Scientific Competition

Edited by
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and STEFAN VOIGT

Conferences on New Political Economy
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Introduction

by

Max Albert

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

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[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).

decision making. Markets use the price mechanism. Open science uses a sophisticated version of the voluntary contributions mechanism based on competition for status.

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

[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.

selection mechanism that tries to deal with the scarcity of funds or of attention. The primary selection mechanism – selection of inputs for further research – could work without peer review, although possibly less efficiently.

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 ministerial control – the so-called "System Althoff", named after the responsible civil servant. With the help of a network of personal contacts, Althoff extracted the information circulating in the scientific community and 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 overexploitation. 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).

into account. The EU commission (2003, 2005), for instance, never mentions scientific competition, under this or a different name. This is like reforming capitalism and forgetting about the price mechanism. It is hard to believe that 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, proxied by the number of habilitations. He analyzes the differences in the relative number of professors, their determinants and the resulting cross border student migration for the German federal states.

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 market. Royalty stacking is a result of two distinct mechanisms, one proprietary, the other contractual. The proprietary mechanism is rooted in the expansion of patents into the traditional domain of open science. The contractual 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.

The volume concludes with two papers devoted to collective decision making in science. Jesu's Zamorra Bonilla applies the perspective of constitutional political economy to methodological rules in science. Combining 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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Job Market Effects on Scientific Productivity*

by

PAULA STEPHAN

1 Introduction

Much of the discussion in science policy circles today focuses on the question of whether the production of basic knowledge is threatened by a shift of emphasis in the public sector towards facilitating technology transfer. There are at least two variants of the crowding-out hypothesis. One variant argues that in the changing university culture scientists and engineers increasingly choose to allocate their time to research of a more applied as opposed to basic nature.[1] Another variant of the crowding-out hypothesis is that the lure of economic rewards encourages scientists and engineers (and the universities where they work) to seek intellectual property (IP) protection for their research results, eschewing (or postponing) publication, and more generally to behave more secretively than in the past.[2] Much of the work of Blumenthal and his collaborators (1996) focuses on the latter issue in the life sciences, examining the degree to which university researchers receive support from industry and how this relates to publication. A related concern is that the granting of intellectual property can hinder the ability of other researchers to build on a given piece of knowledge. This anti-commons hypothesis, articulated by Heller and Eisenberg (1998) and David (2001), postulates that the assignment of intellectual property rights discourages the use of knowledge by other researchers.

How changing property rights in science affect the production of new knowledge is clearly of great relevance to the future of scientific productivity. But there are other reasons to be concerned about the production of scientific knowledge. This paper focuses on these. To wit: who will do science? Will they work in an environment conducive to doing research? The premise of the paper is that researchers' productivity is affected by the environment in which they work and the conditions of their employment. For example, access to

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[1] The model examined by Jensen and Thursby (2003) suggests that a changing reward structure may not alter the research agenda of faculty specializing in basic research.

[2] Clearly, these two variants are not mutually exclusive.

equipment and colleagues clearly affect productivity. Productivity is further enhanced by researchers' having a certain amount of autonomy. Moreover, a research horizon, facilitated by job security or funding security, encourages scientists to choose more risky projects than they might otherwise choose. And it doesn't hurt if scientists work in such environments when they are young. Research consistently finds evidence of a relationship between age and productivity (Levin and Stephan 1991, Stephan and Levin 1992 and 1993, Jones 2005, Turner and Mairesse 2005). For what we might call journeymen scientists, the relationship is not pronounced. But for prize-winning research, there is considerable evidence of a strong relationship (Stephan and Levin 1993). While it does not require extraordinary youth to do prize-winning work, the odds decrease markedly by mid-life. Stephan and Levin (1993) report that the median age that Nobel laureates commenced work on the problem for which they won the prize is 36.8 in chemistry; 34.5 in physics and 39.0 in medicine/physiology for the first 92 years that the prize was awarded. For the more recent period, they find that the median age in chemistry is 38.5; in physics it is 36.0 and in physiology/medicine it is 35.0 (Stephan, Levin and Xiao, unpublished data). They conclude (1993, 397) "that regardless of field, the odds of commencing research for which a Nobel Prize is awarded decline dramatically after age 40." Research opportunities for young scientists affect not only the productivity of the current generation of scientists. They also affect the scientific enterprise in years to come, since the supply of new scientists is responsive to the job opportunities and job outcomes that the current generation experiences.

Historically, scientists and engineers received doctoral training with the goal of achieving a research position either at a university or, depending upon the country, a research institute. In some instances, scientists and engineers worked in large industrial research labs, although in the 20th century this pattern was more common in the U.S. than in Europe.

In many western countries today young scientists face problems obtaining research positions that have characteristics conducive to doing good research. Here we discuss problems facing young scientists, drawing examples from the United States, Italy, and Germany. We also discuss factors contributing to the dismal job outlook faced by young scientists today. We focus on those working in the fields of the physical, life and mathematical sciences, as well as engineers, excluding those working in the social sciences from our discussion.

2 Problems facing young scientists

2.1 The situation in the United States

Public sector research in the United States occurs primarily in the university sector, although some public research is produced at Federally Funded Research and Development Centers (FFRDCs) and at national laboratories, such as the National Institutes of Health. Within the university sector, by far the lion's share of research is conducted at what are known as Research One institutions, institutions such as Harvard, MIT, University of Michigan, University of Wisconsin, etc., classified by Carnegie as a "one" based on the amount of research funding that they receive and the number of PhD students that they educate. There is also a long tradition in the United States, as noted above, of scientists and engineers working in large industrial labs. Three noteworthy examples of such labs that flourished during the 20th century were those at Bell, DuPont and IBM.

Graduate students in the U.S. have a strong tradition, albeit the tradition is field dependent, of aspiring to a tenure track position at a research university. A survey of U.S. doctoral students in the fields of chemistry, electrical engineering, computer science, microbiology and physics during the academic year 1993–1994 found that 36% of the respondents aspired to a career at a research university; 41% aspired to a career in industry/government (Fox and Stephan 2001).[3] The preferences vary considerably by field; in microbiology and in physics more than 50% of the men preferred academic research positions as did 40% of the women surveyed. In chemistry and electrical engineering, which have a long tradition in the United States of employment in industry, a substantially lower percent prefer research positions in academe compared to research positions in industry or government.

The university sector in the United States has been characterized by a tenure system that determines, within a period of no more than seven years, whether an individual has the option to remain at the institution or is forced to seek employment elsewhere (Stephan and Levin 2002, 419). If the individual receives tenure, s/he is promoted to the rank of associate and subsequently full professor if the research record continues to merit promotion. Prior to being hired as an assistant professor it has become increasingly common to take a postdoctoral position.

The importance of tenure makes it crucial for young scientists to signal to older colleagues that they have the "right stuff" for doing research. A nec-

[3] The mail survey was administered by Fox to a national sample of 3800 doctoral students. The response rate was 62%. Respondents were asked "After receipt of your PhD, do you prefer to pursue an academic or nonacademic (industrial, government) career? The response categories were: (1) "academic with emphasis upon research;" (2) "academic with emphasis upon teaching;" and (3) "nonacademic."

essary component of this signal is the ability to establish a lab of one's own. And while startup capital is generally provided by the institution (Ehrenberg, Rizzo and Jakubson 2003), finding the funds necessary to run the lab (not only to buy supplies and equipment but also to hire graduate students, fund postdoctoral positions, and hire technicians) is the responsibility of the individual (Stephan and Levin 2002, 419).

Typically the scientist applies to a research institute of the Federal government for a research grant, although some resources for research come from the private sector (such as the Howard Hughes Medical Institute) and some (and an increasing portion) come from the university itself. In 2001, for example, 59% of the funds for research in the academic sector came from the Federal government; 7.1% came from state and local governments, 6.8% came from industry, 7.4% came from other places and 20% came from universities themselves (National Science Board 2004, chapter 5).

The field that has grown the most rapidly in the United States is that of biomedical sciences. Growth has occurred both in terms of the number of PhDs produced and the amount of funding available for research. For example, PhD production in the slightly broader area of the biological and agricultural sciences grew from 2711 in 1966 to 6798 in 2000 (National Science Foundation 2002). Funding from the National Institutes of Health doubled over a recent five-year period, going from $13.648B in 1998 to $27.181B in 2003.[4] Here we examine the prospects of young PhDs trained in the biomedical sciences in the United States to be hired into a permanent position at a Research One university, as well as their prospects to get funding.

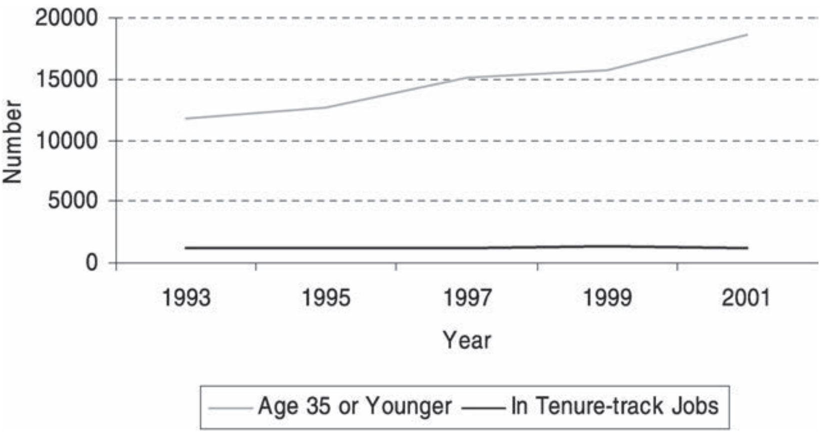
Figure 1 shows the dramatic increase in the number of PhDs age 35 or younger trained in the biomedical sciences in the United States. Data for the figure come from the Survey of Doctorate Recipients (SDR), a biennial survey overseen by Sciences Resources Statistics of the National Science Foundation and drawn from the sampling frame of the Survey of Earned Doctorates (SED), a census of all new PhDs in the U.S.[5] We see that the number of PhDs 35 years of age or younger, trained in biomedical sciences in the United States, grew by almost 60% during the short interval of eight years, going from 11,715 to 18,671. We also see that the number of tenure-track positions has grown by only 7% during the same period, going from 1212 to 1294. Thus, the probability that a young person trained in the biomedical sciences in the United States holds a tenure track position has declined con-

[4] http://www.faseb.org/opa/ppp/fed_fund/NIH_funding_trends_4x13x04_files/frame.htm

[5] The SED is administered to all PhD recipients. The SDR is administered to a sample drawn from the SED. The tabulations presented here use weighted data from the SDR.

siderably in recent years, going from 10.3% to 6.9%.[6] When we focus on Research One institutions, we see a similar pattern. We estimate that 618 PhDs age 35 or younger trained in the biomedical sciences held tenure track positions at Research One institutions in 1993 (5.3% of those 35 or younger). Eight years later, 543 (4.4%) held such positions.

Figure 1 Biomedical PhDs Age 35 or Younger in United States



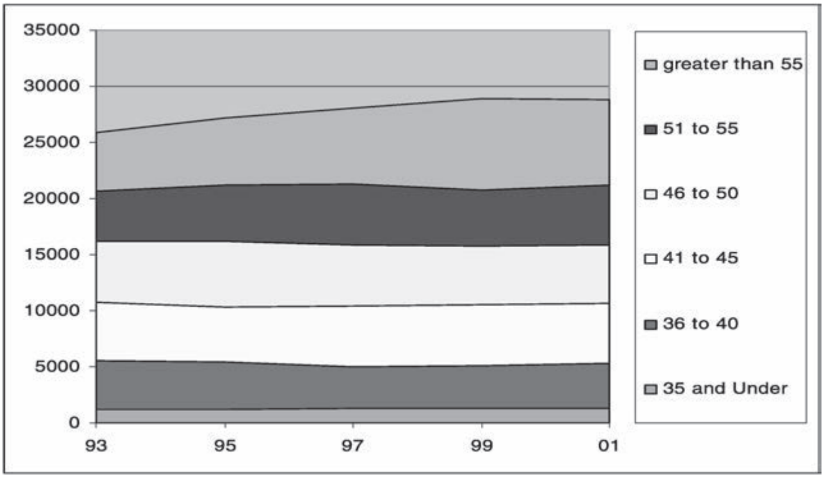
Source: Computations, SDR (see text).

The situation is not limited only to those under 35, as is readily seen in Figure 2 (see p. 16), which shows the number of biomedical PhDs between 36 and 40 in tenure track positions to be almost flat during the period. More generally, the number age 55 and under holding tenure track positions has been fairly constant over the eight-year interval; the only growth has been for those greater than 55 years old.

Not surprisingly, young PhDs trained in the biomedical sciences are having difficulty garnering a first award from the National Institutes of Health, as shown in Figure 3 (see p. 17). While in 1979 NIH made awards to almost 1200 principal investigators (PI's) 35 or younger, by 2003 the number had declined to approximately 200 (National Academies of Science 2005). More generally, the average age at first major independent research support has increased from 37

[6] Increasingly faculty are hired into non-tenure track positions that have the title of assistant, associate or full professor. The number of young individuals holding such positions grew from 389 to 527 in 2001. Including this group with the tenure track group, the probability of being in a faculty rank position has declined from 13.7% to 9.7% during the 1993-2001 period for those 35 and younger.

Figure 2 Tenure Track Biomedical Faculty by Age: United States



Source: Computations, SDR (see text).

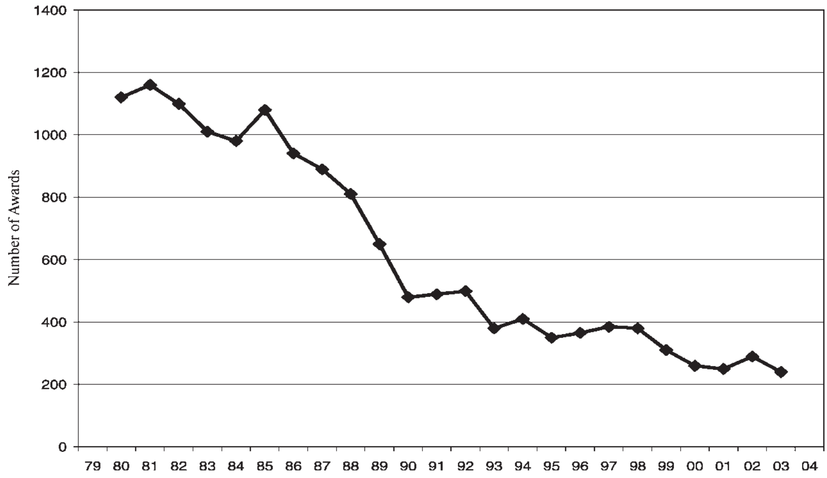
in 1980 to 41.9 in 2002 for PhDs.[7] The decline cannot be attributed to a lack of resources, given the tremendous amount of growth that occurred in the NIH budget during this period. Nor can it be attributed to a decline in supply of young investigators (see Figure 1 p. 15). Neither can it be attributed to the quality of the proposals submitted by those 35 or younger. NIH data indicate that the success rates for new funding are highest for those 35 and younger than for any age group; the second highest success rate is for those 36 to 40. Rather, the decline reflects the older age at which young researchers obtain a first permanent position from which they can apply for funding.[8] The funding situation was of sufficient concern for the National Academies of Science (NAS) to appoint a committee, chaired by Nobel laureate Thomas Cech, to study the issue. Their report, entitled "Bridges to Independence," was issued in 2005.

More generally, the success patterns reflect the changing composition of PhD employment at U.S. universities. Specifically, universities increasingly are hiring more part-time and non-tenure-track faculty; they employ more and more post doctorates and staff scientists. For example, the percent of biomedical PhDs working at universities and employed in non-tenure-track positions grew from 26% to 33% in the eight-year period 1993 to 2002. This

[7] First independent research support consists of either an R01 grant or, in earlier years, an R29 award.

[8] Researchers typically hold a position for two or three years before submitting a grant proposal. One reason for this is that the grant application must show evidence relating to prior results.

Figure 3 National Institute of Health Awards To Those 35 and Under, United States



Source: National Academies of Sciences (2005).

matches a national trend across disciplines and universities. Figure 4 (see p. 18) shows the ratio of full-time non-tenure-track faculty to full-time faculty at Research One institutions (Ehrenberg and Zhang 2005, table 3A.1). The data are displayed for both public and private institutions. In both instances, we see a substantial increase over time. For example, at public institutions, the ratio, which was .245 in 1989, had climbed to .375 by 2001; in private institutions it had started at .312 and eventually increased to .434 by the year 2001.[9]

It should be noted that postdoctoral appointments are usually not included in this data since the postdoctoral position is generally classified as a training position and hence is generally not processed as a hire. During this interval, the number of individuals working in postdoctoral positions has increased dramatically (Ma and Stephan 2005), going from 23,000 in 1991 to 30,000 in 2001.[10] Ma and Stephan find the propensity to take a postdoctoral position to be inversely related to demand for positions in academe. For example, they find the probability to be negatively and significantly related to the per cent change in current fund revenue for institutions of higher education.[11]

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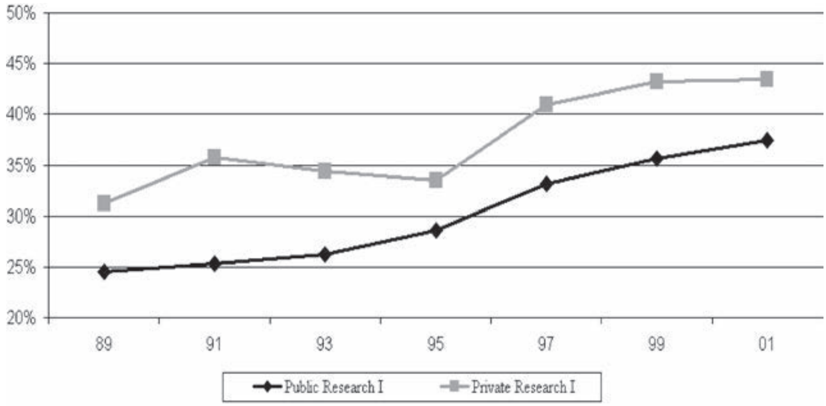
[9] The tabulations are based on data from the biennial IPEDS Fall Staff Surveys.

[10] Richard Freeman (unpublished presentation) estimates that the ratio of post-doctorates to tenured faculty positions in the life sciences went from .54 in 1987 to .77 in 1999, an increase of 43%.

[11] They also find the propensity to be positively related to the size of the PhD's cohort, suggesting that other things equal, as supply of new PhDs increases, recent PhDs are more likely to take postdoctoral positions.

Figure 4 Full-time Non-tenure-track Faculty/Total Full-time Faculty at Research One Institutions: United States



Source: Ehrenberg and Zhang (2005).

Several factors explain these hiring trends. First, cutbacks in public funds and lowered endowment payouts clearly affect hiring. Second, salaries of tenure-track faculty are higher than those of non-tenure-track faculty and research shows (Ehrenberg and Zhang 2005) that this leads to a substitution away from tenure-track positions. Third, funding for non-permanent positions such as staff scientist is available in research grants. The high cost of start-up packages also plays a role in explaining these trends. A survey of start-up packages by Ehrenberg, Rizzo and Jakubson (2003) finds that private Research One institutions spend on average $403,071 on the start-up packages for assistant professors, while public Research One institutions spend on average $308,210. Given these sums, when universities do hire in the tenured ranks, they are tempted to recruit senior faculty away from another university, rather than hire an as yet untested junior faculty member. The financial risk is considerably lower. While the start-up packages are generally higher at the senior ranks, the university gets an immediate transfer of grant money, because the senior faculty generally bring existing research grants with them when they come.

Despite this situation, many young scientists persist in aspiring to a traditional academic career. Geoff Davis's (2005) recent survey of postdocs found that the overwhelming majority of those looking for a job, were "very interested" in working at a research university.[12] While any sample of postdocs is

[12] Davis reports that 1110 of the 2770 respondents indicated that they were looking for a job. Among these, 72.7% were "very interested" in a job at a research university and 23.0% were "somewhat interested."

inherently biased towards those preferring such employment, as the above statistics indicate, the odds that the respondents will achieve a tenure-track position are not good.

The academic labor market in the United States has been characterized by Stephan and Levin (2002) as building upon a series of implicit contracts. Graduate students and postdocs enter a program and provide some "surplus" for the lab through their work as a research assistant or postdoc, and then leave the institution to begin a research career. The professor has an incentive to not cheat on the arrangement. If the student is kept too long, or educated too poorly to be considered employable by a future dean, or provided poor information concerning job outcomes, in theory the professor will cease to be able to attract top graduate students and the source of labor, compensated well below its opportunity cost, will dry up.

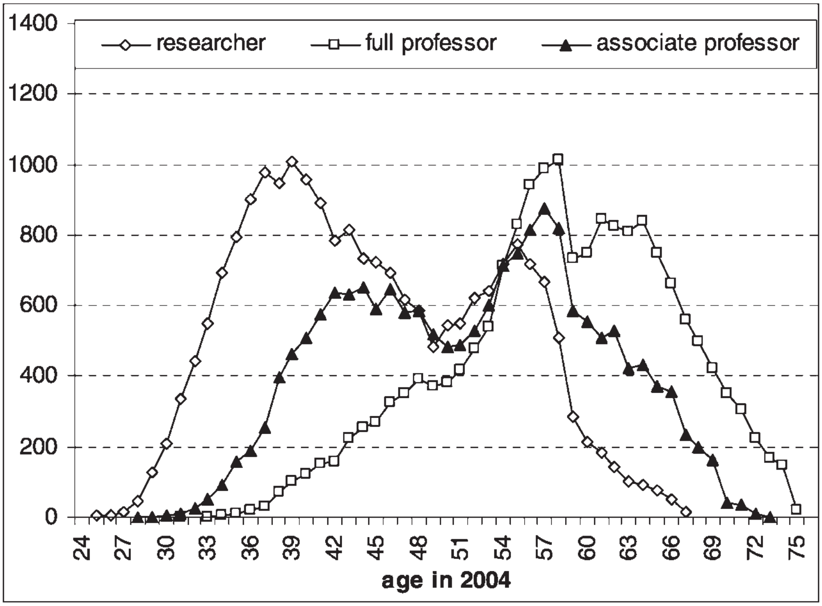
This system, which loosely resembles a pyramid scheme, works reasonably well as long as there is a growing demand for faculty positions. But for this to occur, funding for science must not only grow, but must grow sufficiently fast to absorb the growing workforce of scientists. Such a tremendous growth in resources is something that the U.S. system has been unable to provide, particularly in recent years.

But still the system survives and young scientists continue to be recruited into PhD programs. Stephan and Levine (2002) argue that three factors have allowed it to persist: (1) the demand for college education by the baby boomers in the 1960s and 1970s, which provided fuel for the system to expand; (2) the concept of "postdoctoral study" and (3) the eagerness of foreign nationals to study in the U.S. While the first factor is no longer relevant, the second and third are. The postdoctoral position provides relief for the system in several ways. First, by providing employment opportunities for newly minted PhDs it provides professors an "out" by allowing them to place their students more easily. Second, recipients realize that the postdoctoral position enhances their research record and thus permits them to signal their research capabilities. Finally, and perhaps unwittingly, it diffuses the role that placement plays in recruiting students to study. If applicants to graduate school inquire about job placements in academe, they can be told that academe no longer recruits faculty directly from PhD programs, but instead, only considers applicants with postdoctoral experience. The professor is, so to speak, "off the hook". The large presence of foreign nationals diffuses even more the role that placement plays. Rarely do foreign nationals applying to graduate school inquire about job prospects. In an international context, their prospects are significantly higher as a result of studying in the U.S. than they would be if they were not to study in the U.S. Thus, many of the self-correcting mechanisms that might otherwise result have failed to take place.[13]

2.2 The situation in Italy

Public sector research in Italy occurs in the university sector and at public research institutions (PRIs). Within the PRI sector, the National Research Council (CNR) employs approximately 80% of all PRI researchers.[14] Tenured positions at universities exist at three levels: researcher, associate professor and full professor. Universities also employ contract researchers as temporary employees. Researchers at CNR are hired either into temporary contract positions or into tenured positions (Ricercatore or Primo Ricercatore).

Figure 5 Age of Tenured Academics in 2004: Italy

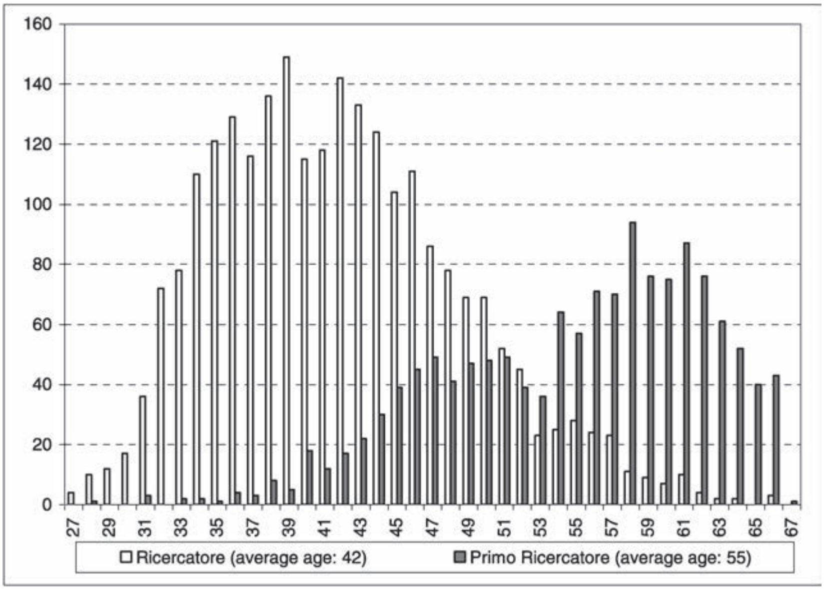


Source: MUIR (Ministry of Italy for University and Research): http://www.miur.it/scripts/visione_docenti/vdocenti0.asp

[13] U.S. students, as opposed to international students, increasingly find careers in science and engineering to be not to their liking. Considerable concern has been expressed in policy circles regarding this decline in interest.

[14] The other public research institutions in Italy are the National Institute of Nuclear Physics (INFN) and the National Institute of Heath (ISS).

Figure 6 Age Distribution of CNR Tenured Researchers in 2004: Italy



Source: National Research Council of Italy.

The job prospects of young PhDs within the university sector have been bleak in recent years and in 2003 a "no new permanent position" policy went into effect. This has resulted in a situation in which the share of temporary researchers at universities has reached 50% in some instances, with young people being heavily concentrated in temporary positions (Avveduto 2005). Figure 5 shows the age distribution for faculty holding tenured positions at Italian universities in 2004. The average age of researchers is 45; those in associate professor positions is 51.7 and those in full professor positions is 58. What is not shown, but worth noting, is that the average age of researchers has increased by more than two years during the seven-year interval from 1997 to 2004.

The situation is no better within the CNR, where a "no new permanent position" went into effect in 2002. The high number of retirements coupled with the hiring freeze has led to a disproportionate number of young scientists in temporary positions; the share of temporary researchers has grown to over 50% and the average age of the CNR researcher is now above 47. Figure 6 shows the age distribution for CNR researchers in tenured positions. The average for those in the position of Ricercatore is 42; for those in the position of Primo Ricercatore it is 55.[15]

One response to the poor job prospects for young PhDs in Italy has been for young scientists to leave the country to find employment. A 2002 CENSIS survey of 1996 Italian researchers working abroad found that the common reason for leaving Italy is lack of access to and progression in a career in the Italian scientific environment.

2.3 The situation in Germany

The article by Schulze (2008) in this book points to the softness of the academic labor market in Germany. For example, figure 1 of his chapter shows that the number of professors at German universities peaked in 1993 at about 23,000 and has been, with few exceptions, steadily declining ever since. In 2004, the last year for which he reports data, the number stood at just slightly over 21,000. The decline is not due to a decline in the number of students. The author shows that during the same period the number of high school graduates increased significantly. He calculates that the ratio of professors per 100 high school graduates "has deteriorated significantly from 11.26 in 1996 to 9.43 in 2004" (section 3.2).

The decline has come at the same time that the number of Habilitationen, a requirement for obtaining an appointment as a professor at most institutions and in most fields, has grown dramatically.[16] To wit, since 1992, when approximately 1300 Habilitationen were produced annually, the number had grown by 2004 to approximately 2200 per year. In terms of Habilitationen per 100 professors, there has been more than a 66% increase during the period.[17] Using a back of the envelope type of calculation, Schulze (2008) estimates that the ratio of new applications to job openings rose from roughly 3/2 to 5/2 during the 14-year period that he analyzes.

It is not only that the job prospects for individuals who have recently received their Habilitationen are poor at German universities. It is also the case that, if and when they do receive a permanent position and the research

[15] The average age of tenured new hires at CNR has increased from 30 to 35 since the late 1980s; the average age of non-tenured new hires is 33.6.

[16] The typical academic career path in Germany involves preparing the Habilitation. After completion, and pending availability of a position, one is hired into a C3 or C4 (now W2 or W3) position which must be at an institution other than where the Habilitation was prepared.

[17] The situation is reminiscent of that in the U.S. with post docs. While the number of tenure-track faculty positions has grown minimally during the last ten to fifteen years, the ratio of postdocs to faculty has grown dramatically (see footnote 10). The incentive to recruit individuals to prepare the Habilitation is similar to the incentive to recruit graduates to hold a post doc position. Both are cheap and productive.

autonomy that comes with a permanent position, they are around 42 years of age (Mayer 2000). Musselin (2005), in her comparison of French, U.S. and German academic career paths, notes that among the three countries studied the age of obtaining a permanent, tenured, position is oldest in Germany. Moreover, the opportunity to be autonomous has not been possible for young scientists in Germany, since independent untenured positions have not existed for young scientists.

Recently Germany has instituted reforms that could have a significant effect on the academic labor market. Specifically, while heretofore individuals could generally not be appointed to a professorial post until they had obtained the Habilitation,[18] the reforms mean, depending upon the state, that the Habilitation could disappear and the post of junior assistant professor would then be accessible directly after the doctorate. Contracts for the junior professor are for three years and renewable one time.[19] In certain ways, this system resembles that of the United States. However, it will not necessarily follow that being hired into a junior position (and renewed) provides for entrée into the position of professor. This will depend not only upon the quality of one's work (as in the U.S.) but also upon availability of posts at the professor level. While positions can be cut in the United States, it is uncommon for an untenured faculty member who merits promotion to be denied tenure and promotion because the position no longer exists. Rather, the position will persist and can be changed from that of an assistant to that of an associate or full over the course of the scientist's career.

A second reform measure involves a move from the "C" to the "W" system. Although the reform was ostensibly designed to provide for performance-based salary increases, it arguably may not succeed in accomplishing this goal. A major component of the change is the way in which base salaries are negotiated. Under the C system, faculty having a competing job offer could negotiate a higher salary at their home institution. The resulting raise was permanent and included in the base used for the computation of pensions. Under the W system, the base salary has been lowered with the idea that performance-based supplements would be possible. The supplements are in principle for a limited period of time. Only if they have been granted for five or more years do they become permanent, although the latter is subject to negotiation.

The W system has the potential of reducing mobility and penalizing productive faculty since for C4 professors it is almost impossible to obtain a competitive W3 job offer. Moreover, not only is the W salary lower, but by

[18] There are exceptions to the Habilitation requirement. For example, one could submit equivalent academic achievements, such as publications, and in technical universities many professors do not have a Habilitation.

[19] In certain cases junior professors can be tenured if they change universities after completing the Ph.D.

switching to a W position, the professor gives up the moderate increases in salary that accompany the C position. Thus, it is likely that the switch will make employment at German universities less attractive for productive academics and increase the incentives to go abroad.

2.4 The situation elsewhere

This situation is not unique to Italy, Germany, and the United States. In France, for example, restrictions have led to poor job prospects for scientific employment in the public sector, which makes up half of R&D employment (European Commission 2004, 34). The number of contract researchers doubled during the 1990s in the United Kingdom. Most European countries are also experiencing a brain drain. By way of example, 75% of the 15,158 Europeans who received their PhD in the U.S. between 1991 and 2000 indicated that they preferred to stay in the U.S. after the PhD to establish their career. About 50% indicated that they had a firm offer of employment (Science and Technology Indicators 2003, chapter 3).

To summarize, young scientists today in many western countries have difficulty getting the type of research position – one that provides for autonomy and a sufficient time horizon – that they anticipated getting when they began their studies. They end up working for long periods in a postdoctorate fellowship or in temporary positions as staff scientist or contract researcher. If and when they do get a position that provides for autonomy they are older.

This situation has negative effects on scientific productivity. First, and foremost, is the loss in productivity of what the young could have discovered if they had had increased autonomy and a longer horizon. A second effect is the loss in terms of the negative signal such outcomes send to younger people that science may not be a choice career. To quote Michael Teitelbaum of the Alfred P. Sloan Foundation (unpublished 2005), “Bad job prospects reinforce lack of interest”. The preface to “Bridges to Independence” makes the case by imagining the year 2029 and a NAS committee assigned to trace the root causes of the U.S.’s fall from preeminence in biomedical sciences. “It was not difficult for the NAS Committee in 2029 to trace the root causes of the U.S. fall from preeminence in biomedical sciences. American college students had always paid close attention to what their peers had to say: The stories of a decade-long post-baccalaureate training period characterized by long hours and low pay were discouraging enough, but when coupled with the slim chance of advancing to an independent research position before the age of 40, few of the most talented American students were enticed” (National Academy of Science 2005, vii–viii). The European Economic and Social Committee observed with regards to the document “Towards a European Research Area”: “One reason for the current lack of new recruits in science and technology is that a few years ago a very large number of young scientists – even those with excellent qualifications – were unemployed" (European Economic and Social Committee CES 595/2000, 15).[20]

3 Shortage

Despite these facts, it is common for policy groups on both sides of the Atlantic to declare an impending shortage of scientists and engineers. A 2003 report issued by the National Science Board (2003) concluded that "Analyses of current trends (in U.S. science and engineering workforce) indicate serious problems lie ahead that may threaten our long-term prosperity and national security." A 2003 European Commission Communication, "Investing in research: an action plan for Europe" concluded that "Increased investment in research will raise the demand for researchers: about 1.2 million additional research personnel, including 700,000 additional researchers, are deemed necessary to attain the objectives, on top of the expected replacement of the aging workforce in research."

Predictions of shortages exacerbate the problem. Encouraging individuals to enter a career when prospects are poor can have serious longer term consequences. Moreover, such forecasts diminish the credibility of the organization declaring the shortage, as the National Science Foundation learned all too painfully in the 1980s.

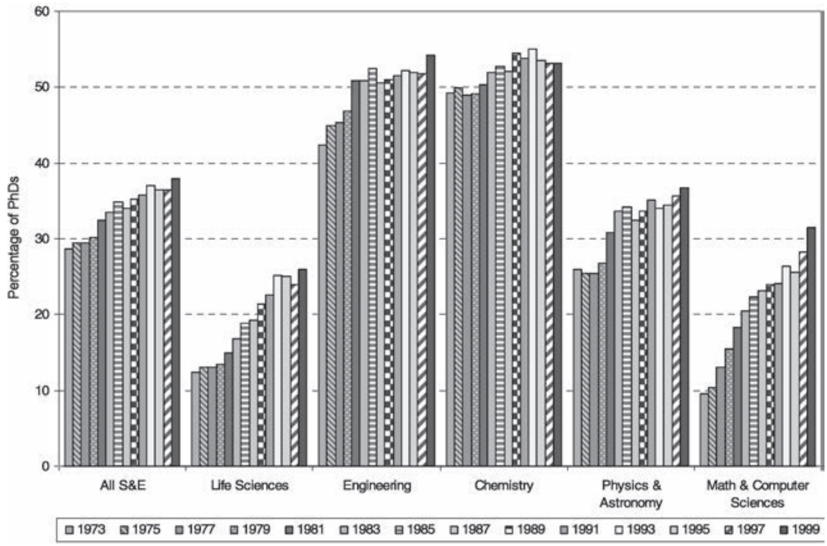
4 Positions in industry

In recent years the employment of scientists and engineers in industry has grown rapidly in the United States, as indicated in Figure 7 (see p. 26). In chemistry and engineering more than 50% of all PhDs work in industry and have for a considerable period. Although the percent is considerably lower in math/computer science and the life sciences, it has grown rapidly in recent years, tripling in the case of math and computer science and doubling in the case of the life sciences. Moreover, it would be incorrect to think of these jobs as only concentrated in development work. A considerable amount of fundamental research is performed in industry in the United States. One manifestation of this is that industry authors were listed on approximately 10% of all scientific articles published in the U.S. in 2001 (National Science Board 2004, table 5-40). Many of these articles are coauthored with colleagues in academe.

Employment in industry is a less salient option for European scientists. This is partly due to the lower rate of spending on R&D in Europe. For example,

[20] Referenced European Commission (2004, 34).

*Figure 7 Percent of U.S. PhDs Working in Industry, by Field, 1973–1999**



* For those five or more years since receipt of PhD and 65 or younger.
Source: SDR tabulations (see text).

on average the EU spends approximately 2% of GDP on R&D; 55% of this is performed in industry. By way of contrast, the U.S. spends 2.9% of GDP on R&D; 64% is performed in industry. Japan spends 3.0% on R&D, 74% is performed in industry. Moreover, the prospects for employment growth in industrial R&D in the EU are not encouraging. The consequences relating to the privatization of research labs of state industries is a case in point. Case studies of labs in Italy and France that have recently been privatized suggest that privatization has shifted the research focus of these labs away from the generation of new knowledge in the national interest to creating value for the company and its clients "by emphasizing the assessment and integration of external knowledge" (Munari 2002). Outsourcing of research is also an issue but the outsourcing is not solely directed towards Asia and countries that have a "cost advantage". Table 1 presents data on R&D expenditures of European majority-owned affiliates operating in the United States (Bureau of Economic Analysis data). We see that over a short span of five years the amount spent by Europe (current dollars) has grown by more than 67 percent and over the 10 year period by 150 percent. A good example of the trend is the recent decision of Novartis to relocate its research headquarters to Cambridge, Massachusetts, in order to take advantage of the research synergies in the vicinity of MIT and Harvard universities. When it opens, Novartis will employ

Table 1 R&D Expenditures of Majority Owned European Affiliates in United States (Billions U.S. dollars)

	1992	1997	2002
Germany	1.8	2.9	5.7
U.K.	2.1	3.0	5.5
Other	4.4	6.4	9.5
Total	8.3	12.3	20.7

400 research scientists; its plans call for it to hire an additional 1000 researchers in the next five years.

5 Conclusion

Young scientists today have difficulty getting the research positions they anticipated at the time they began their training. Many end up holding postdoctorate positions for long periods or as staff scientists, contract researchers or adjunct faculty. When they do get a permanent position, they start out at a considerably older age than did their mentors.

There is much angst in western countries today concerning the prospects for economic growth. The role of scientific productivity in economic growth is widely appreciated. From time to time this angst focuses on problems of the supply of scientists, with the argument that economic growth will be jeopardized if supply fails to keep pace with projected demand. Here we have argued that the problem is not a lack of supply. Instead it is weakness in demand. Decreasing budgets and increasing relative costs have led the public sector to hire fewer scientists – especially into permanent positions. Industry, especially in Europe, has been slow to hire scientists and engineers. The future of science is its ability to attract new generations of scientists and to employ them in a research environment that fosters creativity. Unless fundamental problems giving rise to these employment issues are addressed, we risk the possibility of seriously diminishing scientific productivity in the West.

This risk is occurring in the context of growing competition in an increasingly global economy. Non-western nations are aggressively training and hiring scientists and engineers. The number of PhDs awarded in China, for example, increased more than five-fold between 1995 – 2005 (French 2005); that in India and Korea has also grown dramatically. The ability of a country to innovate and grow relates in part to having a scientific workforce that is generating new ideas. Both Europe and the U.S. are educating large numbers of PhDs. Some of these are “native.” Others come as foreign students. Unless Europe and the U.S. provide work environments in which these scientists and engineers can flourish and be productive, they risk losing the scientific edge from which they have historically profited. The public sector needs to examine ways to enhance the hiring of scientists and engineers into positions that provide a productive work environment.[21] Temporary, piecemeal jobs, which have become increasingly the norm in many countries, are not the solution. Research requires a sufficient time horizon and a degree of autonomy. Countries seeking to enhance productivity need to provide such opportunities for scientists when they are young. Age may not be a fever chill, but prize-winning work is rarely begun when scientists are past the age of 40.

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[21] By way of example, rather than directing Federal research funds in the U.S. to the support of temporary positions such as post docs and staff scientists, ways should be explored to allocate some of the funds to more permanent positions at universities.

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Job Market Effects on Scientific Productivity

Comment by

BERND FITZENBERGER*

Stephan analyzes the interaction between job market prospects and scientific productivity in the sciences.[1] She argues that dismal job prospects (will) reduce considerably entry of highly talented young researcher into an academic career. Stephan focusses on the US and she discusses some developments in Italy and Germany.

Being a labor economist, I think this is a very interesting and needed study because it discusses the important relationship between scientific progress and individual job prospects of the researchers. Scientific progress cannot be produced without suitable incentives (career prospects) for the researchers. This is particularly critical for basic research (nobel prize winning research is only the tip of the iceberg) typically not involving immediate commercial returns.

1 Critical Assessment of Analysis for US

Stephan argues that job prospects for PhDs in the sciences have deteriorated tremendously over the recent decade. The implicit contract between PhD and full professors/research universities, involving remuneration of a successful, hard working PhD/assistant professor by eventual tenure in an academic (university) job has not paid off for an increasing share of the PhDs. The increasing supply of completed PhDs in the US has in fact resulted in universities hiring more cheaper postdocs and less more expensive assistant professors on tenure-track positions. These changes threaten the viability of the implicit contract and, in response, Stephan predicts a severe decline in the willingness to do a demanding PhD in the future.

Clearly, at face value, this argument relies on irrational behavior of the recent cohorts of PhDs because their expectations regarding the implicit contract have not been realized on average. Stephan argues that such irrational beliefs could have been reinforced by a culture of gift exchange where post-docs could be lured into believing for a while that they will eventually would get a tenure-track position. Only with delay these postdocs would

* I am grateful for helpful comments by Dominique Demougin, Martin Kolmar, and other conference participants. I thank Marie Waller for excellent research assistance. All errors are my sole responsibility.

[1] Here, I talk about the sciences when Stephan refers to physical, life, and mathematical sciences including engineering.

realize that this expectation will not materialize. As soon as students in the sciences fully realize that these promises are broken, the supply of PhDs will decline considerably.

I am inclined to investigate potential explanations not relying on irrational behavior. For a rational explanation, the difficulty is to explain the strong increase in supply of new PhDs despite the deterioration of the tenure prospects. I will investigate the following two arguments: (1) Increasing supply of foreigners obtaining PhDs in the US. (2) Good job prospects in industry for PhDs.

Both arguments are also discussed by Stephan, though under a different perspective. The first argument is based on the presumption that foreign students still find graduate education in the US very attractive, even if chances for tenure-track positions have deterioated. Foreign students often prefer staying in the US after completion of their PhD because of better job prospects in the US outside of academia compared to job prospects in their home countries. Postdoc positions are a simple way to extend the stay in the US and to find an attractive job. The huge supply of foreign graduate students is likely to fuel basic research in the US by filling the labs with highly educated and motivated postdocs, unless of course incentives to engage in basic research change themselves as Stephan indicates.

Turning to the second argument, even in the early 1990s almost 90% of biomedical PhDs could not get a tenure-track job (Figure 1 in Stephan's paper). Thus, the majority of PhDs must eventually end up in industry jobs which are likely to be quite attractive because these jobs often combine applied academic research with high salaries.[2] This is confirmed by the discussion in section 4 of Stephan's paper. It seems plausible that a large number of biomedical PhDs in the US saw only small chances to end up in a tenure-track position. Instead, they view obtaining a PhD and working in a low-paid postdoc position mainly as an investment for their eventual career in industry.

In the face of an increasing supply of PhDs, it is a rational response of universities to change hiring policies such that young researcher obtain more tempory positions with lower salaries. These changes increase uncertainty among young researchers, which Stephan argues to lower research productivity. This trend is associated with a shift away from basic to more applied (commercialized) research.

A major problem arises nevertheless for the US, as Stephan emphasizes, if excellency and creativity (prize winning research) in basic research require

[2] It is straight forward to develop an economic model of the decision to obtain a PhD where the degree involves two career alternatives: First, PhDs are eligible to apply for a tenure-track position in academia. Second, they might obtain a well paid research job in industry. Ceteris paribus, an increasing number of PhDs can be explained rationally by the second alternative becoming more attractive, even if the first alternative loses in option value.

independence of the researcher at an age below 40. In addition, increasing competition for tenure-track position might have an ambiguous effect on the research effort of young researchers. On one hand, one might speculate that a more competitive environment might increase incentives to do excellent research in order to get one of the rare tenure-track positions. This way the total research output increases when competition for tenure-track positions increases. On the other hand, the return to research effort declines with the number of competitors, as standard tournament theory suggests, because chances to obtain a tenure-track position (that is, the prize in the tournament) declines at a given level of research effort. In light of the declining returns to the tournament for the tenure-track positions, the share of PhD students engaged in this tournament declines and more of them will focus on applied research enhancing their chances for a well paid position in the industry.

Summing up, the increasing supply of PhDs in the sciences in the US by itself might not reflect irrational behavior but rather the immigration of excellent young researchers to the US and the good job prospects of PhDs in industry. It is not clear that these two effects are going to lose importance in the near future. Thus the only concern might be that young US citizens enroll to a lesser extent in PhD programs. However, the effect of the increasing supply of PhDs on total output in basic research (what is the research production function?) is ambiguous. A related open question is whether the top PhDs still strive and obtain the tenure-track positions allowing them to do basic research. Thus, I am not convinced that the amount of basic research will decline dramatically.

2 The situation in Italy and Germany

I think that the situation in Italy and in Germany is very different from the US and therefore, Italy and Germany can not be used as further examples for the arguments put forward for the US. According to Stephan, Italy has turned into a closed shop with basically no (!) hiring of researchers into tenure-track positions in the sciences. Here, the job propects are clearly so bad that excellent Italian researchers tend to leave the country (e.g. for the US).

The remainder of this section focusses on the situation in Germany. Stephan addresses first the fact that the ratio between habilitations[3] and the number of professorships increased considerably between 1992 and 2004 from roughly 3/2 to 5/2 (referring to the numbers in the paper by Schulze and Warning in this volume). However, one should be aware that this might be a cohort effect because a disproportionately large number of older professors are due to

[3] Traditionally, completing a habilitation was a formal requirement to be considered for a tenured professorship.

retire between 2000 and 2010. Nevertheless, it is likely that prospects to obtain a tenured position have deteriorated over the last 15 years. In the early 1990s, many professorships had to be refilled in East Germany. Nowadays, budgets are very tight and a number of professorships are cut or will be cut by the government.

I have run a small exploratory survey about the job prospects among six young researchers in economics and sociology in Germany (for the sake of brevity, I can not report the detailed results here – it goes without saying, that six responses are not sufficient for statistically valid results).

The following answer in the survey:

What I find worrisome is the current "overproduction" of young researchers due to the promotion of graduate programs and also of post-docs. Combined with a probable decrease in tenured positions and an increased net import of researchers, this may force many of my generation to drop out of academia ... confirms at first glance Stephan's point that job prospects have deterioated in Germany in a similar way as in the US. There is, however, a major difference between Germany and the US. In Germany, the average age after completion of the habilitation is above 40. At this age, it is much more difficult to start an alternative career in industry compared to a postdoc a couple of years after completion of the PhD in the US.

In principle, the introduction of the junior professur without the requirement of a habilitation as the equivalent of the assistant professur should lead to more independence of young researchers. The time limit imposed by the German government should lead to earlier transitions to tenured positions. However, in contrast to the US system, junior professors typically do not have a tenure-track position.

The change of the salary system from the C-system to the W-system involves a considerable decline of the base salary and flexible increases of the salary based on performance. However, upward salary flexibility is severely limited by tight budgets, in fact rendering the new pay system less attractive, especially for those who start their academic career under the new system.

In the short run, again as a cohort effect, the introduction of the W-system might improve job prospects of young researchers as indicated by the following answer in my survey:

Due to the changes in the salary system, the competition from tenured professors from inside Germany is reduced.

This is because established professors find it less attractive to change jobs under the new W-system.

Overall, as Stephan concludes, an academic career in Germany is likely to become less attractive because of the decline in salaries. The positive incentive effects of the new W-system can only work if universities have sufficient resources to honor performance and if the junior professor becomes a true tenure-track position.

Tertiary Education in a Federal System: The Case of Germany*

by

GÜNTHER G. SCHULZE

1 Introduction

In Germany, the responsibility for education rests with the German states, the *Länder*. While the primary and secondary education is largely a regional issue – pupils and teachers do not move across state boundaries in order to exploit differences in educational quality – this is clearly not so for tertiary education. For many fields students are free to move to universities outside the state in which they received their high school diploma. Likewise Ph.D. students seek jobs at universities that best meet their intellectual interests and are conducive for furthering their career. High mobility of high-school graduates, students, university graduates, and doctoral students across states in the presence of decentralized and almost free service provision raises important policy issues. A decentralized education system may well lead to externalities and inefficiencies resulting in suboptimal educational investments by the states, which may either be too low or too high.

Students may receive a free education in one state and subsequently move to a different state where they find employment, pay taxes and increase the local GDP thereby free-riding on educational services of the educating state. If this were a random phenomenon, these in kind transfers between states would cancel out. However, if students react systematically to differences in educational capacities (which may translate into different quality levels) in their choice of university there may be an incentive for states to free-ride on the educational services provided by other states. This incentive could lead to an underprovision of public education provided that the decision where to locate after graduating from university was independent from the decision where to study. This assumption, however, is a stark one. There is evidence that universities produce regional spillovers which create employment in the region and raise regional human capital and GDP (Stephan 1996).

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If these regional spillovers provide an incentive for students to stay in the region after completion of their studies, states may have an incentive to attract the most brilliant minds in order to enhance their pool of high-skilled workers. States find themselves in a different situation of strategic interaction that may result in an overinvestment in educational capacities as educational quality is the instrument to attract out of state high school graduates. In that case the attracting states free ride on the primary and secondary education provided by the home states of migrating students.[1]

Thus the question suggests itself to what extent a federal system of free tertiary education such as the German one gives rise to external effects and strategic behavior either through competitive overinvestment or free riding behavior. This is the concern of this paper.

I model state governments' decision to provide tertiary education in a simple model of endogenous human capital formation. I consider three factors of production: Labor is interregionally immobile and inelasticly supplied. Human capital is produced through the education system and thus a consequence of a political decision. It is mobile across state boundaries but not internationally. This dichotomy of mobility reflects in a simple way the well established notion that mobility increases with educational attainment (e.g., Greenwood 1997, Chiswick 2000, Hunt 2000). Mobility occurs at two stages – after high school individuals decide where to study and after university graduation they decide where to work. At both stages there is *some* inertia – students have a preference to study near home but base their decision also on relative educational qualities, and university graduates' probability to stay in the state where they graduated is larger than the share of employment that this state provides. Lastly, capital is mobile internationally and thus the domestically installed capital stock depends on the amount of labor and human capital in the state. This reflects the observation that each German state is a small open economy with an endogenous capital stock and that the availability of labor and human capital is an important location factor for investment (Burgess and Venables 2004 for a survey). Thus the decision on human capital formation impinges upon interregional capital allocation as well.

In the empirical section of the paper I look for indications of externalities. In particular, I seek to establish whether there are significant differences across states in the level of educational quality as measured by the number of professors per 100 high school graduates and analyze to what extent these differences provide incentives to students to move to a state with better educational capacities.

[1] Likewise states may seek to free ride on the educational services within the university system: They could hire new professors and new PhDs that had been trained elsewhere on a net basis thus saving on education expenses.

The analysis of education provision in the presence of mobility of high-skilled labor dates back to Grubel and Scott (1966) and Bhagwati and Hamada (1974) who analyzed the brain drain from developing to developed countries. Justman and Thisse (1997) show that governments will under-provide education (financed by some immobile factor) if high skilled labor is mobile. Wildasin (2000) shows that the immobile factors have to bear the costs of public education if educated labor becomes mobile, implying a regressive tax system. Südekum (2005) shows in a core-periphery model that educational subsidies in the periphery can miss their regional policy target if increased education leads to higher mobility and therefore stronger migration to the center. Poutvaara and Kanniainen (2000) demonstrate that in the presence of educational spillovers and complementarities between low and high skilled labor, low skilled labor voluntarily subsidizes education. This result, however, breaks down if high skilled labor becomes mobile and moves across state boundaries. It evades high taxes that finance education by moving abroad; similarly immobile uneducated labor seeks to free ride on educational efforts of other states and thus avoid taxes – the public education system breaks down. Poutvaara (2000) analyzes a combination of educational subsidies in the first period with taxation to finance them in the second period and shows that this scheme serves as insurance device against uncertainty in educational productivity. Interregional mobility may ensure against region-specific shocks and thus increase education, tax competition leads to erosion of taxes – welfare effects can go in either direction. All of these papers allow for mobility only *after* education has been completed; in most papers mobility is assumed to be perfect with the only motivation for mobility being differences in net returns. In contrast, I do not assume mobility to be perfect in the above sense, but governed by other considerations as well and I allow mobility of students as well as of high school graduates. Büttner and Schwager (2004) model mobility of high school graduates in Germany's federal system and assume local governments care only about the well-being of their high school graduates (wherever they study or work), some research spillover from universities and the costs of their universities. They have thus an incentive to free ride on the education quality provided by neighboring states as their high school graduates can study there. As a result, investment in universities is suboptimally low. Büttner and Schwager disregard the effect of universities on the regional economy (Stephan 1996) and they assume high skilled labor to be perfectly mobile; thus by assumption a state cannot profit from attracting students. Contrastingly, I model these effects in a three factor stationary state model and show that attracting students increases the remuneration of the immobile factor, attracts capital and raises regional GDP.

The paper is structured as follows: section 2 presents theoretical considerations on the provision of educational services in a federal system. Section 3 provides empirical evidence on the provision of professors in Germany – on the federal level as well as on state level and demonstrates that differences in educational capacities influence students' migration flows across state boundaries. Section 4 provides some concluding remarks.

2 Theoretical Considerations

In a federal system the provision of education may give rise to external effects at various levels as states may import educational services from other member states on a net basis. If states attract high school graduates from other states (for instance by providing a better university infrastructure and quality) *and* the attracted people remain in their new states after graduating from university these states effectively free-ride on primary and secondary education provided by other states. If however university graduates locate in a different state after completing their education, the receiving state free rides on tertiary education provided by another state (and possibly also on primary and secondary educational services). In other words, interstate mobility of students and graduates produce externalities if education is subsidized or even free and thus gives rise to potentially severe inefficiencies.

This situation is described by strategic interaction of states seeking to attract human capital without fully paying for its production at the expense of other states. They do so by providing a high quality university system that promises a good education with high returns. I model the states' calculus to provide tertiary education in the presence of (limited) mobility of students in a game-theoretic model of two jurisdictions in the steady state, which takes into account that capital is mobile international and that human capital will not only attract foreign direct investment. Thereby I am able to model international repercussions of competitive human capital formation in a federal system.

The model proceeds in three steps: In the next subsection optimal human capital formation in a small open economy is derived; the second subsection is devoted to modeling the strategic interaction of small open federal states in providing tertiary education and lastly the properties of the ensuing Nash-equilibrium are described.

2.1 Optimal Human Capital Formation in a Small Open Economy

Assume a small open economy that produces with the help of physical and human capital and labor, K, H, L. The neoclassical production function exhibits constant returns to scale and is described by $Y^d = F(K, H, L)$. Y^d denotes the gross *domestic* product. First partial derivatives are positive, second partial derivatives are negative and cross derivatives are positive (e.g.,

$F_{KH} > 0$, $F_{KL} > 0$ etc.). Inada conditions are assumed to hold. Capital is mobile internationally and therefore earns the given world market rate of return r^w , which implicitly determines the amount of capital installed in the economy (K^*), given its human capital and labor endowment:

$$r^w = F_K(K^*(H, L), H, L) \tag{1}$$

Implicit in eq. (1) is the notion that a broad human capital and labor base will attract physical capital as it increases its marginal return – K depends on H and L. Labor is assumed to be immobile. For now I treat human capital as immobile across boundaries, but will relax this assumption in the next subsection. Gross *national* product is given by:

$$Y = F(K^*, H, L) + r^w(K^S - K^*), \tag{2}$$

where K^S denotes the physical capital owned by the society and K^* denotes the physical capital installed in the economy. Thus $(K^S - K^*)$ denotes the net capital export.

Optimal human capital formation is analyzed in the steady state because educational investment is long-term by nature. For simplicity I assume labor to be stationary. The steady state condition for capital accumulation requires that savings equal depreciation of the capital stock, i.e., $s\,[F(K^*, H, L) + r^w\,(K^S - K^*)] = \delta\, K^S$. This determines K^S:[2]

$$K^S = \frac{s}{\delta - s\,r^w}\,[F(K^*, H, L) \;-\; r^w K^*] \tag{3}$$

K^S may be larger or smaller than K^*.[3] Unlike physical capital, which is accumulated through (private) saving and investment dynamics, human capital is produced through the public education system. Private schools and universities, although they exist, play a very minor role in Germany. Thus human capital accumulation is the result of a political decision. From eq. (2) the effect of increased human capital on steady state national income is derived as:

$$\frac{\partial Y}{\partial H} = \underbrace{\frac{\partial F}{\partial K^*}\frac{\partial K^*}{\partial H} + \frac{\partial F}{\partial H}}_{\frac{\partial GDP}{\partial H}} + r^w\left(\frac{\partial K^S}{\partial H} - \frac{\partial K^*}{\partial H}\right) = \frac{\partial F}{\partial H} + r^w\,\frac{\partial K^S}{\partial H}, \tag{4}$$

[2] Note that $\delta > s\,r^w$ needs to hold, otherwise a non-degenerate steady state does not exist. Realistic parameter constellations always satisfy this condition.

[3] Whether the economy exports or imports capital depends on its saving rate relative to the world saving rate, its production technology and depreciation rate relative to the world. This is elaborated in detail in appendix 5.1.

where I have made use of the fact that $\partial F/\partial K^* = r^w$. Differentiating (3) w.r.t. H and using the same relationship I obtain $\frac{\partial K^S}{\partial H} = \frac{s}{\delta - s r^w} \frac{\partial F}{\partial H} > 0$. Thus (4) simplifies to

$$\frac{\partial Y}{\partial H} = \frac{\delta}{\delta - s r^w} \frac{\partial F}{\partial H} > 0. \quad (5)$$

The accumulation of human capital has three effects: not only does it increase GDP directly; it also attracts physical capital because it initially raises the return to capital ($F_{KH} > 0$). This portrays the importance of a skilled labor force as a location factor for foreign direct investment. Lastly, the increase in human and physical capital raises the remuneration of the immobile factor: labor. As a consequence GNP and the domestically owned capital stock rise – the society has become more affluent.

The government, however, may not only care about pro growth policies, especially education. The incumbent may want to engage in redistribution, the provision of public goods, which may not produce growth stimuli, or special interest group policies (through transfers or subsidies) in order to maximize political support.[4] Spending resources for education or alternative uses described above and denoted by R, is constrained by the size of the budget. As I want to portray German federal states' optimization calculus I take the tax rate τ as given, because it is set by the federal government.[5] Taking the alternative uses for the budget as composite commodity R and using it as a numéraire the budget constraint reads as $\tau Y = p_H \hat{H} + R$. p_h denotes the (relative) price for the production of new human capital $\hat{H}$ which in the steady state replaces exactly the depreciated human capital $\delta_H H$ (retiring skilled personnel, obsolete technologies etc.), i.e., $\hat{H} = \delta_H H$. The budget constraint is endogenous as it depends on H.

Government's optimization problem may now be formulated as

$$\max V(Y, R) \;\; s.t. \;\; \tau Y = p_H \hat{H} + R, \quad (6)$$

where V is the objective function of the government. Optimality requires that

$$\frac{\partial V/\partial Y}{\partial V/\partial R} = \frac{p_H \delta_H - \tau \partial Y/\partial H}{\partial Y/\partial H}, \quad (7)$$

[4] For a survey of the political economy of redistribution and public goods provision and special interest rate policy see Drazen (2000).

[5] Income and corporate tax rates as well as VAT rates are set at the federal level; states receive a share of the income taxes roughly according to their share of GNP. In other federal systems states have the authority to tax independently of, or in addition to the federal level, such as Switzerland and the US. In these systems tax rates are additional policy parameters for the state governments.

where $\partial Y/\partial H$ is given by eq. (5). The LHS of (7) gives the marginal rate of substitution between Y and R which needs to be equal to the marginal rate of transformation, the RHS of eq. (7). A marginal increase of steady state income through increased human capital stock reduces R by the marginally increased expenses for new human capital necessary to balance human capital depreciation ($p_H\,\delta_H$), minus the increase in budget due to the growth stimulus of enhanced human capital investment ($\tau\,\partial Y/\partial H$). Condition (7) determines implicitly the optimal value of human capital, H^*. Explicit solutions can be derived by specifying functional forms for production and utility functions.

For instance if I specify the utility function as

$$V(Y,R) = (1-\theta)\;Y + \theta\;R \quad \text{with} \quad 0 \le \theta \le 1, \tag{8}$$

eq. (7) may be rewritten by using (5) as

$$\frac{\partial F^*}{\partial H} = \frac{\delta - s\,r^w}{\delta}\,\frac{\theta\,\delta_H\,p_H}{1-\theta\,(1-\tau)}.$$

Furthermore, if I specify the production function as

$$F(K,H,L) = K^{\alpha}H^{\beta}L^{1-\alpha-\beta}, \tag{9}$$

the optimal human capital stock is given by

$$H^* = \left[\frac{\delta - s\,r^w}{\delta}\,\frac{\theta\,\delta_H\,p_H}{1-\theta\,(1-\tau)}\,\beta^{-1}\,K^*(H)^{-\alpha}\,L^{\alpha+\beta-1}\right]^{\frac{1}{\beta-1}}. \tag{10}$$

Yet, K^* is a function of H. If I use the functional form in eq. (9) I can explicitly derive the optimal capital stock, given H, from eq. (1):[6]

$$K^* = \left[\frac{\alpha}{r^w}\,H^{\beta}\,L^{1-\alpha-\beta}\right]^{\frac{1}{1-\alpha}} \tag{11}$$

Plugging (11) into (10) and rearranging yields

$$H^* = \left[\frac{\delta}{\delta - s\,r^w}\;\frac{1-\theta\;(1-\tau)}{\theta\,\delta_H\,p_H}\;\beta\;\left(\frac{\alpha}{r^w}\right)^{\frac{\alpha}{1-\alpha}}\right]^{\frac{1-\alpha}{1-\alpha-\beta}}\;L. \tag{12}$$

This result can be summarized in

Proposition 1: *In a small open economy with internationally mobile capital the optimal amount of human capital is proportional to the labor force; it is lower, the higher the political preference for alternative uses of the budget such*

[6] One can easily show that $\frac{\partial K^*}{\partial H} > 0$, $\frac{\partial^2 K^*}{\partial H^2} < 0$.

as redistribution and special interest policies. It rises with the saving rate and declines with the price of human capital formation and the price of physical capital.

If countries have the same production function and the same depreciation rates of human capital and of physical capital[7] the ratio of optimal human capital stocks in the steady state is given by:

$$\frac{H_1^*}{H_2^*} = \left[\frac{\delta - s_2\, r^w}{\delta - s_1\, r^w}\;\frac{(1-\theta_1\,(1-\tau))\,\theta_2}{(1-\theta_2\,(1-\tau))\,\theta_1}\right]^{\frac{1-\alpha}{1-\alpha-\beta}}\frac{L_1}{L_2} \tag{13}$$

This gives us

Lemma 1: *For two small open economies 1 and 2, not linked by human capital or labor mobility, the ratio of optimal human capitals, H_1^*/H_2^*, rises with the ratio of savings rates, s_1/s_2, and the relative population L_1/L_2 and declines with the relative preference for alternative uses of public funds θ_1/θ_2. For equal savings rates and preferences the per capita human capital stock is equal across states, which implies that for identical linear homogenous educational technologies the number of professors per capita is equal across states.*

Lemma 1 has been derived for small open economies assuming that there is no strategic interaction in the education market. Yet, in a federal system human capital is formed not by a unitary state, but by many member states which compete for high skilled labor. This situation of strategic interaction is produced by different degrees of factor mobility – labor is interregional immobile and thus its remuneration is determined by the employment of the other factors; capital is internationally mobile making each member state of the union a small open economy and a price taker in the international capital market. Human capital is assumed to be interregionally mobile, but internationally immobile; its production is determined by interdependent political decisions of a few state governments to provide university capacities. States may seek to attract human capital formed by other states thereby free-riding on human capital investments of other member states. Thus states' optimization calculus needs to take into account the mobility of individuals with high skills and those that seek a higher education.

[7] These are very reasonable assumptions for integrated markets as technology transfer should ensure the same – optimal – technology in both countries. Integrated factor markets for professors and teachers should ensure equal production costs for human capital. Indeed, in Germany professors' wages have been set on the federal level by law ('Hochschulrahmengesetz') and through agreements of ministers of science and culture ('Kultusministerkonferenz').

2.2 Competition for Human Capital in a Federal System

2.2.1 Students' location choice

High school pupils reside where their parents locate; this decision is determined by job market and other considerations of the parents but not by differences in educational quality across states. Interstate mobility of people for educational purposes occurs only after they have graduated from high school. High school graduates may take up their university studies in their home state or in a different state, depending on the relative quality of the education system, and they may seek work in the state of their university education, or in some other state. Interstate mobility leads to externalities in provision of education by states and to strategic interaction in the education sector.[8]

In order to understand the nature of the strategic interaction on the educational market I need to analyze the decision of students where to study as well as the decision of university graduates where to seek employment. These decision parameters will be taken into account in optimization calculus of state governments which produce human capital and compete for it with other states.

Since I focus on tertiary education I assume that the number of high school graduates ('*Abiturienten*') is given for each state, i.e., $HG_i = \overline{HG_i}\ \forall i$. Without loss of generality I assume that each high school graduate wants to study and that there is no capacity constraint on the federal level.[9] I confine our analysis to two states.

High school graduates of state i may either study in their home state or in the other state. Students have a bias for studying in their home state as this may reduce costs – they might still live with their parents – and preserves their established social contexts.[10] Yet they base their decision also on the relative educational capacities of both states, which I proxy by the relative number of

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[8] If they study and work in a state different from the state in which they received their primary and secondary education, the state they work in free rides on the educational services provided by the state in which they went to school. If they return to their home state after graduating from school the home state free rides on the tertiary education provided by the state in which they went to university.

[9] In reality, not all high school graduates study at the university or technical college ('Fachhochschule'). In 2000 only 78.3% of all high school graduates ('Abiturienten' and 'Fachabiturienten') had enrolled in a university, technical college and similar institutions; most of whom enrolled in the same or the following year after graduation (Statistisches Bundesamt, Fachserie 11, Reihe 4.3.1). The analysis could be easily adjusted for a transfer rate smaller than one.

[10] About two-thirds of prospective students prefer to study at the university that is close to their parents home (Kultusministerkonferenz 2002). Therefore, there is a "home bias" which induces students to study in the same state where they graduated from high-school.

professors.[11] The number of professors in a state is an indicator for the variety of subjects offered and the specialities within the subjects and thus for the probability to study the most preferred subject. Moreover, given the less than perfect mobility of students, the relative number of professors determines the student-professor ratio, which in turn determines the quality of teaching and the possibility to participate in research.[12] Thus the number P_i of professors is the strategic variable of state governments' to attract students.

The number of new students in state i, S_i $(i=1,2)$ is given by

$$S_1 = a_1 HG_1 + (1-a_2) HG_2 \text{ and } S_2 = a_2 HG_2 + (1-a_1) HG_1, \quad (14)$$

where $a_i \leq 1$ denotes the share of high school residents that study in their home state. It is determined by the relative capacity of the state as well as the home bias $b_i \geq 1$ of the students in that state:[13]

$$a_1 = b_1 \frac{P_1}{P_1 + P_2} \quad \text{and} \quad a_2 = b_2 \frac{P_2}{P_1 + P_2} \quad (15)$$

If there were no home bias (i.e., $b_1 = b_2 = 1$), students would simply allocate themselves according to relative capacity regardless where they received their high school diploma: $S_i = \frac{P_i}{P_1 + P_2}(HG_1 + HG_2)$. With a home bias they may trade off better study conditions away from home against being close to home. For that reason also the number of high school graduates in a state matters for the number of students in that state; without home bias only the relative number of professors would matter.

For simplicity, and without loss of generality, I assume that every student graduates from university. Upon completion of their studies a share of students ρ decides to seek a job in the state i where they graduated. The remainder of the graduates have no regional preference and seek jobs in state i according to the relative prospects of finding a job which is proxied by the share of state i in the federal GDP: $\gamma_i = \frac{Y_i^d}{Y_1^d + Y_2^d}$.

[11] Of course, temporary staff including post docs and temporary researchers, equipment, buildings and student housing are important as well for students' location decision. I assume differences in these factors follow differences in the professor-student ratio which I consider the most important factor determining the quality of teaching and research.

[12] If students were perfectly mobile in the sense that differences in the student-professor ratios were the only argument for moving, these ratios would be equal in equilibrium. In section 3.4 I provide evidence on students' mobility being influenced by differences in university capacity and thus in quality. For evidence on student home bias and quality as determinants of migration decision see also Büttner et al. (2003).

[13] Since $a \leq 1$,we assume that b is small enough not to violate the restriction that $b_i \frac{P_i}{P_1 + P_2} \leq 1$.

The number of new university graduates employed in state *i*, denoted by UG_i, is given by

$$UG_i = \rho S_i + \gamma_i \, (1-\rho) \, (S_1 + S_2) \,, \quad i = 1,2. \tag{16}$$

This formulation portrays a situation where employment of university graduates is not completely demand determined (according to the value of γ_i), but in which universities can have large spillovers for the regional economy in *creating* high-skilled jobs, a prime example being Stanford University's impact on the 'silicon valley'.[14]

2.2.2 State governments' optimization

Governments maximize their utility (eq. 8) subject to the budget constraint which is endogenous to the decision how much of the funds to allocate for consumption and redistributive purposes and how much to invest into human capital formation. Since I focus on tertiary education the relevant policy instrument is the number of professors that a state employs in the steady state, P_i.[15] As the market for professors is integrated and the salary schemes are the same for all states the annual price for a professor (including equipment, support staff, and researchers in the research unit), p_P , is the same for all states. State governments' optimization problem can be re-stated as

$$\max_{P_i} \; V_i(Y_i(H_i), R_i) \;\; s.t. \;\; \tau \, Y_i(H_i) = p_P \, P_i + R_i \,. \tag{17}$$

The gross national product of a state *i*, Y_i , depends on human capital production as shown in section 2.1, which in turn depends on how many university graduates a state is able to attract (eq. 16). In the steady state $UG_i = \delta_H \, H_i$. The number of university graduates however depends on how many students a state educated – eqs. (14) and (16) – which is a function of the *relative* number of professors that a state employs (eq. 15). The utility function can be rewritten as

$$\begin{aligned} V_i(Y_i, R_i) &= (1-\theta_i) \, Y_i + \theta_i R_i = (1-\theta_i) \, Y_i + \theta_i(\tau \, Y_i - p_P \, P_i) \\ &= [1 - \theta_i(1-\tau)] \, Y_i - \theta_i \, p_P \, P_i \,, \end{aligned}$$

where I have used the budget constraint. As the policy variable is the *absolute* number of professors the first order condition reads as

$$\frac{\partial V_i}{\partial P_i} = [1 - \theta_i(1-\tau)] \, \frac{\partial Y_i}{\partial H_i} \, \frac{\partial H_i}{\partial UG_i} \, \frac{\partial UG_i}{\partial P_i} - \theta_i \, p_P = 0. \tag{18}$$

[14] This assumption reflects the availability of high skilled labor as important location factor for mobile (high-tech) firms.

[15] This implies that the steady state replacement need is given by $\delta_P P_i$, where δ_P denotes the average replacement rate for Professors. Since the average age of obtaining the first tenured job is around 42 and mandatory retirement age is 65, this depreciation rate is about 3 percent per annum.

$\partial Y_i/\partial H_i$ is given by eq. (5), $\partial H_i/\partial UG_i = 1/\delta_H$ in the steady state. The term $\partial UG_i/\partial P_i$ which describes the strategic interaction, still needs to be determined. Plugging eqs. (14) and (15) into (16) and differentiating w.r.t. P_1 yields

$$\frac{\partial UG_1}{\partial P_1} = \rho\,\{b_1\,HG_1 + b_2\,HG_2\}\;\frac{P_2}{(P_1+P_2)^2} + (1-\rho) + \frac{HG_1+HG_2}{[Y_1+Y_2]^2}\left(Y_2\,\frac{\partial Y_1}{\partial P_1} - Y_1\,\frac{\partial Y_2}{\partial P_1}\right).$$

The terms in the last parenthesis contain the term $\partial UG_1/\partial P_1$.[16] Solving (18) for $\partial UG_1/\partial P_1$ gives:

$$\text{(19)}\quad \frac{\partial UG_1}{\partial P_1} = \frac{\rho\,\{b_1\,HG_1 + b_2\,HG_2\}\,\dfrac{P_2}{(P_1+P_2)^2}}{1 - \dfrac{(1-\rho)}{\delta_H}\,\dfrac{HG_1+HG_2}{[Y_1+Y_2]^2}\left(Y_2\,\dfrac{\partial Y_1}{\partial H_1} - Y_1\,\dfrac{\partial Y_2}{\partial H_2}\right)} = -\,\frac{\partial UG_2}{\partial P_1}$$

2.3 Nash-Equilibrium

Now I can derive the first order conditions. Differentiating the state's utility function with respect to the professors that a state employs and setting this expression equal to zero yields the reaction function of that state. From eqs. (18) and (19) I obtain for state 1:

$$[1-\,\theta_1(1-\tau)]\,\frac{\partial Y_1}{\partial H_1}\,\frac{\rho\,\{b_1\,HG_1 + b_2\,HG_2\}\,\dfrac{P_2}{(P_1+P_2)^2}}{\delta_H - \dfrac{(1-\rho)\,(HG_1+HG_2)}{[Y_1+Y_2]^2}\left(Y_2\,\dfrac{\partial Y_1}{\partial H_1} - Y_1\,\dfrac{\partial Y_2}{\partial H_2}\right)} = \theta_1\,p_P \qquad \text{(20)}$$

An analogous expression is obtained for state 2. Solving (20) for P_1 and dividing the equation by the analogous equation for state 2 gives us the ratio of professors in both states:

$$\frac{P_1}{P_2} = \frac{\theta_2\,[1-\theta_1(1-\tau)]}{\theta_1\,[1-\theta_2(1-\tau)]}\;\frac{\partial Y_1/\partial H_1}{\partial Y_2/\partial H_2} = \frac{\theta_2\,[1-\theta_1(1-\tau)]}{\theta_1\,[1-\theta_2(1-\tau)]}\;\frac{\delta - s_2 r^w}{\delta - s_1 r^w}\;\frac{\left(\dfrac{L_1}{H_1}\right)^{\frac{1-\alpha-\beta}{1-\alpha}}}{\left(\dfrac{L_2}{H_2}\right)^{\frac{1-\alpha-\beta}{1-\alpha}}}, \qquad \text{(21)}$$

[16] The last term in parentheses can be restated as $Y_2\dfrac{\partial Y_1}{\partial P_1} - Y_1\,\dfrac{\partial Y_2}{\partial P_1} = \left(Y_2\,\dfrac{\partial Y_1}{\partial H_1}\,\dfrac{1}{\delta_H} + Y_1\,\dfrac{\partial Y_2}{\partial H_2}\,\dfrac{1}{\delta_H}\right)\,\dfrac{\partial UG_1}{\partial P_1}$, where I have made use of $\partial UG_1/\partial P_1 = -\partial UG_2/\partial P_1$.

where for the second equation I have made use of the eq. (5) and the fact that the amount of mobile capital installed at home is given by (11).

This gives us

Proposition 2: *Assume federal states with identical technology which produce with fixed labor endowments, internationally mobile physical capital (the price for which is given) and human capital which is produced by the member states and is mobile across member states. In the Nash equilibrium states will provide the more capacity for tertiary education relative to their competitors, measured by the relative number of professors,*

(I) *the larger their political preference for pro-growth policies relative to consumptive and redistributive use of public funds is relative to that of other states;*

(II) *the larger their saving rates are relative to those of the other member states;*

(III) *the larger their relative population.*

While effects (i) and (ii) are linear in relative preferences and savings rates, effect (iii) is sub linear; that is, ceteris paribus, larger states have a worse human capital endowment per capita.

Proof:

(I) follows directly from differentiating (21) with respect to (θ_1/θ_2). Other things being equal, the smaller the preference for redistribution and consumption of public funds (i.e., the smaller θ) the larger the number of professors relative to its neighbor: Algebraically $\partial\,[(1-\theta(1-\tau))/\theta]/\partial\theta = -\theta^{-2} < 0$.

(II) follows directly from differentiating (21) with respect to (s_1/s_2).

(III) follows from differentiating (21) with respect to $[(L_1/H_1)/(L_2/H_2)]$. Higher relative labor to human capital endowment ratios lead to higher relative numbers of professors and thus to higher numbers of university graduates, other things being equal. However this relationship is sub-linear, that is a larger state (in terms of its population) will have a less than proportionally larger number of professors. This is seen from the exponent of the last term of eq. (21):

$$0 < \frac{1-\alpha-\beta}{1-\alpha} < 1.$$

The last finding starkly contrasts the result of Lemma 1, which states that human capital endowment is proportional to the size of the labor force. In other words, competition between member states for mobile human capital puts larger states at the receiving end.

3 Empirical Analysis

This section first portrays overall trends in the provision of professors over time and points out differences between East and West Germany. It then studies the distribution of professors across the 16 German '*Länder*' in order to see whether the observed pattern is consistent with the theoretical predictions of the model. Lastly, it provides evidence for one of the model's central assumptions that differences in the number of professors translate into migration flows of students across states.

The tertiary education in Germany has basically three tiers: (1) the universities, (2) technical and other colleges ('*Fachhochschulen*')[17], (3) vocational and technical schools ('Fachschulen') and universities of cooperative education ('Berufsakademien') and comparable institutions at each level. I focus on professors at the highest level who needed until very recently a *habilitation* in order to become a professor.[18] This kind of "Super-PhD" required writing a book significantly more comprehensive than a PhD dissertation and passing an oral exam; it typically took at least as long as a normal PhD.[19] The custom was that candidates could not get their first tenured position at the university where they had received their habilitation. The second and third tier positions do not require a habilitation and are much more applied in their academic approach.

I thus look at professors of all fields that are employed full time at a university, a technical university, or a pedagogic university.[20] I exclude professors that typically required no habilitation (or equivalent scientific output) for their appointment; in particular, those at technical and other colleges ('Fachhochschulen') and colleges of art ('Kunsthochschulen'). The professors I look at are almost always tenured;[21] I exclude assistant professors ('Juniorprofessoren') which were introduced only recently and are overwhelmingly non-tenured. All data sources are detailed in appendix 5.2.1.

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[17] They often refer to themselves as "universities of applied sciences".

[18] Habilitation was not an indispensable requirement in order to receive a professorship, equivalent scientific achievements could substitute for the habilitation. However the vast majority of university professors held a habilitation. The only notable exception is professors of engineering, many of whom do not have a habilitation.

[19] Now Germany moves into the direction of the Anglo-Saxon system with non-tenured assistant professors, while the possibility of writing a habilitation and being part of a research team under the supervision of a tenured professor still coexists (and arguably still is the dominant form of preparation for the tenure decision). Recently the cumulative habilitation, a collection of papers, has become popular.

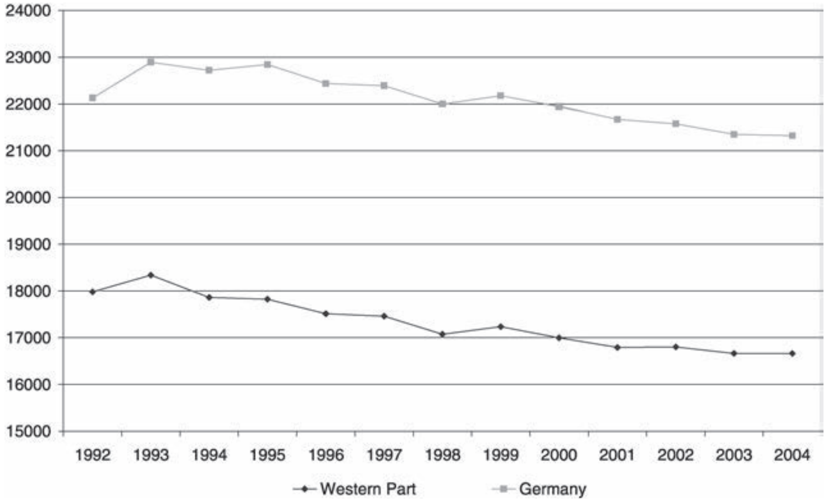
[20] *Pädagogische Hochschule*, which existed in Thüringen and Sachsen-Anhalt until 1992, in Schleswig-Holstein until 1993, and still exists only in Baden-Württemberg. My sample includes the Catholic University Eichstätt and the two universities of the armed forces ("*Bundeswehrhochschulen*").

[21] These are professors of the salary bracket C2, C3, C4 and after introduction of the new classification in 2004/5 W2, W3. Very few of these positions are non-tenured.

3.1 Trends in the Number of Professors and Habilitations at the Federal Level

Overall, the number of professors shows a clear downward trend starting in 1993, when Germany had 22,892 professors; in 2004 Germany had only 21,323 professors. That is a reduction of seven percent in twelve years. This is shown in Figure 1.

Figure 1 Professors in Germany, 1992 – 2004

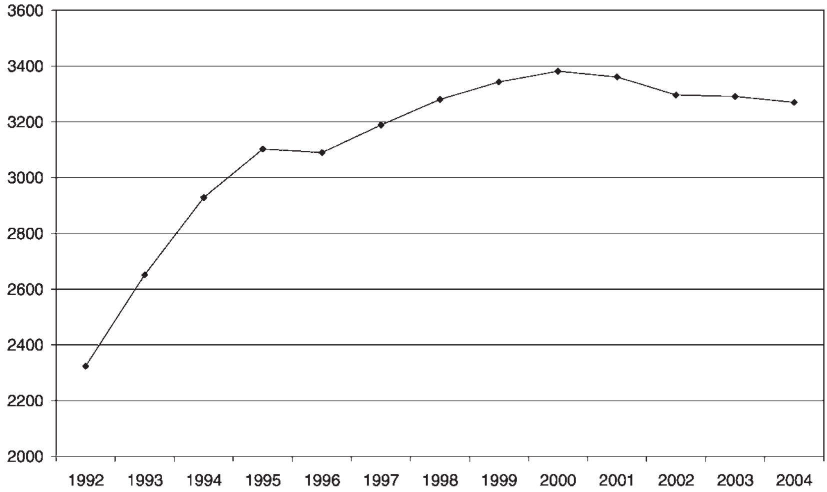


The development in the new 'Länder' runs somewhat counter to the overall trend. Due to the reunification of Germany in 1990 and the ensuing layoff of East German professors hired by the GDR and the restructuring of universities in the East, new professors needed to be hired in order to re-create existing universities. Thus the number of professors rose in the East and leveled off only in 2000 (Figure 2, see p. 50). Yet the overall trend is only mildly affected by that as the share of Professors in the new 'Länder' is less than 20 percent.[22]

Even though the restructuring of the East German universities may have opened up a time window of exceptional opportunities for new professors between 1992 and 1995, the overall trend in professorships continued to deteriorate for those seeking a career in academia. This, however, is not reflected in the number of habilitations, which continued to rise from 1311 in

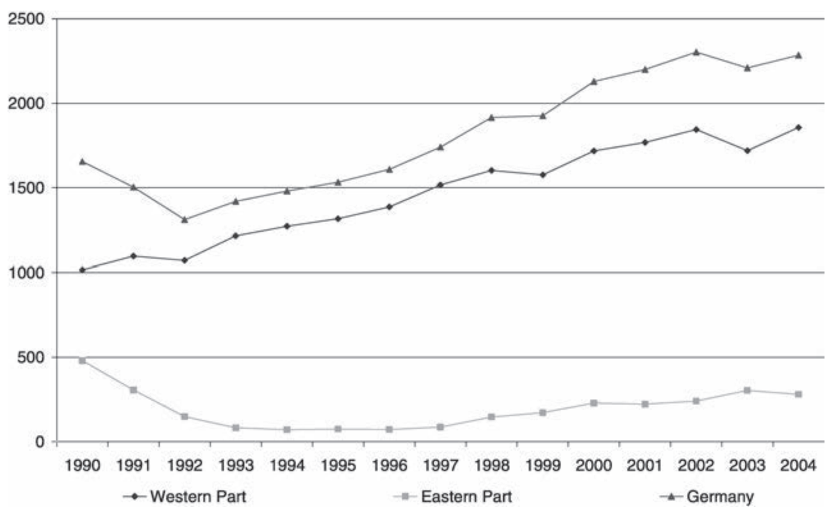
[22] In Figures 1 and 2 the graphs for West and East Germany both exclude Berlin as it was reunified as a state as well and thus has two universities of the former West Berlin and one of the former East Berlin. The graph for Germany includes Berlin, of course.

Figure 2 Professors in East Germany (without Berlin)



1992 to 2283 in 2004, or by 74 percent (cf. Figure 3)! The drop in habilitations in the East in 1990 to 1993 is due to the fact that the "Promotion B", the GDR equivalent to the habilitation is included in that figure. East German scientists sought to finish their "Promotion B" before or shortly after they or their

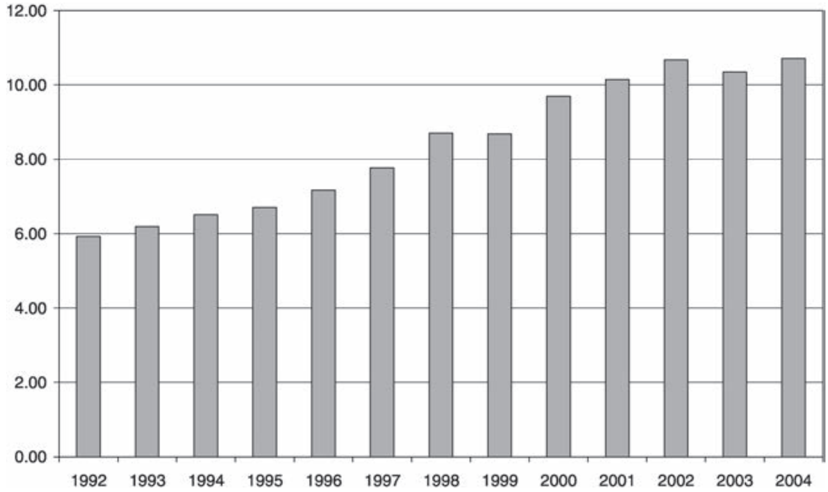
Figure 3 Habilitations in Germany, 1990–2004



professors were laid off. The low level of habilitations in the East between 1993 and 1997 is explained by the fact that a new generation of Ph.D. students needed to be channeled through the system before new habilitations were finished in larger numbers.

Consequently, the number of habilitations per 100 professors rose very significantly during the period from under 6 in 1992 to over 10 in 2004 as shown in Figure 4.

Figure 4 Habilitations per 100 Professors in Germany



If I assume an average age for first appointment as tenured professor (‘Erstberufung’) of 42 years[23] and standard retirement at the age of 65 the average replacement need would be 4.3 people newly awarded habilitation per 100 professors each year.[24] Thus the ratio of *new* applicants to job openings rose from roughly 3/2 to 5/2.

[23] This number was given by the Deutsche Hochschulverband (the German association of professors and those awarded habilitation), cf. Hartmer (2001). Berning et al. (2001) find that the average age of habilitation in Bayern was 39.5 years in the period 1993 – 98, although with large differences between fields.

[24] With the newly increased retirement age of 67 the replacement need is only 4 percent p.a. Obviously these are just an illustrative back-of-the-envelope calculations as the age structure of professors is not uniform in particular due to large expansions of universities in the seventies and the reunification.

3.2 Differences in the Number of Professors across States

I seek to identify differences in the provision of university services across states, measured by the number of professors adjusted for the relevant state size. I use two variables for size – the number of residents and the number of high school graduates.[25] The number of professors divided by number of residents does not account for different age and industry structures and socio-economic profiles of the population between states; the number of professors divided by number of high school graduates seems the more appropriate number as the number of high school graduates measures the demand for professors. It would be endogenous if the decision to seek a high school diploma was dependent on the university quality in that particular state; which seems unlikely. However, if there were significant differences between states in the share of high school graduates that wish to study, a normalization by high school graduates could bias results. As number of high school graduates I use the average of the last five years as I assume that most students need five years to complete their studies.[26]

Figure 5 presents the overall trend in the number of professors per 100 high school graduates. It shows that the ratio has deteriorated significantly from 11.26 in 1996 to 9.43 in 2004 (or by 16%) indicating a substantial aggravation of the German university quality.

Hidden behind this overall figure for Germany is a wide disparity in this indicator. Figure 6 gives an overview of this pattern.

Two stylized facts are evident. First, city states (Hamburg, Berlin, Bremen) have higher ratios of professors to high school graduates than the other states. Cities should be expected to have higher ratios: they draw students from the hinterland because universities tend to be more concentrated in cities or larger towns (and many towns do not have a university at all) and high school graduates are more evenly distributed. While there are usually no large external effects of this pattern as the hinterland mostly belongs to the same state as the city, this does not hold for city states, which draw students from neighboring states. Thus they provide external benefits to the surrounding states.

Second, the new 'Länder' have smaller ratios than the old 'Länder'. An apparent combination of these two effects is found in the case of Brandenburg and Berlin where the former is free-riding on the latter's universities. The

[25] I do not use the number of students in that state as control for size as it is endogenous to the capacity/quality of the universities because students may migrate from states with relative low numbers of professors adjusted for size to those with relative large numbers. Thus, the professor – student ratio underestimates the high performers and overestimates the low performers.

[26] Therefore, if all high school graduates in that state wanted to study and migration were absent, the professor-student ratio would be five times lower.

Figure 5 Professors per 100 high school graduates in Germany, 1996–2004

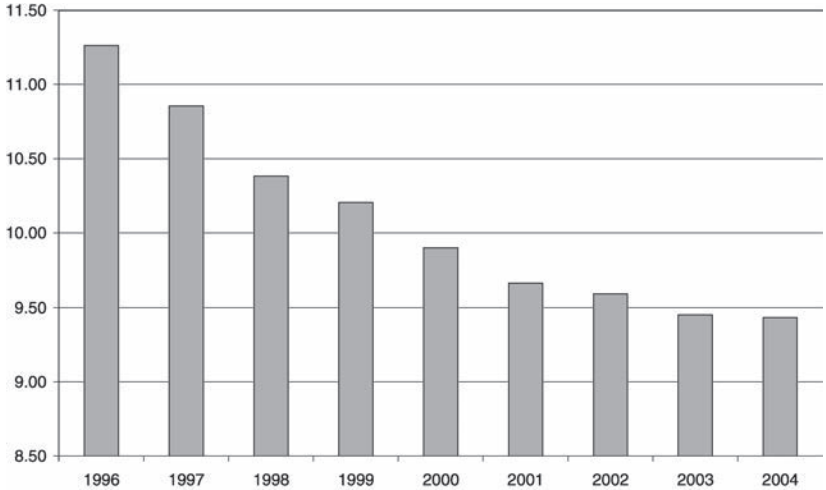
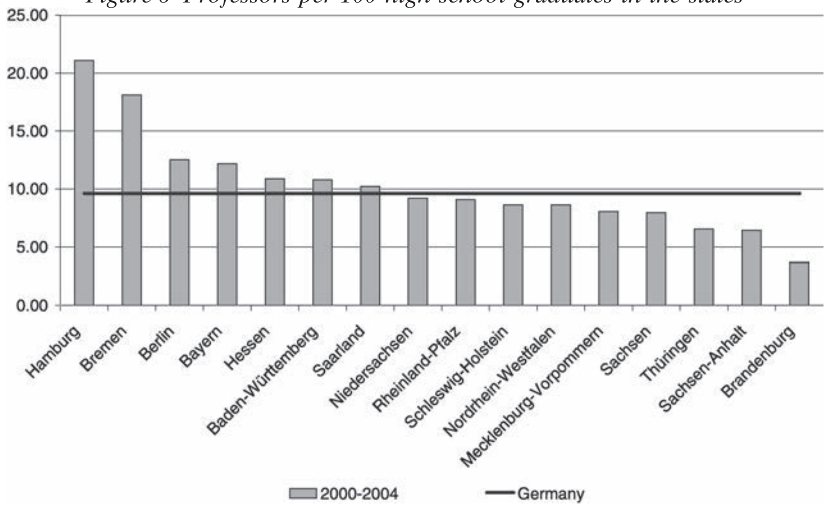


Figure 6 Professors per 100 high school graduates in the states



East-West divide may have financial reasons as eastern states are less affluent and they might still be affected by the transition from socialist command and control society and economy to a democratic society and market economy.

Table 1 (see p. 54) provides a more detailed picture for different time periods as there is a strong time trend. It also shows that the normalization by

Table 1 The provision of professors across states in absolute and relative terms

	Professors (annual mean)		*Professors per 100,000 Residents* (annual mean)		*Professors per 100 High School Graduates* (annual mean)		
Federal State	*1992–1997*	*1998–2004*	*1992–1997*	*1998–2004*	*1992–1995*	*1996–1999*	*2000–2004*
Baden-Württemberg	3011.17	2693.14	29.27	25.45	13.13	13.46	10.79
Bayern	3092.00	3075.71	25.89	25.02	12.78	13.33	12.17
Berlin	1857.83	1511.43	53.66	44.59	27.50	17.08	12.53
Brandenburg	297.17	387.29	11.66	14.97	–	4.52	3.69
Bremen	347.17	377.29	51.05	56.93	15.36	16.71	18.11
Hamburg	1097.17	999.00	64.43	58.04	20.03	19.74	21.09
Hessen	1985.50	1812.29	33.15	29.84	12.10	11.66	10.88
Mecklenburg-Vorpommern	442.83	501.57	24.21	28.50	–	8.26	8.06
Niedersachsen	1843.33	1708.86	23.86	19.79	9.48	9.64	9.21
Nordrhein-Westfalen	4785.33	4562.57	26.82	25.30	9.26	9.43	8.64
Rheinland-Pfalz	913.67	895.14	23.08	22.13	9.76	10.12	9.08
Saarland	272.33	257.29	25.13	24.13	11.93	11.88	10.20
Sachsen	1136.67	1232.43	24.85	28.08	–	8.50	7.97
Sachsen-Anhalt	471.67	585.57	17.17	22.68	–	6.32	6.45
Schleswig-Holstein	482.17	509.00	17.74	18.17	7.49	8.74	8.64
Thüringen	532.67	610.86	21.22	25.35	–	7.18	6.54
Deutschland	*22568.67*	*21719.43*	*27.65*	*25.96*	–	*10.68*	*9.61*
West (without Berlin)	17829.83	16890.29	27.89	25.40	11.11	11.34	10.36
East (without Berlin)	2881.00	3317.71	20.28	24.17	–	7.08	6.55

Data sources: see appendix.

the number of residents produces a somewhat different picture. City states still provide a much higher relative number of professors, and Brandenburg is still the taillight. Yet the ranking is different and there is no longer a clear East-West difference. The ratio of high school graduates to population obviously differs substantially across states.

Is it possible to shed some light on the reasons behind these stylized facts? I offer some very tentative evidence on this issue in the section below.

3.3 What Determines the Provision of Professors? – Some Mickey Mouse Econometrics

In this section I seek to determine whether there is any pattern in the provision of professors per 100 high school graduates. I thus run an OLS regression with robust standard errors with the number of professors per 100 high school graduates as endogenous variable for the sample of 16 German states in 2003. As explanatory variables I use a West dummy, budget per capita and a political index. The West dummy is one if the state is an 'old' – Western – state, zero if it is a new state and 0.5 in case of Berlin. The political index is one if the prime minister is from a conservative or liberal party (CDU, CSU, FDP, Schill party) and zero if he or she comes from a left/labor party (SPD, Green Party, PDS).[27] As changes in the university system take effect only gradually I use budget data and political index data for the last ten years and discounted the values for past years with 5 percent p.a. Lastly, I used as a measure for relative size of states the share of state GDP in federal GDP.

The results are given below.

Table 2 Cross-state OLS regression, endogenous variable: number of professors per 100 high school graduates

Variable	***(1)*** *Coefficient* *(t-statistics)*	***(2)*** Coefficient *(t-statistics)*	***(3)*** *Coefficient* *(t-statistics)*
West-Dummy	7.281299*** (5.41)	7.36203*** (5.80)	7.252577*** (6.30)
Political index	.2484231* (1.35)	.2550074 (1.43)	.2459507[a] (1.46)
State share of population		– 3.076724 (– 0.26)	
Share of state GDP	– .0052421 (– 0.05)		
Budget per capita	325.512*** (5.21)	318.4054*** (4.99)	326.8929*** (6.18)
Constant	– 4.886064 (– 2.02)	– 4.593907* (– 1.80)	– 4.93211** (2.32)
No. obs.: 16	$F(4,11) = 13.96$ $Prob > F = 0.0003$ $R^2 = 0.836$	$F(4,11) = 14.06$ $Prob > F = 0.0003$ $R^2 = 0.837$	$F(3,12) = 20.31$ $Prob > F = 0.0001$ $R^2 = 0.835$

***/**/* indicate significance at the 1/5/10 percent level. [a] significant at the 17 percent level. Data sources and data description are found in appendix 5.2.2.

[27] Alternatively I used the party affiliation of the science or education minister, which affected results only mildly. Arguably the finance minister and the prime minister have more influence on the education policy than the education minister.

The preliminary results show a strong East-West divide in the level of professor provision and a strong positive effect of per capita budget, but not an effect of relative size as measured by state GDP as a share of federal GDP. The effect of budget per capita captures some of the city state effect as city states have larger budget per capita. Lastly, there is some weak indication that the level of provision of professors is influenced by the political stance of the incumbent with conservative politicians tending to spend more on professors in relative terms. The variable never reaches normal significance levels; yet this may be due to the low degrees of freedom.

These results are only suggestive because of the very small number of observations, but they show a direction in which a more profound econometric analysis could go. There seems to be some supportive evidence for the importance of different policy stances as expressed in the variable θ in the model as well as the endogenous budget constraint.

3.4 Student Migration across States

Next I seek to determine whether students do in fact react systematically to differences in university quality across states as measured by the number of professors per 100 high school graduates in that state. This was one of the guiding assumptions in the theoretical part of the paper; it makes the quality of the educational system a strategic variable in the competition for high skilled people.

I employ a standard gravity equation that has been used widely in empirical analyses of international trade and factor movements (Deardorff 1995, Frankel et al. 1996 and many others). It relates positively the gross flows of freshmen from state *i* to state *j* (FRESHMEN_ij) to the relevant sizes of the two states, which are approximated for our problem by the numbers of high school graduates in both states (HS-GRAD). Gross flows are negatively influenced by the DISTANCE between states as measured by the distance in kilometers between state capitals or, in case a state had two dominant centers, by the population weighted distances between these centers and the corresponding state's center.[28] As usual, I included a dummy for adjacent states (ADJACENT) because the variable DISTANCE may not capture the relevant distance appropriately in that case. Because there is a strong East-West difference in Germany, separate dummies EAST were included that took on the value 1 if the home state or the host state was an East German state, 0.5 in the case of

[28] For instance, for a migration from Saxony to the Saarland I used the distances between Leipzig and Saarbrücken and Dresden and Saarbrücken weighted by the relative population shares of Leipzig and Dresden. All details on the construction of the variables including their sources are in appendix 5.2.3.

Table 3 Determinants of student migration flows between German states dependent variable: ln(FRESHMEN_ij)

	(1) Coefficient (t-value)	(2) Coefficient (t-value)
ln(HS-GRAD_i)	.8756688*** (13.81)	.9010989*** (8.97)
ln(HS-GRAD_j)	1.000499*** (19.41)	– .0590754 (– .53)
ADJACENT	.7550757*** (6.99)	.7452368*** (7.08)
EAST_i	.2727333** (2.16)	.2468845** (1.97)
EAST_j	.1931158* (1.66)	.3207784*** (2.60)
PROF_i	.0010049 (0.08)	
PROF_j	.086771*** (7.36)	
ln(ABS_PROF_i)		– .0302408 (– 0.23)
ln(ABS_PROF_j)		1.028326*** (8.02)
ln(DISTANCE)	– 1.03381*** (– 11.81)	– 1.055287*** (– 11.60)
constant	– 7.697556*** (– 6.89)	– 4.010802*** (– 6.07)
F-statistic	F(8, 231) = 153.40***	F(8, 231) = 148.73***
R^2	0.86	0.87
Number of obs. = 240		

***/**/* indicate significance at the 1/5/10 percent level.
Data sources and data description are found in appendix 5.2.3.

Berlin, and zero otherwise. PROF measures the number of professors per 100 high school graduates and was calculated separately for the sending and the receiving state. Alternatively I use the log of absolute number of professors in that state (ln(ABS_PROF)).[29] The regression model thus is

$$\begin{aligned}\ln(\text{FRESHMEN\_ij}) = {} & b_0 + b_1 \ln(\text{HS-GRAD\_i}) + b_2 \ln(\text{HS-GRAD\_j}) \\ & + b_3 \ln(\text{DISTANCE}) + b_4\, \text{PROF\_i} + b_5\, \text{PROF\_j} + b_6\, \text{ADJACENT} \\ & + b_7\, \text{EAST\_i} + b_8\, \text{EAST\_j} + u_{ij}\ ,\end{aligned}$$

where u_{ij} is a disturbance term with zero mean and normal distribution. Regression was made with robust standard errors. Results are given in Table 3.

As expected, the size effect is strong and significant: larger states receive more out of state students and conversely more students emigrate from large states for their studies. Migration is strongly negatively affected by distance between states and 2.1 times as many students migrate between adjacent states. East German states (and Berlin) receive more out of state students and more students emigrate from them. There is no statistically significant evidence that states with higher university quality experience less *gross* outflow

[29] I used a Box-Cox transformation which pointed towards the superiority of the log-log specification. I could not have used the log of professors per high school graduates as this would have estimated twice the size effect of the number of high school graduates.

of students. However, states with a better ratio of professors per 100 high school graduates attract out of state students more strongly. That makes net flows strongly respondent to differences in educational quality.[30] The second model paints basically the same picture, yet the strong correlation between HS-Grad and abs_prof makes an interpretation of the point estimates and significance levels of these four variables very difficult (see appendix 5.2.3). Student migration is thus shaped by pull factors rather than push factors: The results provide empirical support for my basic hypothesis that university quality is a strategic variable for the states to attract students and enhance human capital endowment.

4 Conclusion

The decentralized provision of free education in a federal system is bound to produce externalities that give rise to inefficiencies. Because people are mobile within a federation they are free to let one state invest in their human capital and have another state benefit from the returns to this educational investment. Previous literature has focused on the *underprovision* of education because high-skilled labor is mobile: states anticipate that they may produce human capital for the benefit of other states (Justman and Thisse 1997, Poutvaara and Kanniainen 2000, Büttner and Schwager 2004, Südekum 2005).

This paper has taken a different perspective: Because mobility occurs largely first at the stage of tertiary education, when prospective students have already invested significantly in their human capital, states may have an incentive to *overprovide tertiary* education to attract students, thereby free-riding on the primary and secondary education provided by other states. (They may underprovide primary and secondary education.) Such a strategy makes sense if students are imperfectly mobile after graduating from university, for example because universities create spillovers for the regional economy and create jobs for high skilled individuals. If this is so, the number of students influences the human capital endowment of the state.

I have modeled two federal states which produce with immobile labor, internationally mobile capital and interregionally mobile, but internationally immobile human capital. The decision to provide educational capacity is political, and it impacts on the market allocation of mobile capital which is attracted by a higher human capital endowment. Mobility of human capital occurs at two stages – high school graduates deciding in which state to take up

[30] Of course, ideally migration flows and explanatory variables should have been on a university level because students go to specific universities rather than to certain states. Unfortunately this data is not available.

their studies and university graduates seeking employment throughout the federation; it is imperfect at both stages. It turns out that other things being equal larger states invest relatively less in tertiary education than smaller states – the latter have a better professor-to-student ratio than the former. This creates a distortion which is due to the strategic interaction states find themselves in.

The empirical part sought to portray stylized facts of the German situation in tertiary education. First, there is a strong downward trend in the capacity of universities relative to its demand, i.e., the number of high school graduates. This indicates a significant deterioration in the quality of tertiary education. Second, the number of professors per 100 high school graduates differs significantly across German 'Länder' with city states producing large positive effects for their neighboring states. There is a clear East-West gap with the new 'Länder' providing a lower number of professors per 100 high school graduates. Except for the city states, Bayern, Hessen, Baden-Württemberg and the Saarland show an above average provision of professors. There are some indications that this pattern can be explained by differences in budget constraints and possibly in political preferences. Third, in their decision to migrate, students react systematically to differences in relative educational capacities.

These inefficiencies obviously call for policy interventions if education is to remain in the authority of the states rather than the center. There are good reasons to keep education decentralized (and possibly to decentralize even more): competition is a revelation procedure for new – better – solutions, serves as a laboratory for better educational policies and (thereby) produces dynamic efficiency gains. Competition enhances educational outcomes, *if* externalities can be adequately internalized. These externalities occur at each stage of the system at which mobility occurs. One possibility to internalize these externalities is to privatize the costs of education through user fees thereby eliminating states' incentives to free-ride. This may create other distortions if individuals are credit constrained or risk averse and thus shy away from investing in education. Thus if this solution is ruled out for efficiency or distributional reasons, a voucher system at the federal level may constitute such a solution. Students (and possibly pupils) are given vouchers for their education which they can convert into educational services at any institution within the federation. Institutions can redeem them with a central clearing institution which is financed at the federal level. Thus, financing and authority to regulate educational institutions are effectively delinked, thereby ensuring the advantage of decentralized organization and the efficiency of central financing.

Yet, the current university system may create a third form of externality, which has not been subject of this paper. A state provides external benefits to other states if it exports more educational services in educating and promoting junior staff that is hired by other states than the educational services that it imports by hiring junior staff or newly appointed professors from out of state. This is the case if junior staff – Ph.D. candidates, assistant professors, or research associates seeking a habilitation – are paid more than their marginal value product to the university minus the annuity of their educational investment. This issue is left for future research.

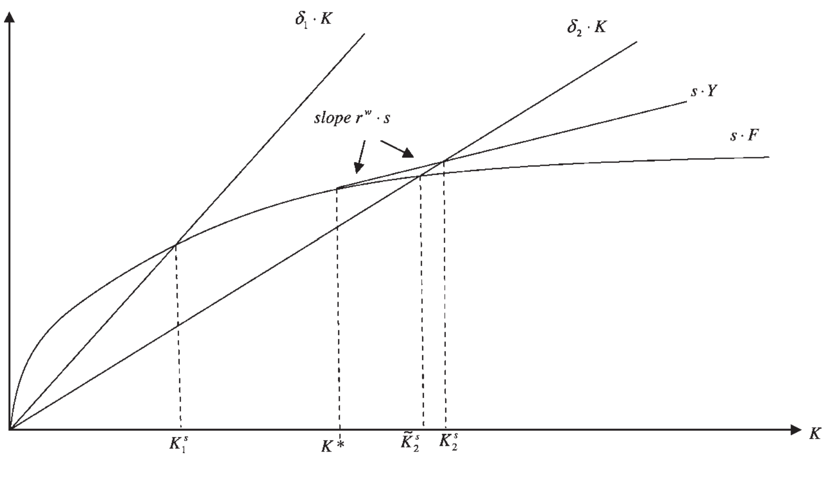
5 Appendix

5.1 The determinants of net capital exports

With constant population the steady state condition for capital accumulation requires that the capital stock owned by a society is constant. In other words, depreciation is exactly replaced by new investment financed out of savings, regardless where the income was generated or the capital installed: $sY = \delta K^S$. If the depreciation rate is high, the capital owned by the society will fall short of the domestically installed capital, $K_1^S < K^*$. As marginal returns to capital for the domestically owned capital stock exceeds world returns $(F_K(K_1^S, H, L) > r^W)$, the economy attracts foreign capital. This is shown in figure 5.1. Recall that K^* is given by the requirement that its marginal rate of return equal the world rate of return, cf. eq. (1).

For low depreciation rates domestically owned capital will exceed domestically installed capital. The exported capital earns the world market rate of return; therefore the equilibrium condition is represented by the intersection

Figure 5.1 Steady state capital stock of a small open economy



of the ray from the origin with slope δ representing total depreciation and the graph that depicts total saving which has the slope $r^w s$.

Specifying the production function of the Cobb-Douglass type

$$Y^d = F(K, H, L) = K^\alpha H^\beta L^{1-\alpha-\beta} \tag{5.1}$$

and using eq. (1) I derive the amount of installed capital as

$$K^* = \left[\frac{r^w}{\alpha} H^\beta L^{1-\alpha-\beta}\right]^{\frac{1}{1-\alpha}}. \tag{5.2}$$

In a closed economy the steady state condition $s\,Y = \delta\,K^S$ would read as

$$\delta \tilde{K}^S = s\,(\tilde{K}^S)^\alpha H^\beta L^{1-\alpha-\beta}$$

and the steady state capital stock would be

$$\tilde{K}^S = \left[\frac{s}{\delta} H^\beta L^{1-\alpha-\beta}\right]^{\frac{1}{1-\alpha}}. \tag{5.3}$$

Obviously the closed economy steady state capital stock equals the open economy capital stock if it falls short of the domestically installed capital stock K^*, but it is smaller if capital is exported as the marginal return to capital is r^w in case of capital export, but lower if the economy is closed. Moreover, if the closed economy capital stock in the steady state exceeds K^* then if this economy is opened it will export capital.

$$\begin{array}{lllll} \tilde{K}^S > K^* & \Rightarrow & K^S > K^* & \wedge & K^S > \tilde{K}^S \\ \tilde{K}^S < K^* & \Rightarrow & K^S > K^* & \wedge & K^S = \tilde{K}^S \end{array} \tag{5.4}$$

Thus capital export occurs if $s\,\alpha > r^w\,\delta$, where I have made use of eqs. (5.2) – (5.4). Assuming that the world production function is of the same Cobb-Douglass type (but possibly with other parameter values) and noting that $F_K = \frac{F}{K}$ and $\frac{F}{\tilde{K}^S} = \frac{\delta}{s}$ the small open economy will import capital iff $s < s^w \frac{\delta}{\delta^w}\frac{\alpha}{\alpha^w}$ where superscript w indicated world values. For the same technologies this condition reduces to $\tilde{K}^S > K^* \Leftrightarrow s < s^w$. If the economy's savings rate exceeds (falls short of) the world savings rate, it will export (import) capital.

5.2 Data Sources

5.2.1 Data for Sections 3.1 – 3.2

Data used in section 3.1 and 3.2 were provided by the Federal Office of Statistics (Statistisches Bundesamt), number of professors and number of habilitations by state are based on data in *Fachserie 11 Reihe 4.4* and were provided as excel files. Data for professors start in 1992 only as the new law on university statistics (*Hochschulstatistikgesetz*) of 1990 was implemented beginning 1992. Population was taken online (www.destatis.de); high school graduates by state were provided as files by the Statistisches Bundesamt.[31] Professors per 100 high school graduates were averaged over the last five years. Students cross-state migration flows were obtained by the Statistisches Bundesamt, Fachserie 11, R 4.1 and refer to the wintersemester 2004/05.

5.2.2 Data and Descriptive Statistics for Section 3.3

Budget data for section 3.3 are taken from the following sources: *Bundesministerium der Finanzen,* "*Finanzbericht 2005*"; Köln 2004, *Bundesministerium der Finanzen,* "*Finanzbericht 2000*"; Bonn 1999, and *Statistisches Bundesamt,* "*Volkswirtschaftliche Gesamtrechnung – Bruttoinlandsprodukt* at http://www.statistik-portal.de/Statistik-Portal/de_jb27_jahrtab65.asp."

The political index (PI) was based on the political dummy PD_i for year i which is one if in that year the state prime minister is conservative (or liberal, or from the Schill party) and zero if she or he is socialdemocratic (or green or socialist) and is aggregated over the last ten years according to following formula:

$$PI = \sum_{i=1994}^{2003} PD_i \, (1.05)^{i-2003}$$

An analogous aggregation was made for budget per capita used in the regression. Data for the political index were taken from www.deutschland.de and the pages for the states linked to this page.[32]

Descriptive statistics and cross correlations are provided below.

[31] Earlier data (up to 1992) are also published in *Kultusministerkonferenz* (http://www.kmk.org/statist, *Veröffentlichung Schüler, Klassen, Lehrer und Absolventen der Schulen 1982 bis 1991,* Nr. 121).

[32] For instance, http://www.baden-wuerttemberg.de/fm/1899/Regierungen_BW.pdf, http://www.niedersachsen.de/master/C2860930_N1461178_L20_D0_I198.html, http://www.landeshauptarchiv.de/geschichte/kabinette.html. See also Biographisches Handbuch der deutschen Landesregierungen nach 1945 (2006) München: K.G. Saur.

Table 5.1 Descriptive statistics for variables used in the regression of Table 2 (section 3.3)

Variable;	Obs.	Mean	Std. Dev.	Min	Max
Professors per 100 high school graduates	16	10.17341	4.379247	3.610794	20.88965
Budget per capita	16	.0291702	.0101174	.0197562	.0495509
Political index	16	3.295331	3.119904	0	7.7217
Share of state GDP	16	6.25	6.467188	1.087774	22.07029
West-Dummy	16	.65625	.4732424	0	1
City state dummy	16	.1875	.4031129	0	1
Population	16	5158229	4841514	663129	1.81e + 07

Table 5.2 Cross correlation for variables used in the regression of Table 2 (section 3.3)

	Profs per 100 high school grads	Political index	Share of state GDP	West-Dummy	Popula-tion	Budget per capita
Profs per 100 high school grads	1.0000					
Political index	– 0.0125	1.0000				
Share of state GDP	– 0.0092	0.1682	1.0000			
West-Dummy	0.5491	– 0.1952	0.4659	1.0000		
Population	– 0.1258	0.1224	0.9851	0.3827	1.0000	
Budget per capita	0.5392	– 0.0459	– 0.5289	– 0.2654	– 0.5591	1.0000
City state dummy	0.8007	– 0.1147	– 0.2671	0.1857	– 0.3310	0.8854

5.2.3 Data and Descriptive Statistics for Section 3.4

Data sources are given in appendix 5.2.1. Distance was calculated as the distance in kilometers between state capitals. Data were compiled by online distance tables and if not available with the help of route planers “Tank & Rast” (http://www.tank.rast.de/services/entfernungstabelle/entfernungen.htm, November 2005) and (http://www.tank.rast.de/services/routenplaner/), respectively. If a state had more than one important metropolitan area, a population weighted average of the relevant distances was calculated (cf. fn. 28). In particular, I used for Baden-Württemberg, Stuttgart and Karlsruhe, for Bayern, München and Nürnberg-Fürth-Erlangen, for Hessen, Frankfurt, for Nordrhein-Westfalen, Düsseldorf and Köln, for Sachsen, Dresden and Leipzig and for all other states the state capital.

Descriptive statistics are given in Table 5.3 below.

Table 5.3 Descriptive statistics for the student migration model of section 3.4

Variable	Obs	Mean	Std. Dev.	Min	Max
FRESHMEN_ij	240	327.525	458.1392	1	2898
(HS-GRAD_i	240	16153.44	14479.45	2575	57409
ADJACENT	240	.2416667	.428988	0	1
EAST_i	240	.3125	.4644811	0	1
PROF_i	240	10.17341	4.24905	3.6108	20.8897
ABS_PROF_i	240	1555.245	1406.356	315.8367	4815.697
DISTANCE	240	409.375	185.5323	33	812

Cross correlations are given in Table 5.4 below.

Table 5.4 Cross correlations for the variables in the student migration model of section 3.4

	FRESHMEN_ij	HS-Grad_i	HS-GRAD_j	ADJACENT	EAST_i	EAST_j	PROF_i	PROF_j	ABS.PROF_i	ABS_PROF_j
FRESHMEN_ij	1									
HS-Grad_i	0.3041	1								
HS-GRAD_j	0.3435	– 0.0667	1							
ADJACENT	0.6555	0.0735	0.0735	1						
EAST_i	– 0.0605	– 0.3052	0.0203	0.0604	1					
EAST_j	– 0.1415	0.0203	– 0.3052	0.0604	– 0.0667	1				
PROF_i	– 0.1607	– 0.1438	0.0096	– 0.1420	– 0.5681	0.0379	1			
PROF_j	– 0.0007	0.0096	– 0.1438	– 0.1420	0.0379	– 0.5681	– 0.0667	1		
ABS_PROF_i	0.2684	0.9628	– 0.0642	0.0522	– 0.4368	0.0291	0.0491	– 0.0033	1	
ABS_PROF_j	0.3503	– 0.0642	0.9628	0.0522	0.0291	– 0.4368	– 0.0033	0.0491	– 0.0667	1
DISTANCE	– 0.4068	0.0824	0.0802	– 0.5512	– 0.1222	– 0.1222	0.0259	0.0259	0.1146	0.1146

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Tertiary Education in a Federal System

Comment by

Stefan Voigt

Schulze's paper deals with a straightforward question: to what extent do federal systems of free tertiary education (such as Germany's) suffer from positive externalities that give rise to strategic behavior of the states in the provision of university education? Schulze is definitely not the first to ask this question but he adds an interesting twist to it: previous papers assumed that states tried to free ride on the free tertiary education provided elsewhere; in the aggregate this would lead to an underprovision of university education. Schulze now takes into account that university graduates might have a bias to stay close to where they graduated and that they could produce positive regional spillovers which might raise GDP regionally. This implies that there might not be incentives to underprovide tertiary education after all. Indeed, its overprovision is a possibility. But states overproviding tertiary education might have an incentive to underprovide both primary and secondary education.

His model is based on a neoclassical production function and Schulze is interested in steady states. The model nicely captures that human capital formation (a consequence of the education budget) does not only increase GDP directly but also indirectly by attracting physical capital (because the return to capital rises initially). Finally, the increase in both physical and human capital leads to wage increases (as labor is assumed to be immobile). This means that GNP increases. The Nash-equilibrium in a model with two states predicts that larger states (in terms of population size) will provide more tertiary education.

The empirical section is interested in identifying differences in the provision of tertiary education – operationalized as the number of professors per high school graduate. The provision of professors is endogenized as the next step. The empirical section further deals with the question whether the differences in the number of professors between the German states are a significant variable in explaining student mobility. This is implemented by drawing on a gravity model. The results show that there is considerable variation in the number of professors which can be explained by a dummy for the West (states in the western part of Germany employing significantly more professors) and the state's per capita budget. There is, however, no clear-cut effect of state size on the number of professors which means that the theoretically derived prediction is not confirmed.

The interesting twist in Schulze paper is the possibility that states in a federation might be oversupplying tertiary education while undersupplying primary and secondary education. My comment will, hence, focus on this aspect. Before picking it up, it might, however, be in order to shortly discuss the proxies used in the empirical section.

As a proxy for the provision of tertiary education, Schulze's uses the number of professors employed full time at universities, technical universities and pedagogic universities normalized by the number of high school graduates. He thus excludes professors employed at universities of applied sciences and other more applied organizations. This is one of a number of potentially possible quantitative measures. I am not sure, however, whether it is the most straightforward one. Positive regional spillovers might also be generated by the more applied universities; their claim to being more applied suggests that they aim at such spillovers. Using the number of professors as a proxy implies that it is primarily their number that is determining differences in the quality of tertiary education. A host of other factors might be relevant too: the number of assistants, the quality of the library, the laboratories, the computer pools and so forth. I wonder whether simply drawing on a state's budget for tertiary education would not have been as good a proxy.

Student migration flows across states are conjectured to be influenced by the number of professors in both the "exporting" as well as the "importing" state. Schulze finds that the number of professors in the importing state is highly significant whereas the number in the exporting state is insignificant, in other words that there is more of a pull than a push effect. This seems to make perfect sense, except that students do not seem to choose a state to study in but rather a university to study at. Schulze is well aware of this problem (see footnote 30) but it remains a problem. Some German state governments are said to consider some universities as their prestige projects whereas others barely survive. This will supposedly not be reflected in Schulze's proxy.

It seems straightforward to assume that student migration flows are at least partially determined by differences in the quality of university education. Yet, it would also seem straightforward that students base their decisions on readily available indicators. It would, hence, be interesting to look at the correlation between the Schulze measure and other readily available rankings that have been widely publicized and discussed in recent years in Germany.

But let us move to the possibility that tertiary education could be overprovided while primary and secondary education could be underprovided. The paper shows that the professor-per-100-high-school-graduates ratio has deteriorated from 11.26 in 1996 to 9.43 in 2004. This is a decline by about one sixth in less than a decade. There certainly does not seem to be any race to the top with regard to university education in Germany. The recent reform of the German payment scheme for professors basically means that salaries were substantially reduced. To be a professor has thus become less attractive relative to other occupations. It is straightforward to predict that average quality will suffer from this reform, another indicator for the absence of a race to the top.

Schulze might argue that the attempt to free ride on primary and secondary education was the more important part of his argument anyways. Free riding on others presupposes that high school graduates are highly mobile before beginning tertiary education (and substantially less so after having finished tertiary education). Empirically, we know, however, that around two thirds of all high school graduates who go on to university remain in the state in which they received their high school diplomas. The notion that states could free ride on primary and secondary education implies that basic education could be low quality whereas university education could be top-notch. Yet, empirically, the correlation between states offering high quality basic education and those offering high quality university education seems to be extraordinarily high (e.g. Bavaria, Baden-Württemberg). And this seems to make intuitive sense too: parents who have graduated from university and stayed in the state would supposedly consider moving out if basic education for their children was not high quality too.

In sum, this paper deals with an interesting question and adds a fascinating new twist to it. The theoretical model nicely captures the most relevant basic ideas. The empirical section presents interesting data – and shows that part of the theoretical predictions cannot be confirmed. Yet, there is scope for more detailed empirics as Schulze terms some of his econometrics "Mickey Mouse" – as the number of observations is very low.

The Productivity of UK Universities

by

Gustavo Crespi and Aldo Geuna*

1 Introduction

There is increasing recognition in the UK and other OECD countries of the importance of scientific research in providing the foundations for both innovation and competitiveness. This has resulted in increased public funding for research in the UK and elsewhere. At the same time, there is a lack of systematic evidence on how such investments can lead to increasing levels of scientific output and, ultimately, to better economic performance. Much of the available literature concentrates on the effects of public funding of basic research on either firms' innovative activities (see among others Cohen, Nelson and Walsh 2002; Klevorick et al. 1995; Jaffe 1989; Narin, Hamilton and Olivastro 1997) or firm performance (Adams 1990), bypassing the question of how to measure scientific output. The reasons for this are the difficulty of identifying a stable causal relationship between the resources spent on the science budget and 'intermediate' scientific outputs. This difficulty originates from the dynamic nature of this relationship. There is a persistent and therefore recursive feedback between inputs and outputs, which is exacerbated by lack of appropriate information for analysis. Among the few studies that have attempted to address the problem are Adams and Griliches (1996) and Johnes and Johnes (1995). This study is based on and further develops Adams and Griliches's methodology.

The national science budget comes from several sources. For example, the UK higher education sector received a total of £4,035 millions for research and development in 2001, financed by the Office of Science and Technology (OST) via the research councils (£942), Higher Education Funding Councils (HEFC) (£1,474), other UK sources such as direct government (£238), higher education institutions (£166), non-profit organisations (£660) and business enterprises (£250), and funding from other countries or supranational institutions (£304). These contributions are allocated within the system according

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to scientific field and research institution, to provide the resources needed for research.

The scientific process produces several research outputs that can be classified into three broadly defined categories: (1) new knowledge; (2) highly qualified human resources; and (3) new technologies. This paper focuses on the determinants of the first two types of research output, which are the most closely related to the science research budget. There are no direct measures of new knowledge, but several proxies have been applied in previous studies. The two that we use in our study, which are also the most commonly used measures are publications and citations. These are incomplete proxies for the production of new knowledge and have several shortcomings (Geuna 1999). Highly qualified human resources have been proxied by the total number of graduate students that have completed their studies.

In this paper we focus on the determinants of university research output (as measured by publications, citations and numbers of graduate students) in the UK. We use an original dataset that includes information for the 52 'old' UK universities across 29 scientific fields for a period of 18 years (1984/85 – 2001/02). The paper does not aim to produce exact indicators of the dynamics of the science system, on the basis of which to draw strong policy conclusions and we fully acknowledge the limitations of the input-output data we use.

The paper is structured as follows. In Section 2 we present the methodology and the data sources; we depart from the traditional static knowledge production function model to estimate different dynamic panel data specifications. In Section 3 we present and discuss the results of the estimations. In Section 4 we use the residuals of our fitted knowledge production functions to evaluate the evolution of UK scientific productivity. Finally, in the conclusion we discuss the limitations of this study and suggest possible further developments.

2 *Methodology and data sources*

Our methodological approach develops the standard knowledge production function model of Adams and Griliches (1996). They use the expression:

$$y_{it} = \alpha_i + \beta W(r)_{it} + \gamma_i X_{it} + u_{it}, \quad i = 1, \ldots\ldots N \tag{1}$$

where y_{it} is the (log) output of the research 'intermediate' output (papers and citations) by field i and time t. $W(r)_{it}$ is (the log of) a distributed lagged function of real past R&D expenditure and X_{it} is a vector of the control variables. The main focus of this analysis is on β, the elasticity of the research output with respect to research input and the measure of local returns to scale in research. Diminishing (constant or increasing) returns predominate when $\beta < (\geq 1)$.

In order to build a science capital stock we need: the time length over which the past investments in university R&D are considered to be relevant for the current research; and a weighting scheme to account for past university R&D. This is where our methodology departs from Adams and Griliches (1996). While they present the results for three and five year distributed lags of R&D, where the weighting pattern is completely ad hoc, we search for a lag structure, and develop a procedure to estimate a flexible and 'data driven' lag structure.

There are two dominant models. First, the Autoregressive Distributed Lag (ADL) model, which assumes a very flexible and unrestricted relationship between (log) R&D inputs and outputs, but at the cost of estimating a large number of parameters (see for example, Guellec and van Pottelsberghe 2001 and Klette and Johansen 2000). Second, the Polynomial Distributed Lag (PDL) or Almon Model, which specifies the weights as polynomial functions of a particular estimated degree (Crespi and Geuna 2004). These log linear models imply a strong *complementarity* between the knowledge inputs.[1] In other words, the greater the initial knowledge, the greater will be the amount of knowledge obtained from a given amount of R&D. The more knowledge is produced, the more it can be recombined to produce new knowledge. Formally we will assume that:

(2) $$K_{it} = \prod_{j=0}^{J} r_{it-j}^{w_j}$$

In this paper we present the results of the PDL Model; we used the ADL model in another paper and obtained consistent results. Let us now define the following 'finite' distributed lag model:

(3) $$y_{it} = \alpha_i + \sum_{j=0}^{q} \beta_j r_{it-j} + \gamma_i X_{it} + u_{it}, \quad i = 1, \ldots\ldots N$$

Although a model like (3) in theory can be estimated in a straightforward manner, there is the potential problem of very long lags in which case the multicollinearity is likely to become quite severe. In such cases it is common to impose some structure on the lag distribution, reducing the number of parameters in the model. It is in this context that the PDL model can be useful. The approach is based on the assumption that the true distribution of

[1] By complementarity we mean that marginal productivity of current R&D investment tends to zero if past R&D investment also tends to zero. This assumption is particularly apt in the case of science where we 'stand on the shoulders of giants' to build new knowledge.

the lag coefficients can be very well approximated for by a polynomial of a fairly low order:

(4) $$\beta_j = \delta_0 + \delta_1 j + \delta_2 j^2 + ... + \delta_p j^p, \quad j = 0, ... q > p$$

The order of the polynomial, *p,* is usually taken to be quite low, rarely exceeding 3 or 4. By inserting (4) into (3), one can estimate a transformed model where the estimated coefficients are the deltas that can be put back into (4) in order to recover the original weights. In addition to the $p+1$ parameters of the polynomial, there are two unknowns to be determined: the length of the lag structure, q, and the degree of the polynomial, p. Here we follow the non-standard procedure of setting the length of the lags using a priori information and then searching for the degree of the polynomial function.

The usual standard procedure is to use the same dataset first to search for the optimum time lag (using some information criteria) and then, taking the best lagging as true, to search for the optimum polynomial function. However, this sequential search approach carries the problem that unless the test statistics are overwhelming, the true significance levels in the tests remain to be derived, and the true distribution of the resulting estimator is unknown. Following the evidence in Crespi and Geuna (2004) for a large set of OECD countries, we set a lag length of 6 years for publications and research students, and 7 years for citations.

Assuming that we know the right lag length we proceed by looking for the right polynomial function. We start by using a fifth degree function and proceed by testing sequential unit reductions in the degree. It is important to note that in order to retain the appropriate significance level in each step we used a very low individual significance level. The PDL model also implies a set of constraints on the unrestricted model (without a specified functional form for the lags). For example, if the known lag length is 6 and we use a third degree polynomial function, we are implicitly imposing three constraints. In addition, we have endpoint constraints, which allow the lag distribution to be 'tied down' at its extremes. These endpoint constraints capture the idea that there is no effect of R&D on the research outputs *before* the current period and also that there is no effect from the research inputs after the maximum lag. That is, we need to impose:

(5) $$\beta_{-1} = 0 \quad \text{and} \quad \beta_{q+1} = 0$$

In total we have five constraints. One way to validate the PDL model is to test whether these constraints are valid, which can be done by using a simple chi-square test.

Finally, the PDL model also requires exogeneity of R&D. We carried out a bivariate Granger causality test. Following Rouvinen (2002), we implemented the test using a dynamic panel data in differences (DPD-DIF) model, where the first differences of the dependent variables are regressed on lags of the first differences of the dependent and independent variables. The findings (not reported here for reasons of space, but available from the authors) would suggest that there is a two-way causality between R&D, and publications and citations. This may result in a biased estimation of the elasticity coefficients. Given the problems with available data, and the experimental nature of our work, we acknowledge that our estimations will be biased, and, taking a conservative approach, we nevertheless decided to use the PDL model because it allows us to use the level variables and therefore to maintain a high level of information, which is crucial in the case of variables such as ours which are very noisy.

To estimate the model we used the *SPRU* science field database.[2] The dataset includes information on 52 old universities covering 29 scientific fields, over an 18 year period 1984/85 to 2001/02.[3] The 52 old universities considered provide a good representation of the scientific research carried out in UK universities; in 2001/02 research grant and contract income for these universities accounted for 87% of total UK research grant and contract funding. The dataset has four variables (not including institution and field ids): information on total research grant and contract income;[4] number of publications; number of citations;[5] and total number of graduate students.

In the following sections we present the results of the field level estimates of the science production function for publications, citations, and graduate students. Because information about publications and citations is only available at field level, we cannot estimate a knowledge production function for each of the 29 fields. We need to aggregate the micro fields into more broadly defined categories by mapping the 29 fields into the 4 broad categories in the OECD

[2] A detailed description of the procedure used for the development and content of the datasets can be found in Crespi and Geuna (2004).

[3] The 52 old universities do not include the Open University, Cranfield University, the independent University of Buckingham (not in University Statistical Record statistics) or Lancaster University (not in the Higher Education Statistical Agency statistics). Due to problems with the archiving of the University Statistical Record data, London University data are the sum of all its colleges. Not all the universities are active in every scientific field, every year.

[4] Total research grant and contract income includes all direct research funding received from the research councils, industry, the EC, foundations, etc. Total research grant and contract income accounted for 38% of total research income in 1988/89 increasing to about 60% in 2000/01 (http://www.ost.gov.uk/setstats/5/t5_1.htm; accessed 26/1/2006). We were not able to obtain total research income broken down by scientific field because this breakdown of HEFC funding by institution and subject area for the whole period was not available.

[5] The source of publication and citation numbers is the Thomson ISI(R) 'National Science Indicators' (2002) database.

Table 1 UK Research Outputs

Year	Publications				Citations				Graduate Students			
	NS	ENG	MS	SS	NS	ENG	MS	SS	NS	ENG	MS	SS
1984	43.3%	13.8%	32.8%	10.1%	44.7%	7.5%	41.1%	6.7%	43.6%	18.8%	9.3%	28.3%
1985	42.1%	13.8%	33.9%	10.2%	42.7%	7.6%	42.4%	7.2%	43.2%	19.5%	9.2%	28.1%
1986	42.2%	14.0%	33.9%	9.9%	42.2%	7.4%	43.2%	7.2%	43.5%	20.1%	8.7%	27.7%
1987	41.7%	14.0%	34.5%	9.8%	41.9%	7.8%	42.9%	7.4%	43.7%	20.5%	9.3%	26.4%
1988	41.3%	14.9%	34.4%	9.3%	41.8%	8.0%	43.2%	7.0%	44.3%	20.3%	8.9%	26.6%
1989	40.6%	14.1%	35.9%	9.4%	40.2%	7.4%	45.2%	7.2%	44.7%	19.3%	8.9%	27.2%
1990	41.1%	14.9%	35.0%	9.1%	41.1%	8.1%	44.2%	6.6%	43.5%	19.0%	9.2%	28.3%
1991	40.6%	15.4%	34.9%	9.1%	41.0%	7.9%	44.6%	6.5%	43.4%	18.8%	9.6%	28.2%
1992	40.5%	16.0%	34.3%	9.1%	40.9%	8.2%	44.6%	6.3%	42.6%	19.1%	9.6%	28.8%
1993	41.1%	15.6%	34.6%	8.7%	41.1%	8.0%	44.7%	6.2%	40.7%	19.0%	10.1%	30.1%
1994	41.0%	16.5%	33.7%	8.8%	40.9%	8.1%	44.6%	6.4%	38.8%	19.1%	10.0%	32.1%
1995	41.2%	16.3%	33.0%	9.5%	42.1%	7.9%	43.9%	6.2%	37.5%	18.8%	10.3%	33.3%
1996	40.8%	16.5%	32.9%	9.8%	41.6%	8.5%	43.2%	6.7%	35.9%	17.9%	12.0%	34.2%
1997	41.2%	15.8%	33.0%	10.0%	41.7%	7.9%	43.8%	6.5%	35.1%	17.4%	12.3%	35.3%
1998	41.1%	16.1%	33.0%	9.9%	42.7%	8.2%	42.6%	6.5%	34.7%	16.9%	13.1%	35.3%
1999	41.1%	16.5%	32.5%	9.8%	42.2%	8.4%	43.0%	6.3%	34.1%	16.9%	13.4%	35.6%
2000	40.6%	16.0%	32.8%	10.6%	42.3%	8.2%	43.4%	6.1%	33.5%	17.3%	13.4%	35.8%
2001	40.9%	16.1%	32.5%	10.5%	43.6%	7.9%	42.6%	5.8%	32.9%	17.7%	13.3%	36.0%

Source: Evidence.
NS: Natural Sciences; ENG: Engineering; MS: Medical Sciences, SS: Social Sciences.

statistics. The four-macro fields analysed were: natural sciences; engineering; medical sciences; and social sciences.

Table 1 summarises the main research outputs used in this section. Across the entire period, there is a remarkable stability in the distribution of research outputs by field. Broadly speaking, natural sciences and the medical sciences together account for 75% and 85% of total publications and citations respectively, in the UK. The remaining percentage is split between engineering (15% and 8% respectively) and the social sciences (10% and 6% respectively). The picture changes dramatically when we focus on graduate student research output where the importance of the natural sciences declines to slightly over 30% at the end of the period, while the medical sciences increases from 9% to 13%. Taken together, these two macro fields have a much lower output share (45% at the end of the period). Engineering remains stable at around 18% for the period, while the social sciences show a systematic growth from 28%, to 36% towards the end of the period.

In order to account for the 'truncation problem' in the citations for the most recent years, the citations variable was adjusted.[6] One way of controlling for truncation is to use what Hall, Jaffe and Trajtenberg (2001) describe as the fixed effect approach. This involves scaling citations counts by dividing them by the average citation count for a group of publications to which the publication of interest belongs. Thus, a publication that received say 11 citations and belongs to a group in which the average publication received 10 citations, is equivalent to a publication that received 22 citations, and belongs to a group where the average number of citations is 20. The groups were defined in terms of scientific field and year and the scaling index was computed using the ISI dataset at world level.

On the basis of these data sources we

– estimate the science production function for the OECD macro fields (natural sciences, engineering, the medical sciences, and the social sciences) using information on 29 science fields available for the UK,

– examine the changes in productivity growth across fields.

3 The UK knowledge production function estimates

In this section we present the results of the field level estimates of the science production function for publications, citations, and graduate students.[7] The

[6] The citation count is affected by the time span allowed for the papers to be cited: for example, papers published in 2000 can receive citations in our data only from papers published in the period 2000–2001; they will be cited by papers in subsequent years, but we do not observe them.

[7] A national level science production function model was statistically rejected in favour of four very broadly defined macro-fields.

four macro fields analysed are: natural sciences, engineering, medical sciences, and social sciences. For each of these macro fields, the aim was to estimate a science production function as follows:

$$(6) \qquad y_{it}^{F} = \alpha_{i}^{F} + \beta^{F} W(r)_{it}^{F} + \gamma^{F} X_{it}^{F} + u_{it}^{F}, \quad i = 1, \ldots\ldots N; F = 1, \ldots J$$

where y_{it} is the (log) output of the research 'intermediate' output (papers, citations, and graduate students) by scientific micro field i (we have 29 scientific micro fields classified into the 4 broad fields listed above) and time t (period 1984–2001). $W(r)_{it}$ is (the log of) a distributed lagged function of real past research grants and contract income by scientific micro fields and X_{it} is a vector of the control variables described below. As explained above, a six-year lag for publications and graduate students, and a seven-year lag for citations were applied; then, conditional on them, we tested the shape of the lag function using fourth, third and second degree polynomial functions. In all cases we could not reject that the third degree polynomial function was the correct one. Also in all cases we tested an unconstrained model and could not reject the constrained model as valid.

The vector X_{it} refers to a series of control variables included to assess two important phenomena.

– First, we want to control for the way in which time is allocated by the researchers. One of the most important decisions regarding time for many (but not all) university researchers is how it is allocated between research and (undergraduate) teaching activities. Because we have information about the number of undergraduate students by field and year, we can control for the impact on research output of teaching intensity in the different fields.

– Second, research output can be affected by factors specific to the university (Geuna 1999). We test for three effects: a) localisation (London based universities); b) research propensity (Russell group universities versus Group 94 universities); and c) reputation (when the university was founded).

The control variables are as follows:

– *Teaching Load*: is the ratio of undergraduate students to total staff, computed by field and year.[8]

– *London*: refers to the proportion of research income in each field that is invested in universities located in London.

[8] Information on teaching intensity ratio is only available from 1993. As the estimation sample starts in 1989, we had to reconstruct the 'missing' period. The best imputing mechanism was using university level linear interpolation, which respects the heterogeneity across universities and fields.

– *Russell*: refers to the proportion of research income in each field that is spent in universities affiliated to the Russell Group (self-selected group of research-led universities).

– *Group 94*: is the proportion of research income in each field that is spent in universities that belong to the Group 94 (self-selected group of research-led universities that are, on average, smaller than the Russell Group, more oriented to teaching, and with less prestigious research reputations).

– *Medieval Universities*: is the proportion of research income in each field allocated to universities founded before the 18th century.

– *19th Century Universities*: is the proportion of research income in each field allocated to universities founded in the 19th century.

– *20th Century Universities*: is the proportion of research income in each field allocated to universities founded in the first half of the last century.

– *Post WWII universities*: is the proportion of research income in each field spent in universities founded after the Second World War, mostly redbrick universities.

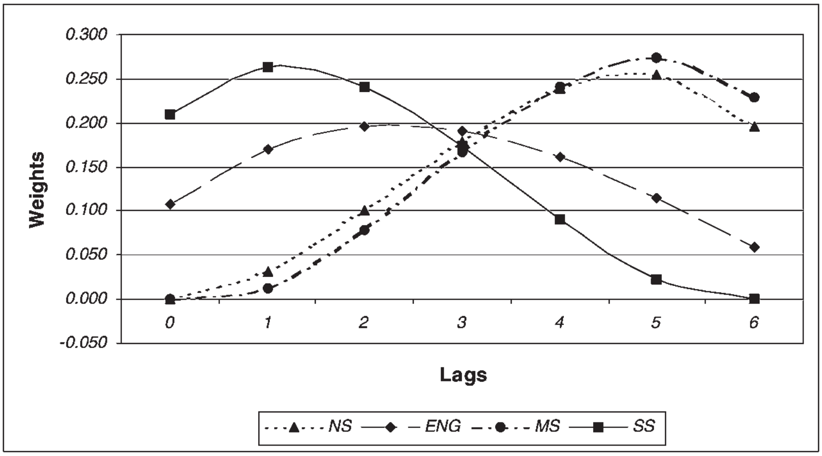
The coefficients of these control variables capture, to some extent, the differences in research productivity of the various institutions. The available literature on university research production allows us to hypothesise a negative coefficient for the undergraduate teaching variable: we can expect a negative impact on research production due to the allocation of more time to undergraduate teaching activities. The localisation of universities in the London area should create positive externalities for research, which increases the productivity of those institutions located in London. We expect a positive value for the variable London. With regard to the other control variables no clear a priori expectation can be formulated; to our knowledge this is the first study that has attempted to evaluate these effects. A possible hypothesis is that those universities that are more research-led and more prestigious tend to assign more importance, and therefore more support, to research, which should translate into higher research productivity.

We estimate the model for the three research outputs: publications; citations; and number of graduate students.

3.1 Publications

We first show the pattern of weights and then proceed to the results of the model. As is clear from Figure 1 (see p. 80), a first important result of our estimation is that the lag structures are significantly different across fields. The social sciences show a relatively important impact in the short run (during the first two years) but the effects diminish over time; the situation in the natural and medical sciences is the reverse, the bulk of the impact being concentrated

Figure 1 Restricted Pattern of Weights (Publications), by fields



towards the end of the lag span. Finally, in the case of engineering we have a clear parabolic function, which suggests a concentration of impact towards the middle of the time period. These differences in the weighting function are very important because they point to a differential impact of a given increase in the science budget over time. The research output generated in the social sciences is much more immediate than in the other sciences, leading to an increase in the share of socials sciences in total publications in the short run. This situation is reversed over time in favour of the natural and medical sciences.

Table 2 presents the results from using the described weighting pattern to compute the sector knowledge stock and to estimate model (6). The first interesting result is that the long run elasticity between knowledge stock and publications varies widely across broadly defined fields. The highest elasticity is found in the medical sciences (0.46) and the lowest in the natural sciences (0.20). In all four cases elasticities are significant. The year effect, which captures the long run trend in scientific opportunities affecting research output, is always positive. It is important to note that as this model does not include a specific variable for spillovers from abroad (an international co-authorship matrix in each science field would be needed) the time trend also captures international spillover effects. The year trend value is highest for engineering, and smallest for the medical sciences.

In terms of the impact distribution of changes in the research budget, the last two rows of Table 2 show the median lag (the year that accumulated at least 50% of the impact) and the 90th percentile lag. Consistent with the weight patterns, the most immediate impact is in the social sciences where

Table 2 UK Levels Estimates, Publications (Method: field fixed effect)

	NS	ENG	MS	SS
R&D	0.208	0.216	0.461	0.340
	0.112*	0.132*	0.145***	0.086***
Year	0.014	0.036	0.011	0.033
	0.007*	0.009***	0.009	0.006***
Undergraduate Teaching	– 0.032	– 0.014	– 0.052	– 0.017
	0.010***	0.014	0.009***	0.007**
London	0.001	– 0.012	0.003	– 0.001
	0.003	0.003	0.004	0.005*
Group94	0.001	0.004	0.003	– 0.008
	0.004	0.005**	0.003*	0.005
Russell	0.004	– 0.004	0.006	0.000
	0.003	0.003	0.003	0.005
Medieval	0.002	0.005	– 0.017	0.013
	0.005	0.004	0.007**	0.004***
19^{th} Century	0.001	0.007	– 0.008	0.009
	0.004	0.003**	0.004*	0.004**
20^{th} Century	0.008	0.005	– 0.001	0.018
	0.005	0.007	0.004	0.012
Constant	– 21.630	– 67.578	– 19.889	– 64.677
	12.254*	15.157***	16.129	11.184***
Observations	108	84	72	84
R-squared	0.83	0.78	0.88	0.86
Chi(2)	2.97	7.10	7.75	7.44
P > Chi(2)	0.71	0.21	0.17	0.19
50% Quartile Lag (years)	3.8	2.1	4.0	1.1
90% Quartile Lag (years)	5.5	4.6	5.6	3.1

Robust standard errors reported below each coefficient. Within R-squared reported.
(*) significant at 10%; (**) significant at 5%; (***) significant at 1%

90% of the effect is observed after 3 years, compared to the medical sciences where it is 5.5 years before 90% of the effect is seen.

We obtained statistically significant and important coefficients for some of the control variables. First, the variable capturing teaching load is statistically significant and important for all fields except engineering. The coefficient is always negative, confirming that large undergraduate teaching loads have a disruptive effect on scientific production. The biggest effect is in the medical sciences. In this case, an increase of one additional undergraduate student per research staff member has the effect of reducing research output by about 5%. Second, and rather surprisingly, a higher allocation of funds to London based universities results in a slightly less productive system in the social sciences. Third, also contrary to expectation, we found some evidence to support the view that a bigger allocation of funds to the Group 94 universities would result in an overall higher research output in engineering and the medical sciences;

no significant effect was identified for the Russell Group universities.[9] Fourth, a larger share of funds to the Medieval universities was shown to result in a more productive system in the social sciences, but has a negative impact on the medical sciences. A larger share of funds to the 19th Century universities had a positive effect on engineering and the social sciences research output, and a negative impact on the medical sciences. The comparator groups for university history is the group of universities founded after WWII. Finally, the tests for validity of the constraints were never rejected.

3.2 Citations

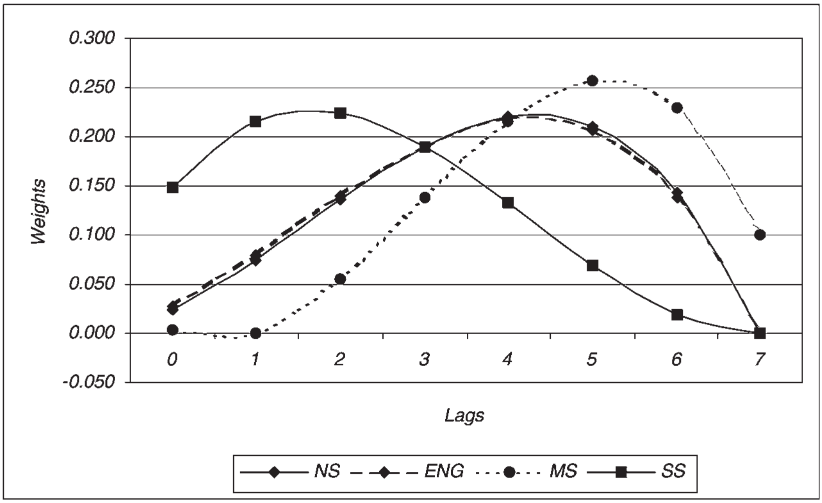
Citation output was analysed following the procedure used for publications. Figure 2 shows the pattern of weights for the different disciplines. The results appear similar to those for publications. The citation output tends to respond more quickly to an increase in R&D investment in the social sciences than the other scientific fields. The medical sciences shows its largest research impact only at the end of the time period, while the polarisation is less strong for the natural sciences and engineering. The main difference from the publication lag structure results is the very similar pattern for engineering and the natural sciences: engineering has a less symmetric profile and behaves much more like the natural sciences.

Table 3 (see p. 84) presents the results for the estimation of model (5) in the case of citations output. In terms of long run science budget elasticity the results are very similar to the results for publications. The highest elasticity is found in the medical sciences (0.61), while the lowest is in engineering (0.15), which is non-significant. The time trend variable is always positive and significant in three of the fields, once again pointing to an increase in scientific opportunities and the existence of international spillovers. Regarding the impact distribution of changes in the research budget, the earliest impact is in the social sciences where 90% of the effect is observed after about 4 years, while in the medical sciences 90% of the effect is achieved only after 6 years.

Regarding the remaining control variables, the results tend to be consistent with those for publications, with the exception of the control variable for the Russell Group universities: a larger allocation to Group 94 universities does not have a positive effect on the system output, while a higher share of funds to Russell Group universities has a positive impact on citations in the medical sciences, but a negative impact in engineering. The teaching variable is again

[9] The higher output could be due to two phenomena: higher productivity of the Group 94 universities, or the competition effect from the other universities which received less funds. University level micro data would be needed to disentangle these two effects. This reasoning applies to the other resource allocation control variables.

Figure 2 Restricted Pattern of Weights (Citations), by fields



negative, with the highest absolute value in the medical sciences. A bigger allocation to London based universities does not provide any positive advantage. Larger proportions of direct funding to Medieval universities result in a higher citations output in the social sciences and lower returns in the medical sciences, similar to the 19th Century universities, with the adjunct

Figure 3 Restricted Pattern of Weights (Graduate Students), by fields

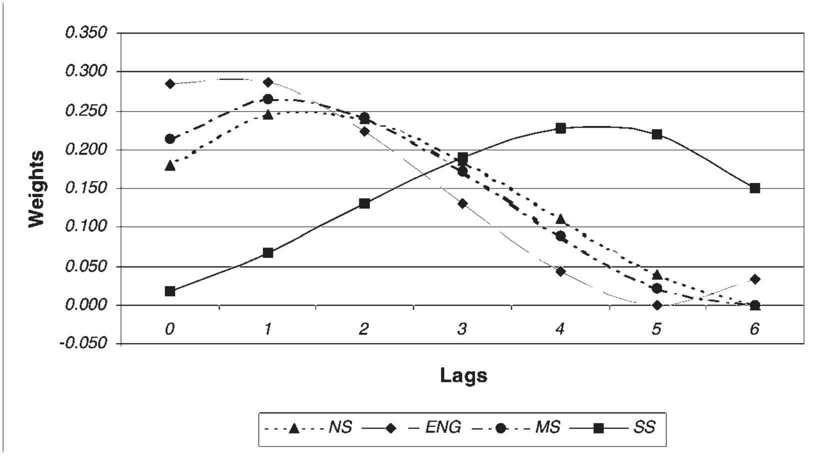


Table 3 UK Levels Estimates, Citations (Method: field fixed effect)

	NS	ENG	MS	SS
R&D	0.212	0.146	0.617	0.353
	0.123*	0.196	0.160***	0.095***
Year	0.014	0.037	0.005	0.032
	0.008*	0.009***	0.011	0.007***
Undergraduate Teaching	– 0.031	– 0.018	– 0.059	– 0.016
	0.010***	0.014	0.012***	0.007**
London	0.001	– 0.011	0.007	– 0.008
	0.003	0.003	0.003	0.005
Group94	0.001	0.004	0.003	0.001
	0.004	0.003	0.003	0.005
Russell	0.004	– 0.004	0.004	0.000
	0.003	0.005**	0.004*	0.005
Medieval	0.002	0.005	– 0.018	0.013
	0.005	0.005	0.007***	0.005***
19th Century	0.001	0.006	– 0.008	0.009
	0.004	0.003**	0.004*	0.004**
20th Century	0.008	0.004	0.002	0.018
	0.005	0.007	0.006	0.012
Constant	– 22.493	– 67.690	– 10.691	– 63.943
	12.946*	16.240***	18.718	12.118***
Observations	108	84	66	84
R-squared	0.68	0.77	0.84	0.67
Chi(2)	1.398	2.760	8.635	7.160
P > Chi(2)	0.966	0.838	0.195	0.306
50% Quartile Lag (years)	4.4	3.3	4.3	1.6
90% Quartile Lag (years)	5.5	5.3	6.0	3.9

Robust standard errors reported below each coefficient. Within R-squared reported.
(*) significant at 10%; (**) significant at 5%; (***) significant at 1%

of a positive impact for engineering. Finally, as before, the constraints implied by the model were not rejected.

3.3 Graduate students

The third science output we examined at the field level for the UK is the ‘production’ of graduate students. Figure 3 (see p. 83) shows the lag structure. The most interesting result of this analysis is that the patterns appear quite different from the patterns for publications and citations. This result might be because graduate students are a research output of a completely different nature to publications and citations. In the case of graduate students, the medical sciences, engineering and the natural sciences show the strongest impact quite quickly (in the first three years), while the impact in the social sciences does not become evident until towards the end of the time frame. We do not have a definitive explanation for this result, but we incline to the view that the combination of a different mix of graduate courses (MSc, Mphil and PhD) across the different fields might be generating these sorts of differential impacts.

Table 4 (see p. 86) shows the results of estimating model (6) using field fixed effects. The largest elasticities regarding this type of research output are found in the natural and the medical sciences with values of 0.54 and 0.65 respectively. The corresponding elasticities for the social sciences (0.21) and engineering (0.11 and non-significant) are much lower. The time trend had a positive coefficient in all fields except the natural sciences. This points to an increase in productivity in the social and the medical sciences and engineering regarding the 'production' of graduate students. In terms of the impact distribution of changes in the research budget, the most immediate impact is in engineering, where 90% of the effect is observed after about 3 years, while the most delayed impact is in the social sciences where it takes 5.3 years for 90% of the effect to be felt.

Regarding the remaining control variables there are some interesting results. First, the undergraduate teaching variable is negative in the natural sciences, the medical sciences and engineering, pointing to the fact that in these fields an increase in the undergraduate teaching load negatively affects the time allocated to supervising and guiding graduate students. In contrast, in the social sciences we have a positive impact from undergraduate teaching towards graduate teaching, pointing to an apparently different nature of graduate studies in this field, a possibility that requires much more analysis for it to be confirmed. Second, as before, there was no evidence of a positive localisation effect for a higher allocation of grants and contracts to London based universities. Third, a bigger allocation of funds to the universities belonging to the Group 94 had a positive premium in engineering, while for those in the Russell Group the biggest premium was in the social sciences. Fourth, in terms of age, a higher share of funds to the Medieval universities has a positive effect in the social and the medical sciences, while more funding to the 19th Century universities induces an increase in the university system output in the medical sciences, but a decrease in the natural sciences. This last result also applies to the 20th Century universities, which also showed a positive premium in the social sciences. The comparator group, as in the previous two estimations, was the category of the Post WWII universities. Finally, as before, in all the models the constraints were not rejected.

Field level estimates provide us with an interesting set of results. Most of these are novel to the literature on the economics of science and, thus, should be seen as preliminary and exploratory, to be confirmed by further analyses. First, in the case of the medical science, the social sciences, and the natural sciences we can identify positive and significant returns for publications,

Table 4 UK Levels Estimates, Graduate Students (Method: field fixed effect)

	NS	ENG	MS	SS
R&D	0.542	0.107	0.656	0.214
	0.200***	0.173	0.158***	0.091**
year	– 0.024	0.027	0.044	0.064
	0.011**	0.009***	0.011***	0.007***
Undergraduate Teaching	– 0.062	– 0.044	– 0.024	0.015
	0.015***	0.020**	0.010**	0.006***
London	0.002	0.009	– 0.01	– 0.005
	0.004	0.004	0.004	0.003
Group94	0.003	0.003	0.005	– 0.005
	0.006	0.005*	0.006	0.002
Russell	0.005	– 0.003	– 0.009	0.003
	0.004	0.005	0.006	0.002*
Medieval	– 0.015	0.004	0.028	0.006
	0.009	0.005	0.014*	0.004*
19th Century	– 0.014	– 0.005	0.017	0.003
	0.007**	0.004	0.008**	0.004
20th Century	– 0.014	0.008	0.002	0.015
	0.008*	0.007	0.003	0.007**
Constant	47.113	– 48.338	– 93.097	– 124.08
	18.987**	16.708***	19.154***	13.174***
Observations	99	77	66	77
R – squared	0.70	0.65	0.87	0.92
Chi(2)	3.005	0.635	10.952	9.803
P > Chi(2)	0.699	0.986	0.052	0.081
50% Quartile Lag (years)	1.3	0.8	1.1	3.8
90% Quartile Lag (years)	3.5	2.8	3.1	5.3

Robust standard errors reported below each coefficient. Within R-squared reported.
(*) significant at 10%; (**) significant at 5%; (***) significant at 1%

citations, and graduate students from investment in higher education R&D. Although positive, the effect for engineering is only significant in the case of publications, pointing to the fact that the research output from this scientific field is better captured by measures other than citations and research students. Second, the four scientific fields tend to have different lag structures. This is particularly noticeable in the case of the social sciences. While investment in R&D in the social sciences affects publications and citations more immediately than in the other three fields, in the case of gradate students most of the returns to research grant and contract funding are concentrated at the end of the period. Third, we found strong evidence that a high undergraduate teaching load negatively affects the research outputs of UK universities. Only in the case of graduate students in the social sciences did we find a positive effect. Fourth, we constructed a set of control variables to assess the importance of allocation of grants and contracts to different subgroups of universities (university specific effects). Some of these were significant and important, pointing to the fact that different allocations of funds to universities result in higher or lower university system scientific production. The higher or lower output may be due to higher productivity in the institutions that received more grants and contracts or a competition effect from the universities that received less funds. Micro data at the level of the university would be needed to identify which of the two effects is dominant.

4 The UK knowledge productivity analysis

This section focuses on the efficiency with which the domestic stock of knowledge (science budget and other grants and contracts) is applied in order to generate the different research outputs. Has this efficiency grown over time or has it declined across disciplines? Building on the results in section 3 we computed field specific total factor productivities (TFPs). These TFPs capture the evolution of the scientific opportunities in each field, and also the effects of changes in organisational practices, resources allocation, and management.

For each macro field we computed the residual of the knowledge production function (6) as:

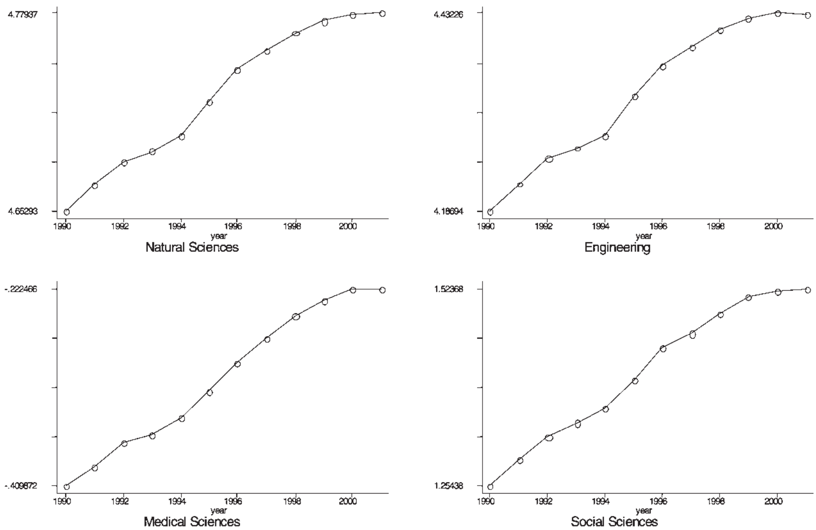
$$tfp_{it}^F = y_{it}^F - \beta^F W(r)_{it}^F, \quad i = 1, \ldots\ldots N;\ F = 1, \ldots J \tag{6}$$

where tfp_{it} is the knowledge production function (semi) residual after controlling for changes in $W(r)_{it}$, the distributed lagged function of real past R&D expenditures. In order to compute (6) we first need an estimation of the elasticity coefficients by field (the βs). We use the field level results shown in Tables 2 (see p. 81) to 4. Given the lags used in the construction of $W(r)_{it}$ we can only focus on productivity evolution during the 1990s.

Figures 4 (see p. 86), 5 and 6 (see p. 90/ 91) show the evolution of the TFP index by field over time for each of the research outputs. Two clear patterns emerge. In all macro fields and research outputs there is an upward trend in the productivity indices, suggesting that there is a clear improvement in the efficiency and technological opportunities of the system. In all four major scientific fields and for the three traditional outputs of scientific research, the productivity of UK science has increased along the 1990s. However, from the mid 1990s, in all the macro fields, there has been a marked slowdown in productivity growth rates as highlighted by the less steep slopes of the productivity indices at the right of the figures.

Across the whole period the TFP growth rate in the case of publications has fluctuated between 1.2% and 2.4%, with the lowest value in the natural sciences and the highest in the social sciences. Taking the cut-off point of 1996 (chosen to coincide with the 1996 RAE), the average TFP growth rates during

Figure 4 TFP Publications



the first half of the 1990s was compared to the same indicator for the 1990s to 2001. The data show a remarkable slowdown in productivity, TFP productivity growth rates declined by more than 50% in the natural sciences, engineering (the largest decrease) and the social sciences, but by 'only' 22% in the medical sciences. Numbers of citations show a similar profile. The highest growth rate is in engineering (2.4%) and the lowest in the medical sciences (0.8%). There is also a clear slowdown in productivity growth rates, but the degree of the decline is even greater than in publications. Finally, the results for graduate students are similar to those for citations with the exception that the highest growth rate occurs in the medical sciences. The slowdown in the second half of the 1990s is also remarkable: in engineering and the natural sciences TFP growth rates halved, while in the social and the medical sciences TFP growth rates are 60% of their value in the previous period.

It is important to note that the productivity slowdown is not an artefact of the increased spending in UK science. The real increase in science and engineering R&D spending in the UK started in 2000–01. In our model the impact on research outputs of an increase of about 7% in 2000–01 is spread across the succeeding six to seven years; the weight for the first year is small in the case of publications and citations (lower than 10% for all except the social sciences) and about 25% in the case of graduate students (again excepting the social sciences for which it is near zero). A significant increase in R&D spending in a particular year can negatively affect the overall productivity of the system in that year if a simple productivity measure based on the ratio

Figure 5 TFP Citations

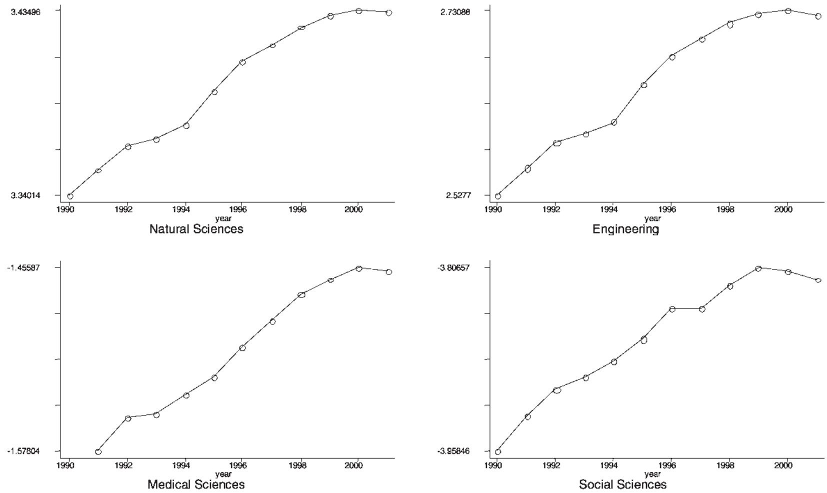
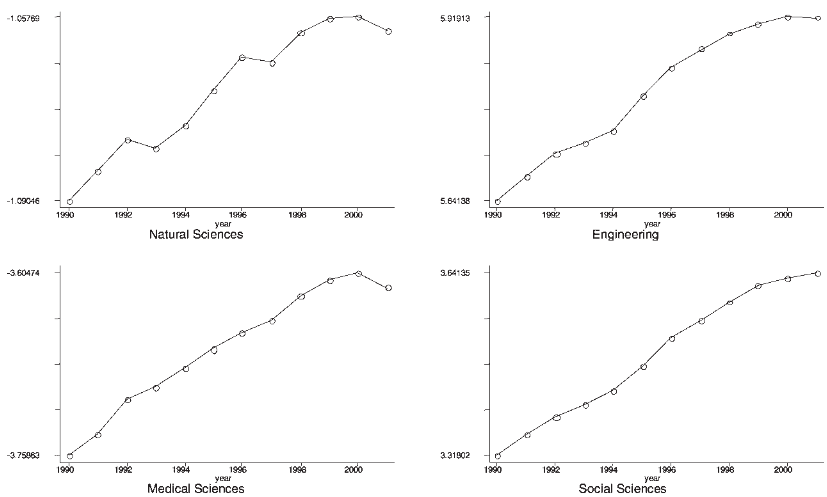


Figure 6 TFP Graduate Students



between that year's inputs and outputs is considered. Our measure of productivity refers to changes in research output that are not explained by changes in the stock of scientific knowledge as proxied by current and past R&D spending. Our estimation of stock of knowledge already controls for the

Table 5 TFP growth rate decompositions for the natural sciences and engineering

Time	Natural sciences					Engineering				
	A	B	C	D	TOTAL	A	B	C	D	TOTAL
91–96	1.5	1.4	1.4	1.2	1.1	3.0	2.8	3.1	2.8	2.7
96–01	0.7	0.8	0.7	0.6	0.6	1.3	1.2	1.4	1.2	1.3
Total	1.1	1.1	1.1	1.0	0.9	2.2	2.1	2.3	2.1	2.1

Note: A controls only for R&D spending; B is A plus controlling for resources allocation by London, Group 94 and Russell Group; C is A plus controlling for University Age; D is A plus controlling for teaching intensity; and Total is A plus (B + C + D).

fact that there are some adjustment lags and that a given increase, for example, in the science budget, is not going to have an immediate impact on research outputs. In the case of a traditional productivity measure, such as the ratio between papers and HERD, the UK has witnessed a very clear decline in the 1990s due to the significant increase in the science budget and not to a deterioration in the performance of the system (Evidence 2003). Our measure of productivity controls to some extent for this and tries to capture organisational or managerial changes in the system.

The TFP estimations above take account only of the spending on research grants and contracts. We now introduce the other control variables to see whether they explain the productivity slowdown. There are several different, and overlapping, explanations. One is that in the period 1996–2001 the distribution of higher education funding led to the system being less productive within each scientific field (B and C estimations). Another is that increased enrolment rates at undergraduate level were not compensated for by an equivalent increase in staff, leading to a reduction in available research time (D estimation). To investigate these two possibilities we re-estimated the TFP indices controlling for how resources are allocated across types of institutions and for teaching intensity ratio. The results for publications are presented in Tables 5 and 6.

Two trends emerge from Tables 5 and 6. First, at field level the process of resource allocation has no serious impact on productivity growth because controlling (or not) for how resources are distributed across university types and geographical location (columns B and C compared to column A) only marginally affects average productivity growth. The exception is the social sciences where the distribution of higher education funding in the first period compared to the distribution in the second period, which led to the system being less productive, reduces the unexplained productivity slowdown (for example, in column A the difference between the two time periods is 1.6; in column C the difference is 1.2).

Table 6 TFP growth rate decompositions for the medical and social sciences

Time	Medical sciences					Social sciences				
	A	B	C	D	TOTAL	A	B	C	D	TOTAL
91–96	2.0	2.0	1.9	1.3	1.2	3.2	3.2	2.7	3.2	2.8
96–01	1.4	1.3	1.3	1.4	1.2	1.6	1.8	1.5	1.7	1.8
Total	1.7	1.7	1.6	1.3	1.2	2.4	2.6	2.1	2.5	2.3

Note: A controls only for R&D spending; B is A plus controlling for resources allocation by London, Group 94 and Russell Group; C is A plus controlling for University Age; D is A plus controlling for teaching intensity; and Total is A plus (B + C + D).

The results controlling for teaching intensity are similar and, again, are relatively invariant. The exception is the medical sciences where, after controlling for teaching intensity, productivity growth rates reduce from 1.7% to 1.2% (row total) and the two sub-periods show no productivity slowdown. Interestingly, after controlling for teaching intensity TFP in the first period drops to 1.3, pointing to the fact that the reduction in teaching intensity in this discipline actually contributed to the higher productivity during the first time period.[10] For the other research outputs the conclusions are similar to those for publications.

Controlling for research allocation and teaching intensity partially explains the productivity slowdown in the medical sciences (especially in the case of publications), but does not account for the productivity slowdown in the second half of the 1990s for the other scientific fields.

There are four possible reasons for this unexplained slowdown. First, there might have been a deterioration in the organisational efficiency of production of traditional science outputs within each field (and even within departments) due, for example, to the creation of incentives for the development of third stream type activities. Second, there might have been a reduction in human capital (the quality of labour), i.e., in the research staff. Underlying this hypothesis is the possibility that the lag in the relative compensations paid to researchers in the universities could have led to some high skilled staff leaving academia (for positions overseas or for jobs in industry), being replaced by an equivalent number of lower quality personnel. Third, due to the increase in other countries' publishing in English, UK researchers are facing increased

[10] Student to staff ratios in the medical sciences decreased from 8.4 to 7.4 students per staff across the whole period. A more detailed inspection shows that any decrease was mostly during the first sub-period. The ratio of students to staff in 1991–95 declined annually from 8.4 in 1991 to 6.9 in 1995. This variable was more volatile in the second sub-period oscillating between 6.9 and 7.3.

competition for publication in ISI journals, raising the bar to getting published (a quality effect).[11]

All of these are pessimistic explanations for the productivity growth slowdown. There is a fourth possibility, which is more optimistic, which is that the RAE has an impact. We can think of the RAE as a sort of institutional shock in the research incentive system for academic units. That is, the introduction of the RAE at the end of the 1980s/beginning of the 1990s produced a positive shock, which induced a productivity increase on the part of UK scientists. If this shock were affecting productivity levels rather than growth rates, after a transition period the system would return to its average growth rate. In other words, the effect of the RAE may have been more dramatic in the early 1990s, but subsequently declined. This could explain the productivity slowdown in the second sub-period considered in our analysis.

It is very difficult to identify which of these potential explanations is the most relevant. Alternative models based on micro data at the university and unit of assessment levels could help to clarify the current dynamics of the UK science system.

5 Conclusions

This paper has analysed the determinants of the three most common university research outputs: publications (as a proxy for the production of codified research knowledge); citations (as an impact adjusted proxy for codified research production); and Masters and PhDs awarded (as a proxy for the production of tacit knowledge accumulated in human capital) for the UK case.

The analysis of the UK science system as represented by the old universities (which account for about 90% of R&D expenditure) points to the existence of different science production functions. We rejected the model of a global science production function for the UK in favour of four very broadly defined macro-fields: the medical sciences, the social sciences, the natural sciences and engineering. In each of these fields either the weight patterns or the R&D elasticities (and also some of the coefficients of control variables) were significantly different.

For publications and citations we estimated significantly different lag structures, with a long lag for the medical sciences before full returns from an increase in R&D spending were achieved, but the social sciences seeing results in the first few years. This means that the science system does not

[11] There is some evidence of this phenomenon in the discussion in the *New York Times* (May 3, 2004) about the loss of dominance of the US in the sciences to non-English speaking countries.

respond uniformly to changes in funds. For example, an increase in the overall science budget will have a rather sequential impact: first, the changes will be felt mainly in the social sciences, then in engineering and the natural sciences and finally, in the medical sciences. For graduate student research output the results are different, with the short term impact being concentrated in engineering and the long term impact in the social sciences.

In the case of the medical sciences, the social sciences and the natural sciences we identified positive and significant returns for publications, citations and graduate students from investment in higher education R&D. Although positive the effect in engineering is only significant in the case of publications, pointing to the fact that the research output of this scientific field is better captured by other measures than citations and research students.

We included in the models a set of control variables. We found strong evidence that a large undergraduate teaching load negatively affects the research outputs in UK universities. Only in the case of graduate students in the social sciences did we see a positive effect. Overall, the higher the teaching load the lower the research productivity. This result denies the validity of the policy model followed in the 1980s and 1990s, which assumed that the number of students per lecturer could be increased without a decrease in the overall quality of the HE system.

We also controlled for the impact of different allocations of funding across types of institutions; the results are mixed and vary according to the different research outputs. Some were significant pointing to the fact that different allocations of funds to universities result in higher or lower university system scientific production. Due to the limitations of field level data, the results on university specific factors, though interesting, should be considered as preliminary: they require validation through analyses based on micro data.

Finally, we developed an analysis of the productivity of UK science and the changes in it during the 1990s. UK TFP has grown across the whole period. This result contrasts with the most standard publication per HERD measure of productivity, which presents a remarkable drop in British productivity, mainly due to a combination of increased budget and publication lags. However, we also identified a clear slowdown in TFP growth in the second half of the 1990s compared to the first. This decline is not due to an increase in the research spending in the later period, nor to the way that resources were allocated across institutions (although this did have some effect in the medical sciences and the social sciences), nor to an increase in teaching loads (which were fairly static in the second half of the 1990s). We speculate that this slowdown in productivity is due to mainly unobserved systemic effects (a policy shock during the first half of the 1990s such as the RAE) or very micro factors related to the (relative) reduction in researchers' rewards, the introduction of more transferable research or a 'brain drain' of high skilled researchers. This slowdown can also be ascribed to increased competition for publication in ISI journals from overseas research. Without more micro data it is not possible to tease out from these alternative explanations their relative importance. These results are consistent with the results of international analysis, which point to a decrease in the relative productivity of UK science. Indeed, it is possible to envisage that, during the 1990s, UK science showed positive productivity growth, but that this growth was less marked than in other countries, especially in the second half of the 1990s.

This paper aimed to test the feasibility of using econometric models to produce results that could contribute to the development of science policy, the aim being not to produce exact indicators of the dynamics of the science system, on the basis of which to draw strong policy conclusions. Rather, the inherent shortcomings in the measurement of the output (and ultimately of the outcomes) of the scientific activity, and the limitations on the available input data call for extreme caution in the interpretations of our results. The conclusions presented above should be taken as a first and preliminary attempt to develop a better understanding of the relationship between the allocation of resources and scientific research output.

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The Productivity of UK Universities

Comment by

Christian Pierdzioch

In an integrating world economy, scientific research leading to the invention of new products and the development of unconventional ideas forms the basis of the international competitiveness of firms, industries, and entire economies. It is for this reason that it has often been emphasized in academic research and in policy circles that developing a deeper understanding of the factors that affect the quantity and quality of research output is of key importance for the prosperity and international competitiveness of economies. One factor that may affect research output is the availability of financial funds. But how exactly does the availability of financial funds affect research output? How to measure research output? Are the effects of the availability of financial funds on research output different across disciplines? Does the availability of financial funds affect research output immediately or only with a time lag? These are all important questions, and finding answers to these questions is a difficult task. Gustavo Crespi and Aldo Geuna have used a novel database on the productivity of scientific research at UK universities to empirically tackle this task. Their empirical analysis is highly welcome because it yields interesting insights into the factors that affect research output, and because it has been competently and thoroughly done.

In order to conduct their empirical analysis, Crespi and Geuna have collected data on the productivity of scientific research at 52 UK universities. Their data cover the period 1984–2002. They present results for four major fields of science: engineering, the natural sciences, the medical sciences, and the social sciences. They have used the number of publications, the number of citations, and the number of graduate students to measure research output. In their empirical analysis, they have used a production function to link their measures of research output to expenditure on research and development (R&D). In order to capture potential time lags between expenditure on R&D and research output, Crespi and Geuna have estimated a polynomial distributed lag model. They have estimated this model using techniques available for estimating panel data models. Their model contains a number of control variables, including a measure of the undergraduate teaching load and measures of the localization and reputation of a university.

Crespi and Geuna present a number of interesting arguments and results, and every single argument and result deserves to be discussed in detail. In the following, I shall focus on potential problems that may arise in the measurement of research output, the specification of the production function, and the interpretation of the empirical results.

As concerns the measurement of research output, one problem is that counting the numbers of publications is maybe a good indicator of the quantity of research output, but not necessarily of the quality of research output. For example, not all academic journals are highly ranked "top" journals, and getting a paper published in an international "top" journal is much more difficult than getting it published in a national journal or a highly specialized field journal. For this reason, the scientific community has developed sophisticated ranking schemes in order to capture the importance and the impact of academic journals. In consequence, it would be interesting to analyze how the results Gustavo Crespi and Aldo Geuna report would change when rankings of journals were used to weigh publications. Rankings of journals may also be useful as a weighting scheme for citations because it could make a difference whether a research paper is mainly cited, for example, in a specialized field journal or a general interest journal.

As concerns the specification of the production function, it would be interesting to learn more about potential problems caused by omitted variables and the potential influence of control variables different from those used by Crespi and Geuna. As concerns potential problems caused by omitted variables, it could be the case that the positive link between research output and expenditure on R&D reported in the paper is at least in part due to the influence of a third variable not yet included in the empirical model. One such variable could be a measure of the stance of the business cycle. For example, in a business cycle boom, tax revenues and, because of a stock market boom, the budgets of private foundations increase. This may lead to an increase in expenditure on R&D. At the same time, expenditure on R&D by firms is likely to increase, firms may hire researchers, and the salaries paid by firms may also increase. This could strengthen the competition between firms and universities for researchers, resulting in an increase in research output.

As concerns control variables, it would be interesting to include, for example, the number of researchers per field, the number of research seminars and scientific conferences that took place at a university, and the number of visiting researchers as control variables in the production function. These variables may give a good account of the reputation of a university. Moreover, these variables may proxy the quality of the research environment at a university. Of course, collecting data on these variables could turn out to be very difficult. Given that path dependencies may play an important role for the reputation of a university and the quality of the research environment, it could be interesting to use lagged research output as a control variable in the production function.

As regards the specification of the production function, it would also be interesting to learn more about the interpretation and the statistical properties of the explanatory variables. For example, the authors have included a time trend in the vector of explanatory variables, and they argue that the time trend captures spillovers from abroad. It would be interesting to learn more about these spillovers from abroad. Do they represent the international exchange of ideas? Or do they represent the importance of forming international research teams? One could also ask whether the time trend reflects spillovers from abroad or rather captures structural breaks or stochastic trends in the data.

As concerns the interpretation of their empirical results, the authors mainly focus on the slowdown in productivity of research at UK universities that took place at the end of the 1990s. This certainly is an important result that deserves a comprehensive and thoughtful analysis. The authors, however, report many more interesting results, and it would be interesting to learn more about how these results can be interpreted. For example, Crespi and Geuna report that, regarding the number of publications, the effects of expenditure on R&D are very different across disciplines. Natural questions that arise as regards this result are: Why are there differences across disciplines? Why is it important to learn more about differences across disciplines? Are there any policy implications? Should we spend more or less money on R&D at universities? Is the allocation of expenditure on R&D across disciplines optimal? It is impossible to answer all these questions in one paper. However, I suggest that the paper could benefit from considering one or the other of these questions.

To sum up, Crespi and Geuna have done very interesting research, and they have undertaken their empirical research with care. Their paper is concisely written, and I have learnt a lot from reading it. I believe that their paper will stimulate future research.

Evaluation of Researchers: A Life Cycle Analysis of German Academic Economists

by

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1 Introduction

Evaluations compare certain features of a person with the features observed in a group of peers. A worthwhile evaluation needs to explicitly define the relevant comparison group and to make a case for the employed choice. In many cases, the contemporaries of the person to be evaluated represent the relevant peer group, the best example being the standard IQ test whose name even refers to the fact that intelligence is measured in relation to some denominator, which is, of course, the respective person's age. In sports, where evaluation almost represents the raison d'être, it is also quite common to compare contestants of the same age group, but other comparison groups, based, for example, on body weight or professional status, are also widely employed.

Research evaluations that are based on scientometric methods are still surrounded by a touch of controversy. Nevertheless, it is generally accepted that reasonable scientometric evaluations need to focus on narrowly defined disciplines; how the disciplines should be delineated is, of course, another matter. Many scientometric studies are, moreover, restricted to specific geographic regions and types of institutions. Apart from these public-domain characteristics, the relevant peer group is also described by personal characteristics, arguably the most important one being the researcher's age.

Age features two distinct dimensions that are relevant in the evaluation context: vintage and career age. Both of these dimensions are liable to have a strong impact on research productivity because research production heavily relies on human capital that is determined, on the one hand, by the initial endowment (i.e., by ability and initial training) and, on the other hand, by experience and obsolescence of knowledge. Since initial training (graduate education) is related to the *age cohort*, whereas experience and obsolescence of knowledge are related to *career age*, both of these age dimensions represent personal characteristics that are associated with generally recognized peer groups (class of 2005, assistant professors in their sixth year, etc).

Precisely because life-cycle and vintage effects are liable to influence any researcher's productivity, research evaluations which are undertaken to

* We thank Robert Hofmeister and Philipp Stützle for valuable research assistance.

implement incentive-compatible managerial reward or penalty schemes need to take these age dimensions into account. In principle, this statement is not controversial. Tenure and promotion committees have always compared the track records of the applicants with precedents. Alternatively, they have judged whether the track records are compatible with an established policy or standard. These standards, however, have evolved over time by investigating research oeuvres of applicants who, by the very fact that they aspired to take a certain career step, constitute a peer group defined by *career age*. Decisions with respect to performance-related pay have likewise been based on comparisons of track records. Since remuneration, unlike tenure and rank, does not represent a time-invariant prize, the applicant's age at the time of the application, i.e., his or her *cohort* or *vintage*, is always implicitly taken into account by the responsible authorities.

Even though of great importance for management decisions, studies dealing with the evaluation of *economic* research have hitherto rather neglected the age dimensions. This neglect applies especially to studies that evaluate entire groups of researchers, for example university departments or research institutes. An exception is the ranking study by Combes and Linnemer (2003). These authors, who rank 600 economic research institutions from 14 European countries, present, among others, one research productivity index that takes the respective researcher's career age into account. Even though the employed method of normalization with respect to career age is purely ad hoc, and the career age of the economists is estimated by rule of thumb, this study is groundbreaking because it spells out the demands that high-quality rankings should meet.

The available literature on life cycles in research productivity is oddly disconnected from the evaluation issue. The studies investigating life cycles are usually motivated by Gary Becker's human capital theory that predicts that investment in human capital decreases over the life cycle, thereby generating hump-shaped individual life cycles in labor productivity and earnings. Some scholars have extended the human capital approach to analyze the processes which are specific to research production. Others have used the standard human capital approach in order to guide their attempts to empirically identify the determinants of labor productivity; these scholars focus on research production mainly because measuring research productivity is, in many respects, easier than measuring labor productivity in other fields. The AER paper by Levin and Stephan (1991) followed both of these routes and was instrumental in kicking off the field that is now known as the *economics of science*.

Surprisingly few studies on life cycles in research productivity were written by economists or investigate the economics profession. This has already been deplored by Paula Stephan in her (1996) JEL survey. Recent work on the economics profession include Kenny and Studley (1996), Oster and Hamermesh (1998) and Baser and Pema (2004) whose empirical results are compatible with a hump-shaped progression of individual research productivity over the life cycle as hypothesized by Becker's human capital theory. Goodwin and Sauer (1995), on the other hand, who do not clamp the life cycle in the Procrustes bed of a quadratic specification, identify a bi-modal life cycle. Hutchinson and Zivney (1995) and Hartley et al. (2001) do not find any evidence supportive of the standard life cycle hypothesis at all.

Among the many considerable econometric problems that arise when estimating life-cycles in research productivity, the most challenging one arguably consists of separating career age and cohort effects, an endeavor that is confounded by the fact that publication behavior has changed over time. In order to estimate life cycle and cohort effects separately, an extensive panel data set comprising many cohorts is indispensable, otherwise the potentially considerable cohort-specific influences cannot be estimated, and the resulting estimates of the life cycle pattern will be biased.[1] It is conceivable that, because of these econometric problems, the empirical evidence with respect to cohort effects is somewhat elusive. Basar and Pema (2004) do not find any cohort effects at all, and Goodwin and Sauer (1995) report only marginally significant effects that are tainted since they may well reflect the fact that the members of the analyzed cohorts differ in age, implying that the older cohorts are composed of academic survivors and thus liable to have been more productive on the average.

The identification problem becomes even more challenging if one acknowledges that the publication behavior of economists has changed over time. Even if these changes have been relatively small, they may become significant in the course of a time period that allows estimating cohort effects. Since, however, career time, historical time and cohort affiliation depend on each other in a linear manner (career time = historical time – cohort "birth" year), only two out of the three effects can be estimated subject to some assumption about the development of the third one. This is the reason why all estimates of life cycle and cohort effects need to be interpreted with some caution.[2]

This paper unfolds as follows. In the next section we present a new data set that describes the research behavior of German academic economists, and in section 3 we describe the heterogeneity of research production with respect to both age dimensions (career age and cohort affiliation). Our investigation of

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[1] Cohort-specific influences are, for example, the knowledge base transmitted during graduate education, the rate of obsolescence, access to resources, opportunities provided by the socio-economic environment, and modes of behavior imprinted on the fledgling scientists. See Stephan (1996, 1216-7).

[2] For a detailed exposition of the econometric methods that have been proposed to identify age, cohort, and period effects on individual research productivity, see Hall et al. (2005).

heterogeneity culminates in the presentation of a simple formula that translates any German economist's research oeuvre into a ranking vis-à-vis his or her peers. Section 4 describes the results of some life cycle regressions. Since tenure represents the arguably most important special feature of the academic labor market, we analyze, in section 5, the persistence of individual research productivity in order to assess at what career stage promotion to a tenured position is justifiable. In section 6, we turn to evaluations of whole research units (German economics departments) and present some rankings that take the age dimension into account.

2 The data set

Most studies of research productivity over the life cycle employ a sample of scientists who are relatively active in research. The rationale for this approach is twofold. On the one hand, the behavior of choice researchers is better documented than that of less active ones. On the other hand, the standard econometric methods are better suited to process steady streams of activities than time series with many periods of inactivity. Since it is our intention to develop an evaluation scheme for all kinds of scientists, we did not follow this restricted approach and compiled a dataset that comprises, in principle, all academic economists currently working in Germany.

Since we use the EconLit data base we had to restrict ourselves to economists who received their doctoral degree at the earliest in 1969, the first year covered by EconLit. Considering that German academic economists receive their doctoral degrees when they are about 30 years old, this implies that the oldest economists in our data set were about 65 years old in 2004, the last year covered in our study. For these economists, we thus have complete life cycles. For the younger ones, the available life cycle becomes, of course, increasingly shorter. The shortest life cycles that we decided to consider have a length of six years which corresponds to a career age at which promising academic economists are granted tenure. We thus only consider scholars who received their doctoral degrees between 1969 and 1998 and who were employed at a German university in the year 2004 or have retired from such a position shortly before.

On the basis of these restrictions we have analyzed the publication records of more than 600 economists. To be more precise, our data set is comprised of all EconLit-listed journal publications (up to the year 2004) authored or coauthored by the economists included in our sample. Evaluating only the set of journals referenced in EconLit excludes journals whose scope is not aligned with the current mainstream of economic research, new economics journals, and journals that do not meet EconLit's quality standards. Whereas scope and timeliness are issues to be considered (scholars with peripheral or interdisciplinary specializations and scholars working on emerging fields may be underrated), exclusion because of insufficient quality does not appear to be an issue since the minimum quality standard set by EconLit is rather soft.

The quality standards set by the journals indexed in EconLit are of course quite diverse. Any study working with this data base therefore needs to capture quality differences in one way or another. If a reward scheme does not take these quality differences into account, the scientists would no longer attempt to produce research output of the highest possible quality but would rather shift their efforts towards producing results that are just about publishable in the journals with the softest quality standards. In other words: "Gresham's law of research evaluation" would see to it that mediocre research drives good research out of circulation.

A popular approach to controlling for journal quality is to use a subset of journals whose prime quality is uncontested. The ranking study by Kalaitzidakis et al. (2003), for example, followed this strategy. Restricting the journal set in this manner comes, however, at a significant cost. First of all, information especially about less accomplished scientists who do not publish in prime journals is lost with the consequence that reward schemes based on such a set of journals would not provide any incentives for this class of employees. A second drawback of restricting the journal set is that this strategy would prohibit us from investigating changes in research *quality* over the life cycle. For these reasons we decided to work with the whole set of journals indexed in EconLit, and to explicitly control for journal quality.

The evaluation of journal quality represents a field of its own. From the plethora of weighting schemes we chose the "CLpn" scheme proposed by Combes and Linnemer (2003) because it is based on the journals' relative (subjectively perceived) reputation and (objectively measured) impact, and thus appears to provide a well-balanced rating over the whole quality range.[3] The CLpn-scheme converts each journal publication in standardized units of AER-page equivalents. The quality weight of the five top-tiered journals is normalized to unity. The sixteen second-tiered journals' imputed weight amounts to two thirds. Weights then decline in discrete steps (one half, one third, one sixth) down to the minimum weight of one twelfth. Our variable that measures research productivity of researcher i on an annual basis (year T) is defined as follows:

$$CLpn_i(T) = \sum_k \frac{p_{k(i)} w_{k(i)}}{n_{k(i)}}, \tag{1}$$

[3] One disadvantage of this method is that journal quality is kept constant over the period of investigation that covers, after all, a time-span of 36 years.

where $p_{k(i)}$ and $n_{k(i)}$ denote the number of pages and the number of authors of researcher i's publications k, while $w_{k(i)}$ denotes the appropriate journal quality weight. The CLpn-index thus not only controls for quality but also for the number of authors and the length of the journal articles.[4]

In order to obtain comparable individual life cycles of research productivity, we merged the annual records of individual research productivity with the year in which the respective researcher obtained the doctoral degree, i.e., we align the individual life cycles by this reference year. Our data set also contains some coarse information about the included economists' field of specialization, and we also documented the researchers' gender. Only about 7.5% of our academic economists are women. 15% of the economists in our sample specialize in microeconomics, 26% in macroeconomics, 34% in public economics and 16% in econometrics. Economists who could not be assigned to any one of these fields were assigned to the field OTHER.

3 Describing the landscape of German academic research in economics

In order to obtain a first impression of the size and distribution of the oeuvres of German academic economists, we cumulate the annual research outputs defined in equation (1) from career year -5 until career year t, where 0 denotes the year in which the economists were granted their doctoral degrees:

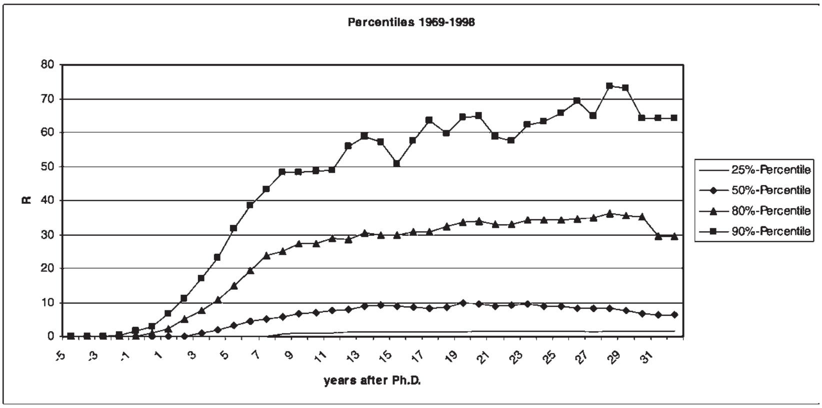
$$R_i(t) = \sum_{T=-5}^{t} CLpn_i(T), \tag{2}$$

and then compute for all career ages t the borderline values of R for the following percentiles: 25%, 50%, 80%, and 90%. The resulting information is depicted in figure 1.

Averaging over all economists in our sample we observe, first of all, that the oeuvre of the median researcher is quite modest. During his whole career the median German economist does not manage to produce more than 10 AER-equivalent pages. Assuming that all of this research has been published in journals belonging to the lowest quality tier, this implies that the median economist publishes about 6 journal articles (20 pages each) during his research career, i.e., one article every six years. Second, figure 1 reveals that the distribution of the individual research oeuvres is skewed to the right and exhibits a large variation. These characteristics do, of course, not come as a surprise. Rather, they constitute stylized facts that have transpired from many

[4] We did not, however, take into account that the number of words per page differs across journals.

Figure 1



related studies.[5] More interesting is the fact that the percentile borderlines are not monotonous and exhibit a marked "overall" concavity. The violation of monotonicity of the stock variable *R* is not as puzzling as it might appear at first sight; it simply reflects cohort effects in our unbalanced panel. If research productivity increases dramatically across cohorts, the stock of the scientists at a young career age (measured across *all* cohorts) may well be larger than the stock of the scientists at an older career age (measured across only those cohorts who have reached this career age). The concavity of the percentile borderlines admits two interpretations: it may either reflect decreasing marginal productivity over the life cycle or it may again represent an artifact of cohort effects in our unbalanced panel.

In order to discriminate between the *decreasing marginal productivity* interpretation and the interpretation that presumes *cohort effects*, we analyzed the career-time oeuvres of different cohorts. For that purpose, we divided our sample of economists into five cohorts, each comprising six age groups. The oldest cohort comprises the age groups 1969 – 1974, and the youngest one the age groups 1993 – 1998. The members of the oldest cohort thus look back on a career of at least 30 years, while the members of the youngest one have had a career of at least six years. The percentile borderlines are now monotonous, indicating that vintage effects within the cohorts are relatively small. Figure 2a (see p. 108) presents the percentile borderlines for the oldest cohort.[6]

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[5] The highly skewed nature of publication was first observed by Lotka in 1926 in a study on physics journals (cf. Stephan, 1996, 1203).

[6] The working paper version of this article also presents the evidence for the other cohorts.

Figure 2a

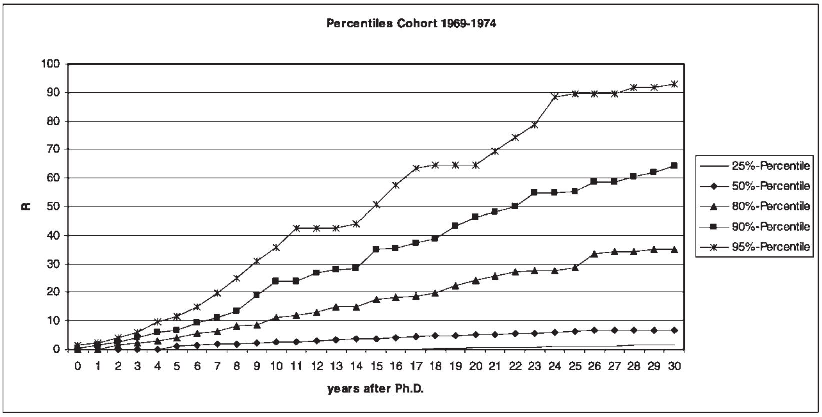
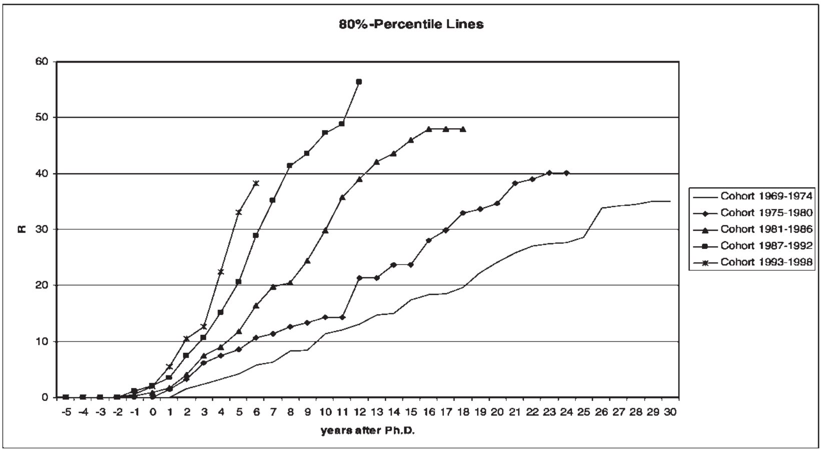


Figure 2b



Two interesting insights transpire. First, eyeballing of the cohort-specific percentile borderlines does not suggest any pronounced concavity. An S-shaped life cycle productivity pattern supporting the factors portrayed by the standard human capital model thus cannot be identified, at least not at the aggregate level. To shed some more light on this issue, we will, therefore, further investigate our economists' life cycles with the help of micro-econometric methods in section 4. The second feature that emerges is more conclusive. The German economics profession is characterized by striking cohort effects in research productivity: the percentile borderlines become increasingly steeper for younger cohorts. The increase in cohort-specific research productivity is illustrated in figure 2b in which the 80%-lines of the five cohorts are superimposed. This representation shows that it took an economist who tops 80% of his peers in the oldest cohort about 18 years to accumulate an oeuvre of 20 AER-equivalent pages, whereas a top-80% economist of the second cohort managed to do so in 12 years. This time span is reduced to 8 and 4.5 years for the two following cohorts, respectively, and the top-80% economist of the youngest cohort only needs 3.5 years to produce 20-AER equivalent pages.

From our data set we can extract information that is directly relevant for the evaluation of individual researchers. In particular, we can assign each economist a peer-specific performance rank at each point of career time. This kind of information is of prime importance for a university management that wants to pursue a rational performance-related remuneration policy. Information about the standing of individual researchers vis-à-vis their peers is, moreover, a prerequisite for department rankings that are insensitive to the age structure of the evaluated faculties. We will turn to this issue in section 6. Whole career profiles in terms of relative performance are, finally, of vital importance to assess the persistence of research performance. The crucial question in this context is whether it is possible to forecast a scientist's research performance from his track record, and if so, at which stage of a scientist's career such forecasts are sufficiently accurate to serve as a basis for management decisions such as granting tenure or awarding substantial research grants. The persistence issue will be dealt with in section 5. Here we will follow up the first issue and ask ourselves how the information about the *current* cohort-specific ranking of *individual* economists can be condensed in such a way that it can serve as a simple management information device.

To do so, we consider the standard situation faced by a university management or a research foundation that would like to assess an economist's relative research standing in the German academic profession. Usually, the evaluator has only access to this person's CV including publication list. With the help of the publication list it is easy enough to compute via equations (1) and (2) the accumulated research output R at the end of the year 2004. Dividing this output R by the *adjusted* career age τ ($\tau = 2010\text{-}Y$, where Y denotes the year in which the evaluated economist received his or her doctoral degree) yields the average research productivity P.[7] How does the average research productivity P of an economist translate into a ranking vis-à-vis his or her peers? Since the relative research standing depends on the average

[7] We let the productive time of a researcher start five years before the doctorate. Since the doctorate takes place in career year $t = 0$, the adjusted career age $\tau = 2004\text{-}Y + 6 = 2010\text{-}Y$.

research productivity as well as on the cohort age of the person to be evaluated, we are seeking a formula of the form $S = f(P,Y)$, where S denotes the evaluated economist's relative research standing in percentiles.[8] Regressing S on Y and P yields the following formula:

$$S = 18.3 - \frac{9.2}{1000} \cdot Y + 0.55 \cdot \sqrt[3]{P}. \tag{3}$$ [9]

For evaluation purposes, the negative residuals of our regression (overestimation) clearly present the relevant downward risk. Since the distribution of residuals resembles a normal distribution with a standard deviation of 0.077, the probability of overestimating a candidate by 10 percentiles is about 10%. This appears to be a risk well worth taking in a situation in which the alternative is to rely on peer evaluations and recommendations that are notoriously biased.

4 A micro-econometric investigation of life cycle productivities

The empirical evidence presented in the previous section suggests that life cycles in economic research productivity are rather flat. This evidence refers, however, to highly aggregated data. In order to do justice to the heterogeneity in our population of economists we exploited the micro-structure of our data set by regressing individual research productivity not only on career-time and cohort membership, but also on the field of specialization, on a gender dummy variable, and on a measure of ability. Following Goodwin and Sauer (1995), we ranked the researchers according to their cohort-specific average life-time productivity. We then defined quintile ranks within the distribution for each three-year cohort and assigned each researcher the appropriate *ability rank*.

Since about three quarters of our observations of the dependent variable (research productivity of economist i in year t) are zeroes, one cannot apply OLS. To accommodate this high degree of censoring we used the *hurdle model*, i.e. we allow the decision making process to be more complex than the one captured by a standard Tobit model.[10] The first part (being active) is portrayed with a Probit model, whereas the distribution of the positive counts is modeled with the help of a truncated Negative Binomial model since the observed density distribution of our dependent variable resembles the pattern of count data.

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[8] The relevant peer group always consists of five age groups, namely the age group of the person to be evaluated and the four neighboring age groups.

[9] Our formula approximates our regression result which explains 93% of the variance of *S*.

[10] For details and other estimation techniques, see the companion paper: Rauber and Ursprung (2007).

The results of our regressions are shown in the working paper version of this article. Our hurdle model focuses on heterogeneity with respect to ability, i.e., we include dummy variables for each ability rank and also allow the life-cycle polynomials to differ across the ability ranks 5 (top researchers), 4 (accomplished researchers) and 1 – 3 (journeymen researchers).[11] Figures 3a and 3b (see p. 112) visualize the fact that the time polynomials differ across ability ranks and that there are significant differences between the time polynomials of the *Probit* and *NegBin* part, thereby suggesting different forces governing the two respective processes. Our results indicate that the top-researchers manage to increase their publication incidence over time while their research productivity somewhat declines in the second half of their careers. It thus appears that the best researchers in the profession focus in the beginning of their careers on fewer research projects (articles) but execute them with more effort which gives rise to higher quality (better journals) and more extensive results (longer articles), and all this is achieved with fewer co-authors. Later on in their careers these researchers get involved in more projects that are, however, executed with less effort. The two processes (number of projects and research effort put into each project) neutralize each other and, in conjunction, give rise to the flat life cycles in overall research productivity already observed. Decomposing our measure of research productivity and regressing average quality, article length, and number of co-authors on our explaining variables indeed shows that older economists work together with more collaborators (co-authors), write shorter articles, and publish in lower quality journals. Interestingly, however, top researchers manage to maintain quality much more than their less gifted peers.[12]

As compared to the top-researchers, the "accomplished" researchers' publication incidence and research productivity declines more sharply over their life cycles. These life cycles are thus better in line with the predictions of the human capital approach to explaining labor productivity. The "journeymen" researchers, finally, have rather flat and nondescript life cycles.

The coefficients of the cohort dummies, not surprisingly, increase over time. This result is consistent with the joint hypothesis of more productive younger cohorts and a *constant* historical time effect. We admit, however, that it is not inconceivable that our regressions somewhat overestimate the identified vintage effects since the gradual substitution process towards publishing research results mainly in journals may still have been at work in the beginning of our period of observation. The estimated coefficients of the gender dummy variable indicate that female economists publish significantly less than

[11] It was necessary to bundle the first three ranks together because of the high degree of censoring within these ranks. Nevertheless, we still allow for different intercepts for each rank.

[12] See our companion paper: Rauber and Ursprung (2007).

Figure 3a

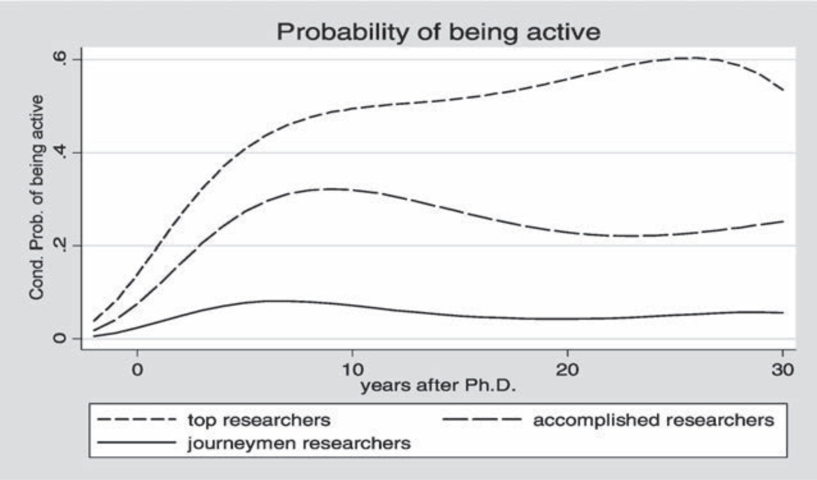
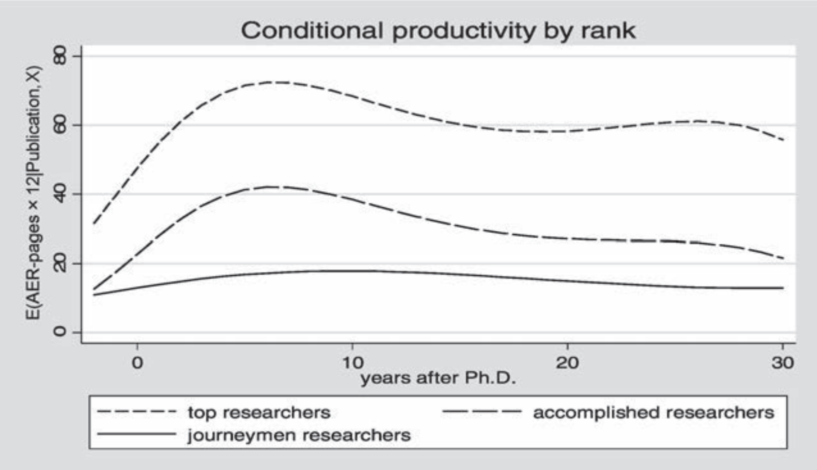


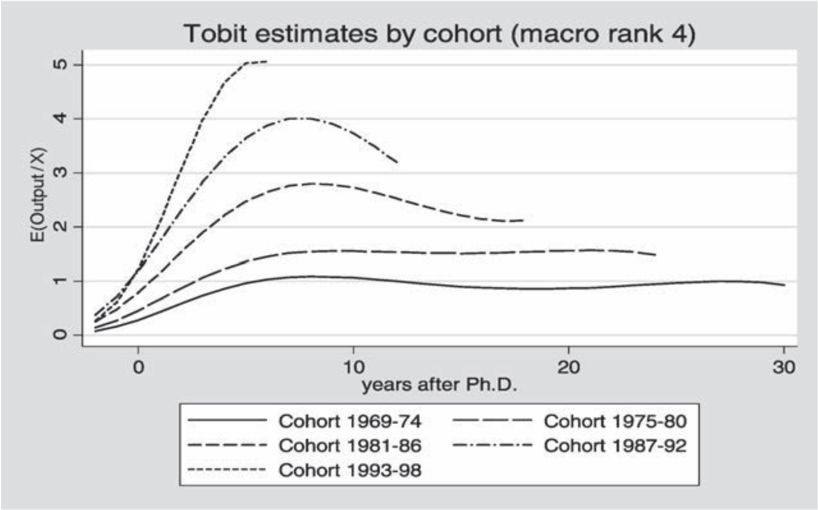
Figure 3b



their male peers. This negative effect, however, arises from the fact that female academic economists seem to be more likely not to engage in research at all. If female economists decide to be active researchers, then they are just as productive as their male peers. Our field dummies, finally, show that researchers specializing in macroeconomics are less likely to be active researchers, and active micro-economists publish more than their peers. Even though these effects appear to be relatively small and fragile, it might be worthwhile to bear these field effects in mind when evaluating individual economists.

In a second (standard Tobit) regression we focus on heterogeneity with respect to *cohort membership*. As in the hurdle model, we allowed the life cycle polynomials to differ, this time across our six cohorts. Figure 4 visualizes

Figure 4



the cohort specific time polynomials. It can be seen with the naked eye that the shape of these life cycles differs across cohorts: younger cohorts have more hump-shaped life cycles than older cohorts. With respect to the other explaining variables nothing changes dramatically.

We thus arrive at the result that the life cycles of younger cohorts – as far as we can tell from the initial phases of these cycles – correspond more closely to the predictions of the standard human capital approach to explaining changes in labor productivity than the evidence we have for older economists. Various hypotheses lend themselves to explaining this result. The first and arguably most plausible one maintains that the academic environment has become increasingly more competitive over the last 35 years. In a more competitive work environment, employees who want to succeed are forced to optimize under the pertaining constraints. It is thus not surprising that their behavior more closely corresponds to the predictions of the human capital model that narrowly focuses on labor market incentives. An alternative hypothesis is that doctoral students of older cohorts have been exposed to different role models than the younger cohorts. This hypothesis relates to the preference formation process which works through sociological imprinting. The last hypothesis does not assume a change in preference formation but different preferences of the people who decide to pursue an academic career. Whether it is possible to empirically discriminate between the three hypotheses (which are, of course, not mutually exclusive), remains to be seen.[13]

5 Persistence of research productivity

The *economics of science* literature has clearly demonstrated that an academic scientist's research productivity has a noticeable influence on his or her labor market success. First of all, research productivity varies positively with pay (cf. Kenny and Studley 1996 and Moore et al. 2001 for empirical evidence relating to the economics profession). A strong research record has, moreover, also a positive influence on the obtainable job status in terms of the employing university's reputation (cf. Grimes and Register 1997 and Coupé et al. 2003), and scientists with strong research records are more likely to be granted tenure and to be promoted to higher academic ranks (cf. Coupé et al. 2003). Tenure and promotion to the highest level of the academic hierarchy may, on the other hand, have detrimental effects on research productivity because these types of upgrading are irrevocable and thus reduce incentives to work hard. Backes-Gellner and Schlinghoff (2004), for example, have show that research productivity of German (business) economists increases before the only crucial career step (appointment to a professorship) and is reduced afterwards. An early study on the impact of tenure that arrived at similar results for the United States is Bell and Seater (1978).[14]

Precisely because irrevocable career steps are liable to have a certain influence on research productivity, it is important to know at what stage of the academic career the research potential of a scientist can be assessed with reasonable accuracy and to what extent this potential is liable to be used in the post-tenure period. In other words, it is (from a managerial point of view) important to possess firm information on the *persistence* of individual research productivity. Inspection of our aggregate and individual data has already revealed that research productivity in our sample of economists is characterized by a great deal of persistence. In this section, we focus on the question whether the traditional American policy to grant, postpone, or decline tenure

[13] See Frank and Schulze (2000) for an experimental design to test a related set of hypotheses.

[14] For a recent theoretical study of tenure and related incentive schemes in academia, see Dnes and Garoupa (2005).

after a review period of six years does make sense in the light of our empirical evidence. Many knowledgeable observers agree that young scientists have to wait too long to be promoted to a professorship in the German university system. On the average, the implicit probation period amounts to eight years (German economists obtain their doctoral degrees when they are about 30 years old and are, on the average, appointed to their first professorship at the age of 38). The objective of the investigation presented in this section is to inquire whether the review period could indeed be shortened without great loss in terms of evaluation accuracy.

As compared to *tenure-induced effects* on research productivity, the optimal *timing of the tenure decision* has not found a great deal of attention in the scientometric literature dealing with the economics profession. A notable exception is the study by Hutchinson and Zivney (1995). These authors regress the average annual post-tenure productivity (measured in numbers of journal articles) on the pre-tenure oeuvre of economists using two hypothetical review periods, namely the standard six years and four years. Their regression analysis leads them to concur with Bell and Seater's (1978) conclusion based on cross-sectional data "that granting of tenure seems to have negative effects on individual publishing performance" (1978, 614). "Yet, because the negative effect is so small numerically, 0.01 articles per year, our results indicate that publishers maintain essentially constant pre- and post-sixth-year rates of publication over their post-doctorate years. Moreover, shortening the review period from six years after the doctorate to four, relying upon our 1969–1979 doctorates, only slightly reduces the ability to predict future journal publication rates based on existing journal publication information while also producing almost constant pre- and post-fourth-year rates of publication" (Hutchinson and Zivney 1995, 74).

In order to check whether the German economists' academic standing reached by their sixth year after the doctorate is a good indicator for their mid-career reputation (at the approximate age of 42, i.e., in the twelfth year after the doctorate), we ranked all economists in our sample at career time $t = 6$ according to the size of their oeuvres in relation to a special five year cohort for each class.[15] We then define quintile ranks and assigned each researcher the appropriate rank. Repeating this procedure for the career year $t = 12$, we arrived at the mid-career ranking of the same economists and were then able to compute the probability of moving from one quintile rank to another within the observation period. These transition probabilities are shown separately in table 1 for the older economists in our sample (classes of 1969 to 1980) and for the younger ones (classes of 1981 to 1992). Due to the inescapable problem of

[15] Members of the class of 1981, for example, are ranked in the cohort comprising the classes of 1979 up to 1983.

Table 1 Transition probabilities: year 6 – year 12

		1 & 2	3	4	5
1 & 2	Coh. 1	**0.80**	**0.14**	**0.04**	**0.02**
	Coh. 2	0.83	0.13	0.04	0.00
3	Coh. 1	**0.30**	**0.41**	**0.24**	**0.05**
	Coh. 2	0.28	0.44	0.23	0.05
4	Coh. 1	**0.02**	**0.37**	**0.47**	**0.14**
	Coh. 2	0.00	0.37	0.56	0.07
5	Coh. 1	**0.00**	**0.00**	**0.19**	**0.81**
	Coh. 2	0.00	0.00	0.14	0.86

Cohort A: 1969 – 1980
Cohort B: 1981 – 1992

research-inactive scholars we had to group the first two quintiles together with the consequence that the probabilities in the columns do not add up to 100%.

The results summarized in table 1 once more show that research production is indeed characterized by a great deal of persistence. The probabilities on the main diagonal are substantially larger than the off-diagonal probabilities, implying that marked changes in the academic standing are low probability events. Table 1, in particular, shows that appointing a young professor with a high reputation is a relatively safe bet these days. On the other hand, appointing a professor with a bad publication record and hoping (perhaps based on hearsay) for the best, is not much more than wishful thinking. The probability of a bottom group researcher making it in the first six years of his or her full professorship to the top 40% is nowadays not more than 4 out of 100.[16] Table 1 also documents that the research track record has become a better indicator of future research productivity over the years. The transition probabilities of the younger economists are more centered on the main diagonal than those of the older economists.

The evidence summarized in table 1 documents that, currently, a six year review period provides ample evidence for an informed tenure decision. The question therefore arises as to whether the German method of appointing professors (i.e., after an average review period of eight years) is indeed significantly superior in terms of avoiding bad appointments to justify the cost (especially the attendant loss of appeal to pursue an academic career). To investigate this question, we have computed the transition probabilities of the

[16] Notice that the persistence documented in table 1 is, of course, to some extent predicated by the question we ask, i.e. by the fact that we use stock data that reflect reputation. Using flow data would certainly increase the inter-quintile transition probabilities.

Table 2 Transition probabilities of cohort B

		1 & 2	3	4	5
1 & 2	**4–12**	**0.80**	**0.13**	**0.05**	**0.02**
	6–12	0.83	0.12	0.05	0.00
	8–12	*0.88*	*0.11*	*0.01*	*0.00*
3	**4–12**	**0.30**	**0.39**	**0.26**	**0.05**
	6–12	0.28	0.44	0.23	0.05
	8–12	*0.24*	*0.62*	*0.14*	*0.00*
4	**4–12**	**0.07**	**0.35**	**0.45**	**0.13**
	6–12	0.00	0.37	0.56	0.07
	8–12	*0.00*	*0.19*	*0.70*	*0.11*
5	**4–12**	**0.00**	**0.02**	**0.19**	**0.79**
	6–12	0.00	0.00	0.14	0.86
	8–12	*0.00*	*0.00*	*0.12*	*0.88*

younger German economists also for hypothetical review periods of eight and four years. The results are summarized in table 2. Given that we work with stock variables, it is not surprising that the predictions become somewhat sharper when using an eight instead of a six year review period, and somewhat more diffuse when using a four year period. More interesting is the fact that reducing the review period from the German standard of eight years to the American standard of six years does not appear to come at an inordinate loss of information. Research excellence, in particular, can be detected after six years just as well as after eight years. In many cases of truly superior young scientists, a review period of four years may well be sufficiently long to make a reasonably safe appointment decision. Our conclusion is thus in line with the results derived for the United States by Hutchinson and Zivney.

6 Some new rankings for German economics departments

If one agrees that the evaluation of individual researchers should take career age and cohort affiliation into account, then these age dimensions should also be considered when ranking whole departments. After all, meaningful department rankings are supposed to reflect the research competence of its members and not the age structure of the departments' faculty. In this section we therefore present some rankings of German economics departments that reflect the life cycle dimension of the evaluated faculties. The objective is to demonstrate how, *in principle*, such rankings can be conceptualized and to show how rankings that incorporate life cycle information compare to traditional rankings that do not do so.

We decided to produce rankings that are comparable to the research rankings published by the *Centrum für Hochschulentwicklung* (CHE) because the CHE-rankings, even though criticized by an impressive number of knowledgeable observers of the German research landscape, nevertheless are quite influential. The reference groups of the CHE-rankings are the *tenured* professors of the respective departments. Whether this reference group constitutes a meaningful basis for an evaluation is questionable. Nevertheless we adopt here this approach in order to provide results that are easily comparable to an established German standard.

The rankings that are presented in table 3 refer to 52 economics departments. All of these departments confer degrees in economics and belong to a German university; we thus do not consider economics departments of second-tier universities, the so-called *universities of applied sciences*. One of the main (but little appreciated) challenges of *current potential rankings* as compared to *work-done-at rankings* consists in the identification of the respective faculty members. Since some of the faculty lists used by the CHE are grossly at variance with a truthful representation, we decided to base our rankings on a revised set of faculty lists that is reproduced in the appendix of the working paper version of this article.

Our first ranking (see column A in table 3) simply represents the mean of the individual research standings of the respective faculty members, where the individual research standing is defined via the percentile value of average lifetime research productivity within a three years cohort comprising all economists who received their doctoral degrees in the same year as the evaluated individual or in a neighboring year. Since these overlapping three-year cohorts are rather small for some years, we also show a ranking using cohorts of five years (column B). The rankings appear to be quite insensitive to the chosen cohort size: only three out the 52 ranked departments move by three ranks and one (Lüneburg, one of the two smallest departments with three professors) by four ranks across the two rankings. The two first rankings are thus very similar which is confirmed by a rank-correlation coefficient amounting to 99.6%.

As far as the top-ranked departments are concerned, the results of the first two rankings confirm, in essence, the results of earlier studies and the assessment of informed observers of the German economics profession (see, e.g., Ursprung 2003). Somewhat surprising is perhaps the fact that the LMU Munich is only placed 9th.[17]

The first two rankings do not take into account that the research standing of individual economists is sensitive to their respective field of specialization. As

[17] More important than the rank is of course the numerical value of the variable on which the ranking is based (these values are reported in the working paper version of this article). In this respect *ratings* are more meaningful than *rankings*.

Table 3

	Life Cycle				Standard	
	A	B	C	D	E	F
FU Berlin	6	6	7	6	9	4
HU Berlin	5	5	1	5	8	5
HWP Hamburg	49	48	44	48	49	47
LMU München	9	9	6	7	6	3
RWTH Aachen	11	12	19	13	4	9
TU Berlin	25	24	27	27	23	29
TU Chemnitz	43	44	46	45	42	43
TU Dresden	18	16	16	14	14	16
Uni Augsburg	30	28	37	28	31	37
Uni Bamberg	37	37	42	31	35	38
Uni Bielefeld	7	7	10	9	11	13
Uni Bonn	1	1	2	1	1	1
Uni Bremen	46	47	40	50	51	50
Uni Dortmund	10	11	8	11	12	12
Uni Duisburg-Essen	44	43	45	44	44	42
Uni Erfurt	41	41	41	38	25	20
Uni Erlangen-Nürnberg	24	25	31	25	24	22
Uni Frankfurt/Main	13	13	11	12	10	10
Uni Frankfurt/Oder	8	10	12	10	13	18
Uni Freiburg	33	33	29	35	32	30
Uni Gießen	35	36	43	39	46	49
Uni Göttingen	34	34	34	29	26	28
Uni Halle-Wittenberg	40	39	33	41	40	41
Uni Hamburg	32	32	26	34	33	31
Uni Hannover	20	19	25	23	19	26
Uni Heidelberg	23	26	18	16	29	7
Uni Hohenheim	26	23	22	24	22	27
Uni Jena	52	51	47	49	47	48
Uni Karlsruhe	36	35	32	33	37	34
Uni Kiel	3	2	5	3	2	8
Uni Köln	27	27	36	26	36	33
Uni Konstanz	2	3	3	4	3	6
Uni Leipzig	48	49	48	47	48	46
Uni Lüneburg	12	8	9	8	5	11
Uni Magdeburg	19	20	20	21	15	25
Uni Mainz	16	18	15	19	21	17
Uni Mannheim	4	4	4	2	7	2
Uni Marburg	39	40	38	32	30	14
Uni Münster	42	42	35	42	38	36
Uni Oldenburg	21	21	21	20	20	15
Uni Osnabrück	14	14	14	18	28	35
Uni Paderborn	50	50	51	51	50	51
Uni Passau	31	30	30	36	34	32
Uni Potsdam	28	31	24	37	39	39
Uni Regensburg	17	17	13	17	18	24
Uni Rostock	45	46	49	46	45	40

Table 3 (cont.)

	Life Cycle				Standard	
	A	B	C	D	E	F
Uni Siegen	29	29	28	30	27	23
Uni Stuttgart	47	45	52	43	43	45
Uni Trier	51	52	50	52	52	52
Uni Tübingen	15	15	17	15	16	19
Uni Würzburg	22	22	23	22	17	21
UniBW Hamburg	38	38	39	40	41	44

A: Life Cycle 3 years
B: Life Cycle 5 years
C: Life Cycle 3 years with field correction (mean $+3/-3$ years)
D: Formula
E: Standard Approach: ranking within total Dataset
F: Standard Approach: simple average of productivity

we have shown in section 4, the field of specialization has a statistically significant influence on our measure of research productivity. The ranking presented in column C of table 3 therefore adjusts for these field-specific differences in publication behavior by aligning the field-specific means. This ranking is still closely correlated to the former ones: the rank-correlation coefficients amounting to 96.6% and 96.5%, respectively. Now we observe however quite a few larger deviations in individual rankings. Nevertheless, the *group* of leading departments does not change as compared to the baseline rankings.

Thus far our rankings were based on orderings of individual scientists within narrow peer groups. One could argue that relying exclusively on actual data of relatively small cohorts may, in some cases, bias the evaluation of individual scientists and thereby give rise to unfair rankings. If, for example, unusually many first-rate scientists happen to be of approximately the same age, scientists who have the “bad luck” to be their contemporaries appear to be mediocre even when their overall research record is quite good, simply because they are compared only to their immediate cohort peers who are, coincidentally, very good. This kind of bias can be avoided by using our formula presented in equation (3) – albeit at the cost of losing some information. The ranking presented in column D of table 3 is based on the ranking of the respective faculty members according to our formula. Since the formula-based ranking in some instances does markedly differ from the baseline ranking that uses actual cohort data we conclude that the identified bias may have an undue effect even in the aggregate.

The last two rankings presented in table 3 do not take the life cycle dimension of individual research productivity into account. They are based on a method that is similar to the method used by Combes and Linnemer (2003) in their "career" rankings, i.e., we compute the average research productivity of each department member and then either use the department-average of the respective percentile rankings (column E) or the average of the individual productivities (column F). Comparing these standard rankings with our baseline ranking demonstrates that life cycle effects are not only significant for the evaluation of individual scientists but also for the ranking of whole departments (the rank correlations between ranking E and F and the ranking A amount to 94% and 88%). Consider, for example, the department of the LMU. According to the standard ranking E, the LMU is ranked 6th while according to our life-cycle rankings A and B it is ranked only 9th. This drop is apparently due to the fact that the most productive members of the LMU department are relatively young; neglecting the fact that young economists are in general more productive than older ones thus gives rise to an over-estimation of the department's research standing. The cases of Frankfurt a.M., the two small departments of the RWTH Aachen and Lüneburg, and Erfurt are similar. The departments of the FU and HU Berlin, Mannheim, Bielefeld, Frankfurt a.O. and Osnabrück represent the counterpart category. These departments do significantly better when life cycle effects are taken into account. In these departments it is thus the old guard that is more productive – at least in relative terms.

The last ranking (F) is more sensitive to outliers than ranking E because there is no upper bound for individual productivity. Extremely productive scientists thus give rise to a non-representative department average. Which of these two standard rankings is to be preferred depends of course on the context of the investigation. In any event, these two standard rankings clearly support our main argument: life cycle considerations also matter for research rankings of whole university departments.

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Evaluation of Researchers

Comment by
Werner Güth

1 Introduction

My assigned task is to comment in an academic way on the academic exploration of German academic researchers. This may be compared to students of psychology who study psychology to learn more about themselves. And let us not deny it – the fact that this is about us renders the paper as something we would not want to miss reading.

Of course, we are all aware of some of the research by some of our colleagues. So what is new here is the very systematic collection and aggregation of publication success of German academic economists with doctoral degrees from 1969 to 1998 who were employed by a German university in 2004. These data are not readily available and, although one may argue that the data are rather selective and possibly even biased, providing such a data basis has to be highly appreciated.

The analysis of the data is impressingly thorough by

– distinguishing different cohorts of researchers with probably very different research environments during the various stages of their career,

– different types (top, frequent publisher) of economists in each cohort,

– following the life cycle regarding publications, and

– decomposing publication scores into their components (journal quality, number of pages, number of coauthors).

Clearly, as demonstrated by the authors, such results can be used for both, evaluating an individual researcher, e.g., by comparing her or him with the average researcher in her or his cohort, as well as evaluating faculties, e.g., by determining their cohort or age adjusted average quality.

2 Measuring publication record

Like in all empirical work, one might complain about the data which the authors analyze. Economics is just one of the social sciences which can do both, gain by importing ideas from neighboring fields and inspire research in neighboring fields. Researchers who engage in such interdisciplinary exchange might complain about using just economic literature data bases. It would probably not question the main conclusions but it would be comforting to know that by broadening the data base, e.g., by including law journals (to account for a field like "law and economics") or journals of social, cognitive and economic psychology, nothing essential changes.

The authors do not consider citation data, presumably since they are manipulable, e.g., by forming citation cartels. If so, this would not be entirely convincing. Editors should soon find out such attempts and take precautionary measures. And why are publication scores not at all or at least much less manipulable? Actually, the share of citations to research reports (monographs, non-economic journals, etc.) outside the economic literature data base used by the authors could indicate the selection bias and how representative the data are for all the scientific work of the included German academic economists. The description: "... that the oeuvre of the median researcher is quite modest. During his whole career the median German economist does not manage to produce more than 10 AER-equivalent pages ..." could be read as a warning that the data source is quite selective rather than stating that German economists are quite unproductive. Citation data, job offers, etc. might reveal how the acknowledgement of individual researchers by their peers and the publication scores, as measured by the authors, are correlated. So far, this is still questionable.

3 Cohort-specific life cycles

Not only industrial but also scientific production experienced quite dramatic changes in the impressingly large time span covered by the data. We now use analytical and statistical software, much improved computing hardware, better data, text systems, etc. and the academic labor markets are, of course, now much more competitive. So comparing a young and a senior researcher's publication score would not be fair. The authors avoid this by distinguishing quality types (top, frequent, ... publishers) only within a given cohort and by cohort weights for individual researchers when comparing faculties. This is entirely convincing. Estimating the life cycle not only for different cohorts but also for different quality types of each cohort renders the analysis even more convincing.

The explorative analysis via time polynomials up to the fourth degree yields convincing results which could be checked for robustness by piecewise linear life cycles or dummies for time intervals of one's career. Due to the many researchers with 0-publication score, the overall estimation (table I of the working paper version) first estimates the probability of publishing at all and only then how much one publishes where this, furthermore, can depend on the field (micro-, macro-, public economics, econometrics).

From the perspective of an individual researcher, the idea of a life cycle suggests, of course, a strong path dependence, e.g., in the sense that past publications reflect human capital or habit formation as, for instance, captured by a (possibly weighted) previous publication score as an aspiration level for future research. The authors analyze this by the transition probabilities of "quintile" persistence (tables II and III of the working paper version) for two time points (career years 6 and 12) and confirm quite some persistence of publication habits.

4 Faculty ranking

When publication activity is cohort and career age specific, as convincingly illustrated by the authors, evaluating faculties by just comparing per capita publication scores is usually done but appears quite arbitrary by favoring faculties whose members predominantly belong to more productive (the later) cohorts and/or are in their most productive career stage. The authors use their method to correct this, i.e., by assessing for each faculty member the publication type by her or his percentile ranking in view of her or his total lifetime publication score in the respective cohort (varying the length of cohorts to check for robustness). The fact that the authors rely partly on field-specific (micro-, macro-, public economics, econometrics) adjustments suggests that faculties nowadays specialize by trying to focus on specific fields.

This, naturally, changes the ranking of faculties with some losing in rank (e.g., the University of Munich (LMU) whose faculty members were mainly young but not necessarily overproductive) whereas others gain (e.g., the faculties in Berlin (FUB, HUB) whose "old-timers" are relatively productive). Given the authors' affiliation, members of the losing faculties might suspect a self-serving attitude. But even they should concede that it would be a rather sophisticated and intuitive way of evaluating in a self-serving way. One also would like to ask: if one accounts for the age composition of a faculty, why does one not account for heterogeneity in other aspects like number of students, PhDs, habilitations, type of graduate education, etc.?

5 Suggestions

The authors motivate their study by the apparent need to assess the promise of individual scholars as well as of faculties when deciding academic promotion and when designing funding schemes for faculties and universities. Do they really believe that we do it only just for the money? Did not old German academic economists without any monetary incentive to publish prove the opposite?

It definitely is interesting to explore the best reward and funding schemes when money rules the academic world. But one should not forget that academic life leaves us a lot of freedom what to research – as the authors illustrate, research can be even self-reflective –, with whom we cooperate and offers a lot of exciting experiences by attending workshops and conferences and spending sabbaticals abroad. For many of us this seems to be quite important. In this sense, the study is very informative and inspiring but one should refrain from policy recommendations before having discussed how decisive some of its shortcomings are. The authors selectively measure (publication) success, neglect habit formation and intrinsic interest in academic research, and do not pay attention to other success measures like citation impact, external funding, external offers, etc.

On the other hand, the data basis offers several chances to answer new questions (Are American coauthors more helpful than German ones in improving one's life cycle?) or old ones anew (Are scholars with names early in the alphabet more successful? Are there especially good faculties for certain fields?) to mention just a few.

Markets versus Contests for the Provision of Information Goods

by

Martin Kolmar*

1 Introduction

"It appears that patent policy is a very blunt instrument trying to solve a very delicate problem. Its bluntness derives largely from the narrowness of what patent breadth can depend on, namely the realized values of the technologies. As a consequence, the prospects for fine-tuning the patent system seem limited, which may be an argument for more public sponsorship of basic research."

(S. Scotchmer, Journal of Economic Perspectives, 1991).

"In the field of industrial patents in particular we shall have seriously to examine whether the award of a monopoly privilege is really the most appropriate form of reward for the kind of risk bearing which investment in scientific research involves."

(Friedrich Hayek, Individualism and Economic Order, 1948).

The classical justification for patents emphasizes the positive effects of patents on the incentives to invest in innovation. Granting a temporally restricted monopoly right increases the incentives to invest in research; however, if perfect price discrimination were not possible there would exist a tradeoff due to the welfare loss associated with the monopoly right. These welfare losses restrict the optimal term of the patent.

Recent research has challenged this orthodoxy by focussing on complementarities in environments where production requires the use of multiple patents. The analysis of a complementary-goods oligopoly dates back to Cournot (1838) who found that prices tend to be higher and quantities tend to be lower than in a monopoly. For obvious reasons this result is also called the tragedy of the anti-commons because it is exactly the *existence* of property rights that leads to an inefficiency (Buchanan and Yoon 2000, Depoorter, Parisi, and Schulz 2001).

In addition to the anti-commons problem that is based on a negative externality between different patent holders (contrary to the negative externality in the case of a substitutive-goods oligopoly), a number of other patent-system related problems are being discussed, ranging from holds-ups when patents are complementary, excessive information costs due to an excessive number of potential patent infringements, inefficient designing-around efforts that raise costs and/or reduce product quality, etc. (Shapiro 2001, 2004).

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Shapiro (2001) has coined the term ‘patent thicket’ to focus attention on the transaction costs that are associated with highly fragmented property rights.

The inefficiency of a specific mechanism, however, cannot be considered to be problematic as long as it has not been demonstrated that alternative mechanisms exist that support equilibria that – given a normative criterion – dominate the equilibrium of the patent mechanism. The literature mentions alternative mechanisms, however, without explicitly analyzing their specific properties.

One obvious class of alternative mechanisms is a contest (see Tullock 1980) or tournament where researchers compete for a prize, for example, research grants, tenure positions, etc. Contests award the price according to a relative-performance measure, for example, scientific publications. Each researcher can influence his probability of winning the prize by increasing his research activities. There is a large literature on contests and tournaments that analyzes the properties of this type of mechanism, mostly for the case of private goods (see Lazear 1997).

There is one specific feature of research or information goods, namely, that they are non-rivalrous in nature (see Che and Gale 2003, Kolmar and Wagener 2004, Morgan 2000). Similar to the tournament literature, investments in a contest are socially productive. The specific feature of the production process of information goods is, however, that the resulting goods are non-rival. Basically, the idea is to introduce a compensating (i.e., negative) externality to resolve the under-provision dilemma present in voluntary-contribution games in the provision of public goods. Information goods are transformed into public goods if the exclusion mechanism is not applied. The idea of a compensating externality to promote the production of public goods can already be found in Cornes and Sandler (1984, 1994).

One of the novel aspects of this paper is to understand exclusion as a social agreement and not an exogenous property of the specific good. If the society decides to grant patent rights on innovations, the exclusion mechanism is applied. If, on the other hand, the society decides not to grant patent rights, an information good becomes public property. The decision whether to grant property rights or not should therefore depend on the economic costs and benefits of the alternative mechanism.

In a transaction-costs free world one would assume that both types of mechanisms turn out to be equally efficient. It is therefore of crucial importance to understand the idiosyncratic transaction costs of both types of mechanisms. In this paper we identify a new potential problem of the patent mechanism, namely, that the holder of a patent may have an incentive to inefficiently restrict the number of licenses sold. We restrict attention to information goods that have the character of a process innovation that – if applied – reduces the production costs of firms in a downstream market. Hence, a patent holder can influence the competitive structure on the downstream market with his license-policy. We show that for the case of an oligopolistic downstream market with Cournot-competition the innovator will inefficiently restrict the number of licences if the cost-differential associated with the application of the innovation is relatively large. Under these circumstances the patent system has welfare costs in terms of a reduced sum of rents on the market.

However, these welfare costs may be unavoidable as long as it is not possible to characterize an alternative mechanism that avoids these inefficiencies without creating new types of welfare losses that are even worse. For the special case of risk-neutral innovators we show that under a mild restriction the contest mechanism in fact dominates the patent mechanism. However, if one allows for risk-averse innovators the introduction of a contest increases the individually perceived risk. No clear-cut results concerning the optimal balance of both types of mechanisms have been derived for this case. However, we show for a special case that both types of 'corner solutions' may turn out to be second-best optimal. Depending on the specific structure of the model it can turn out that the patent mechanism dominates the contest mechanism and vice versa.

The paper proceeds as follows. The model is introduced and solved in Section 2. The model has three stages and is solved by backwards induction. Therefore, the equilibrium on the downstream market is analyzed in Section 2.1. The optimal license policy of a patent holder is analyzed in Section 2.2. Section 2.3 analyzes the incentives to innovate and the optimal balance of incentive schemes. In Section 2.3.1 the analysis is restricted to risk-neutral innovators. It is extended to risk aversion in Section 2.3.2. Finally, the optimal balance of incentive schemes with risk-averse innovators is derived for a specific functional specification in Section 2.3.3. Section 3 concludes.

2 *The model*

We assume an economy with one representative innovator from a set of n potential innovators (researchers) with generic index i. Every innovator can devote $l_i > 0$ units of time and effort to the production of innovations. He derives utility out of monetary income, y_i, (positive) and time and effort spent for research (negative). All innovators have identical utility functions that are linear in l_i and concave in y_i, and that are consistent with the von-Neumann-Morgenstern axioms for expected utility,

$$u_i(y_i, l_i) = v(y_i) - l_i. \tag{1}$$

For simplicity we assume that there is a measure of scientific output, for example, published research papers, that is perfectly correlated with l_i. Hence, we will use l_i as a measure for scientific output directly in the following analysis. Scientific output is an information good, which means that it is nonrivalrous in a sense that will be exactly defined below.

We assume that scientific output has the character of a process innovation: if an innovation occurs it has the potential to reduce production costs in a downstream product market. To be more specific, there are m potential firms with generic index j who produce quantities $x = \{x_1, ..., x_m\}$ of a consumption good by means of a linear cost function $C(x_j) = c \cdot x_j$. Without scientific innovation the unit costs are equal to $c_h > 0$. If an innovation occurs, any firm that uses this innovation is able to reduce its unit costs to $c_l \in [0, c_h)$. The innovation is nonrival because the use of it by firm j does not preclude firm k from using it and the costs of an additional user are equal to zero. The market exists for a span of time T, and for simplicity we abstain from discounting.

An innovation is called *indispensable* or *perfectly complementary* to the production process, if $c_h \rightarrow \infty$. This notation allows it to refer to other results in the literature that focus on the role of complementarities between different inputs. We assume that all firms supply a homogenous good and engage in Cournot competition.

The incentives to devote time and effort to the production of scientific output are given by means of two basic mechanisms.

– First, there may exist a tournament-type incentive scheme that can be thought of as a contest among scientists for research grants or for lecturer or tenure positions at research institutes or universities. This contest maps any vector of individual scientific output $\boldsymbol{l} = \{l_1, ..., l_n\}$ into a vector of probabilities $\gamma = \{\gamma_1, ..., \gamma_n\}$ for getting a fixed prize $z \geq 0$. To be more specific we assume that this contest is of the Tullock-type

$$\gamma_i(l) = \frac{l_i}{\sum_j^n l_j}. \tag{2}$$

– Second, there exists a patent system that protects the innovation for $t \in [0, T]$ periods of time. We assume that the time and effort spent for research influences the probability of generating a patentable idea. Let $\rho_i(l_i) \in [0, 1]$, $\rho'(.) > 0$, $\rho'(0) \rightarrow \infty$, $\rho''(.) < 0$ be the probability of generating a patentable idea. For simplicity we assume that the direct and opportunity costs of getting a patent are zero. If researcher i has a patent on an innovation, he can sell it to $s \in (0, 1, \ldots m)$ in the downstream market. For simplicity we assume that there is no discounting of future payoffs.

Both incentive schemes can be summarized by a vector $\{z, t\}$. Denote by $s \cdot r_i$ the sum of royalties paid by s firms using the innovation. Taken together, innovator i has the following modified utility function:

$$E[u(l_i)] = \rho(l_i)\gamma(\mathbf{l})v(z + s \cdot r_i) + (1 - \rho(l_i))\gamma(\mathbf{l})v(s \cdot r_i) + \rho(l_i)(1 - \gamma(\mathbf{l}))v(z) - l_i, \quad (3)$$

where we have w.l.o.g. normalized the utility of $y_i = 0$ to zero. For the special case of risk neutrality, the underlying preferences can be represented by a utility function

$$u_i(l_i) = \gamma_i(\mathbf{l})z + \rho_i(l_i)s \cdot r_i - l_i. \quad (4)$$

If neither incentive system exists, it follows from (1) that $l_i = 0$ for every researcher i. If society decides to use only the contest mechanism and not to protect the innovation by a patent system, $t = 0$, the innovation falls into public domain, and every firm j can use it for free. If society decides to increase incentives by means of the patent system, every firm that uses the innovation within the first t periods has to pay royalties n to the innovator. Afterwards the innovation again falls into public domain and can be used for free.

The sequence of the game is as follows:

1. At stage 1 the potential innovators simultaneously and non-cooperatively choose l_i and the prize is awarded according to (2). Because we are ultimately interested in the optimal design of an incentive scheme $\{z, t\}$, the comparative-static behavior of this equilibrium would have to be taken into consideration.
2. At stage 2 an innovator who has been successful in developing a patent bargains with the potential users of the innovation, firms 1, ..., m about the royalties paid. The outcome of the bargaining determines the number and identity of low-cost firms on the downstream market during the time span t that is protected by the patent.
3. At stage 3 the firms at the downstream market determine their optimal production plan.

The game is solved by backwards induction.

2.1 Stage 3

There are two possible scenarios on the downstream market. During the period of patent protection, only those firms that have paid royalties to the innovator have access to the low-cost technology, whereas afterwards all firms have access to it. Fortunately the first case is a special case of the second and we can therefore restrict attention to the solution of the following game: assume that $s \leq m$ is the number of low-cost firms in the market. Market demand is given by the function $p(\mathrm{x}) = \left(a - b\sum_{j=1}^{m} x_j\right)$. The maximization problem of both types of firms is given by

$$\pi_h(\mathrm{x}) = t(p(\mathrm{x}) - c_h)x_j, \tag{5}$$

if it is a high-cost firm, and by

$$\pi_l(\mathrm{x}) = t(p(\mathrm{x}) - c_l)x_j - r_i, \tag{6}$$

if it is a low-cost firm. It is straightforward to show that a Cournot-Nash equilibrium of this game has the following structure: all low-cost firms produce the same quantity and all high-cost firms produce the same quantity that is given by

$$x_h^*(s) = \frac{a - (1+s)c_h + sc_l}{(1+m)b}, \tag{7}$$

$$x_l^*(s) = \frac{a - (1+m-s)c_l + (m-s)c_h}{(1+m)b} \tag{8}$$

for an interior solution. The associated profit levels are equal to

$$\pi_h^*(s) = t\frac{(a - (1+s)c_h + sc_l)^2}{(1+m)^2 b}, \tag{9}$$

$$\pi_l^*(s) = t\frac{(a - (1+m-s)c_l + (m-s)c_h)^2}{(1+m)^2 b} - r_i. \tag{10}$$

Total output is therefore

$$\begin{aligned} X^*(s) &= s\frac{a - (1+m-s)c_l + (m-s)c_h}{(1+m)b} + (m-s)\frac{a - (1+s)c_h + sc_l}{(1+m)b} \\ &= \frac{am - (m-s)c_h - sc_l}{(1+m)b}, \end{aligned} \tag{11}$$

and the equilibrium price is equal to

$$p^*(s) = \frac{(a + c_h(m-s) + c_l s)}{(1+m)} \tag{12}$$

In an efficient solution, only the s low-cost firms would produce until price is equal to marginal costs. Hence, we can calculate the deadweight loss as

$$DL^*(s) = t\frac{(p^* - c_l)((a - c_l)/b - X^*)}{2},$$

which is equal to

$$DL^*(s) = t\frac{(a + c_h(m-s) - c_l(m-s+1))^2}{2b(1+m)^2}. \tag{13}$$

We can use the expression for the excess burden to establish a first result. Differentiating (13) with respect to s yields for all $s < m$ and $t > 0$

$$\frac{\partial DL^*(s)}{\partial s} = t\frac{(c_l - c_h)((a - c_l) + (m - s)(c_h - c_l))}{b(1+m)^2} < 0, \tag{14}$$

which implies

Proposition 1: The deadweight loss is decreasing in the number of licences s.

It is useful to refer to two special cases before we turn to a discussion of the general solution.

All firms use the low-cost technology. In the case where all firms have free access to the innovation, quantities, profits, and the deadweight loss are equal to:

$$x^*(m) = \frac{a - c_l}{(1+m)b}, \tag{15}$$

$$\pi^*(m) = \frac{(a - c_l)^2}{(1+m)^2 b}, \tag{16}$$

$$DL^*(m) = \frac{(a - c_l)(a + mc_l)}{2b(1+m)^2}. \tag{17}$$

High-cost firms drop out of the market. According to (7), the supply of the high-cost firms is equal to zero if and only if $c_h \geq \bar{c}_h(s) := (a + c_l s)/(1+s)$. In this case, s firms remain active in the market, and quantities, profits, and the deadweight loss are equal to:

$$x^*(s, \bar{c}_h(s)) = \frac{a - c_l}{(1+s)b}, \tag{18}$$

$$\pi^*(s, \bar{c}_h(s)) = t\frac{(a - c_l)^2}{(1+s)^2 b}, \tag{19}$$

$$DL^*(s, \bar{c}_h(s)) = \frac{(a - c_l)(a + sc_l)}{2b(1+s)^2}. \tag{20}$$

2.2 Stage 2

At stage 2 an innovator who has been successful in patenting his innovation has to determine the royalties he charges the firms who buy a license of the innovation as well as the number and identity of the firms to whom a license is sold. From the point of view of a single firm j that assumes that $s - 1$ firms pay royalties, paying royalties is rational if and only if

$$\underbrace{\pi_l(s) - \pi_h(s-1)}_{:=\Delta\pi(s)} - r_i \geq 0. \tag{21}$$

For simplicity we assume that the innovator can extract a fraction $\alpha \in (0,1]$ of the additional profit $\Delta\pi(s)$ as royalty from each firm buying a license. Hence, the only problem of the innovator is to determine the optimal number of licenses s. His optimization problem is

$$\max_s \alpha s \Delta\pi(s). \tag{22}$$

Indispensable innovation. We start to analyze the solution of this optimization problem for the special case that the innovation is indispensable/perfectly complementary. With this we can concentrate on a potential efficiency problem of the patent system for the case of non-rival goods in the most focused way. In fact we can relax the assumption of indispensability by assuming that $c_h \geq \bar{c}_h(m)$. In this situation, $\Delta\pi(s) = \pi_l(s)$. Treating s as a continuous variable, it follows that

$$\frac{\partial(s\Delta\pi(s))}{\partial s} = -t\frac{(a-c_l)^2(s-1)}{b(1+s)^3}, \tag{23}$$

which is equal to zero if and only if $s = 1$.[1]

Proposition 2: If the innovation is indispensable, the innovator will sell only one license.

In the light of Proposition 1 this solution demonstrates that the patent system as an incentive mechanism may be problematic from the point of view of economic welfare. At least in the case of an indispensable innovation the innovator has an incentive to restrict access to its innovation and to create a monopoly in the downstream market. The economic intuition for this result is straightforward: It is a standard result from oligopoly theory that aggregate profits in a Cournot-market are decreasing in the number of competitors.

The general case. In the general case, setting the partial of (22) equal to zero and solving for s yields:[2]

$$s^* = \min\left\{\frac{1}{4}\left(\frac{2(a-c_l)}{(c_h-c_l)} + m\right), m\right\} \tag{24}$$

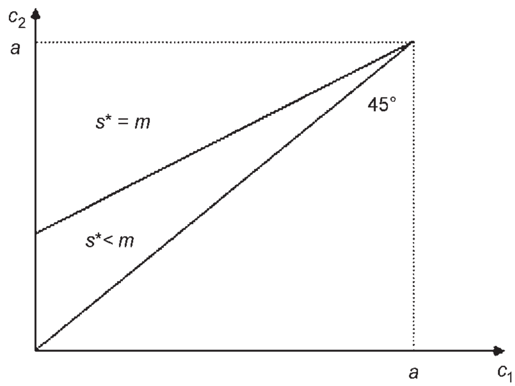
It is easy to check that s^* is increasing in c_l, m, and decreasing in c_l. This implies that the optimal number of licences is decreasing in the cost difference

[1] It is straightforward to check that this solution constitutes the global maximum of the optimization problem.

[2] Again, it is straightforward to check that the second-order conditions are fulfilled.

$(c_h - c_l)$ that can be realized by buying the licence. An analysis of (24) shows that the optimal number of s^* is equal to m if and only if $c_h \geq c_h(s^* = m) := c_l + 2(a - c_l)/(3m)$. This condition is illustrated in Figure 1.

Figure 1 Equilibrium number of licenses



In the figure above, the area below the solid line defines all parameter values for which the optimal number of licences is equal to the number of firms operating in the market. The area above the solid line gives all parameter values for which the optimal number of licences is smaller than the number of firms. Both areas are divided by $c_h(s^* = m)$, which crosses the 45°-line at $c_l = c_h = a$, the point where unit costs are equal to the maximum willingness to pay in this market. Hence, we get a generalization of Proposition 2.

Proposition 3: If the innovation reduces unit costs by less than $c_h(s^* = m) - c_l$, the innovator will sell m licences. If unit costs are reduced by more than $c_h(s^* = m) - c_l$, the innovator will sell less than m licences.

What is the economic rationale for this result? Selling a licence to an additional firm has two opposing effects. First it increases the profit of the innovator directly because an additional licence is sold. Second it changes the competitiveness on the Cournot-market because the number of low-cost firms increases. This reduces the profits of the other firms using the licence. If the potential for cost savings of the innovation is relatively small, the second effect is dominated by the first, whereas the opposite occurs for large cost savings.

The result establishes an additional explanation for the inefficiency of the patent mechanism as a means to shape incentives for research. The standard literature on patents has focused on the deadweight loss created by the monopolistic holder of a patent if he is not able to perfectly discriminate prices between users of the patent. This source of inefficiency is absent in our model because the royalties paid by the firms using the innovation are sunk when they operate on the downstream market. The second argument dates back to Cournot (1838) and emphasizes the "anti-commons" problem if different patents are complementary from the point of view of the potential users (see Buchanan and Yoon 2000, Depoorter, Parisi, and Schulz 2001, Shapiro 2001, 2004). In this case, equilibrium prices tend to be higher than those set by a monopoly holder of the whole set of complementary patents. Our argument rests on the observation that (i) a number of innovations are process innovations that influence the competitive structure in a downstream market and (ii) innovations are non-rivalrous. Non-rivalry and efficient marginal-cost pricing imply that the innovation should be used as widely as possible from the point of view of economic efficiency. However, the patent holder may have an incentive to suppress access to his innovation in order to maximize profits.

We conclude this section with an analysis of the optimal regulation of the patent system *ceteris paribus* that an innovation has occurred. If the normative criterion is to minimize the deadweight loss in the downstream market, two cases have to be distinguished.

1. If $s^* = m$, the duration of a patent t is irrelevant with respect to the associated deadweight loss. t has, however, an impact on the distribution of rents between the innovator and the downstream firms. Hence, if the innovator voluntarily sells m licences (the innovation reduces unit costs by less than $c_h(s^* = m) - c_l$), the patent system has only an impact on the distribution of rents. It is therefore possible to vary t in order to shape incentives for innovation at Stage 1 of the game without efficiency costs in the downstream market. This observation has important consequences for a comparison with the alternative contest mechanism. If both, patent as well as contest mechanisms, have in principle the same incentive effects and impacts on individual utility, the choice of a specific type of mechanism is irrelevant with respect to economic welfare. However, if a contest mechanism has idiosyncratic welfare costs or if individual incentives cannot be adequately shaped by this class of mechanisms, the patent system turns out to be superior. Hence, for innovations implying "small" cost reductions the burden of proof rests on the contest mechanism.

2. If $s^* < m$, the duration of a patent t has an impact on the associated deadweight loss: the longer t, the higher DL. Hence, if the innovation reduces unit costs by more than $c_h(s^* = m) - c_l$, patents *ceteris paribus* reduce economic welfare. Extending t in order to improve incentives for innovation at Stage 1 therefore has efficiency costs. Hence, if the incentive and utility consequences of the contest as well as the patent mechanisms are identical for the class of innovators, we have an argument in favor of a contest mechanism if the cost reductions of an innovation are sufficiently big.

2.3 Stage 1

By (10), the innovator's profit from the royalties is equal to

$$s^* r_i = \alpha t \frac{m(2(a - c_l) + m(c_h - c_l))^2}{8b(1+m)^2} =: \Theta_i. \tag{25}$$

Note that these profits are independent on the innovator's investments l_i at stage 1.

2.3.1 Risk neutrality

If the innovator is risk neutral, we can use y_i as a measure of utility. At Stage 1, every potential innovator anticipates the potential profit Θ_i that results if his research leads to an innovation that can be patented. Hence, his optimization problem (4) becomes

$$u_i(l_i) = \gamma_i(l_i)z + \rho_i(l_i)\Theta_i - l_i. \tag{26}$$

The derivative with respect to l_i is equal to

$$\frac{\partial u_i}{\partial l_i} = \frac{\partial \gamma_i}{\partial l_i} z + \frac{\partial \rho_i}{\partial l_i}\Theta_i - 1 = 0. \tag{27}$$

First note that $l_i = 0$ if $z = 0$ and $\Theta_i = 0$, which means that incentives for innovations are neither provided by the patent system nor by a contest mechanism. In order to see whether it is possible to provide optimal incentives by an adequate design of both mechanisms, we first have to specify optimality.

Optimality. In order to determine the optimal incentive scheme to promote innovations, it is necessary to characterize the conditions for an optimal solution. We define optimality by the maximization of the expected sum of consumer and producer surpluses plus the sum of utilities of the innovators and start with a characterization of the first-best.

Given that an innovation takes place, it is obvious that all m downstream firms shall have access to it for the whole time T. The sum of consumer and producer surpluses is therefore $S^m = T(a - c_L)^2/(2b) - DL^*(m)$. Utility of researcher i is equal to $u_i(y_i, l_i) = y_t - l_i = -l_i$. In addition and to close the model we assume that z can be financed by a lump-sum tax imposed on the downstream market, which implies that $z \leq \bar{z} := \sum_{i=1}^{n} S^m$. Given risk neutrality and additivity, this tax cancels from the equation that characterizes aggregate welfare:

$$W(\mathbf{l}) = \sum_{i=1}^{n} \rho_i(l_i)(S^m) - \sum_{i=1}^{n} l_i. \tag{28}$$

The first-best investments in innovation are therefore characterized by the following first-order conditions:

(29) $$\frac{\mathcal{W}}{\partial l_i}(\mathbf{l}^o) = \frac{\partial \rho_i}{\partial l_i}(\mathbf{l}^o)S^m - 1 = 0, \quad i = 1, \ldots, n,$$

where we denote the optimal values of $\mathbf{l}$ by the superscript "o".

A comparison of (27) and (29) shows that it is necessary to induce optimal research incentives for researcher i to have

(30) $$\frac{\partial \gamma_i}{\partial l_i}(\mathbf{l}^o)z + \frac{\partial \rho_i}{\partial l_i}(\mathbf{l}^o)\Theta_i = \frac{\partial \rho_i}{\partial l_i}(\mathbf{l}^o)S^m.$$

Assume first that $z = 0$. In this case, (30) becomes

(31) $$\Theta_i = S^m.$$

However, S^m is the maximum surplus that results from the innovation and is therefore always strictly larger than Θ_i, the maximum profit of the innovator from licensing his innovation, even if the patent span is extended to its maximum T.

Proposition 4: It is impossible to provide first-best optimal incentives to innovate by the patent system alone.

Next assume that $t = 0$. In this case, (30) becomes

(32) $$\frac{\partial \gamma_i}{\partial l_i}(\mathbf{l}^o)z = \frac{\partial \rho_i}{\partial l_i}(\mathbf{l}^o)S^m.$$

For the case of the Tullock CSF and in a symmetric equilibrium, the condition becomes

(33) $$z = \frac{\frac{\partial \rho_i}{\partial l_i}(\mathbf{l}^o)S^m}{\frac{(n-1)}{n^2\mathbf{l}^o}}$$

The right-hand side of (33) is a positive finite number. The feasibility constraint $z \leq \bar{z}$ imposes the additional condition $l^o(\partial\rho/\partial l(\mathbf{l}^o)) \leq (n-1)/n$. This leads to the following conclusion.

Proposition 5: There exists exactly one positive and finite prize z for which efficient research incentives can be induced. A contest mechanism can be used to induce efficient incentives if $\mathbf{l}^o(\partial\rho/\partial l(\mathbf{l}^o)) \leq (n-1)/n$.

This in principle positive result depends crucially on the assumption of risk neutrality of the researchers. Any degree of risk aversion would make it impossible to implement the optimal allocation because individuals would be exposed to additional risk which would *ceteris paribus* decrease their expected utility.

2.3.2 *Risk aversion*

In this section we will focus attention on the behavior of a single innovator who treats the other innovator's choice of effort as exogenous. We set the complete analysis of the comparative static behavior of the Nash-equilibrium choices of l_i aside with the associated technical complexity. For the case of risk aversion, the expected utility of an innovator is given in (3). Given incentive scheme $\{z, t\}$, the innovator will set $l_i(z, t)$ such that

$$\frac{\partial E[u(l_i)]}{\partial l_i} = 0. \quad (34)$$

Due to the strict concavity of $\rho(.)$ and $\gamma(.)$ in l_i and the boundary behavior of both functions, a unique solution exists. Inserting this solution into the expected utility function yields an optimal-value function $\Phi(z, t)$.

For every incentive scheme $\{z, t\}$ and $l_i(z, t)$, the expected total welfare on the downstream market is equal to

$$\begin{aligned} W(z, t) = {} & \rho(l_i(z, t)) \cdot ((1-\alpha) \cdot t \cdot \pi_l^s + (1-\alpha) \cdot (m-s) \cdot t \cdot \pi_h^s \\ & + (T-t) \cdot m \cdot \pi_l^m + \rho(l_i(z, t))(t \cdot CS^s + (T-t)CS^m) \\ & + (1-\rho(z, t))(T \cdot m \cdot \tilde{\pi} + T \cdot \tilde{C}S) - (\gamma(l_i(z, t)) \\ & + \lambda(1-\gamma(l_i(z, t)))z. \end{aligned} \quad (35)$$

Given our focus on a representative researcher, z can have two different interpretations from the point of view of society. First, and this is the interpretation consistent with the idea of a contest, society has to pay z with probability 1 because it is only the relatively most successful researcher who will win the prize. In this case, $\lambda = 1$. Given that we restrict attention to a representative researcher in this section, it can also be interpreted as the contingent payment to researcher i that occurs with probability $\gamma(l_i)$ from the point of view of society. In this case, $\lambda = 0$. We will differentiate between both interpretations in the following analysis because the results turn out to be sensitive with respect to the specific interpretation. CS^S and CS^m denote consumer surplus if s, m firms use the innovation, and $\tilde{\pi}$, $\tilde{C}S$ denote profits and consumer surplus if no innovation occurs. Furthermore it is assumed that the Prize z can be financed by means of a lump-sum tax.

An optimal solution is again assumed to be characterized by the maximization of the sum of the expected utility of the innovator and the welfare on the downstream market, $\Phi(z, t) + W(z, t)$. If an interior solution exists, it is characterized by the following first-order conditions:

$$\frac{\partial \Phi}{\partial z} + \frac{\mathcal{W}}{\partial z} = 0, \quad \frac{\partial \Phi}{\partial t} + \frac{\mathcal{W}}{\partial t} = 0. \quad (36)$$

In order to better understand the structure of the optimal solution, we start by defining the set of $\{z, t\}$ for which the innovator is indifferent. Totally differentiating $\Phi(z, t)$ and applying the Envelope theorem it follows that this set can be described by the condition

$$\frac{dt}{dz} = -\frac{\partial\Phi/\partial z}{\partial\Phi/\partial t} = -\frac{\partial E[u]/\partial z}{\partial E[u]/\partial t}. \tag{37}$$

The total differential of W with respect to z, t can be written as

$$\frac{dW}{dz} = \frac{\mathcal{W}}{\partial l_i}\frac{dl_i}{dz} + \frac{\mathcal{W}}{\partial t}\frac{dt}{dz} + \frac{\mathcal{W}}{\partial z}. \tag{38}$$

The term dl_i/dz can be approximated by

$$\frac{\partial l_i}{\partial z} = -\frac{\partial^2 E[u]/\partial l_i \partial z}{\partial^2 E[u]/\partial l_i^2}. \tag{39}$$

Using (37), we can write

$$\frac{dW}{dz} = -\frac{\partial\mathcal{W}}{\partial l_i}\left(\frac{\partial^2 E[u]/\partial l_i\partial z}{\partial^2 E[u]/\partial l_i^2}\right) - \frac{\partial\mathcal{W}}{\partial t}\left(\frac{\partial E[u]/\partial z}{\partial E[u]/\partial t}\right) + \frac{\partial\mathcal{W}}{\partial z}. \tag{40}$$

(40) has the following interpretation: Considering only pairs $\{z, t\}$ for which the expected utility of an innovator is constant, total welfare increases in z if (40) is positive. We will discuss the sign of all terms in turn.

– The sign of $\partial W/\partial l_i$ is positive if the net-welfare on the downstream market in the presence of the innovation exceeds the welfare without innovation. Whether this is the case depends on the fraction of the profits that can be extracted by the innovator and the welfare without innovation. The condition is unambiguously positive if the innovation is indispensable.

– The sign of $(\partial^2 E[u]/\partial l_i \partial z)/(\partial^2 E[u\}/\partial l_i^2)$ is ambiguous in general. The denominator is negative because l_i characterizes a maximum of the innovator's optimization problem. It can be shown, however, that the numerator is positive for the class of probability functions $\rho = l_i/(l_i + D)$, $\gamma = l_i/(l_i + E)$, $D,E > 0$, and the class of exponential utility functions, $u(x) = x^q$, $q \in (0,1)$.

– The sign of $\partial W/\partial t$ is negative if the net-welfare on the downstream market in the presence of the innovation exceeds the welfare without innovation. Again, whether this is the case depends on the fraction of the profits that can be extracted by the innovator and the welfare without innovation. The condition is unambiguously negative if the innovation is indispensable.

– The sign of $(\partial E[u]/\partial z)/(\partial E[u]/\partial t)$ is unambiguously positive because $\partial E[u]/\partial z = \rho((1-\gamma)v'(z) + \gamma v'(ast\pi_l + z))$ and $\partial E[u]/\partial t = as\gamma\pi_l((1-\rho)v'(ast\ \pi_l) + pv'(ast\ \pi_l + z))$.

– Finally, the sign of $\partial W/\partial z = -(\gamma + \lambda(1-\gamma))$ is unambiguously negative.

Given the level of generality of the model, it is impossible to derive more constructive results. We will therefore continue with a functional specification of the model that allows us to better understand the tradeoffs of the model.

2.3.3 *Functional specification*

In order to get a better intuition for the implications of the general tradeoff, we will use a functional specification of the model in the following. W.l.o.g. we specify the oligopoly market as follows: innovation is indispensable, and the parameters that characterize market demand and cost functions are $a = 100$, $b = 1$, $c_l = 0$, $T = 1$. Recall that these specifications imply that for the time-span $[0, t]$ the innovator sells one licence and that the resulting monopoly profit and consumer surplus is equal to $\pi_l^{s=1} = a^2/4b$, $CS^{s=1} = a^2/8b$. For the time-span $(t, 1]$ the oligopoly profit is equal to $\pi_l^{s=m} = (m-1)a^2/(1+m)^2 b$, and consumer surplus is equal to $CS^{s=m} = m^2a^2/(1+m)^2 2b$.

In addition, we assume that the potential innovator has a utility function $v(.) = \sqrt{(.)}$. Furthermore, the probabilities to win the contest and to generate a patentable idea are perfectly and positively correlated, $(1-\rho(.))\gamma(.) = \rho(.)(1-\gamma(.)) = 0$. For convenience we denote the joint distribution by $\rho(.)$ in the following analysis and assume the functional form $\rho(l_i) = l_i/(l_i + 1)$. The innovator's expected utility simplifies to

$$E[u(l_i, z, t)] = \rho(l_i)v(z + st\alpha\pi_l) - l_i = \frac{l_i}{l_i + 1}\sqrt{z + t\alpha a^{2/4}b} - l_i.$$

Welfare on the downstream market, (36), becomes:

$$\begin{aligned}
W(l_i, z, t) &= \rho(l_i) \cdot ((1-\alpha) \cdot t \cdot a^2/4b + (1-t) \cdot m \cdot (m-1)a^2/(1+m)^2 b) \\
&\quad + \rho(l_i)(t \cdot CS^{s=1} + (1-t)CS^{s=m}) - (\rho(l_i)z - \lambda(1-\rho(l_i))z), \\
&= \frac{l_i}{l_i+1} \cdot ((1-\alpha) \cdot t \cdot \pi_l^{s=1} + (1-t) \cdot m \cdot \pi_l^{s=m}) \\
&\quad + \frac{l_i}{l_i+1}\left(t \cdot \frac{a^2}{8b} + (1-t)\frac{m^2a^2}{(1+m)^2 2b}\right) \\
&\quad - \left(\frac{l_i}{l_i+1} z + \lambda\left(1 - \frac{l_i}{l_i+1}\right)z\right).
\end{aligned}$$

The maximization of the innovator's expected utility with respect to l_i results in an effort level of

$$l_i/t, z, m) = \max\{0, (250000t + z)^{1/4} - 1\}. \quad (41)$$

It is straightforward to check that the second-order conditions hold true. The expression demonstrates two things: first, the marginal expected revenue (measured in marginal utility) must exceed 1, the marginal loss of utility from an additional unit of effort. Second, patent rights and prizes are perfect substitutes from the point of view of the researcher. This latter finding is a direct consequence of the assumption that both risks are perfectly correlated.

Inserting (41) into the expected utility function and the function measuring welfare on the downstream market gives rise to optimal value functions $\Phi\{z, t)$, $W\{z, t)$. We assume again that an optimal solution is characterized by the maximization of the sum, of both terms, $W(z, t) = \Phi(z, t) + W(z, t)$.

Contingent payment, $\lambda = 0$: In this case we get

$$
\begin{aligned}
&W(z, t) = -(250000t + z)^{1/4}((250000\,t + z)^{1/4} - 1) \\
(42)\quad &-\frac{(5000\,(3m-2)(1-t)/(1+m)^2 - 246250\,t - z)((250000\,t + z)^{1/4} - 1)}{(250000\,t + z)^{1/4}} \\
&+1 - (250000\,t + z)^{1/4}.
\end{aligned}
$$

An analysis of (42) yields the following result.

Proposition 6: (1) For $z = 0$, the optimal duration of a patent is positive, $t > 0$. (2) Without patent protection ($t = 0$) the optimal prize is positive, $z > 0$. (3) Numerical simulations show that the optimal prize-patent mix leads to $z > 0$ and $t = 0$.

Proof:

ad 1: differentiating (42) with respect to t, setting $z = 0$ and evaluating the resulting equation at $t = 0$ yields

$$\text{sign}\left[\frac{\mathcal{W}}{\partial t}\right]_{z=0,t=0} = -\text{sign}\left[\frac{1}{125}(2-3m)m\right],$$

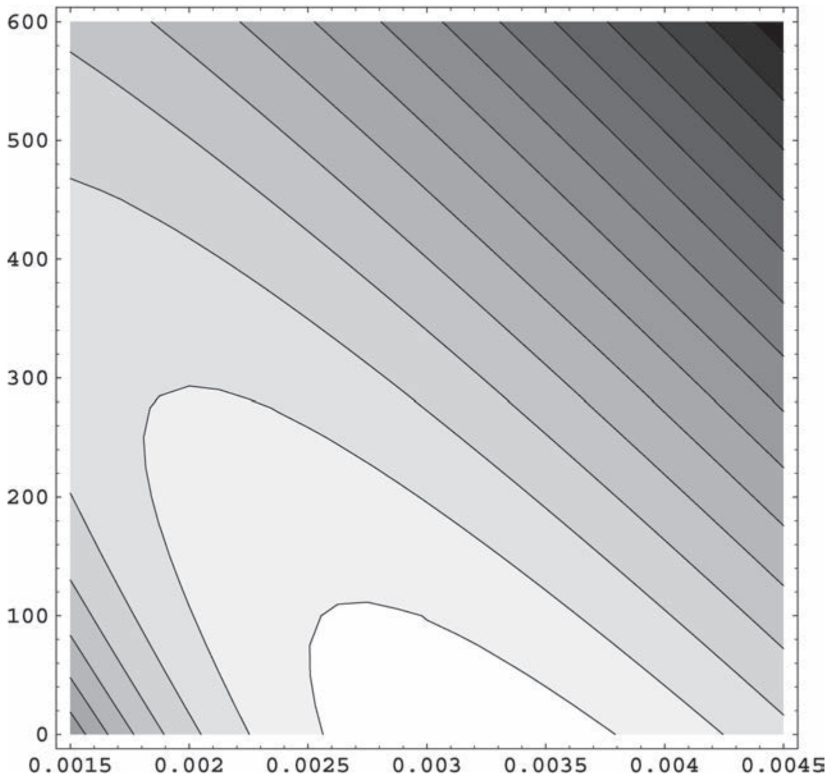
which is unambiguously positive because $m \geq 1$.

ad 2: Differentiating (42) with respect to z, and setting $t = 0$, we get from (41) that $l_i = \max\{0, z^{1/4} - 1\}$, which is positive only if $z > 1$. Hence, for all $z \leq 1$ an increase in z has no impact on incentives while increasing costs. However, for $z > 1$ we get

$$\text{sign}\left[\frac{\mathcal{W}}{\partial z}\right]_{z=0,t=0} = \text{sign}\left[-1 + m(-10002 + 14999m)\right],$$

which is unambiguously positive because $m > 1$. Obviously, $\mathcal{W}(0,\ 0) = 0$. Hence, it remains to be shown that $\mathcal{W}$ does not converge to $r < 0$ for increasing z, which is straightforward to prove.

Figure 2 Welfare-indifference curves for different levels of (z, t) with probabilistic costs (darker shades = lower level of welfare).



ad 3: Figure 2 provides the intuition behind this result.

In the figure, t is plotted along the abscissa and z is plotted along the ordinate for $m = 100$. Each graph represents the locus of welfare-indifference curves. It can be seen for 'small' values of z, there exists an interior local maximum for t, whereas the local maximum for t is equal to 0 for larger values of z. The global optimum is at a point $\{z, 0\}, z > 0$. Unfortunately it has been impossible to derive a closed proof of this result, but it turns out to be robust for all values $m = 1, 2, ..., 200$ for which a simulation has been run.[3]

Part 3 of the proposition highlights that even in the presence of risk aversion the economic costs of the contest mechanism can be dominated by the welfare costs of an inefficient licensing-policy that exists with a patent system.

Non-contingent payment, $\lambda = 1$: In this case we get

[3] Details of the simulation will be provided by the author upon request.

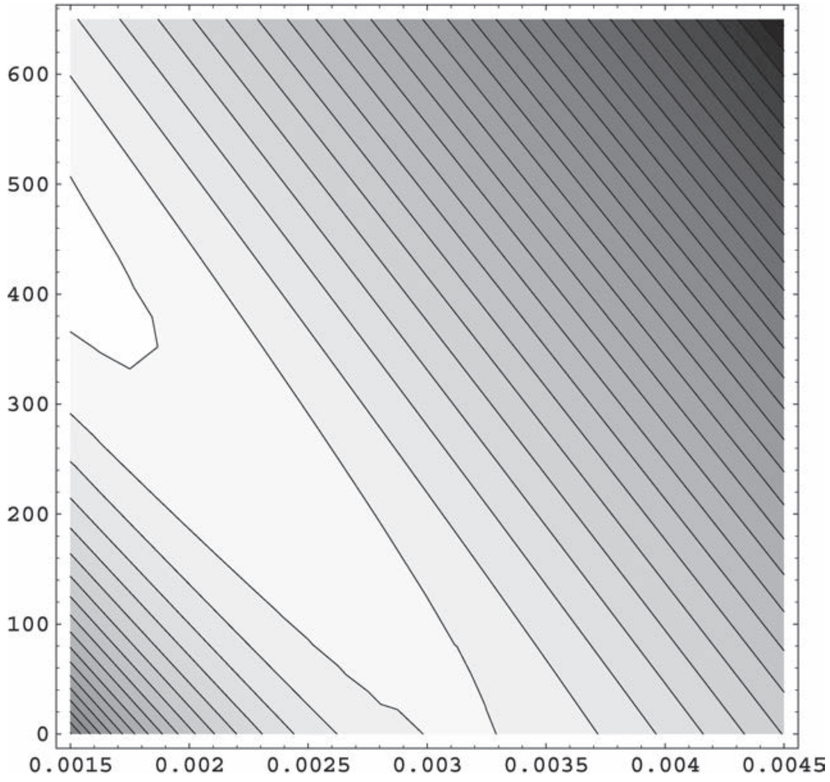
$$\mathcal{W}(z,t) = -(250000\,t+z)^{1/4}((250000\,t+z)^{1/4}-1) \tag{43}$$
$$\frac{(1250\,(197\,t+m)(8+386\,t+m\,(209\,t-12)))(1-(250000\,t+z)^{1/4})}{(1+m)^2(250000\,t+z)^{1/4}}$$
$$+1-z-(250000\,t+z)^{1/4}.$$

An analysis of (43) yields the following result.

Proposition 7: (1) For $z=0$, the optimal duration of a patent is positive, $t>0$. (2) Without patent protection ($t=0$) the optimal prize is positive, $z>0$. (3) Numerical simulations show that the optimal prize-patent mix leads to $z=0$ and $t>0$.

Proof: The proof of parts (1) and (2) are similar to parts (1) and (2) in Proposition 6. Figure 3 provides the intuition behind part 3.

Figure 3 Welfare-indifference curves for different levels of (z, t) with definite costs (darker shades = lower level of welfare).



As in Figure 2, t is plotted along the abscissa and z is plotted along the ordinate for $m = 100$. Each graph represents the locus of welfare-indifference curves. It can be seen that for 'small' values of t, there exists an interior local maximum for z, whereas the local maximum for z is equal to 0 for larger values of t. The global optimum is at a point $\{0, t\}$, $t > 0$. As before, it has been impossible to derive a closed proof of this result, but it turns out to be robust for all values $m = 1, 2, ..., 200$ for which a simulation has been run.[4]

Part 3 of this proposition highlights that there exists no clear evidence in favor of or against a contest mechanism or a patent mechanism. The significance of the idiosyncratic transaction costs differ dependent on the allocation problem at hand.

3 Conclusions

In this paper we have focused attention on the tradeoff between two types of mechanisms that can be used to induce incentives for scientific research, the patent and the contest mechanism. The relative transaction-costs of both types of mechanisms are a result of (a) the incentives of a patent holder to sell licences and thereby influence incentives on a downstream market and (b) the additional innovator-specific risk generated by a contest. It has been shown that the optimal licensing policy of an innovator tends to be suboptimal if the cost-reduction of the innovation is relatively large. In this case, reducing access to the innovation is profit maximizing for the innovator. If the cost reduction of the innovation is, however, relatively small, the innovator has an incentive to sell the optimal number of licences. In the latter case, an increase in the term of the patent merely shifts rents from the firms on the downstream market to the patent holders.

A comparison of the patent and the contest mechanism as means to induce optimal incentives to invest in research must identify the transaction costs of both types of mechanisms. In the case of risk neutrality of the innovators it follows that optimal research incentives can be induced by an adequately designed contest as long as the necessary prize does not exceed the budget. It is, however, impossible to induce optimal incentives by the use of the patent mechanism because the patent holder can only participate in the *surplus* the licence generates for a *firm*. This surplus marginally (which is relevant for incentive design) differs from the *social surplus* generated by the innovation.

The optimal incentive structure is very complicated when risk aversion of the innovators is taken into consideration, and no clear-cut results can be derived. This result is not very surprising, given the anything-goes results from the literature on contest behavior with risk-averse bidders (see for example

[4] Details of the simulation will be provided by the author upon request.

Cornes and Hartley 2003, Konrad and Schlesinger 1997, Skaperdas and Li 1995). However, we have shown for a specific example that in a second-best world with inefficient licensing incentives, extreme solutions can be second-best optimal. Depending on the specific structure of the model it can turn out that the patent mechanism dominates the contest mechanism and vice versa. The results of this section therefore raise more questions than answers are given.

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Scientific Competition: Beauty Contests or Tournaments?

Comment by

Roland Kirstein

1 Basic idea of the paper

Pure public goods are characterized by non-rivalness in consumption (the marginal costs of serving additional consumers are zero) and by non-exclusion (consumers who do not pay for the good may also have access). While non-rivalness is a natural and unavoidable property of information goods, this is not necessarily true with regard to the exclusion principle. In his paper, Martin Kolmar points out that it is a matter of choice whether the exclusion principle applies to information goods. This is the case if society establishes patent right protection. Thereby, society decides whether information goods are pure public goods or club goods. This decision should take into account the respective welfare outcome in a positive transaction cost world. From a property rights point of view the question could be restated: Should the property rights to a specific information good be in the hands of a single decision maker (this would establish a club good), or should it be in the hands of all members of society (this would turn it into a pure public good)?

Society faces two stylized types of mechanism to provide incentives for potential innovators: the contest and the patent. A contest of the Tullock type induces the players to compete for a prize which is awarded according to their relative effort. If innovations are generated, they can be used for free (no exclusion). In a patent system, on the other hand, a patent right and the license fees generated thereof are introduced as the prize for the successful innovator. To University researchers, contests appear to be very familiar. It seems to be the archetype of scientific competition. Researchers are less interested in filing for patents, as their main interest is directed towards prestigious publications, well paid chairs, and the access to research funds. If, however, an innovation has been published in a scientific journal, it cannot be patented anymore (according to German patent law), as it is already publicly available.

2 Political relevance

Kolmar's paper contributes to an ongoing debate in Germany on the abolishment of the "professorial privilege" in the German patent law. A few years ago, the federal government initiated a reform of the Employees' Inventions

Act to abolish the "professors' privilege". Until then, the choice between contest and patent, as described by Kolmar, was left to the professors themselves. Having made an invention, a professor could choose whether to file for a patent (and collect 100% of the royalties) or to publish his results immediately, thereby forgoing the patent. The federal government, however, claimed that the number of patents filed by German universities was "too small", and therefore deprived the university researchers of their privilege. From now on, an invention belongs to the researcher's university (unless the employer rejects it). The university is supposed to install a professional patent management and may file for a patent; the researcher is eligible for only 30% of the royalties.[1]

Kolmar's model provides an explanation why it may have been beneficial not only for the professors, but also for society not to employ the patent system. The researchers were compensated with a scientific career, while society gained free access to their inventions (only the universities were left empty-handed). The federal government seems to have completely overlooked this. To the contrary, it considered the existence of a functioning patent rights system a prerequisite for inventions to have economic value. Moreover, the former German government seems to have been unaware of the fact that prices and value are different economic concepts. A patent right system may be a prerequisite for an invention to have a positive market price. Without exclusion, the market price will be zero, but the invention can still bear value. Even worse, a positive price for a non-rival good would create an ex post inefficiency.

3 Discussion of Kolmar's results

By choosing patent right protection, the exclusion principle applies and a patent holder may charge a license fee from other users of the right. Kolmar shows that a patent holder has an incentive to restrict the oligopolistic competition by selling a smaller than efficient number of licenses if three conditions are met:

- the patent holder competes in a Cournot oligopoly,
- the innovation in question is one that decreases marginal costs,
- and this cost differential is relatively large.

The resulting welfare loss, however, has to be compared with the one generated by the best alternative incentive mechanism. When comparing contests

[1] The economic analysis demonstrates that the new law may miss its goal to increase the number of filed patents; see Kirstein and Will (2006) and Will and Kirstein (2004) with an overview of the relevant literature.

and patents, society should not condemn one mechanism on the grounds that it fails to implement a first-best outcome, but rather look for a second-best solution. For risk-neutral inventors, Kolmar derives that the contest mechanism dominates the patent system. With risk-averse inventors, the results are not as clear cut.

Kolmars paper highlights incentive and welfare aspects of the scientific production process which prove relevant for the recent political debate in Germany. However, some objections deserve to be discussed. The first objection relates to the idea that only "contest" is a tournament, but not "patent". Kolmar models unrelated inventions. In reality, however, the patent system may also be characterized as a rank-order tournament if several researchers explore products or technologies which are substitutes. The extreme case would be a patent race between researchers pursuing the same goal. In such a situation the prize is only awarded to the first inventor. Several patent race models exist,[2] but the Tullock formula may also serve this purpose. The common wisdom is that patent races lead to overinvestment and, therefore, are inefficient.

The second objection may question whether the "contest" between researchers is actually a rank-order tournament. It is a typical property of a tournament that only one prize is used to motivate a group of agents. However, researchers face the prospect of more than one chair or research fund. It may well be the case that several competing candidates all receive a prize. Moreover, it can pay to heterogenize, i.e., to deviate from a strict competition and specialize into an idiosyncratic direction. This may put a researcher into a better position, compared to his competitors, when pursuing a specific prize. In other words, equilibria can be asymmetric. If, however, "patent" may also be a tournament and "contest" is perhaps a much more complicated tournament with multiple prizes and asymmetric specialization, then the comparison of the two mechanisms becomes more complex.

A last objection may challenge the author's view that the Tullock model of rent-seeking actually provides a realistic and correct description of the competition for research funds or tenured positions. What happens in reality is that competitors present their research agendas, and evaluation committees choose the most impressing or promising one. Previous effort may play a role in convincing search committees or referees who decide upon research funds. But the main criterion for its decision is not the past achievements, but the prospects of the candidate or his ideas.

These three objections try to make clear that the difference between "contest" and "patent" is not as clear cut as it was described by Kolmar's model. So what are the differences between the two systems? One difference lies in the respective system's ability to discover decision errors.

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[2] One example is found in chapter 14 of the textbook by Rasmusen (2001).

Scientific competition is characterized to a large extent by ex-ante evaluation, i.e., they are multiple "beauty contests". Candidates may present their plans to several committees or single referees, who decide about awarding one of the available prizes, e.g., a chair or a research grant. For the successful candidate, being awarded a prize implies the access to resources which can be used for either the production of scientific output (research grants, chairs), or for its presentation (publication space, conference slots). In both cases, it may turn out only later whether the winner has actually produced something valuable, either because the production of knowledge only takes place later, or because the published work will be cited.

Ex-ante, the decision-makers are uncertain about the respective merits. Normally, such an evaluation system is characterized by alpha- and beta-errors: the selected candidate may perform worse than expected, and a rejected candidate might have produced a more valuable invention. In extreme cases, when the loser receives no opportunity to pursue his research agenda at all, society will never learn whether the losing candidates would have produced something more valuable. In such a system, an alpha error (the selected candidate has failed to produce anything valuable) can be detected later on, while it would be impossible to discover a beta error.

In the patent system, an ex post evaluation of achieved innovations takes place. The task of the evaluation authority is limited to determining which one of the contestants is the actual winner (the first, the best, the most original), and thereby is granted a monopoly right to use the innovation/invention. This decision can even be made subject to judicial review. It is not excluded that unsuccessful projects, which are not awarded the patent right, may later turn out to be actually superior. Both alpha- and beta-errors can be discovered later, and therefore the quality of the system can be controlled better.

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The Role of Patents in Scientific Competition: A Closer Look at the Phenomenon of *Royalty Stacking*

by

CHRISTINE GODT

1 Introduction

Recently, patents have become both, a product of scientific research and a measure of performance and excellency. Prior to this, patents were confined to industrial development within the market vicinity – aimed at keeping the idea secret inside the corporation as long as possible until the commercialisation of the end product begins. In contrast, basic science was perceived as a separate counterpart to applied science and defended as a patent-free zone. Scientific performance in basic science was conceived as reputation measured by publications. Today, in the field of natural sciences, patents have supplemented publications and citations as an indicator of reputation not only of individual researchers but also of scientific institutions. This development is highly contested in respect to its impact on basic science. Do patents impede or promote science, and in which ways? Will they accelerate research or slow it down? What kind of incentives do they provide for researchers and their home institutions? When patents found their way into the scientific realm in the 1980s, opponents raised concerns that researchers would hold back their results, publish less or later and refuse the exchange of knowledge and material. In the 1990s, concerns were raised that patents would proliferate, thus stifling research and development.[1] Proponents would claim that patents foster scientific competition,[2] that they set an incentive for individuals to invent and for institutions to invest, thus resulting in more innovation.

In the meantime, the debate has become more sophisticated. There is evidence that scientists in private and in public research do both, patent and publish (Stokes 1997, Agrawal and Henderson 2002, Murray and Stern 2005). The long-perceived tension between patenting and publishing does not seem to exist, at least not sharp and measureable. Empirical evidence suggests that access is more willingly granted to patented knowledge than to material (Walsh, Cho and Cohen 2005). Access problems persist in research on clinical

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[1] This discussion is known as the "anticommons debate" – an inversed reference to the famous article "Tradegy of the Commons" by Hardin (1968). The parallel was first drawn by Heller (1998). The debate of how to evalute the process is still ongoing: Is patent protection "too strong" (inter alia Eisenberg 1996a, David 2004) or "too weak" (Heller 1999)?

[2] For the US see, e.g., Nelson (1998), Walsh, Arora and Cohen (2003); for Germany, e.g., Hoeren (2005).

diagnostics, suggesting that problems occur when research is closely related to (or being itself) a commercial activity.[3] Overlapping claims, e.g. related to DNA, make it difficult to know one's own rights and those of others (Verbeure, Mattijs and Overwalle 2005). Special attention is paid to the problem of patented research tools.[4] Consent is growing that patents in science do not function in their traditional sense as incentives for the individual researcher to invent. Researchers respond stronger to other incentives (Agrawal and Henderson 2002). Former high-income expectations of research institutions through patenting and licensing have not been fulfilled, at least not for the average university. Instead, it has become evident that patents play different roles for different actors. In industry, beyond the traditional function of competitive exclusion, patent protection for scientific research results serves two different functions. First, patents commodify information and thus secure the transfer of information between internationally decentralised entities. Second, as patents can be purchased, formally intramural research can be outsourced and re-aquired in a contract-based transaction. In other words, patents are essential for the transfer of knowledge between contractors and the firm. For research intensive, small biotech companies, patents serve to attract venture capitel. For universities, other functions prevail: Patents provide benchmarks for ingenuity and high performance, thus enhancing publicity and profile. Increased international cooperation in every form, between scientists and industry[5] and between scientists across borders,[6] has instigated the claiming of intellectual property rights.[7] Patents can help to establish start-up companies, thus providing career opportunities for graduates.[8] For policy makers in industrialised countries, two functions are important: First, a high

[3] Merz, Kriss amd Leonard et al. (2002), Walsh, Cho and Cohen (2005) – then, patent holders are more likely to assert and researchers are more likely to abandon infringing activities.

[4] The public discussion about research tools (see for the US: National Research Council 2005, Gewin 2005; for the UK: Nuffield Council on Bioethics 2002) has given rise to much research (legal, economic and econometric), see Eisenberg (2000), Holman and Munzer (2000) on the one hand highlighting problems, and Walsh, Arora and Cohen (2003) on the other hand aiming at appreasing and structuring the debate.

[5] See the rationale of the 6th EU Framework Research Programme (recital 1 of the Decision No. 1513/2002/EG from 27 July 2002, Off. J. l 232/1) and the rationale of the funding policies of the German Research Ministry in: Richtlinien für Zuwendungsanträge (BMBF-Formular 0027/01.03, available at http://www.bmbf.de).

[6] See the contributions in Edler, Kuhlmann and Behrens (2003), see also the descriptions of Knorr-Cetina (1999).

[7] In the case of science-industry collaboration, it is the industrial partner who usually has an interest in proprietarily secured knowledge; empirical evidence for the correlation between industry involvement and patent applications of research institutions is provided by Carayol (July 2005, 5 and 13). In the case of science-science collaboration, it is the scientists themselves who are interested in securing their rights to material and knowledge in order to protect their own future research opportunities.

[8] Or can provide additional pension payments – as suggested by Carayol (2005, 14).

patent standard serves as an instrument in global regulatory competition to attract industry, because innovative, high technology firms tend to prefer countries with a high patent standard. Second, patents are meant to enhance the transfer of knowledge from science to industry, thus securing long-term innovation and growth. Therefore, public policy has fostered the collaboration of science and industry, most prominently by funding schemes, and supported the move of patent protection into basic science.[9]

The following article focuses on the patent function of technology transfer and will only cover the technology transfer from basic science to industry. At its center is the question whether there is a causal link between patents in basic research and technology transfer to industry – as often claimed. Thus, it will neither analyse the much debated impact of patents on scientific research behaviour per se,[10] nor will the incentive for the individual researcher be discussed. The article is less interested in the behavioural incentive of patents to invent than in the institutional effect of patents on technology transfer. Thus, it complements the broad debate about the effects of patents in science by providing an additional perspective. It takes patents on scientific results of public research institutions as a given fact, but asks about the commercial logic underlying the assumption of the causal link. It contributes to a better understanding of the functions and different roles fulfilled by research institutions. The modern university systems, especially in Europe, is characterised by a mixture of competition and cooperation which conventional economic approaches are not easily applied to.[11] The article raises the question if a patent is a decisive *sine qua non* condition or just one enhancing factor among many others that instigate technology transfer. Are they important in some sectors, less important in others? Are they beneficial in some, but detrimental in others?

The article focuses on the counterintuitive phenomenon of "royalty stacking". This expression describes the problem of accumulating royalty promises in the research process which results in an ever decreasing profit margin until the research result is "ready" to be transferred to the process of product

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[9] Funding rules require researchers to secure intellectual property rights in their research results. Technology transfer offices are fostered, in Germany as an integral part of the patent reform that abolished the so-called professor's privilege in 2002. This provision had assigned their inventions to them personally. By now, all inventions can be claimed by the university or research institution.

[10] A lot of research has been done in respect of how scientific research has changed under the influence of the hybrid incentive structure of traditional norms and commercial incentives, see only Godt (2007, Chap. 3), v. Overwalle (2006), v. d. Belt (2004), Rai and Eisenberg (2003), Heller and Eisenberg (1998), Blumenthal et al. (1997). Until today, the legal discussion has revolved around the question how science can be shielded and whether the given instruments are sufficient, especially the so-called research exemption in patent law Galama (2000), Holzapfel (2003), Godt (2007, Chap. 6).

[11] Mowery and Sampat (2005, 233) describe this analytical lacuna.

development. Therefore, the phenomenon threatens the very idea of technology transfer from science to industry. It is counterintuitive because it contradicts the very assumption that property rights result into the most efficient distribution of ressources. Therefore, the analysis of the phenomenon of "royalty stacking" may help to understand the conditions required for technology transfer to happen, but may also improve our understanding of the boundaries beyond which the dynamics of the patent system are more detrimental than beneficial to basic science – and in the long run to industrial prosperity and to society as a whole.

The article proceeds as follows. First it describes the phenomenon and its generation (2). It then puts the phenomenon into the broader context of technology transfer in the information society (3). Taking these considerations into account, it portrays some possible policies for the various actors involved (4) before drawing some final conclusions (5).

2 "Stacking Royalties"

The expression "Stacking Royalties" describes the "problematique" of accumulated negotiated royalties by researchers in the subsequent research process. If the profit margins for the commercial developer have already been used up before the developer comes into play, technology transfer from science to industry will not happen. The patent attorney Philip Grubb estimated that a royalty accumulation of 20% is the limit for transfering the research result to the industrial process of product development.[12]

There are two causes for the accumulation of royalty claims, one being proprietary, the other being contractual. The proprietary cause is at the heart of the patent system. Problems with this type of accumulation are in built and, until today, dealt with either statutorily or in corporatist ways. However, problems occur in the modern science system because these practical mechanisms are not available to research institutions and because the ever broadening scope of patent protection affects science in particular. The contractual cause is the one that gives rise to yet unresolved challenges for science. Both are mutually reinforcing.

[12] Oral presentation during the workshop on "Genetic Inventions, Intellectual Property and Licensing Practises", organised by the German Federal Government (BMBF) and the OECD, 24/25 January 2002 in Berlin.

2.1 Property

For the sake of analytic precision, "proprietary royalty stacking", first, needs to be distinguished from "stacking patents". The latter, technically called dependency, is *the* central patent mechanism.

2.1.1 Linear dependency distinguished

Dependency describes the "stacking of patents" (not royalties). It is the key to the patent system as it upholds the incentive to invent during the process of continuous progress. It makes the patent the strongest form of intellectual property in comparison with copyright or plant varieties. First of all, the patent provides an incentive to any innovator by granting him/her a time-limited monopoly.[13] However, any further improvement, in principle, has the potential to destroy the economic value of the former innovation before the patent expires. This is what Schumpeter (1942) called "the process of creative destruction". Therefore, in order to uphold the incentive to innovate in the pursuit of progress, the system links initial patents to subsequent patents of follow-on innovators. The idea is that although the subsequent invention is "novel", "non-obvious" and "inventive" and thus patentable on its own, this patent is still covered by the scope of the basic patent.[14] The legal consequence is that neither the base patent holder nor the improver are allowed to use the invention of the other unless authorised by a negotiated license. This mechanism creates mutual blocking rights[15] and enables the pioneer inventor to reap some of the benefits of subsequent improvements. Dependency provides the balance between the incentive for the pioneer and the incentive for improvers.[16] In principle, dependency does not result in royalty stacking. If one patent builds on a previous one (linear dependency), any follower can promise a share of his/her own profits when using a former invention. Privious royalty promises can only be for shares of this promise; thus they do not accumulate over time.

For applied industrial research, linear dependency has not yet caused insurmountable problems (Kowalski and Smolizza 2000). Although history

[13] However, time limits differ considerably. Patents have a maximum livespan of twenty years after first application (although less than half are prolonged after 10 years by their owners). Copyrights usually last seventy years after the death of the creator.

[14] For the dogmatic distinction between "novelty" of the inventive idea and "breadth of a patent scope" which form the basis of dependency in patent law, see Godt (2003, 11), Godt (2007, Chap. 7).

[15] Merges (1994); for an economic description of the equilibrium between sufficiently strong incentives for the pioneer and the improvers, see Scotchmer (2004).

[16] Although, unsurprisingly, the definition of the „right balance" is highly contested. On the quest for a broad patent scope for the pioneer see, e.g., Kitch (1977), on the quest for sufficiently large incentives for the innovators see, e.g., Nelson (2000), Merges (1996), Scotchmer (1991).

has witnessed situations of blockage in the optics and the aviation industry (Merges 1994, 1996), choosing between the exclusion of competitors and granting a license is a business decision geared by strategic considerations.[17] The hightened concern about rising transaction costs in patent litigation (Fischermann 2005, Kanellos 2005) led economists and lawyers to advise the tightening of patentability requirements (e.g., Merges and Nelson 1990, Barton 2001, 881) by the internal reorganisation of patent offices (Moufang 2003, Straus 2001b, Barton 2000) or by third party review.[18] Besides, ignoring infringements is as widely known[19] as (non-infringing) parallel developments (Scotchmer 2004, 140ff.). Under the threat of compulsory licenses and antitrust motions, industry has usually been willing to find arrangements, preferably via cross-licensing. As a consequence, dependency has until recently attracted little academic attention beyond the field of self-reproductive material.[20]

Problems occur, however, when a patent depends on too many previous independent patents ("property rights complex") (2.1.2) and when too many further developments depend on one basic patent (2.1.3).

2.1.2 Dependency on too many patents: The "property rights complex"

The problem of dependency of one patent on too many parallel patents and the resulting royalty stacking is not a new one for industry and is dealt with under the heading of "property rights complex". The profitable development of an end product is put at risk when too many employees of different firm sections claim a share of the profits from a new (typically assembled) product. In Europe, this problem is explicitly dealt with in remuneration rules for employee inventions in private firms and in public service.[21] As an annex to the law governing employee inventions (German: Arbeitnehmer-

[17] Although the strategic use of patents puts some pressure on the system, see Barton (2000, 2002), European Commission (2003).

[18] Either envisioned as an administrative (Jaffe and Lerner 2004, 22) or a judicial procedure (Lemley 2001).

[19] Schmidtchen (1994, 37), notes two examples: the un-licensed production of light bulbs by Philips and the un-licensed production of plant-oil based butter (margarine) by Jurgens and van den Bergh (later Unilever), both resulting in a market-dominating production.

[20] The classic example is the *sui generis* system of plant varieties, for a concise historic account with an outlook on modern biotechnology see Winter (1992) and Straus (1987).

[21] In Germany: "Richtlinien für die Vergütung von Arbeitnehmererfindungen im privaten Dienst" (RLArbnErfprivD) 20 July 1959 (Bundesanzeiger Nr. 156 v. 18. Aug. 1959), version 1 Sept. 1983 (Bundesanzeiger 1983, 9994). Pertaining to inventions of employees in public service according to "Richtlinien für die Vergütung von Arbeitnehmererfindungen im öffentlichen Dienst" of 1 Dec. 1960 (Bundesanzeiger Nr. 237 from 8 Dec. 1960), enacted as Executive Order of the Minister of Labour after consultation with representatives of employers and employees, based on § 11 ArbnErfG; printed in Bartenbach and Volz (1999, 2002).

erfindergesetz, ArbNErfG), No. 19 of the German remuneration guidelines holds that the value of the whole complex shall be evaluated if a process or a product uses a number of prior inventions.[22] This value (in practice usually 1 to 3% of expected profits) is to be shared by all previous inventors – taking each contribution to the whole into account. Disputes are settled by an arbitral body ("Schiedsstelle") (§ 29 ArbnErfG).

This rule builds on the concepts that each employee is entitled to his/her invention although he/she is paid for making inventions. Technically, only the employer has the right to claim the invention. If the invention is claimed, compensation is due to the employee. This system, installed in Germany in the 1930s, has come under pressure due to the bureaucratic burden for the employer and the risk to miss the four-months deadline (§ 6 sec. 2 ArbnErfG). A national draft reform proposal aims at making the system easier. It proposes the removal of the deadline and of the instrument of the employer to claim the employee's invention ("Inanspruchnahme"). Also the remuneration system is to be simplified. Instead of a share in profits, the employee shall only be entitled to lump sums, with additional royalty promises remaining optional.[23]

In the scientific environment, things differ in three aspects. First, as one single innovative development is usually not confined to one institution, the corporatist mechanism of evaluating "the whole" is not available to a research institution. Typically, dominant patents are owned by a plurality of research institutions. Second, the problem is exacerbated especially in molecular biology by the necessity of using a large array of research tools. Third, according to German law, university scientists are entitled to 30% royalties (§ 42 No. 4 German ArbNErfG).

2.1.3 Too many dependant patents: The inverse "property rights complex"

Problems also occur when too many patents depend on one base patent. This is the problem that has prompted the lively debate about anticommons.[24] Base

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[22] "Schutzrechtskomplexe" Nr. 19 RLArbnErfprivD: "Werden bei einem Verfahren oder Erzeugnis mehrere Erfindungen benutzt, so soll, wenn es sich hierbei um einen einheitlich zu wertenden Gesamtkomplex handelt, zunächst der Wert des Gesamtkomplexes, gegebenenfalls einschließlich nicht benutzter Sperrschutzrechte, bestimmt werden. Der so bestimmte Gesamterfindungswert ist auf die einzelnen Erfindungen aufzuteilen. Dabei ist zu berücksichtigen, welchen Einfluss die einzelnen Erfindungen auf die Gesamtgestaltung des mit dem Schutzrechtskomplex belasteten Gegenstandes haben."

[23] For a critical economic analysis see Will and Kirstein (2004). Kirstein and Will (2004), arguing that the profit share is less efficient than a bonus contingent on the project value.

[24] The anticommons debate as a discussion about "the right patent scope" has displaced the formerly more popular questions with economists about the optimal time length of patents (Merges and Nelson 1990, Scotchmer 1999 and the differentiation of patent protection between industries Lemley 1997).

patents which are too broad might block research and competing developments, following (dependent) patents might be too narrow to be economically useful and therefore poison the system by increasing transaction costs and make research more expensive. However, at first glance, the growing number of dependent patents does not instigate the stacking of royalties – the focus of this article. On the contrary, the smaller the scope of patents becomes, the smaller is the chance that other patents will depend on them.

A closer look reveals something else: Not only does the broadening of the patent scope increase the amount of improvements covered by the scope of a prior patent. The growing scope creates the often deplored "patent thicket" (Shapiro 2001) of overlapping claims. This problem is most virulent in molecular science when a nucleotid sequence or a gene sequence is covered by more than one patent (Jensen and Murray 2005, 240), but it also troubles the information industry (David 2000). It was originally dealt with by the outright exclusion of discoveries and theories. With the move of the patent system to cover research results and information, especially in the fields of biotechnology and information technology, this "easy solution" has been blocked.[25] Problems, formerly crowded out by the discovery/invention distinction, seriously threaten the functioning of the patent system.[26] And they also instigate dependencies which result in the accumulation of royalties.[27]

The discussion about the right definition of patentable subject matter (technically the distinction between invention and discovery), in principle, is an old debate about the proper balance between a sufficiently strong incentive for the inventor and the sufficiently broad leeway for improvers. The concepts were transposed to modern science by the economist Suzanne Scotchmer (1991) in her seminal paper.[28] She holds that "sequential innovation" is a specific characteristic of the modern science system. She re-defines modern scientific progress in ways that were formerly enshrined in considerations on the exclusion of discoveries and theories from patentability. Thereby, she inspired the modern debate about the right scope of patents and problems which are due to patents being either too numerous and too narrow or being too broad and thus impeding subsequent developments.[29]

Yet, this discussion is dominated by a discourse about access rights to research results for scientists. The perceived problem is the exercise of

[25] Bearing in mind that the distinction between discovery (theories) and invention has always been conceived as an "entry" qualification to the patent system rather than a semantic definition. See for the historic example of the chemical dye industry v. d. Belt (1992); for modern biotechnology Straus (2001a), Godt (2007, Chap. 2).

[26] For a considered analysis of scientists not known as radical critics of the patent system see Cornish (2004); also the contributions in Dreyfuss, Zimmerman and First (2001).

[27] Seriously considered as a problem also recently by Jensen and Murray (2005, 240).

[28] Scotchmer (1991), later finetuned in Green and Scotchmer (1995).

[29] See the "anticommons debate" (Will and Kirstein 2004, Kirstein and Will 2004).

exclusion and the rising costs of research. Therefore, reflections aim at shielding science from the exercise of patents via a broad research exemption (Eisenberg 1987, Barton 2000, Gold, Joly and Caulfield 2005) or via access-securing compulsory license type mechanisms.[30] These solutions would also ease the problem of stacking royalty promises that follow from licensing. However, with research institutions becoming normal commercial partners and scientific patenting becoming an everyday phenomenon, research exemptions and compulsory schemes will continue to be narrow and rare.[31] Therefore, the problem of royalty stacking will also remain unresolved.

2.2 Contracts

2.2.1 The beast of the knowledge society

The second mechanism for royalty accumulation are contracts. Contractual arrangements can even be more intricate than the property mechanism. The latter only functions when a patent is technically dependent on a plurality of prior patents. Thus, only "using" a patented method in research without making it part of the new patented invention will seldomly result in a veto right or in a claim to royalties. However, contract clauses might "reach through" the use of the patent to future patents to be created (or future contracts) by stipulating that the owner of the patented reseach tool is entitled to royalties from those patents that will only result from using this research tool.[32] This can result in stacking royalties.

There are various reasons for the owner of an intellectual property to negotiate such clauses. Evidently, it helps to keep track of the market. Tracking future dependent patents is difficult. More important is that information goods are licenced instead of sold. In contrast to the industrial era, property of a patented product is not simply or necessarily transferred – like a high-tech microscope. In the information era, only the use of the technology is consented – i.e. licensed. The transfer of property is not at the center of interest. Important is the control of use. For copyright, contractual clauses

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[30] Such as the newly discussed clearing-house mechanism for patented diagnostics; see contributions to the Conference "Patents and Public Health", organised by Overwalle under the umbrella of the CIPR, Leuven, Belgium on May 27, 2005, http://www.law.kuleuven.ac.be/cir/conference_27may.htm (visited 7/05).

[31] The Supreme Court of the US upheld a decision of the CAFC in *Duke University v. John Madey* which narrowly interpreted the experimental use exemption as not covering academic non-commercial use per se; for a commentary see Eisenberg (2003).

[32] To be clear: These do not necessarily depend on the previous patent.

allow the restriction of duplication[33] In science, these contracts not only include use restrictions which evidently impede scientific freedom[34], they also promote the stacking of royalties.

2.2.2 Information contracts in science

The public debate about "reach through contracts" as a problem for scientific research was first lauched by an expert advisory committee of the US National Institutes of Health in 1998 (National Institutes of Health (NIH) 1998). It was embedded in the broad discussion about research tool patenting. This committee was the first to frame it as a problem for scientists and labeled it "royalty stacking": When scientists do research, they depend on a variety of research tools (material, methodologies, know-how) which need to be licensed.[35] However, in contrast to industry, additional drivers are in place in science when stipulating the contract fostering the accumulation of royalty promises:

When negotiating a license, the typical remuneration are royalties. In principle, royalties are in the interest of both parties. The uncertain value of the information good is captured by a percentage of profits earned later in the development instead of a fixed price. Payment is postponed until the commercial value materialises. The licensee does not have to procure money immediately. The licensor hopes that the share in profits will be higher than an actual payment.

The effects of these basic principles are reinforced in the scientific environment. For the licensor of a patented research tool, science is the only market and the only source of income. Research tools do not usually give rise to "dependency" of subsequent patents because mostly they *enable* research but do not necessarily form part of the subsequent invention.[36] Therefore, as the chances of future proprietary profit participation are small, the immediate selling prize must be high – but this high price is difficult to realize. In fact, at this early stage the value often seems to be low – a point in favor of royalties. Also, the licensee will normally not be the one to develop the final product ready to be commercialized. Therefore, it is in the interest of the licensor to secure some profit from the value enhancing chain by "reaching through" the contract. The license permits the broadening of the group of people obligated to the original licensor. The contract can not only obligate the licensee to pay

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[33] This issue has been intensively discussed as a problem of private legislation undercutting publically secured access rights, see Reichman and Franklin (1999, 964), Samuelson and Opsahl (1999).

[34] This problem was analysed in Godt (2007, Chap. 6).

[35] Type 2 of the three types of cumulativeness of Scotchmer (2004, 144); also coined as "stacking licenses", see Runge (2004, 821).

[36] A big exemption from this rule are gen patents. Both diagnostics and therapeutics will typically be dependent on isolation patents.

a share of his/her profits made when he/she succeeds in improving, patenting and licensing. It can also require him/her to transfer the royalty obligation in favor of the old licensor to the next scientists taking up the research.[37] Assuming that a final research result builds on a broad range of "in-licensed" technologies (apart from previous dominant technologies), such promises accumulate over time.

For scientists as licensees, the royalty promise is of no concern with regard to the problem of the unknown market value of the information good. From their perspective, future royalties will not be debited to their current research budget, but will be borne by the research institution or future aquirers. Therefore, they as well have an incentive to negotiate royalties.[38] In addition, the royalty promise reduces the time investment in negotiations and provides them with quick access to the research tool.[39]

Consequentually, contractual promisses contribute to royalty stacking.

2.3 Discussion

Summing up, with patents being registered in science long before a product becomes reality, two mutually reinforcing factors contribute to the risk of royalty accumulation, a proprietary and a contractual mechanism. The proprietary mechanism touches on the sensitive question of the science/market distinction that was once captured by the invention/discovery distinction. Academically new and challenging, however, is the contractual mechanism. This reason for royalty accumulation deserves more attention. Up to now, patent lawyers and economists have focused on the exclusionary function of property rights and on contracts only as far as the concern the right to exclude. The tectonic shift from sales to lease in information goods has as yet attracted little theoretical analysis.[40]

Under both mechanisms research patents run the risk of accumulating royalty promises before they are finally ready to be commercialised ("royalty stacking"). Thus, the causal link between patents and technology transfer is not as compelling as is often claimed. Patents are one, but not the only condition for technology transfer to happen. Industry will not be interested in aquiring research patents if substantial profit shares have already been assigned to others. Therefore, stacked royalties ultimately threaten the transfer of (patented) knowledge from science to industry.

[37] Type 3 of the three types of cumulativeness of Scotchmer (2004, 145).

[38] Not taking into account institutional long-term interests (like the problem of stacking royalties).

[39] Patience is a decisive factor that influences the "efficient" prize, see Güth, Kröger and Normann (2004).

[40] For a first account see Godt (2007).

3 Technology transfer in the context of the information society

Before addressing policies of how to deal with the stacking of royalties, a brief historical note seems appropriate. The shift of paradigms in research policies came about in the 1980's. In the late 1970s, policy makers had identified a slowing down of innovation in Western economies whereas global technological change was accelerating. Thus, they turned to intellectual property as a classical incentive for innovation and strove for reform, both in the US and in Europe. In the US, the initial idea was to strengthen small and medium sized companies. This was the approach of the celebrious Bayh-Dole Act of 1980. The Act transferred the property of patents resulting from governmentally sponsored reseach to the inventor. Prior to this, those inventions had generally been assigned to the government. However, it came as a surprise that it was the universities and research institutions which primarily profited from the Act. By patenting, they attracted large amounts of investments, gave spin-offs an economic base to start with, and thus not only nurtured, but provided the emerging New Economy with the essential knowledge base. Shortly after its first enactment, the Bayh-Dole Act was adapted to this realization.[41] Even if initial expectations of high revenue only materialised for few universities, the activities of the newly established technology transfer offices strengthened the regional knowledge base of the economy and the reputation of research institutions.

In Europe, the process developed differently. Although driven by the same concern, the legal set-up was fundamentally different. Legally, patents were always assigned to the inventor. In universities, the so-called "Professor's Privilege" safeguarded the inventor's ownership of the invention as part of the academic freedom.[42] Public laws provided for equitable licences granted to everybody when an invention was publicly funded. This mandatory requirement came under pressure, first inside the EU member states,[43] later in EU research policies.[44] Publicly funded research results were diagnosed as not being turned into "useful products", and the mentioned restrictions on the exclusivity of property rights were identified as the reason (Ullrich 1997). By now, public access rights have been either abolished or relegated to administrative regulations.[45] The owner only has the obligation to use the

[41] A short history of the Bayh-Dole Act is provided by Eisenberg (1996b).

[42] Formerly Art. 42 German Employee Inventions Act (Arbeitnehmererfindungsgesetz, ArbNERfG).

[43] See for Germany the advice of the expert group to the Ministry of Science and Technology, Ullrich (1997).

[44] 6th EU Framework Programme, Art. 23 Reg. (EC) No. 2321/2002, Off. J. L 355/23.

[45] E.g. No. 8.1 Internal Regulations of the German Ministry for Education and Research ("Besondere Nebenbestimmungen für Zuwendungen auf Ausgabenbasis") (funding for public research institutions), BNBest-BMBF Juni 2002): Free access has to be provided for other academic research institutions.

results.[46] Patent owners have almost unrestricted power of their intellectual property rights and are even allowed to license them exclusively. Also, the "Professor's Privilege" has been abolished in major EU countries.[47] Like any other employer, the university can claim the intellectual property right with due compensation to the personnel.[48] This reform provided the technology offices with the proper base for professional management of the universities' patent portfolios. Thus, in contrast to the US and in contrast to popular policy perception,[49] the patent was not deployed in its classical way as an initial incentive to invent. The fact that universities come up with innovative ideas is taken for granted.[50] The regulatory core idea was that scientific research patents would instigate technology transfer from research institutions to industry because the knowledge is proprietarily secured. Thereby, the design of scientific research became less geared towards questions valued by the epistemic scientific community but more towards industrial interests. This redefinition of science policies became known as a paradigm shift from science being a "push partner for industry" to industry becoming a "pull partner for science".[51] In other words, it turns the old perspective of science as "producer driven" vis-à-vis the consumers (the colleagues)[52] towards a closer science/industry relation. These motivations of industry and economic policy makers coincided with expectations of policy makers and scientists alike that research institutions could do both, attract additional private funding for research prior to an invention and, after the invention is made, could sell their research results, thus contributing to their funding themselves. Although these expectations have not materialised (not for most US universities, even less in the EU), the effects to improve the knowledge base of the overall economy

[46] For the EC: Art. 23 Reg. (EC) No. 2321/2002, Off. J. L 355/23; for Germany: Nr. 4. 2 BNBest-BMBF June 2002 (ibid); German Research Foundation (DFG): No 13 and 14 "Verwendungsrichtlinie Sachbeihilfe; Vordruck 2.02".

[47] European Commission – Expert Group (2004, 15). In Germany "Gesetz zur Änderung des Gesetzes über Arbeitnehmererfindungen vom 18. Januar 2002", in force since 2 July 2002, BGBl. Part I/2002, p. 414. (Jurisdictions that still adhere to the Professor's Privilege are Finland, Sweden, Norway, and recently installed by Italy).

[48] Although some restrictions apply: e.g. the academic scientist retains the right to publish freely (§ 42 sec. 1 ArbNErfG).

[49] Portraying patents also in the academic sphere as behavioral incentives to invent.

[50] The driving force for academic innovation has been attributed to the scientific norm of esteem in the scientific community, first described in depth by Merton (1938/1973, 1942/1973).

[51] In the EU lauched with the 5th Framework Programme in 1998; in the US through developments instigated by the Bayh-Dole Act 1980, see Godt (2007, Chap. 3); Mowery and Sampat (2005, 224ff).

[52] For an economic behavioral analysis of this relation see Albert (2006).

have been acknowledged. A cooperative system between science and industry has emerged.[53]

From the patent systems' and the behavioural perspective, the key question is whether innovation has become causally stimulated by these reforms fostering technology transfer. As far as preliminary results go, the evidence seems to be mixed. There are other factors that influence the cooperation between science and industry as much as the availablity of patent protection. Beyond institutional and intrafirm organisational arrangements (Owen-Smith and Powell 2001, Bercovitz et al. 2001), there are other legal aspects that foster or impede technology transfer. For instance, in contrast to the US, European provisions on joint ownership do not allow one-sided licensing without the consent of all co-owners, thus slowing down technology transfer (European Commission – Expert Group 2004, 16–17). Property laws in Europe are fragmented. Technology Transfer Offices are still in the process of being built up. Also, the majority of scientists still adhere to classical research norms like instant publishing and cooperative exchange. Both are potentially detrimental to the claim of patents. Where an adaptation to financial incentives in science has occurred, the repercussions of patents on research[54] as well as the repercussions of scientific patenting on the patent system itself (Nelson 2000) have been criticised.

Therefore, it is safe to say that the "problematique" of "royalty stacking" is one facet of the changing environment of the science/industry interface. However, if there is neither technology transfer, nor financial gain for the research institutions, then the suspension of classical research norms cannot be justified. The phenomenon of "royalty stacking" re-traces the profound structural differences of research in academic and industrial settings. It points at problems that were formerly delt with by the exclusion of "discoveries" and "theories" from the patent system. Those problems re-surface and are reinforced by contractual "reach through" arrangements. Stacked royalties undermine both, the policy of why the patents were installed in the realm of science, and the traditional norms of science (as described by Robert Merton). Impeding both patent mechanisms and mechanisms of science will hamper the overall pace of innovation in the long run.

However, it is illusionary to expect that the former invention/discovery distinction can be reinstalled. The convergence is due to the fading distinction between basic science and applied science that is part of the information society. Therefore, other policies must be devised to deal with occurring problems.

[53] Coined by the EU as "innovation system", European Commission – Expert Group (2004, 32).

[54] See only critics like v. d. Belt (2004) and Krimsky (1999).

4 *How to catch the beast?*

How can the various actors deal with the problem of stacking royalties? In the following, the capacities of industry (1), research institutions (2) and governmental public policy (3) will be considered.

4.1 Industry

As a first reaction, industry could consider the acquisition of research results early in the process. However, this motion contradicts contemporary industrial philosophy to reduce R&D costs by acquiring research results at a fixed price later in the process when commerciability becomes a probable option.

Therefore, strategies must be more effectively geared towards avoiding royalty stacking in scientific institutions. A first step, especially for IP managers in industry and lawyers in private practice negotiating these contracts, is to understand the functional differences of how research results emerge in a public and in a private research setting. Although the difference between basic science and applied science in respect of marketability has largely vanished, the process of how research results are produced is still different. This realization should caution against the transposition of contract clauses that may be common to industry, but may have different effects and be ultimately detrimental in science. Whereas industry has its own ways of dealing with burgeoning patents and licenses (mergers and acquisition, closed or open patent pools) (Scotchmer 2004, 157), science is not in the position to apply these strategies.

A starting point for industry involves two aspects. On the one hand, it can acknowledge that proprietarily secured technology transfer is perceived as socially valuable by both public policy and research institutions. On the other hand, it should understand that the dichotomy of the private and public research realm is ultimately favorable to economic evolution. Taking both into consideration, industry has at least two options to prevent the accumulation of royalties in research institutions. First, it can refrain from negotiating royalties. This seems to be a cooperative (information) problem inside industry that needs to be resolved. Any licensor of a research tool has an interest in negotiating as high a percentage as possible irrespective of the danger that the profit margin is used up before any end product has reached the market. The bottom line is, however, that everybody loses out because no product at all will be developed. This consideration might induce industrial associations to draw up a code of conduct aimed at reducing use restrictions and favoring one-time payments instead of royalties when licensing research tools to public research institutions. Second, industry can finance research tools, promote their pooling and open access, either by putting them into the public domain or by pooling them via “one-stop” (clearinghouse) arrangements.

4.2 Research institutions

The most eminent goal for research institutions is to formulate a patent strategy that articulates the profile of the research institution and adopts corresponding rules. These policies will position the institution somewhere on the line between a merely publicly funded institution driven by research interests formerly labeled as basic science (with no obvious commerciability) and an applied science institution aiming at revenue generated by the sale of research results to industry. Such policies will include the duties and freedoms of scientists, principles of their remuneration and publication rules[55] (especially rules on publication if research is funded directly by private companies).

These policies translate into patent policies: If a research institution aims at being a basic science institution, not interested in technology transfer, then it should be easy to convince a licensor of patent tools to sell a tool instead of licensing it. This strategy can be complemented by the recommendations of the Dutch Advisory Council for Science and Technology Policy (AWT) which advises research institutions not to patent very basic and broad inventions (Dutch Advisory Council for Science and Technology Policy (AWT) 2001). From the perspective of the licensor, the revenue in these institutions is uncertain anyway. This might help institutions such as Max Planck Institutes to avoid royalty promises altogether. On the other hand, for institutions working very closely with industry, royalty promises will be unproblematic. Industry is used to the royalty quarrels. The challenge lies with the "middle range" institutions, i.e. most universities. They have to devise procedural strategies to avoid royalties as far as possible. One policy principle might be to oblige their researchers to avoid royalties by first trying to buy the tool. If this is economically unreasonable, they must negotiate the smallest possible royalty. Also, a form of recordkeeping needs to be installed, in order to stay below the 20% margin that impedes later commercialisation.[56]

4.3. Government public policy

Stacking Royalties has to do with the newly emerging commodification of information, with the patenting of research tools and "reach through" contracts. Governments should approach the emerging problems more courageously. Mechanisms need to be devised for the financing of research tools. Administrations can pool them, provide public access, or help industry to find

[55] In respect to clauses relating to publication freedoms, a variety of model contracts are already available, an overview is provided by Peter and Runge (2004).

[56] The record is also important for use restrictions.

"one-stop" solutions, devise policies promoting free access of non-commercial research institutions to research tools.

One important instrument is the regulation of public funding. The licensing of research tools can be limited by obliging recipients of public funding to provide free access to emanating research results. Here, more economic research needs to be done.[57]

5 Conclusion

The phenomenon of "royalty stacking" threatens the very goal of technology transfer from science to industry. In this respect, it is a challenge to research policy. It is a result of two distinct mechanisms, one proprietary, the other contractual. The proprietary mechanism is rooted in the expansion of patents into areas traditionally defined as "discovery" or "theory" and formerly excluded from the patent system. The contractual mechanism is primarily due to the transition from sale contracts to lease contracts in the user market. In combination, these two mechanisms can have detrimental effects on the transfer of technology from science to industry when the royalty share becomes "too large". Two lessons can be learnt: First, the claim of patents does not *per se* secure the transfer of knowledge. A patent is only a *conditio sine qua non*, but other conditions have to be met as well. Second, the phenomenon of "stacking royalties" sheds light on the diverse nature of the scientific process. There are areas which are suited to commercialization, there are others which are not. The latter seem insusceptible to market mechanisms. Patenting in the field of basic science which was formerly classified as a market failure (justifying public funding) gives rise to problems that were once delt with by its exclusion from the patent system. With the fading distinction between basic and applied science, new mechanisms have to be devised in order to conserve scientific norms if science is to continue to serve as an incubator for "fresh knowledge".

Thus, the phenomenon of "stacking royalties" helps to understand changes and continuities in science. Even if the concept of science and the market as opposites seems outmoded, differences persist. Science as a system has become diverse, integrating areas which can be modeled on market mechanisms. Other areas continue to function differently. These differences must be taken into account if research policies want to exploit the potential of both realms, the realm of "intentionless" science with long lasting processes and the realm of science with high susceptibility for economic innovation.

[57] See Scotchmer's (2004, 152) idea of research exemptions counterintuitively favouring the pioneer.

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Royalty Stacking: A Problem, but Why?

Comment by
Christian Koboldt

Christine Godt's paper in this volume describes a problem that, in her words, "threatens the very idea of technology transfer from science to industry". Because scientific research increasingly relies on a multitude of inputs that are protected by patents, and because the holders of these patents individually wish to negotiate royalties which link their remuneration to the commercial value of the output of such research, there is a risk that, by the time research results can be commercially exploited, the accumulated royalties have reduced the potential margin to such an extent that the investment that would be needed for successful cross-over has become unattractive.[1] Put differently, royalty stacking is a problem because it leads to research outputs being so encumbered with royalty promises that they become commercially unattractive and will not be exploited. The paper deals with this problem mainly from a legal perspective, discussing it in the context of the general feature of patent dependency, which supports incremental innovation, but which also creates the risk of accumulated royalties that may eventually stymie commercial success.

In this comment, I will try to draw out more clearly the economic issues that are of interest with regard to the problem of royalty stacking. In particular, I will address the question whether accumulated royalties may indeed exceed the level that would be optimal for all the parties involved in cumulative research (which, in turn, may or may not be socially optimal – a question which I do not discuss), and if this is the case, why patent holders do not solve this problem by using different terms in licensing their intellectual property. Given the scope of this comment, I will raise mainly questions rather than provide answers, but I hope that this (unencumbered by royalties) will lead to further research. I will begin by assuming that patent holders wish to negotiate royalties in order to examine the impact on the overall royalty level, and then discuss whether this assumption is justified.

From an economic perspective, royalty stacking seems to be a clear problem of externalities. By trying to increase their individual share of the prospective cake, patent holders put at risk the commercial success of the research project

[1] It is worth pointing out that the problem is not limited to technology transfer, but could arise with regard to any investment made in advancing a particular research project. Accumulated royalties affect the likely return that such an investment can be expected to earn, and thus royalty stacking may lead to research projects being abandoned at any stage whenever the burden of accumulated royalties has reduced the expected return to a prohibitive level.

– without which there is no cake that could be shared. The basic mechanism is easy to demonstrate.

Assume there are n patent holders whose intellectual property is a necessary input into a research project. The project, if successful, generates a value of 1. For the sake of simplicity, assume further that all of these licensors are identical in terms of the contribution that their patents make to the success of the research project, and with regard to their requirement for compensation, and that licensors make a take-it-or-leave it offer, i.e. set the royalty level.[2] Consider that the probability p of the research project being completed and successfully exploited decreases with the level of accumulated royalties $r = \sum_{i=1}^{n} r_i$, where r_i denotes the royalty negotiated by licensor i. Thus, the expected value of the research project is $p(r)$ with $p' < 0$. For simplicity, assume that the probability decreases linearly with the accumulated royalty level, i.e. $p(r) = 1 - r$.

Individually, each licensor wishes to maximise its share of the commercial value, i.e. set r_i so as to maximise $r_i(p - \sum r_i)$. Using the assumption of symmetry of licensors, solving the first order conditions in a model of simultaneous royalty setting[3] gives an individually optimal royalty level of $r_i^* = 1/(n+1)$ for all $i = 1 \ldots n$. Cumulative royalties are therefore $r^* = n/(n+1)$. Whenever there are multiple licensors, this is in excess of the level of cumulative royalties $\bar{r}$ that would be optimal from the perspective of all licensors together. The collectively optimal royalty level is obtained by maximising $r(1-r)$, which gives $\bar{r} = 1/2$. Unsurprisingly, the problem is worse the larger the number of licensors – and it also is worse (and progressively so) if royalties are being set sequentially.[4]

Thus, it is indeed the case that individual attempts to maximise royalty revenues lead to a collectively sub-optimal outcome. Each licensor enjoys the full benefits from increasing its royalty level, whereas the negative impact on the probability of the research project's success is socialised. This would suggest that collective negotiations of royalties (or the use of one-stop clearinghouse arrangements as suggested by Godt) is one way to reduce the problem, although not one that is guaranteed to succeed. As we know from the economic literature, the existence of gains from co-operation is by no means sufficient for co-operation to succeed.

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[2] This assumption obviously has a bearing on the level of royalties, but not on the general result that individually set royalties are cumulatively higher than collectively negotiated ones.

[3] Royalties are being set simultaneously in the sense that no individual licensor possesses information about the royalty levels set by the other licensors.

[4] Where the *k-th* licensor sets its royalty level knowing the royalties set by all licensors $j < k$, and anticipating the royalties set by licensors $l > k$, it is easy to show that the optimal royalty for the *k-th* licensor is $r_k^* = 1/2^k$, and therefore the cumulative royalty level is $1 - 1/2^n$. It pays to be first in the licensing queue.

Having a clear view of the problem associated with individually negotiated royalties, however, puts the focus on the second part of the question. Why, if the encumbrance of profit opportunities through (accumulated) royalties reduces the likelihood of commercial success, are licensors interested in royalty arrangements in the first place? Would it not be better to negotiate other forms of remuneration, such as an outright sale of, or a fixed licence fee for the use of intellectual property? Indeed, it is easy to demonstrate that both the licensee and the licensor could benefit from negotiating a fixed licence fee whenever royalty promises reduce the probability of successful commercial exploitation of research.

Assume again that a research project generates a value of 1 if its results can be successfully exploited commercially, and that the probability of success depends on the level of (accumulated) royalties. The licensee would prefer to pay a royalty rather than a fixed licence fee l for an input protected by a patent if $(1-r)p(r) > p(0) - l$, i.e. if the licence fee is $l > [p(0) - p(r)] + r(p(r))$. As the second term on the right hand side of this equation gives the expected payment to the licensor under a royalty arrangement, this implies that as long as $p(0) > p(r)$, for any royalty level there must exist a licence fee which is preferred by both the licensee and the licensor. In very simple terms, if a royalty arrangement reduces the chance of success, the gains from avoiding the encumbrance with royalty promises can be split between the licensee and the licensor through a fixed licence fee instead of a royalty. This logic applies to multiple licensors, and suggests that negotiating a licence fee is better for each individual licensor and the licensee regardless of what the other licensees have done, or will do.

Again, the existence of such gains from co-operation is not a sufficient condition for co-operation to occur, but the fact that patent holders and research institution can do better by agreeing to a fixed licence fee rather than a royalty arrangement in cases where encumbrance with royalty promises reduces the potential commercial value of the research raises the question why royalty arrangements are observed in such a context. It certainly suggests that further analysis would be required with regard to the claim by Godt that, in principle, royalties are in the interest of both parties.

In this regard, it is worth pointing out that the fact that research increasingly relies on intellectual property rather than physical capital or labour is not, in itself, a reason for the use of royalties. Although it is true that it may not at all be in the patent holder's interest to 'sell' the patent rights, there should be nothing stopping her from licensing her intellectual property for a fixed fee that is payable irrespective of whether or not the research ultimately has commercial success. Thus, we must look elsewhere for an explanation as to why royalty agreements are entered into even in cases where they reduce the expected value of the research project (and in particular where the problem may be exacerbated through royalty stacking).

From an economic perspective, a number of potential explanations spring to mind:

– There could be a problem of asymmetric information. If the licensee has better information about the likely commercial value of the project, licensors may fear that they do not receive their fair share when negotiating a fixed licence fee. The licensee may understate the likelihood of success or the commercial value of the project, and rather than agreeing to a fixed licence fee based on the value *predicted* on the basis of information provided by the licensee, the patent holder may wish to negotiate a royalty which links payments to the value that will *actually* be realised.

– Licensors may also simply be myopic or unaware of the detrimental impact that a royalty arrangement can have on the likelihood of commercial success, although in this case the licensee should find it easy to alert licensors to the potential downside.

– Conversely, there may be principal-agent issues involved on the part of the licensee in the sense that those agreeing to royalty payments (e.g. indvidual researchers) are not the residual claimants of the commercial value of the research. As Godt points out, they are not the ones who pay future royalties, whereas fixed payments now would have to come out of their research budgets. Thus, researchers motivated by scientific rather than commercial success may well prefer to agree to royalties, as this may allow them to achieve their objective at a lower cost, even though the overall impact is detrimental.

– Licensee and licensor may have different preferences with regard to the risk associated with the research project, or may have different discount rates and therefore attach different relative weights to future royalty payments/receipts and current fixed payments/receipts.

– There may be capital market imperfections that limit the ability of the licensee to fund current payments of a fixed licence fee against future revenues from successful commercialisation.

– There may be other constraints in play which affect the choice of licensing arrangements. For example, it may be the case that royalty agreements perform better in some cases, and that licensors may be worried about allegations of discriminatory treatment that might lead to complaints or private litigation under competition law.[5]

[5] This would obviously be an issue only in cases where the licensor may be deemed to have market power. It is worth pointing out that the reason why licensors may prefer royalties to fixed payments in other settings may have to do with the effect on competition of cross-licensing arrangements where licensor and licensee compete in a downstream market. In such cases, royalty payments have the effect of profit-sharing arrangements, which may affect the intensity of competition downstream.

It would certainly be worthwhile to investigate in greater detail which of these reasons is mainly responsible for the choice of royalties over fixed licence payments, and to what extent their relative importance varies between 'industry' and 'science'.

This would not only provide a better insight into the differences between these two areas, and suggest why the problem of royalty stacking is one that particularly affects licensing of intellectual property in the context of scientific research. More importantly, a better understanding of the underlying causes for using royalty agreements in cases where they reduce the expected value of research is indispensable for identifying potential solutions to the problem of royalty stacking. A 'simply say no' approach to royalty arrangements, the promotion of collective negotiations, or a cross-over to industry before the royalty burden becomes too large, as suggested by Godt, may not be the only responses, and may not necessarily be the best ones.

An Economic Theory of Academic Competition: Dynamic Incentives and Endogenous Cumulative Advantages

by

NICOLAS CARAYOL

1 Introduction

The implicit and explicit rules of academic research stress a specific reward system in which *priority* is essential (Merton 1957). The recognition of a scholar as the intellectual proprietor of the knowledge she produced increases her *credit* within the peer community. In turn scientific reputation translates into increased wages, more prestigious positions and other non-monetary rewards (Dasgupta and David 1994). The academic reward system appears to be fundamentally reputation-based, which has two main implications on the provision of incentives across time (for a given scholar) and across scholars. First, reputation-based incentives tend to distort the distribution of incentives during researchers' careers since the returns of research activity are usually delayed and spread over the remaining professional cycle. Scientists are thus essentially facing dynamic incentives (career concern). Because the expected returns of efforts are decreasing with the remaining activity period, the sharp decline of incentives in the late career is likely to overbalance the experience effect. Thus, an inverse-U shape of scientific production distribution over the career cycle is predicted. Several empirical studies using panel data of publication profiles have corroborated this statement (Weiss and Lillard 1982, Levin and Stephan 1991).[1,2] The second consequence of such a reputation-based reward system is that resources and means are not uniformly distributed across agents but tend to be concentrated in the hands of those who have more *credit*. Cumulative processes bias the academic competition, providing some competitive advantage to the agents who have experienced the best

[1] Stephan and Levin (1997) show that the peak is often attained between ages 35 and 50 in biochemistry and physiology. Diamond (1986) finds that the publication profiles of Berkeley University mathematicians decrease over the whole career. This specificity may be explained by the low experience effect in this discipline.

[2] Such results suggested improvements of human capital theory. Two technical solutions may be used. The first one consists in introducing human capital depreciation. Since it also generates a counter-factual decrease in wages, another solution has been suggested by Levin and Stephan (1991), who introduced a "puzzle solving" argument in academics objective function, namely, the agents do not value only wages but also scientific production itself (or get other satisfactions from it but the wage).

early career accomplishments.[3] Analyses of the distribution of publication records among scholars[4] and time series analysis (Allison and Stewart 1974)) support the cumulative advantage hypothesis.

This paper presents an original model of academic competition[5] which encompasses the two stylized facts exposed above. It relies upon the following simple mechanism. We suggest that academic positions differ both in their associated productivity and in the utility they provide to scholars (both due to different wages and other non-monetary satisfactions). At the different stages of their career path, agents compete to get the best positions. In equilibrium, universities' hiring and promotion decisions are taken according to the scientific production (or credit) ranking at the previous stage. Therefore, the scientists most productive in their early careers are favored in the next stages.

Empirical evidence supports the idea that a mechanism of this kind is at play in academia. The universities, as well as the research positions they offer, are quite heterogeneous in terms of their associated productivity. Stephan (1996) argues that prestigious institutions are endowed with heavy instrumentation equipment that less established ones cannot afford. Cole (1970) shows that the reputation of the hosting institution generally signals

[3] R. K. Merton (1968, 1988) gives the label of *Matthew effect* to the various cumulative advantages affecting the academic sphere. He refers to the quotation of the Gospel according to Saint Matthew: "for unto every one that hath shall be given, and he shall have abundance: but from him that hath not shall be taken away even that which he hath". The first evidence of cumulative advantages in academia is due to H. Zuckerman's Ph.D. thesis defended in 1965 (with Merton as a supervisor), which was dedicated to studying Nobel laureates career paths. In their early work, both Merton and Zuckerman tend to limit the application of the notion to the symbolic mechanism according to which already reputed scholars gain more credit than less reputed ones from a co-authored paper or from a simultaneous discovery. The extension to various cumulative advantages comes later (e.g., in Merton 1988).

[4] This distribution is known to be highly skewed: few researchers publish many articles and many researchers publishing only a few papers each. The shape of the distribution can be well approximated by an inverse power distribution (power law) given by the function $f(n) = an^{-k}$, with $f(n)$ as the number of authors having published n papers and a and k as parameters of the law. When $k = 2$, this expression is identical to the one initially proposed by Lotka (1926). Many empirical studies have confirmed the relevance of this distribution for different scientific domains, see, e.g., Murphy (1973) for the humanities, Radhakrishnan and Kernizan (1979) for computer science, Chung and Cox (1990) for finance, Cox and Chung (1991) for economics, Newman (2000) for physics and medicine, Barabasi et al. (2001) for mathematics and neuroscience, etc.

[5] Several modelling attempts have considered other dimensions of academia. Carmichael (1988) intends to explain the tenure system. Merton and Merton (1989) describe the optimal timing scheme for solving a set of scientific problems. Lazear (1997) models funding agencies. Brock and Durlauf (1999) propose a model of discrete choice of scientific theories when agents have an incentive to conform to the opinion of the community. Lach and Schankerman (2003) model the licensing of scientific discoveries. Carayol and Dalle (2004) propose a model of scientific knowledge accumulation over an increasing set of scientific areas.

researchers' abilities, enabling them both to more efficiently raise funds and to more quickly and widely diffuse their results in the scientific community. Hansen et al. (1978) showed that the quality of the researchers' universities is the critical variable for explaining future production. Cole and Cole (1973) found a university department quality effect on the productivity of physicists. Some empirical evidence also suggests that the allocation of the best positions, either through internal promotions or through hiring decisions (Garner 1979), is mainly based on past scientific production. Zivney and Bertin (1992) showed that the researchers "tenured" in the twenty five most reputed finance departments of US universities previously published perceptibly more than the average tenured researcher. Having studied the mobility of more than 3,800 economists, Ault, Rutman and Stevenson (1978, 1982) showed that the main determinant of the quality of the first position hosting institution is the quality of the training university (both at undergraduate and graduate levels) and the quality of the university where the Ph.D. has been defended. Moreover, they showed that further "upward mobility" (mobility associated with an increase in the quality of the institution) is mainly explained by past publications (even if the effect is limited). The most productive agents will benefit from better research positions and are in turn likely to publish more. In this way, the academic competition is dynamically biased in the sense of Merton's cumulative advantage, because the initial successes tend to further improve productivity and, in turn, favor late successes. Thus, in the very nature of the academic employment relationship lies one of the sources of the cumulative advantage process.

More precisely, we model two employers (we refer to universities, but it could be departments or research labs) which offer at each period research positions at all career stages – Ph.D., junior and senior levels – to overlapping generations of two researchers. While taking promotion and hiring decisions, universities cannot (or just do not) observe both agents' efforts and cardinal values of past productions: such decisions are taken on the basis of agents' past production ranking.[6] At each stage of the career, positions differ both in terms of their remuneration and their associated productivity. There is a productive premium due to both, the accumulated reputation of the host institution and positive spillovers from first-ranked colleagues of other generations within the university. At the junior and senior stages, the previously most productive agents will select the positions they prefer, while others will accept the remaining academic positions or even choose the outside option

[6] The structure of the model has much in common with the biased contests literature initially applied to sequential auctioning (Laffont and Tirole 1988), imperfect measurement of agents' production within firms (Milgrom and Roberts 1988, Prendergast and Topel 1996), and, lastly, career paths within firms that are either autoregressive ("late-beginner effect", Chiappori et al. 1999) or dynamically correlated ("fast track", Meyer 1991, 1992).

(i.e., leave academia). Getting the most productive position improves the chances to win the next competition round, that is, to get again more productive positions and higher wages. This is the way cumulative advantage in researchers' competition is captured in the model. In this paper, we also explicitly introduce competition between universities, which also compete at each period to hire the best researchers at junior and senior stages. Therefore, hiring decisions according to agents' ranking, wages, and cumulative advantages are endogenously determined in a dynamic setting.

Our main results are the following. We derive researchers' equilibrium efforts and show that, as highlighted by several empirical contributions (Zuckerman and Merton 1972, Allison and Stewart 1974), the anticipated cumulative advantage improves early career efforts (because it generates dynamic incentives) while it deters late career efforts. However, the effect of the cumulative advantage on scientific production over the whole career remains ambiguous.[7] As regards universities competition, we derive Markov perfect equilibria of the game under non-restrictive assumptions. The equilibrium is stationary in the long run: a fixed cumulative advantage endogenously arises that the leading university confers to its researchers. We precisely compute the equilibrium and the optimal wages and show that the equilibrium wages offered to second-ranked agents are optimal and that the wages offered by the leading university to the first-ranked agents may be larger or lower than their optimal counterparts. This is because leading universities do not internalize the positive incentive effect of the wages they offer on scholars hired by other institutions at previous stages.

The paper is organized as follows. The model is presented in the next section. The third section is dedicated to agents' behaviors under such a biased competition. The fourth section introduces universities competition. The fifth section compares optimal to equilibrium wages. The last section concludes.

2 The model

2.1 Main features

Let us define the population of academic researchers as overlapping generations of agents whose career lasts three periods. Let $p \in \{1, 2, 3\}$ denote the periods at which the agents can be, respectively, Ph.D., junior researchers, and senior researchers. At each period t of discrete time, a fixed cohort C^t arrives,

[7] In a previous contribution (Carayol 2003), which introduces a more specific model (only one cohort), I specifically studied and found an optimal level of cumulative advantage, i.e., the second stage competitive bias given to the first stage winner that optimally balances early career incentive effect and late career disincentive effect.

composed of two researchers. There are two research institutions, say, universities $\{i, j\}$, which offer a position of each stage.

The outcome of scholars' work is assumed to result in some aggregated output which can be called research production (that is, papers properly weighted to account for quality differences or, equivalently, credit in the peer scientific community). Research production is supposed to be additively separable in efforts over the two first periods of activity. At period t, the research output of the agent employed in university i at stage p of her career is given by

$$y_i^{p,t} \equiv f^p(e_i^{p,t}) + b_i^{p,t} + \varepsilon_i^p. \tag{1}$$

It is a function of effort spent at time t by the agent being at stage p of her career $e_i^{p,t}$, with f^p a positive and increasing function whose derivative gives the productivity of effort at the different stages of the career (Ph.D., junior and senior: $p \in \{1, 2, 3\}$). f^p is assumed to be strictly increasing, concave, and null when the agents exerts zero efforts: $f^{p'} > 0$, $f^{p''} \leq 0$ and $f^p\ (0) = 0$. The term ε_i^p is the specific random shock that affects agent z's production at stage p, where $E\ [\varepsilon^p] = 0$. Let us assume these shocks are *iid* across agents and periods. We define $\Delta\varepsilon^p$ as the difference between the individual random shocks at stage p: $\Delta\varepsilon^p \equiv \varepsilon_i^p - \varepsilon_j^p$. The distribution function of this random variable is denoted G^p and its density function is g^p. The latter is assumed to be unimodal, continuously differentiable, strictly positive over $[-\infty, \infty]$, and symmetric around its unique maximum attained at 0 (which implies that $g^{p'}(0) = 0$). The term $b_i^{p,t}$ gives the surplus of credit which is due to the agents' context of work: it is an attribute of the university in which the agent is working. This position specific component is formed as follows:

$$b_i^{p,t} \equiv \alpha_i^t + \beta_i^{p,t}, \tag{2}$$

with α_i^t as the effect of the accumulated reputation of the institution on agents' production. For simplicity, we assume that it is independent of the stage p. The vector $\alpha^t = (\alpha_i^t, \alpha_j^t)$ synthesizes the reputational advantages of the two universities at t. The term $\beta_i^{p,t}$ is the potential production premium due to the previous period ranking of co-workers in the academic institution. It is a positive externality which can be seen as a reputation effect. Alternatively it might also be thought as a spillover due to costless interactions (like good advisers or next-door office colleagues). It can be computed as

$$\beta_i^{p,t} \equiv \beta \times \mathbf{1}\,\{i|p'|t \,\text{ranks first with}\, p' \neq p\} \tag{3}$$

for $p,p' = 2, 3$, with β a positive parameter and $1\ \{\cdot\}$ the indicator function which is equal to 1 if the condition in brackets holds. The expression "$i|p'|t$

ranks first" simply means that university i employs at t a researcher who is at stage p' of her carrier and who was ranked first at the end of the preceding period (period $t-1$ and stage $p'-1$). This assumption formalizes the idea that junior and senior scientists gain some production premium β to be working with high-ranking scientists of the other generation. Ph.D. students (first stage) are assumed to gain equally from the ranks of the two older generations: $\beta_i^{1,t} = \frac{1}{2}\left(\beta_i^{2,t} + \beta_i^{3,t}\right)$. The term α_i^t is simply constituted of the accumulation of the past premia as follows:

(4)
$$\alpha_i^t \equiv \sum_{p=2,3} \sum_{\tau=1\ldots T} \gamma^\tau \beta_i^{p,t-\tau}$$

with some discount factor γ over a given relevant period of time T. If γ tends to 1, then past and present advantages have nearly equal weights in present production. When γ tends to 0, then α_i^t also tends to zero and the production premium $b_i^{p,t}$ tends to be restricted to the present spillover $\beta_i^{p,t}$.

Agents' instantaneous net utility is given by the function $W(s_i^{p,t}, b_i^{p,t}, e_i^{p,t})$, which is assumed to be additively separable between disutility of efforts and utility as follows:

(5)
$$W_i^{p,t} \equiv U(s_i^{p,t}) + \varphi \times \mathbf{1}\left\{b_i^{p,t} > b_j^{p,t}\right\} - V(e_i^{p,t})$$

where $\varphi > 0$ represents the surplus of satisfaction derived from being in the most prestigious institution. Agents value not only wages but also the prestige of their host institution.[8] U is an instantaneous utility function that assumes agents to be intra-periods risk averse: $U : (0, \infty) \to (-\infty, \infty)$ such that $U' > 0$; $U'' \leq 0$, $\lim_{s \to 0} U(s) = -\infty$. The instantaneous disutility of efforts function $V : (0, \infty) \to (0, \infty)$ is assumed to satisfy $V(0) = V'(0) = 0$, $V' > 0$, $V'' \geq 0$, and $\mathrm{lin}_{e \to \infty} V'(e) = \infty$.

The whole career net utility function is assumed to be additively separable between the three periods of the career. We also assume that agents do not have access to the financial market; so they can neither save nor borrow and, thus, consume their whole revenue received at each period. Thus, the total net utility of agent i of cohort C^t actualized at the initial period t is given by

(6)
$$\overline{W}_i^t = \sum_{p=1,2,3} \delta_a^{p-1} W_i^{p,t+p-1},$$

with δ_a the agents' discount factor.

[8] Levin and Stephan (1991) assume that scholars' objective function has a "puzzle solving" argument, that is, scholars also directly like publishing papers. Our assumption is slightly different in that we rather assume that scholars like being in a distinguished and prestigious institution.

2.2 Information and timing

At each period, universities offer a position at each of the three stages. At the first stage, wages are uniform and exogenously fixed and agents are assigned to Ph.D. research positions. At the junior and senior stages, the universities compete in wage offers. Universities cannot adapt wages to cardinal information on past production: they only compare the scientific production of the two candidates at the previous stage.[9] At both levels, the positions they offer have distinct associated productivities. The universities play simultaneously and have an infinite time horizon. This game is called the Universities Game.

Agents have a life cycle point of view and face intra-cohort competition. They compete during the first round when they are Ph.D. At the end of the first period, they can access junior positions offered by universities. Positions are characterized by an associated utility and a production premium. Given universities competition, the most productive agent chooses the university that offers the preferred junior position. The other agent accepts the remaining junior position offered by the other university or defects and takes the outside option. If not, the two agents compete in the following stage. Again, the most productive chooses the senior position he prefers. There is a cumulative advantage because the first winner can choose the junior position that provides an advantage to get the best senior position. At the end of the third period, they retire.

Agents competition is analyzed in the following section while section 4 deals specifically with universities competition. Time consistency between the two interrelated competitions is due to the fact that efforts are not observable and that cardinal information on production is not available. Previous-stage contest ranking is the only information available. Therefore, scholars care only about future wages and productive advantages that they consistently believe to be stationary. As we shall show, universities care only about previous period ranking, the other university's strategy, and the present efforts of its current employees at previous stages when setting their wage offers.

3 Researcher behavior

We now concentrate on the computation of scientists' equilibrium behavior, leaving aside the issue of universities competition that will be treated in the next section. Important for us now, we shall show there that, at each stage, the agent who wins the competition occupies (as expected) at the next period the

[9] Thereby, we also assume that universities do not consider the ranking of the Ph.D. stage to hire a senior researcher. This information is either neglected, irrelevant, or just lost.

position that provides the highest satisfaction, which also provides the highest productivity. Let the difference in productive advantages b^p at each stage $p = 1, 2, 3$ be denoted Δb^p.[10] We shall also define Π_i^p as the expected utility of the agent employed at university i[11] from stage p to the end of the career given its level of information so far. Formally, we have

$$\Pi_i^p = \mathrm{E} \sum_{q=p}^{3} \delta_a^{q-1} W_i^{q,t+q-1}, \tag{7}$$

with E as the expectation operator.

At each stage, agents maximize expected utility over their remaining career cycle. We use standard backward induction reasoning. Since there is no motive for any competition in the last stage of the career, we have $e_k^3 = 0, k = i, j$ and third stage production thus is equal to the shock and the potential production premium.[12] We thus concentrate on the two first stages of the career: we first study the second stage Nash equilibrium and then the first stage subgame perfect Nash equilibrium.

3.1 Second stage Nash equilibrium

At the second stage, agent i chooses her effort level e_i^2 given j's (e_j^2) in order to maximize her expected net utility from then. Agents believe that they will get more utility if they win than if they loose such that $\Delta U^3 > 0$, a belief which is consistent with universities' behaviors as we shall show in the next section. Thus, i's program at the beginning of stage 2 consists in

$$\begin{aligned} &\tilde{e}_i^2 \equiv \arg\max \\ &\left\{P\left(y_i^2 > y_j^2\right) \times \delta_a\left(\overline{U}^3 + \varphi\right) + \left[1 - P\left(y_i^2 > y_j^2\right)\right] \times \delta_a \underline{U}^3 - V\left(e_i^2\right)\right\}. \end{aligned} \tag{8}$$

$\overline{U}^3$ is the utility derived from the wage if the second contest is won and $\underline{U}^3$ if it is lost. The Nash equilibrium effort level $(\tilde{e}_i^2)$ maximizes the expected net utility actualized at the second career stage. It is equal to the probability of winning the second contest times the utility he will receive if he wins the contest, plus the probability to loose that contest times the utility received if he looses it, net of the disutility of efforts.[13] An identical program could be

[10] Since the analysis in this section will be limited to only one cohort's behavior, we will remove time superscript and uniquely refer to career periods (given by p).

[11] In this section "agent i" and "the agent employed by university i" have the same meaning.

[12] Clearly, senior researchers do exert efforts in real life. Nevertheless, this behavior seems not to respond to the kind of motives that are considered in this model.

[13] We omit second stage utility since it is independent of second stage efforts.

written for *j*. The simultaneous solution of the two programs, detailed in the appendix, leads to a unique Nash equilibrium which is symmetric and given by

$$\tilde{e}^2 = \Theta_2^{-1}(\delta_a(\Delta U^3 + \varphi)g^2(\Delta b^2)), \tag{9}$$

with function Θ_2 defined as $\Theta_2(x) = V'(x)/f^{2\prime}(x)$. This function is null at 0 and strictly increasing from then (and, thus, so is its inverse function Θ_2^{-1}). Therefore, second period efforts are increasing with agents' actualization factor, and the differences in third stage differences in utility whatever they come from, difference in wages (ΔU^3) or in satisfaction derived from being in a prestigious institution (φ). The cumulative advantage (Δb^2) effects are negative (for details, see the appendix).

3.2 First stage subgame perfect Nash equilibrium

We now turn to agents' first period behaviors. Agent i's objective is to determine her first period effort level in order to maximize expected net utility, that is,

$$\tilde{e}_i^1 = \arg\max\left\{\Pi_i^1\right\}. \tag{10}$$

This maximization program is different from the second period one since the first period success influences the second stage competition. The unique and symmetric subgame perfect Nash equilibrium (again, detailed computations are in the appendix) is given by

$$\tilde{e}^1 = \Theta_1^{-1}(g^1(\Delta b^1)\delta_a[\Delta U^2 + \varphi + \delta_a(2G^2(\Delta b^2) - 1)(\Delta U^3 + \varphi)]) \tag{11}$$

with Θ_1 defined as $\Theta_1(x) = V'(x)/f^{1\prime}(x)$ and Θ_1^{-1} increasing. Moreover, since $\Delta b^2 > 0$ and g^2 is symmetric around its unique extremum at 0, $G^2(\Delta b^2) \geq 1/2$ and, thus, $2G^2(\Delta b^2) - 1 \geq 0$. Thus, first stage equilibrium efforts $\tilde{e}^1$ are increasing with the discount factor and with the differences in utilities at the junior and the senior stages (ΔU^2 and ΔU^3) whatever they come from, the differences in wages or the differences in satisfaction to be in the most prestigious institution. The cumulative advantage effects (Δb^2) are positive while the effects of the initial advantage (Δb^1) are negative (for details, see the appendix).

These results are summed up in proposition 1 below. It confirms the results of several empirical studies according to which the cumulative advantage stimulates early career efforts while it diminishes late career efforts.

Proposition 1. *Agents' equilibrium efforts are unique and symmetric at each stage of the career cycle. Equilibrium efforts decrease with the differences in utility provided by positions at the remaining stages of the career. The cumulative advantage which favors* in the second stage *the winner of the first competition increases first period equilibrium efforts while it decreases the second period efforts. The first stage advantage decreases first period efforts.*

4 Universities competition

In this section, we shall focus on the Universities Game. At the end of each period, the scholars' ranking is public knowledge. At the beginning of the next period, the universities offer one position at each stage $p = 1, 2, 3$. To fill their available junior and senior positions (agents are assigned exogenously to Ph.D. positions at the beginning of the career), universities compete to hire the best researchers. Neither do the universities care about the institutions where the researchers were previously employed, nor is relevant cardinal information on past production available (or relevant with regard to the institutional constraints). The universities compete simultaneously in wages. The competition is asymmetric with respect to their respective reputations. Universities consider the game as lasting infinitely.

Let us denote by Qj the objective function of university i at some time period t_0. Without loss of generality, we will consider $t_0 = 0$ in order to avoid cumbersome notation. The objective of university i is its discounted net surplus over an infinite period of time:

$$\Omega_i = \sum_{\tau=0}^{\infty} \sum_{p=1}^{3} \delta^{\tau} (\psi y_i^{p,\tau} - s_i^{p,\tau}) \tag{12}$$

The parameter $\psi > 0$ gives the per-unit value of scientific knowledge captured or just considered[14] by the university. It is assumed to be homogeneous or normalized. Parameter δ is the discount factor.

Let now $\Delta y_i^{p,t}$ denote the net surplus of production university i gets from employing at stage p and period t the scientist who won her preceding academic contest (as compared to hiring the one who did not). It directly derives from the preceding section that this surplus is only composed of the direct productive complement a first-ranked agent provides to other researchers employed by i (at other stages). It is independent of i and can directly be

[14] If the university is controlled by any external institution or body having its own goals (e.g., a state), the rate *ip* in the objective function might be higher than the effective rate of returns of scientific knowledge on the university budget. It would become closer to its social value. For a comparison between *ip* and the real social value of scientific knowledge, see section 5.

computed from agents' production function (1) and from the equation which sets the direct externality:

$$\Delta y_i^{p,t} = \Delta y^p = \tfrac{3}{2}\beta. \tag{13}$$

We denote by $\underline{y}^{p,t}$ the expected production only due an agent's efforts (without considering the premium). It is only affected by her equilibrium efforts (which are unique at each stage as shown in section 3).

The universities' payoffs at period t can be written as a function of the wages offered by the two universities at the two last stages summed up in vector $s^t = \left(s_k^{p,t}\right)_{k=i,j;p=2,3}$:

$$\begin{aligned}\pi_i(s^t) = {} & \textstyle\sum_{p=1}^{3} \left(\psi \underline{y}^{p,t} - s_i^{p,t}\right) \times \mathbf{1}\left\{\Pi_i^p \geq \widehat{W}^p\right\} \\ & + \psi \textstyle\sum_{p=2}^{3} \tfrac{3}{2}\beta \times \mathbf{1}\left\{\Pi_i^p > \Pi_j^p \quad \text{and} \quad \Pi_i^p \geq \widehat{W}^p\right\}\end{aligned} \tag{14}$$

University i must offer agents a wage that provides a higher expected utility than their reserve utility level outside the university system $\left(\Pi_i^p < \widehat{W}\right)$. Otherwise, agents always defect and the university gets a null payoff. The second component of the right hand side of the equation indicates that, at each stage $p = 2$, 3, if university i provides the highest expected utility given the level of information of agents so far $\left(\Pi_i^p > \Pi_j^p\right)$, it hires the researcher ranked first and captures the surplus of production as given in (13). Otherwise, the university hires the other researcher and cannot capture the surplus in revenues associated with the production premia given in (13).

At each period, an action of university $k = i, , j$ in the Universities Game is a vector $s_k^t = \left(s_k^{2,t}, s_k^{3,t}\right)$ of the two wages offered at t. The history of the game so far, denoted $h^t \in H^t$, where H^t is the set of all possible histories in period t, is the collection of all previous actions such that $h^t = h^{t-1} \cup \left(s^{t-1}\right)$. H is the set of all possible histories over time, that is, $H = \cup_{t=0}^{\infty} H^t$. A pure behavioral strategy at t is a function $\rho_i^t \colon H^t \times \mathbb{R}^{+2} \to \mathbb{R}^{+2}$, which gives a couple of wages at t (an action s_i^t) for each possible history at t and each possible wage simultaneously offered by the other university (s_j^t).

We are interested in Markov Perfect Equilibria (MPE, see Maskin and Tirole 2001) of the Universities Game. So we restrict considerations to Markov strategies, that is, strategies that are not functions of the whole history of the game but only of the state of the system. The vector $\alpha^t \in \mathbb{R}^{+2}$, which synthesizes the reputational premia of the two universities at t, is the payoff-relevant state of the system because no other past variable does consistently influence present actions. We consider only strategies of the form $\sigma_i^t : \mathbb{R}^{+2} \times \mathbb{R}^{+2} \to \mathbb{R}^{+2}$. Such strategies give, for each action of the opponent and for each given reputation levels, a couple of wages s_i^t. They are of the form

$\sigma_i^t = \sigma_i^t(s_j^t, \alpha^t)$. An MPE is a couple of strategies $\hat{\sigma}^t = (\hat{\sigma}_i^t, \hat{\sigma}_j^t)$ such that $\widehat{\sigma}_i^t$ and $\widehat{\sigma}_j^t$ are best responses to each other and are Markov strategies.

The MPE notion relies much on the idea that small causes have minor effects. In the dynamic programming spirit, agents are assumed to simultaneously maximize their continuation equations which in the context of the Universities Game can be set as follows:

$$\text{(15)} \quad \theta_i(s^t, \alpha^t) = \sum_{p=1}^{3} \left(\psi \underline{y}^{p,t} - s_i^{p,t}\right) \times \mathbf{1}\left\{\Pi_i^p \geq \widehat{W}^p\right\} + \sum_{p=2}^{3} B \times \mathbf{1}\left\{\Pi_i^p > \Pi_j^p \quad \text{and} \quad \Pi_i^p \geq \widehat{W}^p\right\}.$$

This expression is similar to the payoff function (14), except that now hiring the first-ranked scientist at the junior and senior levels brings some delayed productive surplus due to the increase of reputation to the host university it causes. The increment of reputation depreciates over years at rate γ and is discounted by factor δ over an infinite horizon. The payoff surplus is thus $\frac{3}{2}\beta\psi \sum_{t=1}^{\infty} \delta^t \gamma^t$. When one adds to this term the direct spillovers $\frac{3}{2}\beta\psi$ already present in (14), it becomes equal to $B \equiv \frac{3}{2}\beta\psi \sum_{t=0}^{\infty} \delta^t \lambda^t = \frac{3}{2}\beta\psi(1-\delta\gamma)^{-1}$.

The simultaneous maximization, at each period, of the two universities' continuation equations leads to the MPE. The intuitions for the equilibria are the following. By convention, and without loss of generality, let i be the leading university at the period considered, t, while j is the other university, that is: $\alpha_i^t > \alpha_j^t$. University i can attract the best scientist with a lower salary at stages 2 and 3 because agents value being in the most reputed university and, if they are about to enter the junior stage of the career, they also know that working in this university will increase their probability to win the next contest. Since both institutions value equally recruiting the best researcher, such asymmetry provides university i a decisive advantage. Indeed, university i can offer a wage such that university j cannot set a wage that might attract a first-ranked researcher without having a lower return than when just hiring the second-ranked agent. The best rate at which university j can attract the second-ranked agent is by setting the minimal wage which saturates agents' participation constraints. Theorem 2 states this more rigorously.

Theorem 2. *Given assumptions 3, at any period $t > 0$ of the Universities Game, the MPE $\hat{\sigma}^t = (\hat{\sigma}_i^t, \hat{\sigma}_j^t)$ is such that, if $\alpha_i^t > \alpha_j^t$, the equilibrium wages $\hat{s}^t = (\hat{s}_i^t, \hat{s}_j^t)$ are $\hat{s}_i^t = (\bar{s}^{p,t})_{p=2,3}$ and $\hat{s}_j^t = (\underline{s}^{p,t})_{p=2,3}$ that i) $\Pi_j^p = \widehat{W}^p$ for all $p = 2, 3$, and ii) $\hat{s}_i^t = \arg\max_{s_i^{p,t}, p=2,3} \sum_{p=1}^{3} (\psi \underline{y}^{p,t} - s_i^{p,t})$ subject to the incentive constraints (9) and (11), subject to the participation constraints and subject to competition constraints that ensure i attracts the first-ranked agents. The competition constraints are given by the following condition. For any vector of wages s'^t that differs from $\hat{s}^t$ only in wages offered by j (possibly, both) $s_j^{p,t} = s^{p'} + \underline{s}^{p,t}$ such* *that* $\Pi_j^p \geq \Pi_i^p$*, then* $\theta_j(s'^t, \alpha^t) < \theta_j(\hat{s}^t, \alpha^t)$ *(any move allowing j to hire first-ranked agent(s) would be detrimental to j).*

Proof. See appendix.

Assumptions 3. *A3.1) For all vectors of wages* s''^t *identical to* s^t *with the exception that, at any given stage p (possibly, both),* $s_j^{p,t}$ *is equal to* 0 *instead of* $\underline{s}^{p,t}$*, we have* $\theta_i(s''^t, \alpha^t) \leq \theta_i(s^t, \alpha^t)$*. A3.2) For all vectors of wages* s'''^t *identical to* s^t *with the exception that at any given stage p (possibly both),* $s_i^{p,t}$ *is equal to* $\underline{s}^{p,t}$ *instead of* $\bar{s}^{p,t}$*, we have* $\theta_i(s'''^t, \alpha^t) \leq \theta_i(s^t, a^t)$.

Assumption 3 simply rules out trivial and uninteresting scenarios. It states that, at the equilibrium and at both stages, the university which has not the reputational advantage prefers to hire the second-ranked worker rather than hiring no-one (A3.1); and the university which has the reputational advantage prefers to hire the first-ranked worker at the equilibrium wage at both stages rather than hiring the second-ranked worker at the wage where this agent achieves her outside option utility (A3.2).

Theorem 2 shows how the rivalry with the other university places a *competition constraint* on the leading university. The *competition equilibrium* is said to arise when the leading university saturates the competition constraint. In this scenario, the leading university sets its third stage wage $\bar{s}^{3,t}$ such that

$$U(\underline{s}^{3,t} + B) = U(\bar{s}^{3,t}) + \varphi, \tag{16}$$

that is, the first-ranked agent, at the beginning of the senior stage, would have an equal satisfaction whether being hired by the less reputed university, which would offer (in addition to the wage that saturates the participation constraint $\underline{s}^{3,t}$) to the agent all the value of the productive (direct and delayed) premium this agent would bring in[15], or being hired by the leading university.

The leading university sets its second stage wage $\bar{s}^2$ such that

$$U(\underline{s}^{2,t} + B) = U(\bar{s}^{2,t}) + \varphi + \delta_a[2G(b) - 1](U(\bar{s}^{3,t}) + \varphi - U(\underline{s}^{3,t})). \tag{17}$$

At the beginning of the junior stage, the first-ranked agent would again have the same expected utility whether being hired by the less reputed university, which would offer the value of its productive premium (B), or being hired by the leading university and benefiting from both the direct satisfaction of working there (φ) and the discounted surplus of expected utility due to the increased chance to win the next contest.

Nevertheless, the incentive constraint is still effective because the universities' employees consistently anticipate that the wage offers are constant

[15] B is the maximal amount the non-leading university is ready to offer to the first-ranked agent on top of the wage that it would give to the second-ranked agent.

in time. The leading university knows that the second period wage it offers has an incentive effect on the agent who is currently employed by the university at Ph.D. stage. Similarly, the leading university knows that the senior wage it offers has an incentive effect on both the Ph.D. and the junior who are employed in this university. Thus, there is no reason to exclude *a priori* a scenario in which the competition constraint is not saturated due to pure incentive purposes. The leading university wages might differ depending on whether the competition constraints are saturated or not.

Let us define $(\bar{s}^{p,t*})_{p=2,3}$ as the full incentive maximizing wages of the leading university i, which are solutions of the optimization program of theorem 2 but now irrespective of the competition condition. If $\bar{s}^{p,t*} > \bar{s}^{p,t}$, $\forall p = 2, 3$, then the competition constraint is not effective and the *full incentive maximizing equilibrium* is said to arise (none of the incentive constraints is saturated). If $\bar{s}^{2,t*} < \bar{s}^{2,t}$ and $\bar{s}^{3,t*} > \bar{s}^{3,t}$, the *mixed equilibrium* arises. The incentive effect is then prevalent only for the senior wage[16] while the junior wage offered by the leading university saturates the competition constraints.

Corollary 4. *Theorem 2 leads to three possible Markov Perfect Equilibria which differ only in the wages offered by the leading university depending on whether the competition constraints are saturated: i) the competition equilibrium, where* $\bar{s}^{p,t} > \bar{s}^{p,t*} = 2, 3$; *ii) the mixed incentive equilibrium, where* $\bar{s}^{2,t*} < \bar{s}^{2,t}$ *and* $\bar{s}^{3,*} > \bar{s}^{3,t}$; *and iii) the full incentive maximizing equilibrium, where* $\bar{s}^{p,t*} > \bar{s}^{p,t}$, $p = 2, 3$.

Now we investigate the long run implications of theorem 2, which shows that the leading university always attracts the first-ranked scholars. There is, thus, a path-dependent process since the equilibrium of the university competition game preserves the competitive advantage of the leading university which tends to some fixed value as stated in corollary 5 below.

Corollary 5. *The MPE of the Universities Game preserves the competitive advantage of the leading university over time: the most reputed university hires the winning agents at junior and senior stages and thus conserves full advantage over time. In the long run* $(t \to \infty)$ *the endogenous competition advantages the leading university confers to its employees tend to* $b \equiv \beta(1+\gamma)/(1-\gamma)$ *for an infinite reputation relevant period T.*

Proof. According to theorem 2, the leading institution hires at both stages the first-ranked agents and thus preserves its advantage for ever. When t tends

[16] Senior wages have a higher incentive effect because they affect positively both the Ph.D. and the junior stages efforts, while the junior wages only affects agents holding a Ph.D. position.

to infinity, any productive premia the non-leading university might have had (by any kind of accident) tends to zero (due to discounting). Then, the productive advantage ($\Delta b^{p,t}$) of any agent employed by the leading university is strictly equal to the productive premium ($b^{p,t}$). In the long run, when $t \to \infty$, it also becomes invariant in time and is computed as follows: $b^{p,t} = \beta + \sum_{p=2,3} \sum_{\tau=1,\ldots,T} \gamma^{\tau}\beta$, which tends to $b \equiv \beta(1+\gamma)/(1-\gamma)$ when the relevant period T tends to infinity.

5 *Welfare analysis*

This section is dedicated to the welfare analysis. Optimal wages are computed in the first subsection under some simple specifications of the utility, disutility and production functions. In the second subsection, we compute the equilibrium wages given these specifications and study how they relate to optimal ones.

5.1 Optimal wages

We assume that the social surplus created by the academic activity is simply obtained, through function Φ, as the actualized sum of the individual productions times a given parameter ($\phi > 0$ which gives the per-unit social value of scientific knowledge, which is assumed to be homogeneous (or normalized).[17] Thus, the total surplus generated, actualized at period $t_0 = 0$, is given by

$$\Phi \equiv \sum_{\tau=0}^{\infty} \sum_{k=i} \sum_{p=1}^{3} \delta^{\tau}(\phi y_k^{p,\tau} - s_k^{p,\tau}), \tag{18}$$

with δ the social discount factor.

The program of the central planner is to set the optimal recruitment scheme at each period and stage. We assume that the central planner has exactly the same level of information as universities: at each period, it can only use ordinal information on the previous period ranking.[18] The planner naturally offers the best positions to the first-ranked agents (as universities competition does) in order to fully preserve the career incentives. It sets the optimal wages as follows: $\hat{S}^t = (\hat{s}^2, \check{s}^2, \hat{s}^3, \check{s}^3) = \arg\max_{S^t} \Phi(S^t)$ under the incentive constraints

[17] Notice that we do not assume that the social value of knowledge ϕ and the value considered by the universities ψ are identical.

[18] Therefore, our analysis can be seen as a second-best approach relative to an approach assuming omniscient central planner.

given in (8) and (10) and the participation constraints. By convention, wages $\hat{s}^p$ are offered to the first-ranked agents and wages $\hat{s}^p$ are offered to second-ranked ones.

We now specify the functions U, V and f^p according to their properties given in section 2. The utility function is simply assumed to be $U\ (s) = \ln\ s$. The disutility function is assumed to be quadratic in efforts: $V\ (e) = \frac{1}{2}ce^2$. The production functions of scientific knowledge are assumed to be linear in efforts: $f^2\ (e) = (\mu f^1\ (e) = \mu ae$. The strictly positive parameter μ, gives the increase in agents' productivity between the two first periods of their career. If we have $\mu > 1$, then agents' productivity increases through the career path. Let also the $\Delta\varepsilon^d$ be identically distributed across the different periods of the career, that is $g^d = g, \forall d$.

We focus on the long run wages $(t \to \infty)$, for which we know that $\Delta b^{p,t} \to b$, $p = 1, 2, 3$. Again, the long term wages are consistently expected to be stationary by agents. They anticipate that the next period wages will be the same they observe in the current period. Given such anticipations, the central planner has no reason to modify the long run wages in time.

The central planner sets the lowest optimal wages at junior and senior stages so as to saturate agents' participation constraints. Thus, the wages of the second-ranked agents at both stages are $\check{s}^2 = \exp\ (\widehat{W}^2)$[19] and $\check{s}^3 = \exp\ (\widehat{W}^3)$. The wages offered to the first-ranked agents are simply derived from the two FOCs of the central planner program solved for $\check{s}^2$ and $\check{s}^3$ (detailed computations are in the appendix):

$$\hat{s}^2 = 2\phi\,\frac{a^2}{c}\,\delta_a g(b) \tag{19}$$

$$\hat{s}^3 = 2\phi\,\frac{a^2}{c}\,\delta_a g(b)\big(\delta_a(2G(b) - 1) + \mu^2\big) \tag{20}$$

The optimal first-ranked junior wages decrease with the cumulative advantage b. The effect of b on $\hat{s}^3$ is ambiguous. The optimal wages also increase with the productivity of agents' efforts (a), the per-unit social value of scientific knowledge (ϕ), and agents' discount factor (δ_a). They decrease with the per-unit cost of efforts (c).

[19] We assume here, so as to simplify the notations, that agents compare the second stage outside option with the current period utility (and not with the whole career expected utility flow). In short, agents compare $\widehat{W}^2$ with $\underline{U}^2$ instead of Π^2.

5.2 Comparing optimal and equilibrium wages

Let us compute the equilibrium wages given the specifications introduced in the previous subsection. As stated in theorem 2, i), the lowest equilibrium wages saturate the participation constraints: $\underline{s}^2 = \exp(\widehat{W}^2)$, $\underline{s}^3 = \exp(\widehat{W}^3)$. These wages are strictly identical to their optimal counterparts. Let us now focus on the *competition equilibrium*. The equilibrium wages of the leading university are simply obtained by using equations (16) and (17) and introducing the specifications:

$$\bar{s}^2 = \exp\left\{\ln\left(e^{\widehat{W}^3} + B\right)[1 - \delta_a(2G(b) - 1)] - \varphi - \delta_a(2G(b) - 1)\left(\varphi - \widehat{W}^3\right)\right\} \tag{21}$$

$$\bar{s}^3 = \left(e^{\widehat{W}^3} + B\right)e^{-\varphi} \tag{22}$$

These wages saturate the constraint that the leading university attracts the two first-ranked agents for any profitable offer of the opponent university. The other university is ready to offer a wage to a first-ranked researcher up to the totality of the spillovers and reputation premia he would bring in. The first-ranked researcher accepts the position if such a wage provides her a higher utility than the satisfaction to be in the most reputed university and (at the junior stage only) the surplus of expected utility due to the higher probability to win the next contest. Thus the leading university offers a wage to the first-ranked agent so that the wages the other university should offer to attract her are sufficiently high so that it prefers to hire the second-ranked agent at the best rate.

If the leading university prefers to pay more to its employees just because this increases its payoffs due to the incentive effects of higher wages on its current employees at previous stages, we are in the *full incentive maximizing equilibrium*. It can easily be shown that the wages offered by the leading university in this case (set irrespectively of the competition constraints) can be simply derived from the optimal wages as follows:

$$\bar{s}^{p*} = \frac{\psi}{2\phi}\hat{s}^p, p = 2,\ 3 \tag{23}$$

Universities value scientific knowledge production at rate ψ instead of ϕ, and they only take into account the incentive effect of wages on their own employees. If $\frac{\psi}{2\phi}\hat{s}^p > \bar{s}^p, p = 2,\ 3$ and if the university's associated returns are higher, then the university sets the wage offer so as to maximize the incentives provided to its currently employed Ph.D. agents. Notice that the equilibrium wages will be lower than their optimal counterparts only if $\psi > 2\phi$, that is, if the universities value knowledge at least twice its social value.[20]

[20] For space constraints, we do not examine the mixed equilibrium here, the analysis of which does not bring much.

Proposition 6. *Optimal and equilibrium wages of the second-ranked agents at junior and senior career stages are equally set so as to saturate agents' participation constraints. The competition and full incentive equilibria wages offered to the first-ranked agents can be either greater or lower than the optimal ones depending on the values of the parameters.*

Proof. The proof results trivially from a comparison of (21) and (22) with (19) and (20), respectively, and from considering (23) for the full incentive maximizing equilibrium.

6 Conclusion

In this paper, we have introduced a model of academic competition which intends to capture both the life-cycle effect and the cumulative advantages effect of the academic reputation-based reward system on individual incentives. We have suggested a mechanism according to which such an effect is rooted in the employment relationship. Research positions are intrinsically unequally productive, and the allocation of the best positions is based on a ranking of past scientific productivity. Unequal productivity is essentially due to some positive externality high-ranking agents have on the scholars employed by their university and to a positive effect of the accumulated reputation of the employing university, which is endogenously determined by past successes in the recruitment of the most reputed scholars.

Our results highlight that the cumulative advantage has negative effects on incentives at each stage of the career but the first, at which the effect is ambiguous. The most important results of this paper concern the other side of the coin, namely, competition between universities. In equilibrium, the leading university always hires the first-ranked agents at junior and senior stages. The cumulative advantage the leading university confers to its employees is endogenously generated in the long run and is stationary. The wages offered by the non-leading university are optimal. There are three possible equilibrium wage offers by the leading university. In the competition equilibrium, the leading university sets the wages so as to saturate the competition constraint which ensure it hires the first-ranked scholars. In the full incentive equilibrium, the leading university sets wage offers so as to maximize the incentives provided to the agents it currently employs at previous stages. In the mixed equilibrium, only the junior wage saturates the competition constraint.

In all cases, there is no reason why the equilibrium wages offered to first-ranked agents should correspond to the optimal ones. In the competition equilibrium, the leading university cares only about the capacity of the opponent university to attract first-ranked agents. If competition between universities is very low, the full incentive equilibrium is likely to arise. Then, the leading university may not consider the competition constraint and rather focus on the provision of incentives to agents (as in the optimal scenario) but in a very specific and partial manner. The leading university does not take into account the incentives its wages have on the other university's employees. Moreover, since universities can control neither efforts nor production at any stage, no incentive can be provided to their seniors, and junior wages impact only on the Ph.D. they hire. It is only if the leading university values knowledge twice more than its social value (which is rather unlikely) that the full incentive equilibrium wages of the leading university are higher than their optimal counterparts.

Appendix

Computation of the second stage Nash equilibrium

The first order condition of program (8) is

$$\delta_a(\Delta U^3 + \varphi) \times \partial P(y_i^2 > y_j^2)/\partial e_i^2 = \partial V(e_i^2)/\partial e_i^2. \tag{24}$$

Notice that the probability that i wins the second contest is given by

$$P(y_i^2 > y_j^2) =$$

$$P(f^2(e_i^2) + \Delta b^2 + \Delta\varepsilon^2 > f^2(e_j^2)) = [1 - G^2(f^2(e_j^2) - f^2(e_i^2) - \Delta b^2)].$$

When differentiating that expression with respect to j's efforts, one obtains

$$\partial P(y_i^2 > y_j^2)/\partial e_i^2 = f^{2'}(e_i^2) \times g^2(f^2(e_j^2) - f^2(e_i^2) - \Delta b^2).$$

Introducing this expressions in the first order condition (24), one gets

$$f^{2'}(e_i^2) \times g^2(f^2(e_j^2) - f^2(e_i^2) - \Delta b^2)\delta_a(\Delta U^3 + \varphi) = V'(e_i^2).$$

Let us define the function Θ_2 by $\Theta_2(x) = V'(x)/f^{2'}(x)$. This function is defined on $\mathbb{R}^+, \Theta_2 : (0,\infty) \to (0,\infty)$. Since $V'(0) = 0$, this function is null at 0 ($\Theta_2(0) = 0$). Moreover, since $V' > 0$, $V'' > 0$, $f^{2'} > 0$, $f^{2''} \leq 0$, it can easily be shown that this function is strictly increasing: $V_2 > 0$. Thus, its inverse function $\Theta_2^{-1} : (0,\infty) \to (0,\infty)$ is also increasing. Then, one can rewrite the two first order conditions using these new notations:

$$\Theta_2(e_i^2) = \delta_a(\Delta U^3 + \varphi)g^2(f^2(e_j^2) - f^2(e_i^2) - \Delta b^2),$$

$$\Theta_2\,(e_j^2) = \delta_a\,(\Delta U^3 + \varphi)g^2\,(f^2\,(e_i^2) - f^2\,(e_j^2) - \Delta b^2).$$

Given the assumptions formulated so far, these two equations are of the form $e_i^2 = h\,(e_j^2)$ and $e_j^2 = h\,(e_i^2)$ with h a strictly increasing and continuous function on $\mathbb{R}^+$. Therefore, if an equilibrium exists, it is necessarily symmetric of the form $e_i^2 = e_j^2 = e^2$. This equilibrium then satisfies the following expression:

$$\Theta_2\,(e^2) = \delta_a\,(\Delta U^3 + \varphi)g^2\,(\Delta b^2)$$

Since Θ_2 is strictly positive, null at 0, and strictly increasing, this equation admits a unique solution. Moreover, since $g^2 > 0$, $\Delta U^3 > 0$ and $f^{2'} > 0$, this solution is strictly positive. The unique symmetric second stage Nash equilibrium is thus given by

$$\tilde{e}^2 = \Theta_2^{-1}\,(\delta_a\,(\Delta U^3 + \varphi)g^2\,(\Delta b^2)).$$

Computation of the first stage subgame perfect Nash equilibrium

Let p_i denote the probability that, if the agent employed by university i has won the first stage contest, he will also win the second stage contest. Since the second contest is influenced by the results of the first, p_i is a conditional probability. Since the second stage equilibrium efforts (9) are identical, that conditional probability is independent of agents' identity $\{p = p_i = p_j)$. It can be computed as follows:

$$p = P(e^2 + \varepsilon_i + \Delta b^2 > e^2 + \varepsilon_j) = 1 - P(\Delta\varepsilon < -\Delta b^2) = 1 - G^2\,(-\Delta b^2).$$

Referring to the assumption that g is symmetric around 0, one can write: $p = G^2(\Delta b^2)$.

We denote by $\Delta\Pi^p$ the surplus of expected utility received from stage p, inclusively, over the remaining career cycle that results from winning the contest at stage *p;* formally,

$$\Delta\Pi^p = \Pi_i^p\Big|_{y_i^p > y_j^p} - \Pi_i^p\Big|_{y_i^p < y_j^p},$$

where $\Pi_i^p\Big|_{y_i^p > y_j^p}$ is the expected utility of agent i at stage p conditionally on i winning period p's contest.

Using these notations and definitions, we can rewrite $\Delta\Pi^1$ as follows:

$$\Delta\Pi^1 = \delta_a\,(\bar{U}^2 + \varphi) + \delta_a^2\,[p^2\,(\bar{U}^3 + \varphi) + \,(1 - p^2)\underline{U}^3] - \delta_a\underline{U}^2 - \delta_a^2 \\ +[\,(1 - p^2)\,(\bar{U}^3 + \varphi) + p^2\underline{U}^3]$$

After a few simplifications, we get

$$\Delta\Pi^1 = \delta_a\,(\Delta U^2 + \varphi) + \delta_a^2\,(2G^2\,(\Delta b^2) - 1)\,(\Delta U^3 + \varphi),$$

with ΔU^2 the difference in utility between having won the first contest and having lost it. Introducing that expression in the first order condition of the first period maximization program (10), we get:[21]

$$\Theta_1^{-1}(e^1) = g^1(\Delta b^1)\delta_a[\Delta U^2 + \delta_a(2G^2(\Delta b^2) - 1)\Delta U^3],$$

with Θ_1 defined analogously to Θ_2, that is, $\Theta_1(x) = V'(x)/f^{1\prime}(x)$. The equilibrium is symmetric and unique for the same reason given in case of the second period. The final expression of the equilibrium efforts (11) follows.

The incentive properties of the cumulative advantage

Here, we study the effects of the competitive advantages at the first two stages of the career (Δb^1 and Δb^2) on the equilibrium efforts ($\tilde{e}^1$ and $\tilde{e}^2$).

The second period efforts are independent of the first period advantage. In order to characterize how the cumulative advantage Δb^2 affects the second period equilibrium efforts, we differentiate both sides of (11) with respect to Δb^2:

$$\partial\tilde{e}^2/\partial\Delta b^2 = g^{2\prime}[\Delta b^2]\delta_a(\Delta U^3 + \varphi) \times \Theta_1^{-1}(g^2(\Delta b^2)\delta_a(\Delta U^3 + \varphi)) \quad (\leq 0)$$

We know that Θ_1^{-1} is an increasing function. Moreover, since $\Delta b^2 > 0$, and since g^2 has its unique extremum at 0, $g^{2\prime}(\Delta b^2)$ is strictly negative. Thus, we can conclude that $\partial\tilde{e}^2/\partial\Delta b^2 \leq 0$.

The first period equilibrium efforts are functions of both the first and second stages cumulative advantages. From (9), we obviously have $\partial\tilde{e}^1/\partial\Delta b^1 < 0$, since Θ_1^{-1} is increasing, $\Delta b^1 > 0$, and $g^1(x)$ decreases for all $x > 0$ and, thus, $2G^2(\Delta b^2) - 1 \geq 0$ (g^1 is symmetric around its unique extremum at 0). As regards the effect of the second stage advantage on the first stage efforts, we differentiate both sides of (9) with respect to Δb^2. We get

$$\partial\tilde{e}^1/\partial\Delta b^2 = 2g^1(\Delta b^1)g^2(\Delta b^2)\delta_a^2(\Delta U^3 + \varphi)$$

$$\times\Theta_1^{-1\prime}(g^1(\Delta b^1)\delta_a[\Delta U^2 + \varphi + \delta_a(2G^2(\Delta b^2) - 1)(\Delta U^3 + \varphi)]) \quad (> 0).$$

The second period bias has a disincentive effect on the second period efforts while it increases first period efforts.

[21] The symmetry of the density function around 0 preserves the symmetry of the equilibrium since $g^p(\Delta b^p) = g^p(-\Delta b^p)$, $p = 1, 2$.

Proof of theorem 2

The proof has two parts.

a) *It is an MPE*. Let us show that university *j* has no incentive to deviate. If *j* offers a higher wage at any stage, two cases may arise. This increase is not sufficient to attract the first-ranked agent. Then *j* has higher costs but returns remain unchanged. If the increase is sufficiently high, university *j* attracts the first-ranked agent but, according to ii), *j*'s expected payoffs are lower. If *j* decreases any wage offer, then the second-ranked agent has an incentive to deviate and leave the university system. The university does not fill the position and gets a lower return according to A3.1. Thus, *j* has no incentives to move. Let us show now that university *i* has also no incentive to deviate. If *i* increases any wage, revenues remain the same while costs increase. If *i* decreases wages by any increment, then *j* reacts by setting a wage which allows it to hire the first-ranked agent and increase its returns. This would of course sharply decrease *i*'s payoffs. Thus, *i* has no incentive to move either.

b) *No other MPE exists*. Excluding the above mentioned MPE equilibrium, there are four possible situations for any given stage *p* (which can be treated independently) that can be categorized by comparing the wages offered by *i* and *j* to those offered at $\hat{\sigma}^t$. 1) Both universities offer higher wages at stage *p:* If *j*'s offer is not sufficient to attract the first-ranked agent, then it has a incentive to reduce its offer. If it is sufficient to attract the first-ranked agent, then *j* has an incentive to reduce its offer because its payoffs are then lower than the payoffs from just setting its offer to $\underline{s}^{p,t}$ and hiring the second-ranked agent. This would already be true if *i* were setting its wage offer at $\bar{s}^{p,t}$ (condition ii). Now that *i* makes an even higher offer, it is clearly also true, and *j* has an incentive to deviate. 2) University *j* offers a higher wage at stage *p* and *i* a lower wage: If *j*'s offer is not sufficient to attract the best scientist, *j* clearly has an incentive to deviate. If it is sufficient, then *i* has an incentive to increase its offer so that it will reach the threshold given in ii), that is, where *j* will prefer reducing its offer and aim at hiring the second-ranked agent. 3) Both universities offer lower wages at *p:* Then j certainly cannot hire any agent and gets lower payoffs according to A3.2. University j deviates. 4) University j offers a lower and *i* a higher wage at *p:* Then j certainly cannot hire any agent and gets lower payoffs according to A3.2 and *i* pays a higher salary without compensation. University *j* deviates. In all situations which differ from the MPE, at least one university deviates. Then, there is no other equilibrium.

Computation of the optimal wages

The program of the central planner can be rewritten as follows:

$$\max_{\hat{S}^t, \forall t>0} \Phi = \sum_{\tau=0}^{\infty} \delta^{\tau} \left(2\phi \left[f^1\left(\tilde{e}_1^{\tau}\right) + f^2\left(\tilde{e}_2^{\tau}\right) + f^3\left(\tilde{e}_3^{\tau}\right) + 3\varepsilon\right] \right.$$
$$\left. +3\phi \Delta b^{p,\tau} - \left[2s^{1,\tau} + \hat{s}^{2,\tau} + \hat{s}^{2,\tau} + \hat{s}^{3,\tau} + \hat{s}^{3,\tau}\right]\right)$$

Given the specifications introduced in section 4, we have $\Theta_1(e) = \frac{c}{a}e$ and $\Theta_2(e) = \frac{c}{\mu a}e$. Moreover, one obtains $f^1 \circ \Theta_1^{-1}(x) = \frac{a^2}{c}x$ and $f^2 \circ \Theta_2^{-1}(x) = \frac{(\mu a)^2}{c}x$. Then Φ becomes

$$\Phi = \sum_{r=0}^{\infty} \delta^{\tau} \left(2\phi \left[\frac{a^2}{c} \delta_a g(\Delta b^{p,\tau})(\ln \hat{s}^{2,\tau} - \ln \check{s}^{2,\tau} + \varphi + \delta_a [2G(\Delta b^{p,\tau}) - 1] \right.\right.$$
$$\left. + (\ln \hat{s}^{3,\tau} - \ln \check{s}^{3,\tau} + \varphi)). + \frac{a^2 \mu^2}{c} \delta_a g(\Delta b^{p,\tau})(\ln \hat{s}^{3,\tau} - \ln \check{s}^{3,\tau} + \varphi) + 3\varepsilon \right]$$
$$\left. +3\phi \Delta b^{p,\tau} - \left[2s^{1,\tau} + \hat{s}^{2,\tau} + \check{s}^{2,\tau} + \hat{s}^{3,\tau} + \check{s}^{3,\tau}\right] \right).$$

In the long run, when $\tau \to \infty$, the cumulative advantage becomes stationary $\Delta b^{p,\tau} = b$, as shown in corollary 5. Then, the optimal wages also become stationary: $\hat{s}^{p,\tau} = \hat{s}^p$ and $\check{s}^{p,\tau} = \check{s}^p$, $p = 2{,}3$. The first order conditions of the central planner's program, solved with respect to the lowest wages, lead to negative values. Then, the central planner saturates the participation constraint of the second-ranked agents at the two stages considered: $\underline{s}^2 = \exp(\widehat{W}^2)$ and $\underline{s}^3 = \exp(\widehat{W}^3)$. After some computations and simplifications, the first order conditions computed with respect to the highest wages lead to the equilibrium wages given in (19) and (20).

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An Economic Theory of Academic Competition

Comment by

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Economists and organizational theorists have long overlooked the study of their own institution, the university. What precisely do universities produce? What is their objective function? Why are they structured the way there are? What is the best way to organize them? Align incentives? The enormous increase in higher education together with budgetary restrictions, increased systems competition, the importance of human capital, in particular, in research and development, brain drain, and more, all of these things should induce organizational researchers to analyze the institutions of higher education. Nicolas Carayol's paper is a welcome step in that direction.

Carayol aims at explaining two phenomena observed in the university systems of most countries and referred to in his paper as the life cycle and the cumulative advantage effects. The first effect simply recognizes that for most professors, the early phase of their career is usually also the more productive in terms of research. This observation seems to be true independently of the country examined despite very different university systems. The second effect refers to the impact of reputation which seems to afford a durable advantage to institutions endowed with it. The paper shows that both effects can be explained as resulting from an optimal mechanism problem in the face of moral hazard difficulties.

Intuitively, suppose universities benefit from the research output of their researchers and researchers benefit from the reputation of their respective institution. At the end of their doctorate, more reputed universities will be able to attract the best young professors. In addition, due to the moral hazard problem on the side of professors, universities will offer them an incentive scheme. Here the paper imposes a very strong restriction assuming that universities cannot offer incentive schemes based on output. Rather each period the universities make competitive wage offers to their academic staff. Given the competition between the universities, researchers are faced with a form of tournament. However, given that the universities do not have the same reputation the tournament is biased, favoring last period's winner.

The setup provides a straightforward explanation for the inverse U-shaped productivity. Naturally, in the three periods framework analyzed by the model, the winner in the doctoral phase goes to the better institution. Therefore, due to the positive externality of reputation, his next period's productivity increases. However, in the third period the researcher does not have any incentive to work since the wage scheme only rewards last period's output. Altogether, it guarantees the inverse U-shaped result. More generally, the reputation effect would allow us to generalize the conclusion to a model where academics live longer than three periods. Intuitively, the effect would obtain because reputation is important to the academic in the early phase of his career, but evidently becomes less important over time. The cumulative advantage effect is also very intuitive; better institutions can more easily attract promising academics. They in turn have higher productivity (also due to the reputation effect). On average and over time, the mechanism only reinforces the reputation on the institution.

The paper suggests a few interesting conclusions. For the remaining discussion, we will need to keep in mind that universities produce more than just research, e.g., teaching and administrative tasks. These other tasks are not all as difficult to measure. For example under the old German university system, professors received a bonus related to the number of students participating in their class. In such a context, the foregoing analysis suggests that universities should provide incentives for older colleagues to shift the emphasis of their work away from research and more towards those more easily measurable activities. For example, in the current German system faculties often nominate relatively young professors to become chairman while some of the older colleagues are free from administrative work. Given the inversed U-shaped effect described above, this is most certainly a waist of human capital. From the point of view of the German university system and according to Carayol's model, this should have two negative effects. First, it reduces the accumulated reputation since some of the human capital is wasted. Moreover, it suggests to the better German academics that they would be better of starting outside of the German system, for example by spending the early part of their career in the US. Again from the point of view of the overall system, this effect is negative reducing their current reputation and through the inter-temporal feedback also future reputation.

In addition, the paper makes clear that universities may have an advantage providing tournament contracts, instead of solely creating incentives by the use of outside offers. As discussed earlier, the outside offer scheme distorts incentives, particularly in the latter part of the career.[1] A tournament scheme between academics of single institutions would require for many countries a significant departure from current practice. Since output is not easily verifiable from the outside (e.g., how would an economist be able to judge the research output of colleagues in a medical faculty and vice et versa), it would require either a complicated mechanism with outside referees or delegating decisions to a "powerful" chairman. The later mechanism only functions as

[1] It is likely that distortions are not only found in the latter part of the career. Without solving for the first best solution, I would presume that in the early phase researchers may, under some conditions, get involved in a *rat race* contest.

part of a reputational equilibrium. In that respect, it is interesting to note that North American universities (i.e., Canadian included) have been doing this for quite a while. In practice, it seems to require both a powerful chairman and outside referees.

The paper also emphasizes that an intelligent emeritus policy may be advantageous. It would allow universities to increase research incentives even in the last years of a colleague's career. For example, granting privileged access to research facilities and libraries, office space, secretarial work etc. are all important ways to reward colleagues for their commitment to the last period.

To conclude, I would like to discuss a few critical aspects of Carayol's paper, thereby suggesting future avenues of research. First and foremost, the paper introduces a very *ad hoc* objective function for universities and for professors. Not withstanding the importance of research, universities are also focused on teaching and require a management structure. Due to particularities of higher education, universities are usually managed by academics themselves. What would be important is to create a link between "teaching and research" output and revenues, or, alternatively, directly with society's welfare.[2] Regarding the description of researchers, an extension including multiple tasks would appear essential and promising. Finally, considering alternative hypotheses may also be useful. Suppose, for example, that the incentive problem for research can be easily solved, either because research output is more easily verified than often assumed or because of intrinsic motivation. Moreover, suppose that academics decide on their specialization during their doctoral phase and that changing specialization later on is too costly. Finally, assume that the "hot" topics follow some random drift and that better universities have better information on the drift.[3] First, this would explain why doctoral candidates want to go to top universities and why graduates from top institutions do better on the job market. It would also explain the inversed U-shaped research output. Intuitively, over time "hot" topics shift away thereby reducing the output of researchers. For example, the benefits of having done a Ph.D. with Debreu vanished in the early 80s. Last but not least, it also explains why the reputational advantageous are long lasting.

[2] To emphasize the importance of this point, consider the case of French faculties in economics. Clearly outside of France most would agree that Toulouse is currently the best French research faculty. Nevertheless, recent decisions by the French ministry of education seem to favor institutions in Paris for the development of a top Ph.D. Program.

[3] For example, because the editorial boards of the top journals are selected from renowned universities. These assumptions would also explain why universities often subsidize journals by allowing their researchers to take editorial position and often even rewarding them for doing so.

Research Networks – Origins and Consequences: First Evidence from a Study of Astrophysics, Nanotechnology and Micro-economics in Germany

by

Dorothea Jansen*

1 Introduction

There is a fast growing discourse on the potential benefits of networks for the research process in sociology, business administration and economics. Important concepts are the idea of the emergence of a new mode of knowledge production, the concept of social capital and its role for the production of knowledge or the idea of creating critical mass in research. These ideas have found their way into science policy recommendations and programs such as networks of excellence or the promotion of interdisciplinarity and of university-industry-cooperation by research funding organizations in Germany and abroad.

This paper presents concurring hypotheses on why networks might have a positive impact on research performance. Preliminary evidence from a quantitative and qualitative study of three subfields, astrophysics, nanotechnology and micro-economics, is presented. These fields are characterized by input and output indicators with a special focus on the structure of networks and on strategic network behavior. Next to a bivariate analysis, four preliminary models relating input, networks and output are presented. The data were collected in a research project which is part of a larger research group dealing with the changing governance of the German research system.

2 The role of networks in scientific production

2.1 Networks and a new mode of knowledge production

The information society, the knowledge society and the network society are metaphors trying to catch important characteristics and changes in modern societies. One of these metaphors is the idea of a new mode of knowledge

* The members of the project team are Dorothea Jansen (principal investigator and speaker of the research group), Andreas Wald and Karola Franke at the German Institute for Public Administration. Information on the project and the larger research group is available at www.foev-speyer.de/governance/. Funding by the German Research Association is acknowledged (DFG FOR 517: Ja 548/4-1, Ja 548/5-1, Ja 548/5-2).

production (Gibbons et al. 1995, Nowotny et al. 2001). Scholars from the sociology of science postulate that scientific knowledge today is no longer the domain of scientific disciplines and academic actors. Instead it arises from distributed production connecting producers and users from different societal subsystems. Transdisciplinarity and an orientation toward application, collaboration and networking between these actors are supposed to become vital for the production of knowledge and for its legitimacy. The proponents of these contested theses (for a critique c.f. Weingart 1997, Krücken 2001, Slaughter and Rhoades 2004, Trute 2005) expect that users instead of academic disciplines will have a larger say in the definition and evaluation of research programs. The trend for this new mode of knowledge production is supposed to be particularly strong within fields such as biotechnology or nanotechnology while traditional fields such as astrophysics are seen to hold on to disciplinary lines.

Although there is still little systematic empirical evidence of a positive effect of a mode-2 type of research and collaboration profile on academic performance,[1] the mode-2 ideas soon became topics in the political debates on reforming the German research system. Shortcomings in quality and quantity of research output, in competitiveness and innovativeness of the system were attributed to a deficit in collaboration and networking between disciplines, between different types of research organizations, between academic and industrial actors and between basic and applied research. More collaboration and heterogeneous collaboration and networking are asked for by more and more funding agencies and programs (DFG 2003, Wissenschaftsrat 2003). I will try to give a tentative answer to the question of whether enforcement of heterogeneous networks or of an applied research strategy actually does enhance scientific productivity. In the next paragraph, I argue that networks can be an asset and deliver social capital, but can be a social liability as well.

2.2 Social capital and social liability from research networks

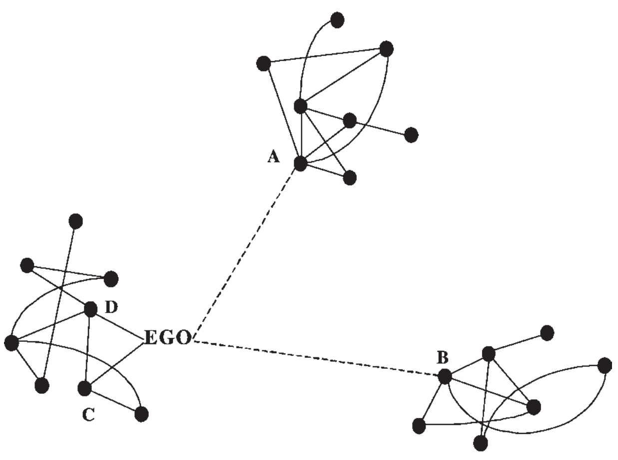
The main producers of new knowledge today are not individual researchers or entrepreneurial inventors but research groups collaborating within and across organizations. These groups are embedded in different types of organizations (academic, government, industry), disciplines and industry sectors. New knowledge and especially basic innovation and new paradigms emerge mostly at the margins of disciplines, organizations and sectors (cf. Hippel 1988, Blackler et al. 1998, Nahapiet and Ghoshal (1998). It is produced by combination and exchange. This is why embeddedness into research networks via

[1] Negative effects found by Evans (2004); no effects found by Gulbrandson and Smeby (2005) and Heinze (2004).

research contacts, the flow of information, knowledge pieces, materials, instrumentation and people can be treated as a kind of social capital.

I define those aspects of a network structure[2] that open or constrain opportunities of action for individual or corporate actors as social capital. Social capital can be converted into other forms of capital. In the social network literature, five types of benefits from social capital are distinguished (Jansen 2002, Lin et al. 2001): information and (tacit) knowledge, trust into and enforcement of norms, structural autonomy, entrepreneurial profits from arbitrage, and finally social influence coming from legitimacy and reputation attributed by other relevant actors. The benefits accrue to individual and corporate actors, or to groups of actors within a social structure.

Figure 1 The bases of social capital: weak ties vs. strong ties, structural holes vs. dense clusters[3]



The different benefits from social capital are supposed to be based on different types of ties and structural configurations. Structures or positions, which are beneficial in one regard can be detrimental for some other goal. The main structural differentiation is between so called strong and trusted ties (solid lines) in densely knit networks and so called weak ties (dotted lines) in sparse extended networks. A sparse network yields information and structural

[2] Networks in a methodological sense consist of a set of nodes (actors, events, ideas) and the edges/ relations that are defined on them (e.g. information flow, influence, membership). This concept is related to but not identical to the governance focused use of the term in transaction cost economics (Williamson 1991) or in neoinstitutional sociological approaches (Powell 1990, Podolny 2001, Jansen 2002).

[3] Adapted from Burt (1992, 27).

autonomy for a broker. Brokers can bridge "structural holes" (e.g. between EGO and the three dense clusters) and thereby combine diverse information/ knowledge, transfer knowledge or extract arbitrages from otherwise unconnected ties. While Burt (1992) in his theory of structural holes claims that dense networks mean constraints and inefficiency for an actor, other scholars underline the positive effects of dense networks with easy going collaboration and knowledge flow, high trust and low transaction costs (Coleman 1990, Powell 1990).

Empirical studies show strong tendencies of interorganizational and personal networks towards homophily and stability. In-depth studies of collaboration patterns (Uzzi 1997) as well as longitudinal quantitative studies of alliance formation (Gulati and Gargiulo 1999, Todeva and Knoke 2002) report that network formation is guided by previous experience with partners or partners of partners. Strong and embedded ties tend to go together with high returns to an actor in the form of stability, profitability, successful innovations, access to tacit knowledge and to finance (Uzzi 1997, Talmud and Mesch 1997, Ingram and Roberts 2000, Hansen 1999, Ahuja 2000, for a review see Jansen 2002). But there is also evidence that an overdose of embeddedness into networks can hamper innovation and produce too much confidence into established routines and products (Burt 1999, Kern 1998, Henderson and Clark 1990).

Studies of academic research networks at the micro level are mostly based on bibliometric data, i.e. copublication analysis. They confirm a positive effect of network embeddedness, particularly of top level and international ties on scientific output and impact (Frenken et al. 2005, Adams et al. 2005). Structural information (clustering, density, brokerage positions) are seldom reported in bibliometric analysis.

Thus the central question is which type of tie and which type of network is more successful in knowledge production in the long run. Will trust breeding cliquish networks bring about stability at the cost of innovation and learning capacities? What is the effect of brokerage between cliques? Since network structures and ties that work in the exchange of codified and public knowledge may not work in the transfer and creation of implicit or proprietary knowledge, the ultimate question will probably be how to balance both types of ties and stability and openness of networks.

2.3 Learning and network strategies as governance mechanisms in networks

There are two important cognitive variables which can explain the effect that networks will have in the long run. The capacity for the production of new knowledge depends on the absorptive capacity (Cohen and Levinthal 1990) of a group. Only those working close to new knowledge can grasp its relevance. This means that a wise learning strategy must invest into the monitoring of research areas other than the current ones. Whether a research group is able to do this depends on its size and organizational slack.

Next, a wise network strategy should try to avoid the ossification of networks. Open and strategic choice of complementary skills and synergy of research capacities should be an important argument in network formation. But such a strategy rests on the provision of a functional equivalent for personal trust as the heretofore prominent governance mechanism in networks. Systems trust into more abstract role structures and positions must be built up endogenously by the network actors.

Centrality and prestige of actors might be able to substitute for personal experience (cf. Powell et al. 1999, Stuart 1998, Podolny et al. 1996). Actors in central positions tend to attract collaboration offers without such previous experience. In particular, actors in the center of role structures succeed in combining high centrality and prestige with a broker position that attracts new ties (cf. Jansen 2000 and 2004, Darr and Talmund 2003, Obstfeld 2005). They are the ones that connect heterogeneous partners. But their position does not – as Burt would have it – support unconstraint brokerage and arbitrage. On the contrary, they are caught between two or more groups or cliques (Krackhardt 1999). They have to integrate divergent demands, research cultures, and disciplinary views. At the same time they are highly visible. Reputation effects are strong for them and they have a lot to loose. Transitive role structures and actors in between several cliques might very well work as governance mechanisms that support trust in an open social structure with changing actors. Those with high influence and prestige act as trustees who can prevent opportunistic behavior in networks by informal more or less horizontal control and sanctioning (cf. Wittek 1999, Lazega 2000).

This idea of stable transitivity and changing actors within an open social structure can only work if actors build their network not only on past experience but on a forward looking calculation of potential trustworthiness (Buskens and Raub 2002). Since ossification of networks could thus be avoided the strategic attitude towards networks should have a positive influence on the absorptive capacity for new ideas and on research performance.

2.4 The role of funding organizations and research policy

The political quest for building networks comes along with another reform issue: the idea of strategic concentration of funds on selected research programs. At the meso-level, organizations are advised to concentrate on core competencies and sharpen their profiles. At the micro level research groups are advised to assemble critical mass by becoming part of a larger research network. However, it is not at all clear how an increase in networking, concentration of internal resources and concentration of external funding will influence innovativeness and competitiveness of the research system.

One of these problems is the choice between generalist and specialist strategies by the research groups. What will research groups do when they are confronted with a highly volatile research area and concentrated resources? Focused funding programs establish a monopolistic demand structure. From the point of view of organizational ecology, they constitute a coarse grained environmental niche (Hannan and Freeman 1977). Science and engineering are characterized by a so-called concave fitness structure, i.e. large differences between the demands of different research lines and methods. Concave fitness structures ideally should lead to high profits from specialization. In interaction with a coarse-grained environment and high volatility, population ecology instead predicts a more generalist strategy as a hedge against long periods of low fit to the demand structure. De-differentiation and a loss of returns on investments in specialization could be the consequence. Generalist strategies would also lower the need for external collaboration and networking.

Another question is how the strategy of research groups depends on its size and on resources of the larger organization (Wernerfelt 1984). It might very well be that under conditions of resource concentration only large and established research organizations are able to profit from specialization by internal differentiation and the management of resources and portfolios. Networks ideally allow for the bundling of resources. Thus, networks might be able to solve the critical mass problem of the smaller research groups. Open networks might be able to profit from their heterogeneity and innovativeness. On the other hand networks combined with the concentration of resources on large programs might lead to lock-in effects. They might undermine the emergence of new research lines, which happen to fall outside of focused programs and profiles. Enforcing network building and critical mass might come to the disadvantage of smaller university groups, who cannot build on the support of large and established research institutions.

2.5 Concurring hypotheses on determinants of research productivity and the role of networks

According to all approaches discussed above the size of networks is expected to enhance research performance (H1). Disciplinary heterogeneity of research groups (H2), an applied research orientation (H3), and industry collaboration (H4) increase research performance in the perspective of the mode-2 theory. The structural holes approach in social network analysis posits that heterogeneity of networks and low social control/ constraints in networks have a positive impact on performance. Thus it is expected that a large amount of industry ties (H4) as well as of international ties (H5) enhance performance.

Low network density in ego-networks (H6) is an indicator of efficient networks and low control/ constraints. Sparse networks therefore could support high performance. On the other hand, the concurring view on the most important asset from networks holds that dense networks will support fruitful scientific exchange and knowledge transfer (H6 reversed). From an actor-centered learning perspective on networks, the strategic relevance of networks, an open choice of partners and the intake of new ties into networks are seen as indicators for a governance mechanism that might avoid the pitfalls of closed ossified networks. Open networks are expected to have a positive impact on performance (H7). Next, arguments from organizational ecology and management posit that specialization and differentiation of research profiles lead to higher performance (H8) as well as to larger networks (H9). Organizational ecology draws attention to a possible negative effect of a coarse grained environment – here a concentration of funding in focused programs – on specialization and performance (H10). Finally, from a resource based view on organizations we expect that groups from large established research institutions such as the Max Planck Society or the National Research Centers are in a better position to attract large networks (H11) and to invest in specialization (H12). This in turn yields a higher performance (H13).

3 Networks and scientific performance – preliminary evidence

3.1 Sampling procedure and data collection

Three disciplinary subfields were chosen for this study. One of them is a typical mode-2 field – nanotechnology – while another one is a typical mode-1 field – astrophysics. To represent the social sciences disciplines micro-economics was chosen. The population includes all German institutions which published at least one article in the selected fields according to the Science Citation Index (SCI) respectively ECONLIT in 2002 or 2003. Fields were technically described by experts from the central data project. The relevant research groups affiliated to the institutions listed were identified with the help of directories and other information available at the web. The research group is defined here as the smallest unit in an organization which conducts a more long-term research program.

The web search resulted in 122 astrophysics groups, 225 nanotechnology groups, and 56 microeconomics groups. After a validation with the help of experts from academia and funding institutions, samples of size $n = 25$ were drawn for each field. Expert interviews with the leaders of these research groups were conducted in 2004 and 2005. The interview consisted of a semi structured qualitative part and a network inventory for the collection of so called ego-networks (JANSEN 2003, 79 – 85). In addition the interviewee was asked to fill in a standardized questionnaire on input and output data of his group.

Bibliometric data on publications, citations and co-publications at the level of the members of each research group were collected and aggregated to a bibliometric profile of each group.[4] For astrophysics and nanotechnology the SCI database, for microeconomics SCOPUS with better journal coverage was used.

3.2 Research input and output and disciplinary differences

The most important resource for research are researchers. Table 1 shows some striking differences in the size, composition and funding of research groups between the fields. Both fields from the natural sciences by far exceed the size of the typical research group in microeconomics. Dispersion of size is high in all fields and particularly in the natural sciences. Groups from universities tend to be smaller than those from non-university research institutes. In microeconomics, professors account for about one third of the manpower of a group, while in nanotechnology they represent just 7%.

Table 1 Structure of staff

	Astrophysics		Nanotechnology		Microeconomics	
	Mean	Std	Mean	Std	Mean	Std
# researchers	13.4	11.5	13.8	13.5	4.1	2.4
% internally funded	48.1%	5.8	31.5%	4.2	85.1%	1.9
% funded by third stream	51.5%	9.1	63.6%	8.9	14.4%	0.8
% of postdocs	67.9%	7.5	48.8%	8.9	43.4%	1.7
% of C3/C4	9.4%	0.8	6.9%	0.6	30.8%	1.0
% of doctoral students	51.7%	6.6	50.6%	6.8	67.9%	2.0
% of research students funded	9.3%	2.7	4.3%	1.0	13.9%	0.9
# technicians	2.2	2.8	2.9	2.4	0.3	0.5
# disciplines in group	1.4	0.6	2.1	1.1	1.6	0.7
Valid cases listwise	22		22		22	

In line with the idea of a mode-2 field, nanotechnology groups fund 64% of the group's manpower by external funds. The difference to astrophysics as a natural science mode-1 field is not that high (52%), only on the edge of being significant. The funding structure of the microeconomics groups is completely

[4] The data on the population institutions and the bibliometric data on the research groups studied in depth were provided by the central data project, ISI Karlsruhe. Thanks to Ulrich Schmoch, Sybille Hinze and Torben Schubert.

different. With almost one third of manpower at the professorial level the degree of internal funding is much higher ($p < 0.01$). Disciplinary heterogeneity in the groups is highest in nanotechnology.

Time for research is lowest in microeconomics with a large amount of professorial group members who devote a lot of time to teaching duties. Percentage of time for research is much higher in both natural science fields with a higher amount of externally funded personnel explicitly hired for research projects. Percentage of time for project acquisition is largest in nanotechnology (13.4%) with the largest amount of externally funded researchers. Time for research is getting scarcer and time for teaching and administrative duties increased in all fields.

Table 2 Allocation of work time of the group

	Astrophysics		Nanotechnology		Microeconomics	
	Mean	Std	Mean	Std	Mean	Std
% time for research	60.4%	17.6	55.9%	17.6	40.5%	17.0
% time for teaching	19.8%	11.9	18.6%	10.6	34.1%	15.0
% time for project acquisition	10.2%	5.7	13.4%	8.4	7.9%	5.6
% time for other work	9.5%	9.8	12.0%	7.8	17.0%	10.7
Change in time for research	2.7	0.8	2.7	0.8	2.6	0.8
Change in time for teaching	3.3	0.8	3.7	0.6	3.6	0.7
Change in time for project acquisition	3.3	0.8	3.5	0.8	3.0	0.8
Change in time for other work	3.6	0.8	4.1	0.8	3.8	1.0
Valid cases listwise	23		20		21	

Change: 1 = much reduced, 5 = much increased

In all fields groups invest by far most of their research time into basic research projects. Astrophysics qualifies as a typical mode-1 field with very few applied research, nanotechnology groups devote a fifth of their capacity to applied research. The high amount of applied research in microeconomics (25%) reflects the work of groups applying microeconomic analysis to environmental, innovation and agricultural problems. It comes at some surprise that there is hardly a difference between nanotechnology and astrophysics concerning the amount of development work (13–15%). Interview data show that this work in both fields deals mostly with the building of new research equipment.

Output indicators show a large amount of dispersion. This is partly due to the large differences in group size. The large standard deviations reflect the typical evidence of highly skewed distributions of research output (Lotka 1926, Price 1976). While publications in national journals still have some relevance for microeconomics, astrophysics and nanotechnology groups exclusively publish in international journal. For all fields publication in

Table 3 Research orientation and type of funding

	Astrophysics		Nanotechnology		Microeconomics	
	Mean	Std	Mean	Std	Mean	Std
% basic research	84.4%	19.1	63.9%	26.3	74.3%	31.0
% applied research	2.5%	5.3	21.1%	21.8	25.7%	31.0
% development	13.1%	16.9	15.0%	12.4		
% of third stream funded research	59.6%	23.5	70.7%	30.6	20.2%	27.0
out of this						
% funded by science foundations	57.2%	33.2	58.2%	31.3	76.8%	36.5
% funded by public/ state institutions	37.0%	33.6	26.6%	23.6	18.2%	33.6
% funded by industry	0.6%	2.2	8.6%	11.4	5.0%	9.2
% other funding	5.2%	14.7	7.1%	18.0	0.0%	0.0
Valid cases listwise	23		21		23	

Table 4 Output indicators – self report

Time period 2002–2003	Astrophysics		Nanotechnology		Microeconomics	
	Mean	Std	Mean	Std	Mean	Std
Papers in international refereed journals	44.3	43.0	39.0	64.6	7.0	8.5
Papers in national refereed journals	0.0	0.0	0.2	0.7	1.4	1.7
Conference papers	48.0	62.3	34.8	48.4	9.3	14.7
Papers international/ researcher	3.8	2.3	2.8	1.6	2.0	3.0
Papers national/ researcher	0.0	0.0	0.0	0.1	0.4	0.5
Conference papers/ researcher	4.0	3.1	2.1	1.3	2.3	2.9
Valid cases listwise	23		20		22	

international journals is the most important output. Even in nanotechnology patents are not of countable relevance.

The differences between the fields shrink a lot when we control for number of researchers. The number of international journal papers per researcher and conference papers per researcher is about twice as large in astrophysics compared to microeconomics. Nanotechnology is in between with regard to international papers and at the same level as microeconomics in conference papers. The lower productivity of microeconomics is probably due to the fact that these groups devote less of their time to (externally funded) research than both natural science fields.

In table 5 self reported data from the standardized questionnaire and the self assessment data from the interviews are compared to bibliometric data. Nanotechnology groups are slightly more productive according to bibliometric data, the profile of astrophysics is slightly lower. Microeconomics ranks third both in bibliometric indicators, self reports and assessments.

Table 5 Comparison of questionnaire and bibliometric output data

Time period 2001 – 2002	Astrophysics			Nanotechnology			Microeconomics		
	Mean	Std	*n*	Mean	Std	*n*	Mean	Std	*n*
Papers in international refereed journals, self report	44.4	43.0	23	39.0	64.6	20	7.0	8.5	22
Papers in international refereed journals, bibliometric data	37.2	46.1	25	45.5	65.8	27	0.7	0.9	25
Citations	8.4	7.2	25	4.9	4.5	26	0.2	7.1	25
Self assessment	1.67	0.92	24	1.95	0.86	21	2.95	1.23	20

Self assessment: 1 = international top group, 5 = not so strong

3.2 Network structures and networks strategies

Data on the ego networks, i.e. focused networks around the research group, were collected with the help of a standardized inventory. Interviewees were asked to name all those actors (so called "alteri") with whom they collaborate in joint projects. For up to 20 mentioned ties structural data (i.e., whether the alteri also know each other) and attributes of the alteri were collected.

Corresponding to the differences in group size, large networks are much more common in the natural sciences than in microeconomics. The difference is even larger when we look at the size of gross networks instead of the smaller networks described structurally in table 6 (see p. 220). Nanotechnology commands the largest networks (28.6) followed by astrophysics (24.8) and microeconomics (13.4). The structurally described networks in the three fields are of similar density (0.41 – 0.43). Almost half of the possible ties between alteri do exist. As a young field, nanotechnology is characterized by rather young ties compared to astrophysics. Mean tie strength does not make much of a difference between the fields. It is slightly lower in nanotechnology. Astrophysics is the most international field. It comes as no surprise that nanotechnology is the only field with a relevant percentage of ties to industry – albeit almost ninety percent of ties relate them to other academic groups. Thus academy still seems to have a large say in research questions.

Concerning the hypotheses on network strategies, we need more qualitative information on how groups perceive their networks, how they use them and what the driving forces of network formation and change are. Data on the change in networks will be collected in follow-up interviews scheduled for 2006 and 2008. In the first interviews qualitative information on the origins of research projects and research networks and on the strategies connected to them were collected. A qualitative content analysis yielded several non-exclusive factors which were coded as multiresponse variables for statistical analysis.

Table 6 Structure of research networks

	Astrophysics			Nanotechnology			Microeconomics		
	Mean	Std	*n*	Mean	Std	*n*	Mean	Std	*n*
Size*	10.080	3.174	25	11.300	4.027	27	7.240	5.797	25
Network density*	0.427	0.235	25	0.412	0.236	27	0.430	0.226	23
Mean duration of ties*	9.674	4.076	21	6.811	3.487	20	7.742	4.379	22
Mean tie strength* **	1.533	0.226	25	1.466	0.231	27	1.533	0.284	23
% of international ties*	0.614	0.233	25	0.381	0.249	27	0.398	0.302	23
% of industry ties*	0.000	0.000	25	0.107	0.154	27	0.032	0.088	23

* Detailed ego-network data
** 1 = weak, 2 = strong

The most frequent motivation given for a project is that it emerged from path dependence naturally. More than three quarters of the respondents in astrophysics and microeconomics and more than half of the nanotechnologists attributed the origin of their projects to path dependency. Scientific relevance often goes together with path dependence reasoning. It is much more important for astrophysics and microeconomics than for nanotechnologists. On the other hand application options are relevant only for nanotechnologists. More than a third of them reported that projects are strategically fitted to external funding programs. This was reported by a fifth of astrophysics groups and only by one in eight microeconomics groups.

Table 7 Origins of research projects and of networks

	Astrophysics			Nanotechnology			Microeconomics		
	Mean	Std	*n*	Mean	Std	*n*	Mean	Std	*n*
% Projects: path dependent	0.84	0.28	25	0.52	0.51	25	0.76	0.44	25
% Projects: scientific relevance	0.32	0.48	25	0.04	0.20	25	0.28	0.46	25
% Projects: application relevance	0.00	0.00	25	0.40	0.50	25	0.04	0.20	25
% Projects: external incentives	0.20	0.41	25	0.36	0.49	25	0.12	0.33	25
% NW: path dependent	0.92	0.28	25	0.72	0.46	25	0.96	0.20	25
% NW: strategic open choice of partners	0.68	0.46	25	0.80	0.41	25	0.16	0.37	25
% NW: effect of external incentives	0.24	0.44	25	0.20	0.41	25	0.08	0.28	25

The differences between the fields concerning the reasoning on the emergence of networks are quite similar. Path dependence is less important for nanotechnology groups. They are more prone than astrophysics groups to choose new partners with a strategic and open perspective. Microeconomics groups make hardly use of an open strategic choice of new partners. They also refer very seldom to external incentives for the establishment of their networks. External incentives for the establishment of their networks do have some effect in both natural science fields.

There is a striking difference in the role of external funds for the research opportunities between the fields. External resources were not a subject at all in many interviews in microeconomics. Obviously the much lower infrastructural and personnel requirements of microeconomic research make external resources and group size not a problem for this field. On the other hand, for more than two thirds of the natural science groups external funding is a condition sine qua non for doing research.

Networks are a strategic asset for the research capacity of most of the natural science groups. Instead, microeconomics has a lower but more divided view on networks. The function of networks is to provide for complementary resources, skills and knowledge. Critical mass reasoning is less important, most common in nanotechnology. Specialization is strongest in astrophysics. More than half of astrophysics groups specialize in both, subject and methods. Modular arrangements of projects or conduction of several unconnected projects are the most common strategies for nanotechnologists. Microeconomics seems to be divided between the two ends of the scale. A large group specializes in subject and methods, and another one conducts diverse unconnected projects.

Table 8 Relevance of external resources, networks and types of specialization

	Astrophysics			Nanotechnology			Microeconomics		
	Mean	Std	*n*	Mean	Std	*n*	Mean	Std	*n*
% research capacity depends on external resources	0.72	0.48	25	0.68	0.48	25	0.12	0.33	25
% external resources provide nice adds	0.28	0.58	25	0.16	0.37	25	0.16	0.37	25
% smallness of group is a problem	0.16	0.37	25	0.08	0.28	25	0.00	0.00	25
NW importance*	2.72	0.46	25	2.88	0.33	25	2.24	0.83	25
% NW function: complementary resources	0.84	0.37	25	1.00	0.00	25	0.76	0.44	25
% NW function: assembling critical mass	0.12	0.33	25	0.16	0.37	25	0.08	0.28	25
% low specialization, heterogenous projects	0.12	0.33	25	0.24	0.44	25	0.40	0.50	25
% modular arrangement of projects and capacities	0.20	0.41	25	0.36	0.49	25	0.08	0.28	25
% specialization in methods	0.16	0.37	25	0.20	0.41	25	0.36	0.49	25
% specialization in subject and method	0.52	0.51	25	0.20	0.41	25	0.24	0.44	25

* Importance of networks: 1 = low, 3 = essential

4 Evaluation of hypotheses

4.1 Bivariate correlation analysis

As an indicator of performance the self reports on the number of international papers in refereed journals are used here. Correlation with the bibliometric indicator is $r = 0.897$ ($p = 0.000$). Since a split of the data for a discipline specific analysis is not possible, in the bivariate analysis the dependent variable is transformed to z-values using discipline specific means and standard deviations.

Despite small sample size, some hypotheses from chapter 2.5 can be corroborated by the data (see table 9). The size of the network – the overall gross size ($r = 0.40$, $p < 0.01$) and the size of the structurally described network ($r = 0.34$, $p < 0.01$) – correlates strongly with discipline specific performance (H1). Some of the central variables of the mode-2 thesis, the percentage of industry

Table 9 Correlation matrix: Bivariate analysis and regression variables

		Bivariate pearson correlations							
		1	2	3	4	5	6	7	8
1	Ln (paper intern. journals)								
2	Z-transformation per discipline	.65***							
3	# researchers	.59***	.58***						
4	% externally funded research	.31**	– .20						
5	% applied research	– .29**	– .08	– .08					
6	# of disciplines	.24**	.23**	.35**	.22*				
7	Size of gross network	.50***	.40***	.69***	.21*	– .13	.42***		
8	Size of described network	.48**	.34***	.33**	.26**	– .03	.35**	.65***	.13
9	% industry ties	.25**	.26**	.38**	.12	.30*	.46**	.30***	.30***
10	(% industry ties)^2	.22*	.22*	.32**	.09	.27*	.43**	.31***	.13
11	% international ties	.14	0.08	– .16	– .02	– .44**	– .09	– .00	.22
12	Density of network	.03	.05	.24*	– .04	– .25*	– .04	0.18	– .41**
13	Strength of ties	.06	.14	.10	– .06	.02	– .21	– .10	– .00
14	Duration of ties	.04	– .06	.14	.06	– .25	– .04	.23*	.16
15	Importance of networks	.38***	.18	.20	.49***	– .01	.27*	.25**	.34**
16	NW origin: path dependent	– .24**	– .18*	– .21*	– .17	– .21*	– .07	– .32***	– .20*
17	NW origin: open choice of partners	.38***	.08	.31**	.51***	.06	.11	.22*	.30**
18	% low specialization	– .07	.09	.11	.10	.10	.14	– .08	– .22
19	% specialization subject & methods	.11	.09	– .04	– .16	– .16	.05	.14	.16
20	field 1 astrophysics	.38***	.00	.08	.19	– .42**	– .30**	.08	.07
21	field 2 nanotechnology	.30***	.00	.22	.43***	.14	.42**	.24**	.27*
22	Non-university group	.44***	.37***	.31***	– 0.18	– 0.12	0.17	.32***	.24**

* p = 0.10 2-sided
** p = 0.05 2-sided
*** p = 0.01 2-sided
n pairwise between 72 and 76, except duration of ties: $n = 62$

ties (H4: $r = 0.26$, $p < 0.05$) and the number of disciplines in the group ($r = 0.23$, $p < 0.05$) are positively correlated to performance. However, a positive effect of the amount of applied research cannot be corroborated (H3: $r = 0.08$).

Concerning indicators of network structure, the only significant effect is the percentage of industry ties (see above, H4). Neither density of networks nor strength or duration of ties displays a significant relation to performance. This sheds some doubts on a purely structural approach to networks (chapter 2.2) and underlines the necessity to look at the learning and network strategies of the actors (chapter 2.3).

Concerning the qualitative aspect of network strategies (H7) we find a relevant negative correlation of a path dependent "non-strategy" in network formation with performance (H7: $r = -0.18$, $p < 0.10$), but no effect of a strategic open choice of network partners or of young ties. Albeit open partner choice does correlate with the size of collaboration networks (gross

Table 9 (cont.)

Bivariate pearson correlations												
9	10	11	12	13	14	15	16	17	18	19	20	21
.97**												
– .28*	– .25*											
– .02	.02	– .05										
– .17	– .17	.17	– .19									
– .18	– .14	.18	.30*	– .02								
.11	.11	.12	.06	– .03	– .02							
– .13	.29**	– .13	– .05	.04	– .01	– .18						
– .06	– .14	– .04	– .01	.04	.02	.38**	– .20*					
.26*	.22	– .25*	.19	– .02	– .23	– .23	– .13	– .09				
– .19	– .11	.08	– .08	– .16	.29*	.06	.27***	.15	– .33**			
– .30**	– .25*	.38**	.01	.07	.27*	.12	.11	.19	– .22	.30**		
.39**	.35**	– .22	– .03	– .13	– .21	.30**	– .31***	.36**	– .02	– .18	– .51**	
.09	.12	.18	– .02	.03	.06	.15	– .15	.26**	– 0.11	.28**	.25**	0.08

networks: $r = 0.22$, $p < 0.10$; detailed networks: $r = 0.30$, $p < 0.05$). On the other hand, a path dependent choice is correlated negatively with both measures of network size. Strategic network behavior in turn is correlated positively with the size of a group ($r = 0.31$, $p < 0.05$) and the amount of externally funded research ($r = 0.51$, $p < 0.05$).

Concerning hypotheses 8 to 10, they are only partly corroborated by bivariate analysis. Neither specialization nor heterogeneity of project strategy has a significant effect on performance (H8). Specialization leads to larger networks, but the effect is not significant. We observe a relevant negative correlation between the percentage of applied projects and specialization ($r = -0.16$), albeit again not significant. A heterogeneous project strategy leads to a larger amount of industry ties ($r = 0.26$, $p < 0.10$) and to smaller networks (H9: $r = -0.22$, $p < 0.10$).

Of greater statistical relevance are the hypotheses on potential advantages of groups from their embeddedness into larger established non-university research organizations. Non-university groups tend to be significantly larger (23.78 vs. 9.41). Most of them come from astrophysics, only few from micro-economics. Non-university groups perform significantly better (H13: $r = 0.37$, $p = 0.001$). They command larger networks (H11: $r = 0.32$, $p = 0.005$) and they are more often specialized than university groups (H11: $r = 0.28$, $p = 0.014$). At the same time they are less dependent on external funding ($r = -0.18$, $p = 0.124$).

As a preliminary conclusion, I hold that networks and particularly network size have a strong effect on scientific performance. Heterogeneity of networks, particularly industry ties, can have a positive effect on performance. However, applied research and a heterogeneous research strategy do not have positive effects on performance. A strategic attitude to networks in contrast to a path dependent attitude has a positive effect on the size of networks. Strategic network behavior leads to and/ or tends to be supported by group size and larger amounts of external research money. It comes at some surprise that specialization is not correlated with performance. The options for specialization seem to be better in the context of non-university research organizations. Groups in these contexts perform significantly better. They are larger and can attract larger networks. At the same time, they are less dependent on external funding. Focused funding is acknowledged as relevant for project choice in the two natural science fields, mostly from university groups. Whether such resource constraints indeed do lead to less specialization of university groups is a research question that needs further attention.

4.2 Preliminary regression models

Small numbers and skewed distributions such as performance data pose severe problems for a thorough regression analysis. The current data file therefore will be enlarged soon in order to come to better founded conclusions. The large differences between the disciplines and their production logics ask for a discipline specific analysis, which unfortunately is impossible with the small data set available now.

What is presented here is a linear regression analysis on the dependent variable self reported number of papers in international refereed journals. For reasons of improved fit to assumptions on the distribution of residuals, the numbers have been transformed to their natural logarithm. Two dummy variables are introduced to represent the field differences. I start from a baseline model with the two field variables and the number of researchers. Packages of variables concerning the hypotheses from the four theory strands (see chapter 2) are introduced then.

In table 10 (see p. 226), beta coefficients (left column) and respective p-values (right column) are reported (see table 9 for a correlation matrix). Because of the small case numbers, strong significance cannot be expected even for sizable beta coefficients.

Model 1 is the baseline model. The differences between the fields are highly significant. Performance compared to the baseline field (microeconomics) is higher for astrophysics (*beta* = 0.613) and nanotechnology (0.529). Difference in group size (# of researchers, *beta* = 0.430) also accounts for a sizable part of the adjusted explained variance of 0.627.

Some of the strong effects of size and fields can be captured by other variables. However, given the high percentage of explained variance reached by the basic model, R^2 increases only very modestly. Model 2 tests for the effects of the mode-2 variables. The number of disciplines is not a relevant factor, but the percentage of industry ties and the percentage of applied research seem to have some effect, despite a lack of significance. As on the bivariate analysis, the percentage of industry ties increases the productivity of the group (*beta* = 0.125) as long as it only maintains a small work time budget to applied research (*beta* = −0.135). This could imply that industry ties are only scientifically productive if the group keeps a strong footing in basic research.

Model 3 introduces the structural network variables (H4 to H6). This is the model with the highest percentage of explained variance (R^2 adjusted = 0.672, p = 0.000). Field differences seem partly to be captured by network variables such as size of network (*beta* = 0.17, p = 0.102), the percentage of industry ties (*beta* = 0.201, p = 0.552) and of international ties (*beta* = 0.128, p = 0.161). Since the quadratic term of industry ties is (not significantly) negative, the question whether there is a curvilinear relation to performance needs further

Table 10 Regressions analysis: Dependent variable = Natural logarithm of self reported number of publications in international journals (OLS regression)

Regression Analysis: Dependent variable LN (# papers international journals)								
	Model 1		Model 2		Model 3		Model 4	
	$n = 72$		$n = 68$		$n = 59$		$n = 68$	
constant		0.000		0.000		0.841		0.393
# researchers	0.430	0.000	0.393	0.000	0.408	0.000	0.370	0.000
% applied research			−0.135	0.130				
# of disciplines			0.046	0.619				
Size of network					0.170	0.102	0.246	0.006
% industry ties			0.125	0.194	0.201	0.556	0.080	0.411
(% industry ties)**2					−0.100	0.759		
% international ties					0.128	0.161		
Density of network					0.046	0.643		
Strength of ties					0.057	0.499		
Duration of ties					−0.069	0.434		
Importance of networks							0.056	0.510
NW strategic open choice of partners							−0.090	0.353
% low specialization							0.047	0.588
% specialization subject & methods							0.046	0.576
field 1 astrophysics	0.613	0.000	0.590	0.000	0.498	0.000	0.597	0.000
field 2 nanotechnology	0.529	0.000	0.439	0.000	0.394	0.000	0.405	0.001
R^2	0.643		0.655		0.729		0.696	
R^2 adjusted	0.627		0.621		0.672		0.648	
p	0.000		0.000		0.000		0.000	

observation. Structural variables (density, tie strength, and duration) are again irrelevant.

Model 4 retains the variables size of network and industry ties from model 3 and adds some variables on network and research strategies of the groups. The model explains 69,6% of the variance, R^2 adjusted is 0.648. Size of network is the only variable with a significant effect. Obviously some of the group size's effect is taken up by it. Thus, while small groups might need networks the most, it is the larger groups who command large networks. Strategic network orientation and importance of networks, which are strongly correlated with performance as well, display no effect when introduced together with network size and size of research group.

In order to be aware of the drawbacks of a joint analysis of the fields, a comparison of the correlations of the two types of performance variable, the discipline specific transformation and the logarithmic transformation, is of some help (see table 9 p. 222/223). There are some striking differences: the amount of external funding changes sign when we regard the discipline specific standardization. The effect of network size stays positive and significant, but is reduced. The negative effect of a path dependent network strategy holds for both indicators. However, indicators of networks' strategic importance and strategic network building loose much of their positive effect. This could mean that research resources such as external research money and large networks take on a very different role in the three fields studied here. Further analysis on the basis of a larger data set and longitudinal data will have to disentangle these effects in detail.

5 *Conclusion*

The preliminary analysis presented here makes clear that networks are an important factor in scientific performance. In particular, the size of networks has a relevant and significant effect on performance. On the basis of this small and heterogeneous sample, a relevant effect of structural network attributes, which are focused by the different strands of social capital theory, could not be found. Instead, an actor oriented approach focusing on the network strategies of a research group seems to be promising. A path-dependent "non-strategy" in network formation not only prevents groups from attracting sizable networks but also has a negative effect on scientific performance. Small groups have a tendency to build their networks in a path dependent way, while larger groups tend to choose a more strategic view on networks. What is cause and effect here is hard to say from cross-sectional data. Follow-up interviews are planned to disentangle these relations.

While networks are important, they seem to have quite a different role in the subfields studied. They may have a strong role in attracting external research money, which can be used to enlarge one's group. But there are disciplines such as microeconomics which are not really dependent on external money and large group sizes. A disciplinary split of model 1 – despite the very small numbers – yielded no effect of group size on performance in microeconomics, but strong significant effects in the natural science fields. A thorough analysis of the economies of group size, network size and amount of external funding is scheduled for the next year when the larger data sets for the fields are available.

As for the mode-2 thesis, it can be stated that nanotechnology indeed displays some of the mode-2 attributes. But except for the stronger orientation towards applied projects the differences to the other fields are not really striking. Nanotechnology groups have on average 10% industry ties, but still 90% academic ties in their networks. Concerning the relation to performance, the picture is still ambivalent. A large amount of applied research and heterogeneous projects seem to hamper research performance, while industry ties as such have a positive effect. Here we need to follow up the question of whether there is an upper threshold of the amount of industry ties, which might lead to an inverted u-shape of its effect on performance.

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Networking in Science and Policy Interventions

Comment by

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Dorothea Jansen's paper deals with the influence of networking and social capital on the production of knowledge. Jansen investigates this topic for Germany, where reforms of the mainly publicly financed science sector promote network building among scientists. The major questions are related to the effects of ego-centred networks of scientists and the output scientists produce (chiefly publications). The study is part of a larger research program on scientific networks in Germany. The focus is on three disciplines, i.e., astrophysics, nanotechnology and microeconomics. The paper combines sociological theory and empirical data, addressing topics that are of relevance because the results may allow an evaluation of some of the reforms in the German scientific system.

The paper consists of two parts. In the first part, theories about the role of networks in science together with the research hypotheses are outlined. In the second part, Jansen presents preliminary results from an investigation of ego-centred networks. Data include 25 research groups from each of the three fields. Jansen tries to answer two questions. The first focuses on the effects of heterogeneous networks on output and the second addresses the effects of an applied research strategy on output. She analyzes existing networks with the standard and well established sociological toolkit and tests several hypotheses that may explain positive network effects.

The prevalent assumptions throughout the paper suggest that networking (and social capital) generally yields positive effects. The idea is that network structures have inherent resources; if individuals collaborate, positive network effects come into existence. These may then foster the production and the spread of knowledge. The idea of the positive network effects has been taken up by politics. Politicians and research funding institutions nowadays implement reforms which favor a high degree of networking (e.g., the Sixth Framework Programme of the EU). One consequence is that resources are redistributed in favor of scientists who are active in networking. It seems that politicians tend to think that networks can cure the shortcomings in the German science system. Thus, the well-known statement of Portes (1998, 2) that "social capital has evolved into something of a cure-all for the maladies affecting societies" seems to apply also for network approaches.

For this reason, I deal with an aspect left out in the paper, the influence of policy reforms on scientists' decisions and science output. I assume that individuals are 'embedded', i.e., institutions matter, and that they behave rationally, i.e., they adjust their behavior to changing incentives. I start with an environment for scientists where no externally-set incentives for networking exist. After that I consider the political interventions in this environment. Within these scenarios, I point out some aspects that could be of interest in the context of Jansen's research.[1]

Let us imagine an environment for scientists in which there is no external intervention that promotes or punishes collaboration in research. Let us further assume that scientists behave rationally by publishing in recognized journals. The more they publish, the better. For the production of publications, they rely on inputs from other scientists. For instance, scientists use published articles from other scientists to develop their own research ideas. However, using published articles or patented ideas is not cost-free. Scientists can reduce transaction costs if they decide to collaborate with each other in the exchange of knowledge so that information can flow faster. Therefore, collaborating units – we may also call them networks – come into existence. Moreover, each scientist within a network can specialize.[2] The gains from such a division of labor are shared among network members (see, e.g., Beaver 2001) and a scientist participates in the collaboration as long as individual gains are positive. In this kind of "self-organising networks" (Wagner and Leydesdorff 2005, 1608), the exchange of knowledge is faster and cheaper than between networks. Transaction costs are low because low entry and exit barriers exist. Presumably such networks have a rather informal character. Assuming that scientists are free to choose with whom and with what intensity to collaborate, it can be argued that for scientists networks are a means of achieving individual objectives.[3] Therefore, networks will have the structure, the density of ties and the size that suit their members best. Obviously, networks prove to be rather heterogeneous with respect to these variables, as the data from different networks and research fields show.

Now let us assume that the environment changes through a policy intervention which rewards scientists who work in networks. I assume further that the total budget for science remains constant. Thus, the policy intervention leads to a redistribution of public resources in favor of those scientists who work in networks (or claim to do so). This change in the institutional setting provides incentives to react to. In order to look at the effects, I distinguish two types of scientists.

The first type does not engage in networking before the policy changes. These scientists now may need to become members of networks otherwise

[1] This comment can be understood as a contribution to the ongoing discussion among sociologists and economists on networks. For a recent debate, see Rauch and Casella (2001) and Zuckerman (2003); for a criticism of social capital and network concepts in a specific context see Egbert (2006).

[2] Cf. Walstad (2002, 14–15) for specialization and exchange in science.

[3] Cf. Melin (2000, 34) for reasons to collaborate.

they lose resources. In extreme cases, collaboration is not anymore a question of whether it is useful for the production of output; it is a precondition for receiving funds. Scientists of this type have to devote more time to networking activities. They may either try to enter existing networks or decide to set up new ones. In both cases, individual efforts are spent on network building instead of the production of output. Consequently, the policy intervention is most likely to have negative effects on output.

The second type is actively engaged in collaborations before the policy intervention. One effect of the new incentive could be that collaborations become more formalized because of two reasons. First, other scientists wish to enter the networks in order to get part of the available budget. For those scientists who are already in the network, it could be reasonable to set up entry barriers for newcomers. Second, funding is related to formality; it is difficult for informal networks to receive funds. With formalization, transaction costs rise.[4] This may cause efficiency losses as compared to a situation of informal collaboration. Another effect is that an increase in the network size also increases the probability of conflicts among members, thus leading to efficiency losses. It is not at all clear whether a policy intervention which promotes networking increases the production of knowledge. There are plausible reasons to believe that the opposite effect is possible.[5]

To sum up, collaboration among scientists evolves naturally. Networks show different structures reflecting the aims of the participants. As long as these collaborations are voluntary, they can be considered as efficient arrangements for the production of scientific output. If the incentives for collaboration change, then the structures of networks (size, density, structure, etc.) change as well. Networks that were efficient before the policy intervention are unlikely to be efficient after the policy intervention. For this reason, it cannot be argued that the observation of a correlation between network variables and output measures provides a justification for a policy that influences these variables. Such a policy can only be justified if it can be shown that, without it, network formation is inefficient.

Jansen certainly contributes to the knowledge on networks in the German science sector by describing particular networks. Her case study provides detailed information on ego-centred network structures. However, it is difficult to understand how descriptions of existing networks will help to evaluate policy interventions in science. Networks are constantly formed and reshaped by individual decisions. If one aims at an evaluation of policy reforms that favor networking in science, one needs a theory on network formation. Since Jansen does not refer to such a theory, important questions remain unan-

[4] The transaction costs of formal networks may even be so high that scientists decide not to collaborate.

[5] See also Cowan and Jonard (2003) for negative effects of growing networks.

swered. To mention but some of the prominent ones: What are the general patterns (according to the optimal size and optimal density) of efficient networks? How do size, density, and structure of networks fluctuate when the resources available for the network increase or decrease?[6] What is the optimal distribution of resources within a network? A more thorough insight into the matter would make it necessary to combine the descriptive material presented by Jansen with a theory that allows predictions about individual behavior upon changes in the environment.

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[6] Cf. the attempt of Cesaroni and Gambardella (2003) in this respect.

A Beauty Contest of Referee Processes of Economics Journals

by

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This paper is a concise report on an internet survey of economists to ascertain their satisfaction with peer review processes of publishing in economics journals. Other problems of peer review processes are not addressed in this paper. Instead we refer to the starred footnote.

1 The Survey

At the end of 2001 and at the beginning of 2002 we addressed some twenty thousands persons twice by e-mail asking them for their online responses to seven questions concerning their experience with referee processes of economics journals. Some 6,000 persons in total had a look at our questionnaire, but only a bit more than 10% of these individuals started to respond to it.

As the professional institutions in the Anglo-Saxon world did not even respond to our inquiry, let alone did offer their cooperation, we used as many sources of mail addresses as possible in the hope of capturing many economists. We had the mail addresses of the members of the *European Economic Association, Verein für Socialpolitik, Economics Bulletin, IZA (Forschungsinstitut zur Zukunft der Arbeit – Institute for the Study of Labor)*, and *Inomics*. Hence, we could rely on some 4,500 academic economists. Many other addresses were those of professional people who had never published. Thus, an overall response rate of 3% may seem to be small, but it increases to some 13% if we count the "certain" academic economists only.

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2 The journals

In our survey we used two groups of journals. We call them *invited* and *contributed* journals, respectively. The invited journals centred on the famous Diamond (1989) list (see Table 1 in EEA), which comprises 27 economics journals. We enlarged this list by 49 journals taken from the A and B categories of the VSNU (Vereniging van Samenwerkende Nederlandse Universiteiten – Association of Universities in the Netherlands) economics journals ranking list, which we considered as important enough to be included. This makes 76 invited journals. Furthermore, we solicited our respondents for contributing journals to their short list at their own discretion.

This procedure yielded a total of 359 journals. Space does not permit to include a list of all journals in this article.[1] However, the structure of responses shows that our choice of the 76 invited journals provided a good match with respondents' experience: Among the 73 journals showing at least 10 responses to Question 1, only four were not listed among the invited journals. All ten journals which attracted one hundred or more responses are also members of the Diamond list. All journals of the Diamond list, with the exception of the *Brooking Papers on Economic Activity,* elicited at least 11 responses to the first question. Moreover, the responses show that the Diamond list ignores some renowned (mostly non-American) journals, which existed well before 1989.

For the presentation of the results of our study, the data were broken down as follows: For the analysis of relationships between respondents' attitudes, we employed *all* data irrespective of how many responses per journal we had. For descriptive documentations with respects to particular journals we arbitrarily settled on at least *five valid responses* for the respective journals. To report on journals with less than five valid responses would probably convey a distorted picture. As some respondents had chosen to drop out during the survey, the set of journals decreases somewhat for later posed questions. For Questions 1 and 2, 110 journals had at least five valid responses, for Question 3 we had 107 journals, and for Questions 4–7 we had 106 journals.

For the purpose of this paper we prepared, moreover, a concise summary documentation of subjects' responses. We narrowed down the set of papers to a cut-off benchmark of at least *twenty valid responses* to Question 7 (the last

[1] The list of all 359 journals can be downloaded from our homepage http://www.wiso.uni-kiel.de/vwlinstitute/ifs/chair/peerreview.php as Table 1*. It contains all journals for which the first question was answered (respondents could answer subsequent questions only by passing Question 1 first). In this table, the invited journals are marked with an *asterisk,* the journals of the Diamond list among them with a *diamond,* and the contributed journals are *unmarked.*

question).[2] 51 journals satisfy this condition. Table 1 (see p. 241) shows them ordered according to the ranking of the journals with respect to Question 7 asking for subjects' general satisfaction with the journals' referee processes. All rank numbers are, however, taken from the more comprehensive tables.

3 Respondents

A survey of researchers' experience with referee processes can follow several routes. One possibility is to address successful authors who managed to get their papers published in a journal. The other way is to address economists at large and thus collect also the experience of the less lucky ones which is, however, crucial for a valid picture of the performance of referee processes. Although this approach might be in danger of attracting mainly frustrated authors who wish to take revenge on allegedly unfair refereeing, we shall demonstrate below that our data do not suffer from such biases. Rather they are biased in the opposite direction.

Taking the second route, we asked several institutions for e-mail addresses of economists. We received help from the *European Economic Association,* the *Verein für Socialpolitik,* the editorial board of the *Economics Bulletin,* from *IZA* and from *Inomics*. Our faculty colleagues Sönke Albers and Joachim Wolf also provided good advice. Some e-mail addresses of economists were collected by us. Several institutions, mostly from the Anglo-Saxon world, did not even reply to our inquiries, let alone offer us their cooperation. These were, in particular, *The American Economic Association, The Econometric Society,* and *The Royal Economic Society,* as well as some less well-known Asian economic associations. This refusal of cooperation implies that American, Asian and Pacific economists are unfortunately underrepresented in our survey (see Table 2 in EEA). We had only the choice to work with the data available or dispensing with our endeavour at all. We decided to continue our analysis.

The data of all respondents underwent a plausibility test. This led to the elimination of the data of 9 respondents, representing their joint responses to 22 journals, for various reasons.[3] As these were typing errors, jokes, or attempts at manipulation,[4] the data of these subjects had to be eliminated.

[2] The documentation of the results based on a benchmark of at least five valid responses can be downloaded from our homepage http://www.wiso.uni-kiel.de/vwlinstitute/ifs/chair/peerreview.php as Tables 3* – 8*.

[3] The elimination criteria were: Response time exceeding 250 weeks (3 respondents), having received more than 10 referee reports (5 respondents), and having received referee reports without having submitted a paper (1 respondent).

[4] We checked also computer IP addresses for similar evaluations, but did not observe suspicious similarities of responses in the cases of multiple uses of the same computers.

Concerning the descriptive results for the particular journals (at least five usable responses), we were left with 630 respondents to the first question, of which 551 participated in the survey through to the seventh question. In the aggregate[5], we could dispose of between 4538 (for Question 1) and 3791 data per question (for the entries to Questions 2 – 7 cf. the number of entries in Table 4, see p. 248).

4 *Reactions*

In addition to responses to our questions, many subjects sent us comments and suggestions. The tenor of their reactions was helpful, sympathetic, or critical.

Numerous sympathetic, some of them even enthusiastic, reactions came from all strata of respondents. Many commentators argued that we should have posed more and more detailed questions. Yet, it is true that we started originally with a far more comprehensive list of questions, but decided to confine the questionnaire to but seven questions for fear of too many drop-outs. Our experience with this survey showed ample evidence that we were right in doing so: Only about eight per cent out of all persons originally interested in our survey embarked on responding to all questions. Other sympathetic scholars took the occasion of our survey to broach their own uneasiness with the current referee situation.

Critical comments were received from only a few prominent economists. For instance, a renowned economist urged us: “Please stop sending me reminders about this research. Such research projects are dangerous and misleading!” Comments like this suggest that research directed at referee processes of learned journals seems not to be favored among some of the profession’s most prestigious scholars.

5 *The questionnaire*

When a responded connected to our server, (s)he was first presented with a general plea to participate in the survey. Then the respondent was shown a list of our 76 invited journals and asked to select those journals with which (s)he had experience as an author. Furthermore, the respondent was prompted to add further economics journals of his or her choice. Both sets together formed the particular respondent’s journal set.

Then the respondent was asked the first question and asked to respond to subsequent items for the selected journals. For the Questions 1, 2, 3, and 7, (s)he was urged to respond to the respective question for *all* journals in his or

[5] Counting all journals irrespective of how many responses we had per journal.

her set. As to the first three questions (s)he could proceed only after the respective question had been answered for all journals in the set.[6] Questions 4 – 6 could only be answered if the respondent had actually received a referee report. As some journals reject manuscripts without having solicited referee reports, a respondent may not have had experience with referee reports of all journals to which (s)he had submitted manuscripts. Therefore, for Questions 4 – 6 we allowed for passing to the next question without having responded to the respective question for all journals. Moreover, during the response process, a respondent could also opt to eliminate some journals altogether from his or her journal set. While the questions answered up to this point were kept in our data, these journals were then dropped for the subsequent questions. This device was intended to encourage respondents to complete the questionnaire even if s(he) realized that (s)he had initially proposed a larger set of journals than s(he) was able or willing to evaluate.[7]

The questionnaire consisted of the following seven questions per journal.[8]

Question 1: "After submission of your paper, how long did it take on average to get a reply other than just a confirmation that your paper had been received?"[9]
Question 2: "How many referee reports did you receive on average?"
Question 3: "How many papers did you submit to this journal and how many papers were accepted?"
Question 4: "Were the referee reports competent?"
Question 5: "Did the decision of the editor match the referee report?"
Question 6: "Were the referee reports carefully done?"
Question 7: "How was your overall satisfaction with the procedure of paper submission to the respective journal?"

Note that the responses to Questions 1 – 3 are numbers such as the spell to get a first reply, numbers of referee reports received, and numbers of papers submitted and accepted. In contrast to that, the responses to Questions 4 – 7 result from mouse click to one out of seven fields on Likert scales. In our results, the worst value is coded with a 0, and the best with a 6, so that 3 forms the mean coded value of each Likert scale if all values were clicked with equal frequency. Notice, of course, that data from Likert scales are necessarily subjective data. An economist told us that he did not participate in our survey because he would have to have consulted all his files. However, authors of manuscripts usually decide to send a manuscript to a journal according to the perceptions in their memory without having consulted their files first. Mimicking this behaviour, we were interested in the immediate opinions of our

[6] This method warrants that subjects could concentrate on meaningful comparisons among the journals of their set for the same aspect of evaluation.

[7] The elimination of journals was easily accomplished. The respondent had only to erase a little hook after the journal.

[8] The screenshots of all seven questions can be downloaded from our homepage http://www.wiso.uni-kiel.de/vwlinstitute/ifs/chair/peerreview.php.

[9] In the respective cell subjects were asked to indicate the response time in weeks.

respondents. Using Likert scales is the proper way to capture their perceptions.

6 Results

To have some kind of measuring rod for comparisons, we often calibrate our results against those journals which are commonly regarded as being the core economics journals. Although there exist several categorizations (e.g., Burton and Phimister 1995), we decided to stick to the well-known Diamond (1989) list.

6.1 Descriptive results

Recall that, for the particular journals, we confined our attention to those journals which commanded at least five valid responses. This reduced the set of journals given attention to a domain of 106 to 110 journals. The journals were ranked according to the given responses, where equal responses led to equal ranks. We used the following ranking criteria:

Question 1: Shorter response times.
Question 2: Greater number of referee reports.
Question 3: Higher individual acceptance rates (papers accepted/papers submitted).
Question 4: Higher competence of referee reports.
Question 5: Higher matching of editorial decision and referee reports.
Question 6: Higher carefulness of referee reports.
Question 7: Higher overall satisfaction with referee process.

Complete data for the present study are presented in tables which can be downloaded as Tables 3* – 9* from http://www.wiso.uni-kiel.de/vwlinstitute/ifs/chair/peerreview.php. For the concise summary documentation of subjects' responses presented in Table 1 of this paper, we employed a cut-off benchmark of at least *twenty* valid responses to Question 7. 51 journals satisfied this condition. In order to provide background information, we indicated in Table 1 the ranks R of the more comprehensive tables. As to Table 1, we had to settle on one ordering criterion; we used overall satisfaction (Question 7). As overall satisfaction is the most important characteristic, we arranged the columns of Table 1 in reverse order of the presentation of questions. The findings of our paper rest, however, on the more comprehensive results.

Table 2 (see p. 243) provides a concise summary of *descriptive results.* With respect to *response time*, the *Quarterly Journal of Economics* stands out as the speediest one with a mean turn around time of 0.613 weeks, that is, 4.29 days. However, given its high subjective rejection rate of 93%, this means that the managing editor(s) of the *Quarterly Journal of Economics* reject(s) many of

Table 1 Journal Satisfaction Ranking (At least 20 valid responses)

Journal	n	Satisfaction (7)			Carefulness (6)			Match (5)			Competence (4)			Acceptance Rate (3)			Number (2)			Spell (1)		
		R	∅	STD	R	∅	STD	R	∅	STD	R	∅	STD	R	∅	STD	R	∅	STD	R	∅	STD
Finanzarchiv	38	6	4.53	1.43	20	4.16	1.39	28	4.97	1.20	29	3.82	1.43	16	0.73	0.38	57	1.63	0.49	14	13.26	7.26
Math. Soc. Sc.	30	8	4.47	1.81	12	4.35	1.52	25	5.03	1.55	9	4.39	1.50	33	0.62	0.45	50	1.69	0.73	39	17.56	11.18
J. Economics/ZfN	70	12	4.33	1.67	25	4.09	1.41	21	5.12	1.38	32	3.76	1.47	27	0.66	0.43	30	1.88	0.64	20	14.58	11.58
J. Math. Economics ◆	24	13	4.25	1.76	11	4.36	1.53	7	5.39	1.31	15	4.26	1.36	26	0.87	0.33	61	1.59	0.83	97	29.07	29.70
Scott. J. Polit. Ec.	20	15	4.20	1.91	51	3.62	1.91	34	4.91	1.64	67	3.23	1.80	53	0.47	0.50	67	1.48	0.65	32	16.33	11.49
Soc. Choice Welfare	32	16	4.19	1.71	14	4.29	1.58	16	5.22	1.18	10	4.36	1.32	17	0.72	0.41	34	1.82	0.71	80	23.71	16.50
J. Inst. Theor. Ec./ZfgStW	49	17	4.18	1.50	19	4.16	1.46	33	4.94	1.28	30	3.78	1.39	28	0.66	0.44	37	1.81	0.71	36	16.76	10.20
Economic Inquiry ◆	24	19	4.13	1.90	30	4.00	1.83	29	4.96	1.75	11	4.30	1.55	24	0.68	0.39	17	2.04	0.43	45	18.57	13.37
J. Ec. Dynamics Control	33	20	4.09	2.07	31	3.97	1.82	31	4.94	1.61	24	3.94	1.59	39	0.58	0.46	16	2.08	0.88	69	22.07	18.25
J. Econometrics ◆	23	21	4.09	2.04	23	4.09	1.66	54	4.64	1.40	14	4.29	1.43	31	0.64	0.44	20	1.94	0.74	82	24.44	16.79
J. Public Ec. Theory	29	23	4.03	1.57	13	4.31	1.29	17	5.20	0.85	18	4.13	1.14	50	0.48	0.49	38	1.81	0.48	74	23.14	12.37
J. Population Economics	24	25	3.92	1.74	25	4.09	1.44	20	5.13	1.10	41	3.65	1.53	14	0.74	0.40	24	1.89	0.63	70	22.10	12.90
Ec. Theory	51	27	3.84	1.96	28	4.04	1.70	18	5.17	1.42	23	3.98	1.52	44	0.54	0.45	71	1.42	0.62	58	21.22	19.65
Eur. J. Political Ec.	76	28	3.84	1.67	45	3.68	1.61	46	4.73	1.54	47	3.56	1.62	35	0.61	0.47	25	1.89	0.60	55	20.65	17.26
Econometrica ◆	98	29	3.82	1.91	17	4.23	1.88	37	4.88	1.36	12	4.30	1.91	88	0.20	0.31	13	2.12	0.80	68	22.04	14.57
Weltwirtsch. Archiv	62	30	3.81	1.77	24	4.09	1.23	24	5.05	1.37	37	3.69	1.50	47	0.51	0.41	80	1.25	0.56	21	14.71	13.73
Canad. J. Economics ◆	37	33	3.73	1.84	36	3.81	1.47	27	5.00	1.33	50	3.51	1.43	54	0.45	0.48	44	1.74	0.55	54	20.58	11.89
Games Ec. Behav.	66	34	3.73	1.76	35	3.81	1.83	10	5.35	0.97	21	4.06	1.69	58	0.43	0.43	47	1.71	0.56	90	26.22	16.23
Management Science	31	36	3.68	2.04	40	3.71	1.70	44	4.81	1.58	43	3.64	1.73	56	0.44	0.46	7	2.36	0.88	51	20.03	17.14
Scand. J. Ecconomics	74	37	3.66	1.93	49	3.64	1.73	53	4.64	1.51	69	3.21	1.68	60	0.43	0.46	19	1.95	0.51	50	19.98	12.41
Economics Letters ◆	154	38	3.63	1.99	71	3.27	1.88	9	5.36	1.13	49	3.55	1.81	49	0.50	0.44	86	0.86	0.55	22	14.77	10.70
J. Public Ecconomics ◆	112	39	3.59	1.81	34	3.83	1.68	68	4.45	1.52	42	3.64	1.57	74	0.34	0.40	27	1.88	0.54	67	21.66	11.91
J. Economic Theory ◆	94	41	3.56	1.93	39	3.73	1.91	23	5.09	1.16	34	3.76	1.77	76	0.32	0.37	48	1.70	0.72	89	25.86	17.00
J. Development Ec. ◆	42	42	3.52	1.94	37	3.78	1.72	51	4.67	1.43	73	3.16	1.73	67	0.38	0.44	35	1.82	0.56	100	32.47	24.29
Empirical Economics	44	43	3.50	1.85	32	3.91	1.61	42	4.83	1.41	44	3.61	1.62	20	0.69	0.44	39	1.80	0.53	76	23.22	17.82
Public Choice	57	47	3.39	2.10	73	3.16	1.87	26	5.02	1.23	64	3.28	1.68	45	0.54	0.46	69	1.42	0.61	31	16.19	14.33
American Ec. Review ◆	171	48	3.37	1.83	55	3.55	1.86	65	4.49	1.61	27	3.86	1.71	91	0.17	0.33	33	1.85	0.75	66	21.64	15.83
J. Ec. Beh. Org.	67	49	3.37	1.91	61	3.51	1.86	58	4.61	1.60	60	3.39	1.64	40	0.56	0.47	60	1.60	0.73	85	25.11	19.30
Review Ec. Studies ◆	84	50	3.37	1.73	29	4.03	1.53	47	4.72	1.58	20	4.08	1.42	87	0.22	0.38	29	1.88	0.66	95	28.33	17.72
J. Industr. Ec.	46	51	3.35	1.69	27	4.04	1.38	43	4.83	1.14	40	3.66	1.40	80	0.30	0.39	32	1.86	0.59	63	21.50	16.10
J. Mon. Credit Banking	29	52	3.35	1.72	44	3.69	1.97	27	5.00	1.23	79	2.97	1.70	72	0.34	0.46	49	1.69	0.60	47	18.67	11.88

Table 1 (cont.)

Journal	n	Satisfaction (7)			Carefulness (6)			Match (5)			Competence (4)			Acceptance Rate (3)			Number (2)			Spell (1)		
		R	∅	STD	R	∅	STD	R	∅	STD	R	∅	STD	R	∅	STD	R	∅	STD	R	∅	STD
Rev. Ec. Statistics◆	71	53	3.34	1.93	54	3.55	1.82	67	4.46	1.60	31	3.78	1.56	69	0.36	0.44	45	1.73	0.70	60	21.36	13.99
International Ec. Rev. ◆	76	54	3.33	1.87	41	3.70	1.59	38	4.87	1.41	39	3.67	1.64	75	0.32	0.44	26	1.89	0.88	93	27.78	15.20
J. International Ec. ◆	60	55	3.32	1.73	57	3.54	1.58	61	4.56	1.38	52	3.48	1.55	84	0.26	0.38	21	1.91	0.48	86	25.14	13.20
Theory and Decision	26	56	3.31	1.87	48	3.64	1.60	36	4.88	1.64	55	3.44	1.58	25	0.67	0.43	72	1.41	0.73	77	23.25	19.43
Int. J. Game Theory	30	60	3.17	2.04	43	3.96	1.83	14	5.24	1.12	36	3.73	1.70	61	0.43	0.44	56	1.64	0.64	101	33.76	30.35
Oxford Ec. Papers ◆	51	62	3.10	1.96	70	3.28	1.93	74	4.31	1.80	76	3.06	1.82	78	0.31	0.43	43	1.75	0.66	62	21.48	14.00
Kyklos	53	63	3.09	2.11	56	3.55	1.74	19	5.14	1.22	54	3.44	1.55	70	0.36	0.46	85	1.11	0.76	4	10.07	8.32
J. Applied Economics	23	66	3.04	2.23	51	3.62	1.69	27	5.00	1.05	56	3.43	1.69	22	0.69	0.45	66	1.48	0.80	104	38.48	28.35
Rand J. Economics ◆	60	67	3.03	1.97	47	3.66	1.92	60	4.59	1.37	37	3.69	1.67	85	0.23	0.38	51	1.67	0.70	83	24.49	16.14
Cambridge J. Ec.	20	69	2.95	1.73	50	3.63	1.80	56	4.63	1.89	77	3.05	1.57	51	0.47	0.48	15	2.10	0.88	79	23.50	16.58
European Ec. Review ◆	187	70	2.95	1.83	69	3.32	1.73	50	4.68	1.40	65	3.26	1.56	77	0.31	0.41	28	1.88	0.61	84	25.08	16.66
Economica ◆	52	71	2.94	1.92	76	3.08	1.91	57	4.63	1.48	75	3.11	1.76	63	0.42	0.47	42	1.75	0.60	75	23.21	14.21
Southern Economic J.	22	72	2.91	2.11	77	3.05	1.96	39	4.86	1.36	78	3.00	1.95	62	0.42	0.49	64	1.56	0.64	46	18.65	14.53
Quarterly J. Economics ◆	106	73	2.90	2.05	75	3.08	2.03	79	4.86	2.02	57	3.42	1.90	94	0.07	0.21	79	1.25	0.82	1	0.61	3.70
J. Labor Economics ◆	32	74	2.88	1.90	63	3.43	1.81	55	4.63	1.65	33	3.77	1.38	90	0.19	0.35	59	1.61	0.60	88	25.79	17.66
J. Human Resources	30	75	2.87	2.11	68	3.36	1.87	78	4.08	2.20	38	3.68	1.60	64	0.42	0.49	52	1.66	0.87	48	19.29	14.06
Oxf. Bull. Ec. Statistics	29	76	2.86	2.18	53	3.57	1.83	30	4.96	1.19	68	3.22	1.76	73	0.34	0.45	83	1.19	0.86	52	20.11	11.08
Economic J. ◆	115	77	2.86	2.01	62	3.50	1.86	71	4.38	1.72	59	3.40	1.71	82	0.27	0.41	23	1.90	0.78	65	21.58	13.54
J. Monetary Economics ◆	34	80	2.79	2.03	66	3.39	2.12	32	4.94	1.41	63	3.29	1.92	81	0.29	0.41	68	1.46	0.72	102	36.65	30.81
J. Financ. Ec. ◆	28	81	2.79	1.85	58	3.54	1.62	63	4.54	1.67	66	3.24	1.70	93	0.10	0.31	73	1.40	0.55	10	12.44	8.04
J. Political Economy ◆	88	85	2.13	1.92	81	2.61	1.96	66	4.47	1.72	80	2.89	1.91	92	0.15	0.33	70	1.42	0.59	91	26.77	21.28

Table 2 Concise Summary of Descriptive Results

Item	Remarks
Response time	Median: 20.5 weeks; QJE: 0.613 weeks; 20 journals of the 26 DIAMOND journals need more than 20 weeks.
Number of referee reports	Median: 1.75; only 24 out of 110 journals provided at least 2 referee reports.
Acceptance rates	Median: 0.5; 23 DIAMOND journals below 0.5.
Competence	Median: 3.641; even distribution of DIAMOND journals; only 12 below 3.
Matching	Median: 4.826; only 7 below 4.
Carefulness	Median: 3.69; even distribution of DIAMOND journals; only 10 below 3.
Satisfaction	Median: 3.524; 36 score 4 or better, 23 worse than 3.

The median values are the medians of the mean values for the individual journals.

the submitted manuscripts without ever having consulted a single referee as to rejection or acceptance of a paper. Indeed we registered n = 91, 89, 88 responses to Questions 4, 5, 6, but n = 106 responses to Question 7 which means that several subjects did not receive a referee report at all. These values resemble those obtained for *Economics Letters*, for which we registered n = 129, 125, 123 responses to Questions 4, 5, 6, but n = 154 responses to Question 7. Given a mean response time of 14.77 weeks and a meager mean of 0.86 referee reports, this leads us to conjecture that the decision to reject a paper without having sent it to a referee takes the editor of *Economics Letters* considerable time. Moreover, we cannot exclude that some authors counted a letter from the editor only as a true referee report.

As compared to other disciplines, e.g., the natural sciences, economics journals seem to take a particularly long time to reach a decision. Hardly any journals decide in fewer than 10 weeks, and more than half of them need 20 weeks and more to take a decision. 20 journals of the Diamond list (out of the 26 remaining ones) need more than 20 weeks to make a decision.

A mean of more than two *referee reports* is the exception rather than the rule. Among the Diamond journals, only *Econometrica* and *Economic Inquiry* reach a mean number of referee reports above two. These are the only Diamond journals which rank among the first twenty ranks with respect to the mean number of referee reports. *Economics Letters* ranks last among the journals from the Diamond list (0.86 referee reports per respondent). Recall that respondents might have considered the managing editors' rejection as a valid referee report.

We used subjects' reports on total numbers accepted by and submitted to a respective journal to compute the journal's individual *acceptance rates*. The data show that the more reputed journals have lower acceptance rates, which was to be expected. Indeed, 17 journals out of the Diamond list figure among the last 25 ranks.[10] Cross-disciplinary comparisons show that manuscript rejection rates are much higher in the humanities than in the natural sciences.[11] By and large a rejection rate of more than 80% in the humanities contrasts with an acceptance rate of some 80% in the natural sciences.

Competence of the referee reports is, on the whole, judged rather favorably. Only twelve out of 106 journals were rated below 3.0 (out of a maximum 6.0), among them only two Diamond journals, viz. the *Journal of Political Economy* and the *Journal of Financial Economics*. Seven out of the journals of the Diamond list (recall that the *Brooking Papers* dropped out) score at 4.0 or better. Competence of referee reports seems to be not positively correlated with the reputation of a journal. Neither the journals of the Diamond list nor of the invited journals bunch at the upper or at the lower end; they appear to be rather evenly distributed among the ranks. For instance, 14 journals of the Diamond list rank ahead of, and 12 rank behind the mean rank of 44.

Our results show that most journals score rather well with respect to *matching* of the managing editors' decisions with the recommendations of the referee reports. However, this signals a good performance of peer review if and only if referees' judgements are valid. If they are just reliable,[12] and the managing editor decides blindly in accordance with them, this need not be a proxy for good refereeing because referee hostility or incompetence may be but insufficiently monitored by the editor.[13] Attentive editors should interfere in the latter case, which would be reflected in lower matching scores. Accompanying letters to the editors may also be harsher than the referee

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[10] However, there are some exceptions to this regularity. For instance, four journals of the Diamond list rank below 50 (out of 94 ranks).

[11] Cf., e.g., Zuckerman and Merton (1971), Lazarus (1982), Adair (1982), Hargens (1988; 1990).

[12] For a discussion of the concepts of validity and reliability see EEA.

[13] With respect to the editorial decision to accept or reject a manuscript, voices have been aired which encourage editors to use their discretionary powers wisely and – if necessary – should not shy at overriding referees' recommendations (Bailar 1991, 138). Stricker (1991, 164) disputes that good editors should behave like psychometric clerks who simply add up the scores that a manuscript gets from the referees. He argues that "good editors are not clerks. They read the manuscript, appraise the reasons reviewers give for their recommendations, and weigh all the information about it ..." He is paralleled in this view by Glenn (1982, 212), Rodman (1970, 355–356), and Goodstein (1982, 213). Crandall (1991) pleads along the same lines that editors should be super referees. He deplores that too many editors do not behave in this way. He suspects "that many editors do not even read the papers for which they are supposed to have editorial responsibility." Scarr (1982, 54), editor of *Developmental Psychology* and the *American Psychologist*, has made a case for editorial responsibility. She refers editors who shirk their duties to one of Harry Truman's wise insights: "If you can't stand the heat, get out of the kitchen." Yalow (1982, 244) blamed reviewer and editorial incompetence for instances as revealed by the Peters and Ceci (1982a, 1982b) experiment. Simon et al. (1986, 270) report that only between 13 and 19% of authors' complaints against referee reports were successful.

reports for the authors. Editorial deviation from referee recommendations may also be prompted by high backlogs of manuscripts which may goad editors' zeal to curb the growth of the queues of papers agreed to be published. Such independent decisions by editors may help explain the lower rankings of some prestigious journals, such as the *American Economic Review,* the *Journal of Political Economy,* the *Review of Economics and Statistics*, the *Journal of Public Economics,* the *Economic Journal,* and the *Quarterly Journal of Economics*. When an editor, because of space limits, is forced to reject manuscripts furnished with good referee reports, (s)he may well give in to favoritism.

Concerning *carefulness* of the referee reports, journals scores were similar to their scores on competence of the referee reports. Only 10 out of 106 journals scored less than 3, among them again the two notorious Diamond Journals, the *Journal of Political Economy* and the *Journal of Financial Economics*. Only 6 out of the 26 journals of the Diamond list (after dropping the *Brooking Papers*) scored at 4 or better than 4. As compared to competence, we observe a minor shift of the reputed journals to lower ranks: 11 journals of the Diamond list were rated above and 15 below the mean rank. Some reputed journals rank among the bottom 20 of carefully done referee reports, viz. the *European Economic Review,* the *Oxford Economic Papers, Economics Letters,* the *Quarterly Journal of Economics, Economica,* the *Journal of Political Economy,* and the *Journal of Financial Economics.*

Overall satisfaction with the whole procedure of paper submission proved as disappointing for the prestigious journals. Out of 106 journals, 36 scored at 4 or better; among them only five journals were from the Diamond list. Out of the 106 journals, 23 scored worse than 3; among them eight journals were from the Diamond list, to wit, the *European Economic Review, Economica,* the *Quarterly Journal of Economics,* the *Journal of Labor Economics,* the *Economic Journal,* the *Journal of Monetary Economics,* the *Journal of Financial Economics,* and the *Journal of Political Economy.*

6.2 Statistical Results

6.2.1 Response Biases

All data obtained from the subjects are, of course, subjective data. However, the data collected for Questions 1–3 have "objective" counterparts. In an attempt to correct for subjective biases, we sent a mail to the editors of all 110 journals for which we had received at least five valid responses to Question 1 and asked them for editorial data on the average response time to authors, the average number of referee reports solicited, and the average acceptance rate of manuscripts. Replies to these questions seemed to be easy, as editors of most journals are wont to keep regular statistics on these figures. Indeed, a few journals do even publish them (see, for instance, The Economic Journal Managing Editors' Report 2005). Editors who did not respond to our mail were sent a reminder. We received responses from the editors of 52 journals (response rate: 47.3%), among them 7 responses from the 26 Diamond journals (response rate: 26.9%). Note that we could not check whether we received the true "objective" data, biased data or mere conjectures of the editors, but they represent an independent alternative data source which allowed inferences on possible biases. For the sake of a shorthand expression we address them as the *objective* data in this paper.

Table 3 gives a concise summary of our results. Their entries are the means (taken over all journals for which we had data) of the ratios of the mean responses of the subjects and the responses of the editorial board. Table 3 shows us that the subjective response time exceeds the objective one by some 50%, that the subjective number of referee reports is slightly lower than the objective number of referee reports, and that the subjective acceptance rate exceeds the objective one by some 150%.

Table 3 Response Biases: Statistics

Subjective value divided by objective value	N	Min.	Max.	Mean	STD
Response Time	52	0.61*	4.73	1.49	0.69
No. of Reports	52	0.53	1.40	0.90	0.16
Accept. Rate	50	0.42**	8.89	2.50	1.34

All means significant at the 1% level (two-sided).
* Only 8 values smaller than 1.
** Only 4 values smaller than 1.

The most spectacular upward bias is noticed for the subjective acceptance rates. We may offer several explanations for that (all of which may have contributed to produce this result):

1. *Self-selection effect:* It seems that the more successful scholars felt more attracted by our survey.[14]

2. *Survey-selection effect:* As our survey was directed to investigate authors' experience with referee processes, we had asked subjects to respond only for those journals with which they had experienced at least one referee process. This rules out manuscript submissions which were rejected immediately by the

[14] Similar effects were observed by Sweitzer and Cullen (1994). They polled 209 authors for their satisfaction with peer review processes. 67% of the AR (accept with revision) authors, 43% of the RR (reject but may resubmit) authors, and only 30% of the RO (reject outright) authors responded to their questionnaires sent to unsolicited authors of the *Journal of Clinical Anesthesia*. Higher response rates of authors whose papers were accepted were also observed by Garfunkel et al. (1990) for the *Journal of Pediatrics.*

editors without soliciting referee reports. Of course, manuscripts which were infused into the referee process have positive chances of being accepted, whereas the crude (objective) rejection rate includes also purely editorial rejections.

3. *Cognitive-dissonance effect:* Successful events are memorized, failures are mentally suppressed.

4. *Trend effect:* True acceptance rates fell with the lapse of time. Respondents who remember their submission history of papers amalgamate past with present experience, which, due to the influence of higher past acceptance rates, biased their perception of acceptance rates upwards.

The upward bias of the response time is most probably associated with the upward bias of the acceptance rates. A well-established result says that it takes journals shorter times to reject a paper than to offer a revision of the paper.[15] This is reinforced when many papers are rejected immediately by the editors without having been infused into referee processes. Given that the more successful authors were over-represented in our survey, this implies longer spells of response time.

Note, therefore, that our data are biased in favor of the more successful authors. However, as we did not pre-select our respondents a priori from the set of the successful ones, this upward bias is certainly less than it would have been, had we addressed only people whose papers were actually accepted for publication. On the other hand, a survey such as ours is endangered of attracting frustrated respondents who wish to deal a blow to those journals which they consider to have treated them unfairly. The entries in Table 3 show that this was certainly not the case.

6.2.2 Favoritism

Favoritism can manifest itself in three ways, to wit, personal, institutional, and regional favoritism. Personal favoritism means that certain persons enjoy preferential treatment with respect to refereeing and/or editorial decisions. The literature abounds with gossip about personal favoritism, yet, in order to demonstrate its presence, one needs inside data on referee processes and editorial decisions, to which we had no access. Hence we could not study personal favoritism. Likewise, we could not study institutional favoritism because, for reasons of respondents' anonymity, we have only a regional, not an institutional breakdown of data. Yet our investigation of regional favoritism does not allow sensible results. Indeed, favoritism seems to manifest

[15] Cf., e.g., Ellison (2002, 955, Table 2); Omerod (2002) remonstrated the long response time of decision processes of the *Economic Journal:* In the year 2000, it took this journal 18 weeks to reject a paper and 28 weeks to offer a revision of a paper. See also The Economic Journal Managing Editors' Report (2005, 6, Table 4).

itself, first and foremost, as institutional favoritism, followed by personal favoritism. The Coupé data (2000a; 2000b) convey some flavor of institutional favoritism; alas, these data rest on *published* manuscripts rather than on submitted manuscripts, as asked for by Hodgson and Rothman (1999). For more information see EEA.

6.2.3 Correlation Analyses

To identify relationships among the responses to our questions, we pooled the data for *all* journals (irrespective of the number of responses per journal), and combined them into a correlation matrix, Table 4.

Table 4 Correlation Matrix of Responses to Questions

	Response Time	Number	Accept. Rate	Compe-tence	Editorial Match	Care-fulness
Number	– .021 4333					
Accept. Rate	– .029 4049	.147** 4049				
Competence	– .112** 3974	.279** 3974	.322** 3974			
Editorial Match	– .056** 3858	.032* 3858	.177** 3858	.265** 3858		
Carefulness	– .099** 3817	.276** 3817	.314** 3817	.722** 3817	.281** 3817	
Satisfaction	– .256** 3791	.199** 3791	.467** 3791	.682** 3791	.312** 3791	.684** 3791

** Significance of correlation at the 1% level (two-sided).
* Significance of correlation at the 5% level (two-sided).
The lower lines in the cells denote the number of cases.

When considering response time, we find that it is negatively correlated with all responses. Although longer response times may also be caused by more and better referee reports, the negative correlation with all responses suggests that longer response times seem to be more associated with editorial inefficiency than with more or better referee reports.

When considering the number of referee reports, we observe a moderate, but positive correlation with the acceptance rate and with the qualitative responses. The positive correlation of the number of referee reports with the acceptance rates seems to be influenced by the occurrence of manuscript rejection without referee reports. These manuscripts have no chance of being accepted. Thus, whenever referee reports are solicited, the chance of acceptance becomes greater than nought. The small correlation between the number of referee reports and the editorial match shows that referee reliability becomes a problem in case of multiple referee reports. As editors of economics journals are wont to reject a paper whenever a single one among several referee reports is somewhat critical, irrespective of how positive the other reviews are,[16] authors perceive an editorial mismatch with referee suggestions. This perception seems to have caused the small correlation. Concerning the rest, more referee reports are associated with the perception of higher competence and higher carefulness, and, by that way, with higher overall satisfaction.

Prima facie one might have expected that the manuscript acceptance rate exhibits the paramount correlation with overall satisfaction. However, while that correlation *is* substantial, it is much lower than the correlation between competence and overall satisfaction, and between carefulness and overall satisfaction. The perceptions of higher carefulness and higher competence of the referee reports are associated with higher acceptance rates. Concerning the correlation of editorial match with referees' recommendations and satisfaction, one would, however, have expected a higher correlation.

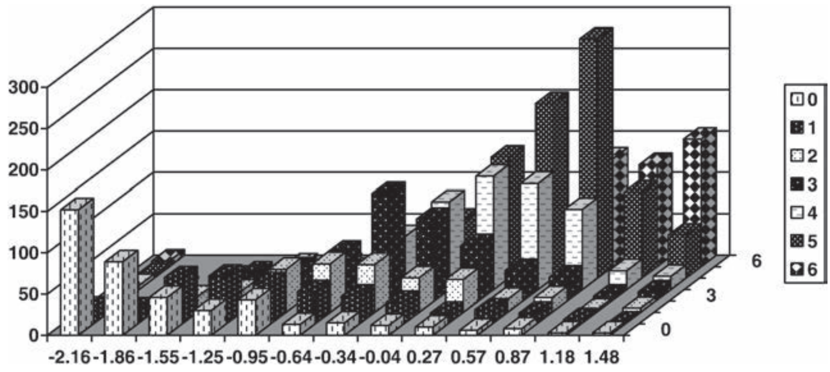
The highest correlation reported in Table 4 is the one between competence and carefulness of the referee reports. Obviously, our respondents hold that a referee who does competent work also does it carefully and vice versa.[17] Both qualitative responses have at the same time the paramount positive correlations with overall satisfaction with the referee process. Thus, competence and carefulness emerge as the most important positive features of referee processes in authors' perceptions. They are even more meaningful for overall satisfaction than the acceptance rate itself.[18] This gives rise to the conjecture that authors accept rejection of their manuscripts more easily when it is based on competent and careful referee reports. And, conversely, they seem to be

[16] Cf., e.g., Zuckerman and Merton (1971, 78), Bakanic et al. (1990, 378), Hargens and Herting (1990, 97), Kupfersmid and Wonderly (1994, 56). In a similar sense cf. also Ingelfinger (1974, 687), Crandall (1982; 1991), Cole (1991), Coleman (1991, 142), and Eckberg (1982).

[17] In our instructions for response to Question 6 we used the following remark to alert respondents that competence and carefulness need not coincide: "Concerning question 4, please note that competence and carefulness may be independent." The full set of questions inclusive of instructions can be downloaded from our homepage http://www.wiso.uni-kiel.de/vwlinstitute/ifs/chair/peerreview.php.

[18] This result accords with the results of Garfunkel et al. (1990), who did not find major differences in review evaluation among authors whose papers were accepted or rejected by the *Journal of Pediatrics*. However, our result for economics authors stands in remarkable contrast to the findings of Weber et al. (2002), who observed for authors of the *Annals of Emergency Medicine* that author satisfaction is associated with acceptance but not with review quality.

Figure 1 Distribution of Satisfaction for Factor Levels



but moderately happy with the acceptance of their paper when it was based on incompetent and sloppy referee reports.

Finally, to make use of similarities among responses, we applied a factor analysis. We employed a principal-component analysis using a varimax rotation with Kaiser Normalization.[19] This produced a factor composed of the two components: carefulness and competence, each with a factor weight of 0.539, which means that their marginal rate of substitution is equal to -1. We call this factor *quality*. It explains 86.114% of the variance among the two characteristics competence and carefulness. Competence and carefulness are, therefore, good proxies for the factor "quality". Our analysis yielded 48 factor levels, which we found to be arranged in terms of 13 distinct groups. Representing each group by its median allowed us to focus on 13 representative factor levels. A negative (positive) factor value means that a subject exhibits a less (better)-than-average evaluation of the respective journal. A factor value of zero corresponds to the average evaluation.

Associating these 13 factor levels with the seven levels of overall satisfaction shows a characteristic distributional pattern: For low quality we observe a positively skewed distribution of satisfaction. As quality increases, the distribution of satisfaction becomes symmetrical, and gradually becomes negatively skewed as quality approaches its peak. Note that, although this pattern is in a way due to bunching effects inherent in categorical measurement, it is, nevertheless, rather distinctive in this case. Figure 1 shows the respective graph, which arranges normalized quality at the abscissa, sat-

[19] For the ease of calculation we shifted the Likert scales of questions 4–7 by 1 to Likert scales from 1 to 7. For the presentation in the figures, we stick to the scale range from 0 to 6.

isfaction at the ordinate. The vertical axis indicates the absolute frequency of our 3817 data points.

Low levels of satisfaction are caused by a positively skewed distribution of factor values having their peak at the lowest factor level for the lowest level of satisfaction. For higher satisfaction levels the distribution of factor levels converges first to a symmetric distribution which is reached at the medium satisfaction level. For still higher satisfaction levels the distribution of factor levels assumes the shape of negatively skewed distributions. The factor distribution for the highest satisfaction level has its peak at the highest factor level.

Figure 1 depicts a mountain extending across the figure from the (−2.16, 0) coordinate point to the (1.48, 6) coordinate point. The steepness of this mountain on both sides of its ridge is captured by the correlation coefficient between the quality factor and overall satisfaction. Its value is 0.735. It is significant (two-sided) at the 1% level. This illustrates a good explanation of overall satisfaction with the referee process by competence and carefulness of the referee reports.

Finally, we have a look at the joint distribution of overall satisfaction and subjective acceptance rates. Figure 2 shows the respective graph. We observe negatively skewed distributions for all intervals of acceptance rates except the lowest acceptance rates (virtually rejections). Subjects whose papers are often

Figure 2 Distribution of Satisfaction with Referee Reports in Terms of Acceptance Rates

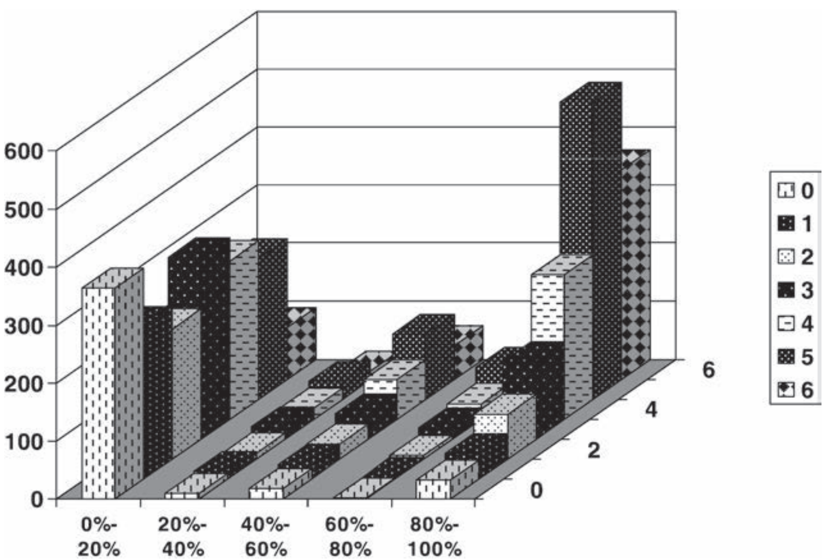
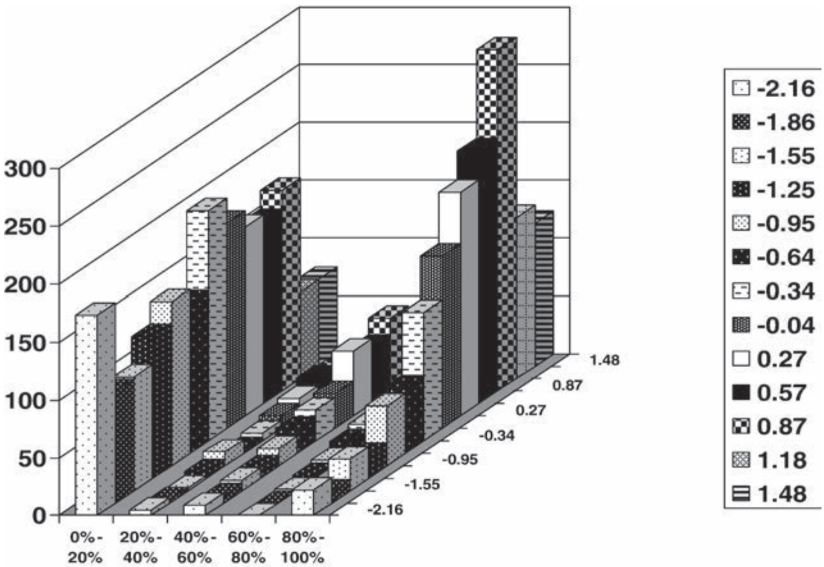


Figure 3 Distribution of Satisfaction with Referee Reports in Terms of Acceptance Rates for Factor Levels



rejected are not distinctly dissatisfied. Although they are not extremely enthusiastic about rejection, we encounter in this pattern a reflection of the appreciation of careful and competent referee reports. Good-quality referee reports may, thus, indeed cause authors to understand rejection of their manuscripts.

Figure 3 repeats this exercise for the 13 representative factor levels. For higher acceptance rates we observe negatively skewed distributions, while for the lowest acceptance rates satisfaction is rather evenly distributed with the exception of extreme happiness.

3 Conclusions

Peer review in science is a tribunal of sorts. It influences decisively personal advancement, research opportunities, salaries, grant-funding, promotion, and tenure. Peer review claims to exert quality control of manuscripts, to improve manuscripts, to promote innovative research, to foster dissemination of new research, to select projects for grant funding, to screen papers for conference presentation, and to serve as a means to rank researchers, journals, and institutions.

Yet journals no longer serve the function of disseminating new research. About three and a half decade ago, Garvey and Griffith (1971) had already demonstrated that the bulk of communication and dissemination of current research runs over informal outlets such as personal communications, technical reports, discussion papers, and preprints. Eventual publication of a paper means that it had entered the archives of science, while its author had long ago started new research. This applies even more so in the electronic age. The main purpose of journal publication nowadays is to imprint a signal of quality on a scholar's research. However, this requires an excellent performance of peer review. When peer review lacks validity, impartiality, and fairness, the imprint of manuscript excellence becomes dubious.

These limitations induced us to conduct an internet questionnaire investigation of authors of economics journals. We found much longer response times than what is customary in the natural sciences. The top journals had on average high rejection rates. While the top journals did not show particular differences from other journals with respects to the distribution of competence and carefulness of referee reports, they perform somewhat worse for overall satisfaction. Moreover, it is always the same group of some eight top economics journals which populate the bottom rungs in the respective rankings.

We observed response biases among our respondents: the subjective response time exceeds the objective one by some 50% and the subjective acceptance rate exceeds the objective one by some 150%. This may be explained by several effects (self-selection, survey-selection, cognitive dissonance, and trend).

A correlation analysis showed that competence and carefulness are highly correlated, and showed the paramount correlation with overall satisfaction, while the acceptance rate exhibited a smaller correlation with overall satisfaction. This suggests that the authors of economics journals have a higher esteem for good referee reports in comparison to a mere focus on the acceptance rate. In other words, they will understand a rejection of their paper if it is backed by well-founded reports.

Combining competence and carefulness into a factor "quality" showed that, as quality increases, the distribution of satisfaction follows, first, a positively skewed distribution, becomes, for higher levels of quality, a symmetrical distribution, and approaches a negatively skewed distribution for the highest level of quality. When juxtaposing overall satisfaction (quality) and acceptance rates, we found negatively skewed distributions for all acceptance rates with the exception of the very lowest acceptance rates, for which the distribution is largely uniform. This confirms that manuscript rejection is tolerated provided that the referee reports are competent and carefully done.

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What Should We Expect from Peer Review?

Comment by

Max Albert and Jürgen Meckl

Seidl, Schmidt and Grösche (henceforth, SSG) report on an internet questionnaire study that asked economists about their experiences and satisfaction with the referee process in economics journals. In this comment, we put the paper in the context of scientific competition and ask whether we should be worried by its results.

1 *Scientific competition and scientific quality standards*

Scientific competition is mainly driven by the quest for status or reputation. Researchers earn status when their contributions are used – and not just cited – by other researchers (see Hull 1988, 283). Status-seeking researchers should use the products of previous research (contained mainly in research papers) if they believe that it will help them to produce output that will, in turn, be used as an input in future research.

In a nutshell, then, research is the production of papers by means of papers. In order to use the results of a paper, researchers must, of course, be aware of the paper and believe it to be relevant to the problems they are working on. Even if these conditions are satisfied, however, they will not use a paper if they consider it (i.e., the results and ideas contained in it) to be of too low a quality.

Scarcity of attention and quality concerns explain the peer review system. Journals try to collect high-quality papers that have something in common, either a specific topic or, in the case of general journals, a potential to interest even off-topic researchers. Some journals are more successful than others in publishing high-quality work, get more attention, and, in turn, attract more submissions of high-quality work. This positive-feedback effect leads to quality rankings among journals.

Ultimately, publishers compete, with the help of their journals, for the attention of the scientific community. There is a hierarchy of delegation, where all agents pursue their own interests. Publishers select and control editors, who, in turn, select and control referees. On each stage, there is competition and moral hazard. Moreover, some deviations from the application of quality standards, like promoting papers sympathetic to an editor's or referee's research, can be viewed as payment in kind for editorial or refereeing services. For this reason and others, we should expect some amount of personal, institutional, or regional favoritism, as well as inner-scientific partisanship, in the selection of papers for publication.

The explanations so far assume given quality standards used by researchers in selecting input for their own research. It is, however, not at all clear how decision-relevant quality standards can become established in the production process we have described above. Even if everybody expects everybody else to use only inputs that satisfy certain standards of high quality, this expectation is not self-fulfilling – unless using high-quality inputs increases one's chances to produce high-quality output. However, the last assumption is quite reasonable for the quality criteria used in science.[1]

Quality standards in science, then, are the outcome of an intertemporal coordination problem among self-interested and forward-looking researchers. As explained above, we expect these quality standards to spill over, if imperfectly, into the peer review process. With perfect coordination, there exists a single quality standard. However, there are several factors working against perfect coordination. First, new methodological arguments can shift the focal point of the coordination game. Second, at a given time, there may exist several candidates for a focal point. Third, researchers have different information about current debates and different reaction speeds. Thus, the coordination process is slow and subject to shocks, working – despite the forward-looking attitude of researchers – like an evolutionary process of short-sighted adaption to one's perception of the current trends, with several standards competing during adjustment. Fourth, quality standards in different research areas (which are defined by relatively low probabilities of use across boundaries) differ, which leads to grey areas where standards are uncertain or disputed. Fifth, quality in science has many dimensions, and weighing these dimensions may be a problem even if there is broad agreement about the dimensions themselves.

Thus, scientific competition involves competition between quality standards. There exists pressure in the direction of harmonizing the standards, but one should not expect perfect harmony, especially with respect to the fine points and when new ideas threaten existing standards. Moreover, quality may be difficult to detect. Even on the basis of common standards, different editors or referees may still come to different conclusions because they prefer different trade-offs between errors of the first and second kind.

2 *Quality standards for peer review*

SSG suggest that journals only archive papers and hand out quality signals. In their conclusions, they write that peer review in science is a tribunal with decisive influence on individual careers. They then list the claims made in

[1] See Albert (2006) for a model explaining quality standards in scientific competition along these lines.

favor of peer review, which, as far as the journal referee process is concerned, are: quality control, improvement of manuscripts, promotion of innovative research, fostering dissemination of new research, and serving as a means to rank researchers. They note that, due to the publication lag, journals no longer disseminate new research; instead, their main purpose is "to imprint a signal of quality on a scholar's research". This, in their view, requires excellent performance of peer review, especially validity, impartiality, and fairness.

Reliability of the referee process means that different referees come to the same conclusion. Validity of the referee process means that quality judgments report the true quality of the paper. For instance, stories about highly successful papers that were frequently rejected before their eventual publication are often viewed as anecdotal evidence of low validity. Typically, the degree of reliability and validity is measured in terms of correlations between different referee conclusions or between quality judgments and quality.

Validity is ill-defined, however, and reliability is not to be expected when several quality standards compete. Only with (almost) perfect coordination on quality standards, low validity and reliability must be due to imperfections in the peer review process. We do not believe that perfect coordination has been reached in economics.

Impartiality and fairness mean absence of favoritism and, instead, reliance on quality standards, which is of course possible even with competing standards. SSG report regional favoritism. However, consider the case of the Quarterly Journal of Economics. This journal rejects most papers without referee reports but (not mentioned by SSG) publishes many previous NBER working papers. Since NBER papers are already subject to quality control, selecting from them might lower the cost of refereeing without lowering quality. Hence, it may be a sign of efficiency if some journals tap such pools of high-quality papers. Due to the nature of the NBER, this leads to regional favoritism.

However, there is no prima facie case that such practices lead to an unfair and partisan publication system. A combination of journals with different biases can lead to a fair system. Moreover, in judging the quality signals produced by journals, it is easy to adjust for known biases: If you publish a paper at a journal biased against you, it just means that the quality of your paper is probably higher than the journal's average.

Editors usually want the referee to point out possible improvements of the paper. Within limits, this is reasonable since the editor would like to be the paper as good as possible and the referee can produce at least some relevant hints as a by-product of quality control at almost no additional cost. However, referees should not invest much in improving a paper. If they did, this would create incentives for an author to abuse referees as unpaid ghostwriters or conscripted audiences. It is unlikely that this would result in an efficient team effort. Hence, it is perfectly alright if bad papers get sloppy and short reports.

This sets incentives to authors to invest more into their papers (and seek for coauthors by themselves) and makes a better use of the scarce time of the referees, who can concentrate on good papers. It also implies, however, that editors and referees should not necessarily aim at the satisfaction of authors.

Nevertheless, trying to measure imperfections is certainly an excellent idea. The question is whether the data of SSG actually point to imperfections, and hence whether we should be worried about the relatively poor performance of top journals.

We both admit that, independently of each other, we started filling in SSG's questionnaire but gave up since, due to lack of time or access, we could not consult our files. We both were resolved to get to it later, but as these things go, we never did. In line with our experience, SSG admit that those who persevered may have answered the questions from memory, which, as they recognize, may lead to systematic biases. They argue, however, that authors' memory is probably what counts in submission decisions and with respect to author satisfaction. We agree. However, we cannot quite see why author satisfaction should be important, especially if, as SSG find, it depends strongly on the competence and care invested in the reports.

SSG note that top journals receive lower-than-average ratings for their referee processes. Even if this indicated lower-than-average quality, this need not be problematic. Authors submitting a paper face costs in terms of submission fees, rejection risks and decision times, which may be more or less mitigated by the quality of the reports. Top journals overwhelmed by submissions should offer worse terms to authors; they could do this by urging their referees not to waste time on any but the most excellent papers.

However, we can think of two plausible explanations for lower-than-average ratings for top journals' referee processes even if these journals offer average quality. First, authors may just expect more from higher-ranked journals and judge referee processes not in comparison with each other but in comparison with their expectations. Second, authors can make two errors when submitting their papers: aiming too high or aiming too low. If they prefer the first error to the second, papers will on average be submitted too high, which, in the worst case, leads to a rejection based on a single sloppy and short negative report. Papers then trickle down to lower-ranked journals until paper quality matches journal rank. If referee reports get more careful as the gap between journal rank and paper quality shrinks, the trickle-down effect implies that author satisfaction with the referee process increases with falling journal ranks, even if journal policies are all the same. In this context, signalling a large gap between journal rank and paper quality by a sloppy reports offers a further advantage: authors may adjust their self-assessment more quickly, which reduces the number of wasted submissions in the trickle-down process.

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Methodology and the Constitution of Science: A Game-theoretic Approach

by

Jesús P. Zamora Bonilla

1 Science as a game

Competition is an essential element of the scientific process as it is usually carried out. Nevertheless, its competitive aspects have been much more frequently studied from a sociological point of view than from a philosophical or epistemological one, and (perhaps with the main exception of Popperian falsificationism and Mertonian institutionalism), the effects of competition and rivalry on the cognitive value of scientific discoveries have tended to be given an anti-objectivist interpretation. Furthermore, although competition is a phenomenon clearly under the scope of game-theoretic analysis (or, in general, of micro-economic analysis), very few attempts have been made until now of formally describing scientific research as a kind of 'game'. Taking all this into account, the aim of this paper is to sketch some guidelines of a philosophical understanding of scientific research as a competitive, game-like process, a point of view which, in the end, will try to provide some analytical tools with which to assess the rationality and objectivity of scientific knowledge.

An underlying idea of this approach will be the notion that scientific research can be described as a game which is played according to some rules. This idea can be traced back to Karl Popper's *Logik der Forschung*, where the notion of scientific method is explicated as something more alike to 'the logic of chess' than to the rules of formal logic.[1] In this sense, methodological rules are *conventions*, as long as they can conceivably be as different as they are (actually, many of them are not equal in different scientific fields or schools, and also vary with time). Popper's attempt was to justify his own preferred rules by somehow deriving them from the goal of maximising the 'criticizability' of every scientific item, although he offered few convincing justifications of why this epistemic value, criticizability, had to be taken as the most important one in science. I will not attempt to determine here what the values of scientific research 'must' or 'should' be: I rather think that most of the answer has better to be left to scientists themselves, as well as to people using the outcomes of science or suffering from them; but I shall nevertheless explore this idea of scientific norms as conventions derived in some way from

[1] As will become clearer below, I suggest that the rules of science resemble even more the rules of sports, like football or tennis, than of 'logical' games like chess.

the goals 'we' want science to fulfil (the question, of course, is who 'we' are?). From a game-theoretic perspective, two different but interrelated sets of questions emerge once we interpret scientific norms as the rules of a game: First, what will scientists' behaviour be *once* certain norms have been established? And second, what norms would they *prefer to have* if they were given the choice? Obviously, a rational answer to the second question can only be given after making some prediction about how people will react under some norms, i.e., after having given some answer to the first question. The theory about how do people choose the norms under which they have to interact is known as 'constitutional political economy' (cf. Brennan and Buchanan 1985), and one particular goal of this paper is, then, to outline a 'constitutional–economic' approach to methodological rules (see Jarvie 2001) for a non-economic, but also 'constitutional' interpretation of Popper's view of scientific norms).

The main elements in the description of a game are the *options* (or 'strategies') of each player (or agent), the *rules* (i.e., an indication of which outcome obtains for every feasible combination of strategies, one for each player), and the *preferences* of the agents (i.e., an indication of how each player evaluates each possible outcome). Once this description has been given, the analysis of a game typically proceeds by trying to determine its 'solutions' or *equilibria*. Technically, an equilibrium of a game is a combination of individual choices such that no player can make a decision better for her than the one she has made, *given* the decisions made by the other players (Nash equilibrium). In general, the goal of a game theoretic analysis of a social fact is to show how some relevant features of the situation can be explained as an equilibrium emerging from the interaction of the agents. As readers familiar with the developments of game theory will know, one typical problem is that many games have more than one possible equilibrium, and in this case, either the outcome of the game remains indeterminate, or some stronger conditions must be added to justify why some specific solution is attained. It is also possible that *no* equilibrium exists, but it can be proved that, under a wide set of circumstances, there always are some equilibria if agents have the option of choosing, not directly an option, but a determinate probability of choosing every possible option.[2] Further mathematical complications result from the analysis of repeated or dynamic games (when players have to take a sequence of decisions, perhaps in a changing environment), of stochastic games (when the outcomes of the players' decisions are not known with certainty), or of games of incomplete or asymmetrical information (when players do not know with certainty some possible states of nature, or some of them know more

[2] This is traditionally called a 'mixed strategy', whereas a 'mixed equilibrium' is one that obtains by a combination of mixed strategies; nevertheless, the analysis presented in this paper will always stay at the level of 'pure', i.e., deterministic, equilibria.

than others). The application of these models will surely be extremely interesting and even unavoidable for understanding many features of science, but this paper will again offer only the most simple analysis.

2 Scorekeeping in the game of science

I proceed now to a description of the basic elements in the game of scientific research, which will essentially be conceived as a game of *persuasion*. That language is extremely important to science can hardly be denied. Authors as different as the logical positivist Rudolf Carnap and the post-modern anthropologist Bruno Latour would agree at least on this point, though obviously for very different reasons. The perspective I am going to take here is closer to Latour's in the sense that I will assume that interaction between researchers mostly takes place through a continuous mutual examination of what each other *says* or *writes*, although I would guess that scientists can agree to evaluate their colleagues' 'inscriptions' (to use Latour's word) by means of rules which a Carnapian would not dislike too much. This does not mean, however, that other things besides language are unimportant; scientists also perform non-verbal actions, they experiment, observe, earn and spend money, draw diagrams, organise meetings, and so on, though it is true as well that a big part of these things is made by speaking, and, on the other hand, that what people say (and specially what they write) is usually more public than what they do, and so it is easier for other people to scrutinise. So, it can be instructive to describe the game scientists play as if their main decisions related to what assertions to make (probably before or after performing some other actions), and as if their rewards would essentially depend on what other people is asserting. This vision of the process of scientific communication as central to the strategies of researchers is not only consistent with a big part of the work on sociology of science of the last two or three decades, but is also close in spirit to some recent proposals in the philosophy of language. I am referring particularly to Robert Brandom's *inferentialism* (Brandom 1994). According to Brandom, what makes a series of noises to count as an *assertion* is the chain of inferences the speech community takes as appropriate to make regarding that assertion, inferences which essentially relate to the *normative* status that each participant in a conversation attributes to the others (i.e., the things participants are *allowed* or *committed* to do by the rules of the language game). For example, my saying 'there is a cat on my roof' can be *taken* as an assertion by my hearers if and only if we share a set of normative inferential practices which allow them to attribute to me, under specified circumstances, the 'obligation' of presenting some relevant evidence from which that sentence can be derived, as well as that of accepting the linguistic or practical consequences which, together with other commitments I have made, follow from it. Using a metaphor suggested by Wilfried Sellars, understanding an expression would amount to mastering its role 'in the game of giving and asking for reasons'. It is important to mention that Brandom's concept of 'inference' does not only cover moves from sentences to sentences, but also from 'inputs' of the language game (e.g. observations) to sentences, as well as moves from sentences to 'outputs' of the game (e.g., actions).

The aspect of Brandom's theory I want to emphasise is that linguistic practice proceeds by each speaker 'keeping score' of the commitments made by the others and of the actions commanded by those commitments, according to some inferential rules which define the language games which are possible within their speech community. It is this idea of 'scorekeeping' that will be put into use here in order to analyse the game of science. I propose to consider the 'inscriptions' produced by a researcher as her set or 'book' of commitments (her 'book', for short). There is no need that every such commitment amounts to the bare acceptance of a certain proposition (say, A), for it is possible to make a variety of *qualified* (or 'modalised') commitments, as 'it seems likely that A', 'there is some evidence that A', 'A deserves some attention', and so on. *The game theoretic nature of scientific research arises because each scientist's payoff depends on what is 'written' not only on her own book, but on the book of each other member of her community.* This payoff is generated by three interacting factors: an internal score, an external score, and a resource allocation mechanism, all of which are determined by several types of norms. In the first place, any scientific community will have adopted a set of *methodological norms* with which to assess the scientific value of any set of commitments; the coherence of a researcher's book with these norms (or, more precisely, the coherence *her colleagues say* it has) will determine the *internal score* associated to that book. Second, and in contrast to the case of everyday language games, in science many propositions are attached to the name of a particular scientist, usually the first who advanced them; one fundamental reward a scientist may receive is associated with the fate that the theses (laws, models, experimental results...) proposed by her have in the books of her colleagues. This 'fame' is what I call here her *external score*. The combination of the internal and the external score associated to a book is its *global score*. Third, the community will work with a set of *rules for the allocation of resources* which will determine how much money, what facilities, what work conditions, what assistants, and so on, will be allotted to each scientist, depending on her global score.

So viewed, the game of scientific research proceeds as follows. The methodological norms of a discipline tell each researcher what things can she do (or must she do) in order to write a book with a high *internal* score; this will make her count as a more or less 'competent' researcher. These norms indicate how to perform and report experiments, what formal methods to employ and how, what types of inductive inferences are appropriate, what styles of writing are acceptable, and so on. By following these norms, she will end up committing herself to the acceptance of some propositions advanced by other colleagues, hence contributing to *their* having a high *external* score. She will also have to comment on the coherence of her colleagues' commitments with the methodological norms of the discipline, contributing to rising or lowering *their internal* score. On the other hand, in order to reach a high *external* score, she has to take advantage of her colleagues' struggle for attaining a high internal score: she has to be able to devise experiments, hypotheses, or models which her colleagues, given their previous commitments, and given the accepted methodological norms, cannot refuse to accept without running the risk of high losses in their internal scores.

3 An example: the first gravity wave experiments

To exemplify the applicability of a game theoretic approach to the analysis of scientific research processes, I shall take H. M. Collins' classic narration of the dispute about gravity waves which followed the experiments of the physicist Joseph Weber (Collins 1985). According to Collins, Weber's results strongly conflicted with accepted cosmological theories (for his experiments indicated an amount of gravitational energy too big for our nearby universe to be stable), and nearly all attempts of replication failed to show the same results (although no one of them was indisputably negative taken in isolation). Under these circumstances, the other members of the scientific community chose to reject Weber's results, and decided that presumed 'gravity wave detectors' detected nothing at all because there was no signal strong enough to be detectable; this means that the community did not assign a high score to Weber, neither an external score (for his presumed results were not accepted), nor an internal one (because deficiencies in his methods were pointed out). In spite of this, Weber went on defending his experiments and trying to improve them. From a game theoretical point of view, the first relevant question is whether all those decisions (both Weber's and those of his critics) were rational and mutually consistent, i.e., whether they constituted a Nash equilibrium. For example, could Weber have made a better decision at some point of the process? It is very likely that he could have *forecasted* the negative reaction of the community; furthermore, he might have acknowledged that he was wrong when non-confirmatory results begun to appear in the experiments of some colleagues. That he did not take this decision seems to indicate that he severely misrepresented the chances of his 'discovery' being recognised; then, though his decisions might have been 'optimal' given his own (over-) estimation of success, this estimation had to be very wrong, and so in a sense he was acting 'irrationally', at least from the cognitive point of view. If we did not want to accuse Weber of irrationality, we would have to look more deeply into his view of the situation.

On the other hand, what about the decisions of his colleagues? Given that most experiments were inconclusive, that the acceptance of Weber's results might have forced a toilsome reformulation of much accepted knowledge, and that this reformulation would demand the cooperation of many theoreticians and experimenters, the decision of waiting till a 'significant' number of colleagues had made a decision seems logical for a majority of researchers. Nevertheless, for those who had more to loose or to win if Weber was right (for example, because their prestige strongly depended on the theories which negated the existence of detectable gravity waves, or because they expected to contribute with new discoveries in the line of Weber's if he happened to win) it seemed rational to attempt to replicate the experiments soon, as many did. In conclusion, the resolution of the debate looks like a Nash equilibrium, since everybody chose her best option, given what the other people were doing, although perhaps Weber himself suffered from a strong confirmatory bias (i.e., his decision was rational according to the beliefs he actually had about the probability of successful replications, but these probability judgements were somehow defective).

The situation, however, is not so simple once we consider more deeply the strategies available to each researcher. For example, imagine you are one of those who are waiting for more information from your colleagues before deciding what to do with Weber's assertions. Your options are not just 'accept' and 'reject', but rather 'accept if at least ten percent of the community accept; reject otherwise', 'accept if at least fifty percent accept; reject otherwise', and so on, or even something more complicated, because you will surely take also into account your own degree of belief in the validity of the disputed hypothesis. The scientific community must be in an equilibrium also with respect to the decisions about *when* there are 'enough' reasons in favour of a proposition for it to become acceptable.[3] On the other hand, what about people trying to replicate Weber's experiments? If it is true that the 'community leaders' have so much prestige that their own conclusions would 'trigger' a consensus around them, they had at least a choice between performing the experiment as carefully as possible or not, as well as a choice between describing the results in the most neutral way or in a way which is favourable to their preferred theories. As long as they suspect that their declarations will virtually close the debate, they will be strongly tempted to choose the second option in both cases, especially when there is only one leader (according to Collins, in the dispute about gravity waves this role was played by Richard Garwin). A full analysis of the episode in game-theoretical terms should indicate, hence, what the reasons of the leaders were for

[3] See also Zamora Bonilla (2002) for an analysis of a possible agreement among researchers about how much 'corroborated' a hypothesis must be for making its acceptance *compulsory*.

behaving 'honestly' (if they did it), and also what reasons the remaining scientists had for accepting the leaders' assertions, especially if it was not clear which strategy the leaders were to use. The mathematical model I present in section 5 allows to explain why scientists may choose to behave 'honestly' very frequently (though not always).

The next question is whether other possible equilibria could have existed. Collins himself strongly sympathises with this possibility, for he repeatedly asserts that every scientific controversy might have been 'closed' in a different way. For example, in the case of Weber, the community *might* have accepted the existence of gravity waves, since (according to Collins) the experiments did not point too much clearly in the opposite direction. In that case, Weber would have been recognised as an important contributor to the advancement of knowledge. But what about the other members of the scientific community? Would all of them necessarily have found it profitable to accept gravity waves given that the others had accepted them? Probably not, because, lacking a powerful theory to explain why these waves are as they were accepted to be (under these counterfactual circumstances), some researchers could still have opted for defending the old theory and rejecting Weber's results. This simply means that, if other equilibria exist, some of them can in principle correspond not to a full consensus around the new result, but to a division of the community into two or more rival 'schools'. Nevertheless, even if a unanimous *acceptance* of Weber's results were a Nash equilibrium, it is very likely that it would be judged by most scientists as worse than an almost unanimous *rejection*. This is again because of the absence of a theory which can accommodate those results: with the help of the old theory they expect to be able of solving still many problems, whereas the prospects for scientific merit under the other scenario are much more uncertain. In a nutshell, had all of Weber's colleagues accepted his results, they would surely have got, on average, a payoff *below* the payoff from almost unanimous rejection.

4 Scientific norms as the constitution of science

Although nearly all the choices individual scientists have to make refer to decisions whose outcome will depend on their coherence with the norms prevailing within their scientific community, the norms themselves must also be selected, for they are, after all, social conventions. As I said in the first section, the perspective advocated in this paper is that of constitutional political economy; so I assume that the norms governing scientists' interactions are to be chosen by those scientists themselves, and I will ask what properties the norms can be expected to have according to that assumption. After all, though it is true that a single scientist can do little or nothing to substantially change the norms of her community, these can be *easily* changed by means of a *collective* agreement. A unanimous or almost unanimous agreement about a norm can sometimes derive from its adoption by only a part of the community, for, if enough colleagues accept it (both in their practice and in their public assessment of the others' internal scores), probably many others will find it profitable to do the same. On the other hand, since most norms are better understood as *regular practices* than as explicit, well-defined precepts, these practices can also change smoothly by small individual adaptations to changing circumstances. In any case, if the norms have to be collectively accepted, it is absurd to assume, as constructivists sometimes claim (e.g., Latour and Woolgar 1979, Latour 1987), that they can be 'imposed' on the whole community by a small group of researchers, save, perhaps, when these have a monopoly over the material resources which are necessary for the rest. For example, a norm designed *just to favour* a particular theory or model would be rejected by those scientists who are proposing different ideas.

Another relevant aspect of norms is that they tend to be in force for a long time (usually, more than the mean life of most theoretical models, for example). This has two important consequences regarding the epistemic properties of the norms. First, it is hard for researchers to guess what models they will be proposing in the future, when the norms which they are choosing today will still be in force; so, under a more or less thick 'veil of ignorance', and assuming that no monopoly over material resources exists, *impartial* (and, in particular, *epistemic*) criteria will preferably be employed in order to discuss the acceptability of a norm. Second, at any moment in the evolution of a scientific discipline, *prevailing* norms will probably have evolved in order to help the community members in their striving to find acceptable results. This entails that methods, models, laws, and even styles of research, which had been accepted after being evaluated with impartial norms, may become a *norm* themselves (for example, your paper can be rejected in a physics journal if it contains a model which contradicts Maxwell's equations, even if it is methodologically sound in any other respect). As a result, the methodological norms a community has at a given moment can be an obstacle for the adoption of new ideas; *scientists will only take seriously the possibility of abandoning the old norms when the prospects of finding out new results which are acceptable according to those norms begin to decrease*, as compared to the new norms. Although the last two points seem mutually contradictory, it is possible to accommodate them in the following way: after all, the arguments employed to defend the new norms must be based on some methodological criteria of a *higher* level; this means that these criteria are thought to be in force during a longer period, and in a wider field; so, these 'metanorms' will very probably be impartial and epistemically sound, or at least, more so than the 'lower level' norms which are assessed with their help.

With respect to the norms which serve to determine the value of *internal* scores, we can distinguish three different kinds. In the first place, there must be some norms about the *disclosure of information*, i.e., rules indicating which of your commitments *have* to be inscribed in your book; they can also determine who is entitled to access each part of another's book, for not all its parts need be equally public. Obviously, the interests of the scientists in establishing some rules of disclosure of information instead of others can depend on the existing technical and institutional possibilities for getting resources or other benefits by using that information in a non public way.[4] Second, some norms (which I shall call *inferential norms*) must establish what kinds of inferences from a set of actual or hypothetical commitments to another are mandatory, discretionary, or forbidden; these are the norms whose fulfilment within a book is easier to check, for usually they only demand to analyse the 'inscriptions' contained in the book (Zamora Bonilla 2002). Third, further norms must refer to the coherence of a book's 'inscriptions' with something external; these norms serve to introduce in the books 'inputs' which are not just inferred from other commitments already contained in them. Usually, norms of this type establish the conditions under which a researcher or group of researchers are *entitled* to introduce a new inscription in such a way that their colleagues are *committed* to accept it *by default*, i.e., unless they manage to present a *justifiable* chain of commitments which lead to a different conclusion. Norms governing laboratory protocols and demanding replicability are of this kind. The most important point about these *norms of observation* is that they do not need to refer to an 'indubitable empirical basis', or something like that, for it is enough that scientists find it advantageous to play the game defined by these norms (amongst others). However, as long as the results of a discipline have some practical consequences, on which scientists' payoffs may depend, it is sensible to assume that a discipline whose rules of inference and of observation lead *systematically* to mistaken practical conclusions will cease to get the resources it needs. So, the members of a scientific discipline will have an interest, if only for this reason, in collectively adopting a system of rules which is efficient in the production of (approximately) true statements.

5 *The effectiveness of the scientific constitution*

The status of norms is one of the most fiercely debated points in the sociology and the philosophy of science. Without assuming that the game theoretic approach can offer a definitive solution to all the problems related with scientific norms, it can be useful, at least, to illuminate some deficiencies of other

[4] Dasgupta and David (1994) is the main reference on this topic.

approaches. For example, functionalists, such as Robert Merton, tend to argue as if indicating the virtues of a norm from a 'collective' point of view were enough for explaining why this norm is *accepted* and *obeyed* by the individuals forming that collective. Obviously, those cases where the interests of the individual and those of the 'group', whatever this means, are in conflict pose a problem for this approach, for it leaves unexplained just why an individual decides, in the first place, to approve the rule, and, in the second place, to act according to it. Constructivists, in their turn, tend to talk about norms as if they were either mere rhetorical devices, or mechanisms for benefiting some privileged group. In this case, the problem is that, although this approach can explain why some people may have an interest in proposing or using some norms, it does not explain why *others* (knowing that the norms are *just* rhetorical strategies for defending the interests of some) actually behave as if they also accepted these norms. In contrast, from a game theoretic point of view, individuals 'obey' the norms just because it is in their own interest to do it (though social influences on individual preferences are not discarded a priori). This means that a system of norms will be *stable* if and only if it constitutes a Nash equilibrium, i.e., if, under the assumption that the others obey the norms, anyone's best option is also to obey. For example, given that most people speak a certain language in a country, it will be in my interest to do the same; given that firms and public administrations hire people according to their academic certificates, it will be in my interest to try to get some; given that judges and policemen do efficiently their work according to the prevailing civil and criminal laws, it will be in my interest to obey these. As it is clearly shown in these examples, when 'obeying certain norms' includes 'punishing those who do not obey', general compliance with the rules is to be expected (Axelrod 1984, Elster 1989). In the case of science, this is reflected in the fact that a researcher's book is permanently evaluated by other colleagues, whose evaluations are contained in their respective books, which are evaluated by other scientists, and so on. For example, I will be punished if my model violates the law of energy conservation, but also if I *fail to criticise* a colleague whose model makes this mistake. So, the fact that a certain norm is followed by a high proportion of my colleagues makes not obeying very costly for me.

Nevertheless, it is clear that disobedience may sometimes provide great advantages, particularly if the chances of not being discovered are high. I can manipulate experimental results, or fail to put enough effort in my work, or fail to disclose some information that the norms command to publish, and so on. The sociological literature is full with case studies showing how scientists 'misbehave', at least according to the rules *they* (scientists) preach, not to speak of the rules preached by the philosophers. Even some institutional mechanisms (which are norms themselves) may have the perverse effect of rewarding this type of misbehaviour (for example, the 'publish – or-perish' practice).[5] The persistence and the spread of an institution like science, where most fundamental things depend on the trust people put on other people's assertions, demands, however, that misconduct is severely limited, particularly in those cases where the fate of a discipline is at stake. Actually, science seems to attain this goal rather well even in the absence of something like a 'police' or 'courts of justice'. The question is, hence, whether the mechanism of mutual checks described in the last sections is strong enough for deterring researchers from systematically disobeying the prevailing rules. If the answer were 'no', then either the public trust in scientific results should be much more fragile than what is usually presumed by the scientistic rhetoric, or the apparent stability of so many portions of scientific knowledge would just be based on scientists' exceptional honesty. I hope, however, that the following toy model may allow to avoid this dilemma.

Let f be the frequency with which a researcher disobeys the norms, and suppose, for simplicity, that all infringements are equally important (if this is not the case, then f can be alternatively interpreted as a normalised average of an individual's infringements). Let $u(f)$ (>0) be the utility received by a scientist if she is *not* discovered and disobeys the norms with frequency f, and let $-v(f)$ (<0) her disutility if she is discovered and hence punished. In this model, punishment basically consists in reducing a researcher's internal score, e.g., by not accepting her papers for prestigious journals or congresses. The probability of being discovered, $p(f)$, is an increasing function of f. I will assume that the functions u, v and p are equal for all the community members. Given these assumptions, an individual's expected utility from disobeying the norms with frequency f is $EU(f) = (1 - p(f))u(f) - p(f)v(f) = u(f) - p(f)(u(f) + v(f))$, and the optimum infringement frequency for her corresponds to that value of f which maximises $EU(f)$. On the other hand, it is reasonable to assume that an individual's utility depends on the frequency with which the norms are disobeyed by *other* researchers: the more frequently norms are infringed by your colleagues, the less utility will you get from the same actions (for example, because by producing outputs of a lower quality, the scientific community obtains less resources from society). Hence, a situation where f were low for all, would also be better for everyone than a situation where f were high for all. The essential question is, of course, whether in a situation of equilibrium the f's will be 'high' or 'low'. In order to answer this question, I will add some more simplifications: first, suppose that $p(f)$ is just equal to f (i.e., the probability of being discovered is the same as the frequency of infringement); second, assume that $u(f)$ and $v(f)$ are linear functions of f, in particular, $u(f) = a + bf$, and $v(f) = cf$ (with a, b, $c > 0$); this entails that $u(0)$ is positive (you get a positive payoff by not disobeying the norms) and $v(0) = 0$ (you are not punished if you obey the norms); lastly, your

[5] See Wible (1998) for a good rational choice approach to the study of scientific fraud.

utility will also depend on the *average* frequency of infringement within the rest of your community, $\mathbf{f}$, so that $u(f,\mathbf{f}) = (1 - \mathbf{f})(a + bf)$ (i.e., even if your infringements are not discovered, you get a null utility if norms are always disobeyed), whereas $v(f)$ does not depend on $\mathbf{f}$ (i.e., you will be punished by your infringements independently of how frequently your colleagues disobey the norms).

Under these assumptions, a researcher's expected utility is given by

$$\begin{aligned} EU(f,\mathbf{f}) &= (1 - f)(a + bf)(1 - \mathbf{f}) - f(cf) \\ &= -f^2(b(1 - \mathbf{f}) + c) + f(b - a)(1 - \mathbf{f}) + a(1 - \mathbf{f}) \end{aligned} \quad (1)$$

Individual maximisation of (1) is reached when $\partial EU/\partial f = 0$, which yields the optimal frequency

$$f^*(\mathbf{f}) = [(b - a)(1 - \mathbf{f})]/2(b(1 - \mathbf{f}) + c). \quad (2)$$

Some useful consequences are the following:

(3) a) $f^*(\mathbf{f}) < 1/2$ if $a < b$

b) $f^*(\mathbf{f}) = 0$ if $a \geq b$

c) $df^*/d\mathbf{f} = -(b - a)\ c/[(2(b(1 - \mathbf{f}) + c))^2] < 0$ if $a < b$

d) $|df^*/d\mathbf{f}| < 1$ if $a < b$

Thus, (3a) says that your optimal frequency of infringement is less than fifty percent. Furthermore, the bigger the reward a from always obeying the norms, and the stronger the punishing reaction c to your infringements, the smaller will this optimum frequency be. For example, if punishment is at least as strong as the benefits you get from disobeying (i.e., if $c \geq b$), then f^* will be smaller than 1/4. Note also that, if $a \geq b$, then f^* will be 0 according to (3b), for in that case EU is decreasing within the interval [0,1]. On the other hand, (3c) says that your optimum frequency of infringement decreases as the average frequency of your colleagues rises. This result essentially derives from the assumption that you are not *less* punished for your transgressions when your colleagues commit *more* infringements in the aggregate. Regarding other types of social norms, this need not be true; for example, when the police works less efficiently, it is *more* probable that you will not be punished because of your crimes (although you can be 'punished' even if you do not commit any crime), and this provides a reason to commit more crimes. However, in the case of science, researchers want essentially to have a global score higher than their colleagues', and this entails that they will hardly miss the opportunity of denouncing your infringements, even when they commit many. So, our assumption that $\mathbf{f}$ affects u but not v simply means that, the higher is $\mathbf{f}$, the less willing will your colleagues be to recognise your merits, though they will always be prone to punish you. Lastly, (3d) will be useful in proving the next theorem; it can be derived from (3c) by taking into account that **f** is according to (3a) necessarily less than 1/2 if each researcher takes a rational decision.

The main result of this simple model is the following:
There is only one Nash equilibrium, which corresponds to the case where all researchers disobey the norms with a frequency **f** such that $f^*(\mathbf{f}) = \mathbf{f}$.

Proof: In figure 1a (see p. 276), this equilibrium corresponds to the point where the function $f^*(\mathbf{f})$ crosses the line of 45 degrees (the identity line); let **d** be the frequency associated to that point. In the first place, it is easy to see that all scientists disobeying the norms just with frequency **d** is a *sufficient* condition for a Nash equilibrium, because in that case the best option for every researcher is choosing exactly $f = \mathbf{d}$. To see that it is also a *necessary* condition, suppose first that all researchers chose another frequency, as $\mathbf{d_1}$ in figure 1a; this can not be an equilibrium, because the optimum response would not be $\mathbf{d_1}$, but *e*, and hence, researchers would not be acting rationally. Suppose next that there were an equilibrium in which not all researchers chose the same frequency. In this case, the average frequency could be equal to **d**, higher (e.g., $\mathbf{d_2}$), or lower (e.g., $\mathbf{d_3}$). Suppose first that it were **d** (figure 1b, see p. 276), and take one of the scientists which disobey the norms with the highest frequency, *h*; if *h* is the frequency chosen by her, this means that the average of the rest of the community (i.e., of every member save *i*) must be **g**, if her decision is rational; hence, the average of the full community is *at most* $(h + \mathbf{g})/2$, which is necessarily *less* than **d**, because $|df^*/d\mathbf{f}| < 1$, and so the community average can not be **d**. In the second place, suppose that the community average were $\mathbf{d_2} < \mathbf{d}$ (figure 1c, see p. 276); in this case, again, a scientist selecting the highest of the chosen frequencies (*h*) will be responding, if rational, to an average of the rest of the community equal to **g**, but then the community average is *at most* $(h + \mathbf{g})/2$, which is again less that $\mathbf{d_2}$ because of the same reason. In the third place, suppose that the average is $\mathbf{d_3} < \mathbf{d}$ (figure 1d, see p. 276); in this case, a rational scientist which chooses the lowest of the selected frequencies (*k*) will be responding to an average of the rest of her community equal to $\mathbf{g}'$, and the average of the full community will be *higher* than $(k + \mathbf{g}')/2$, which is higher than $\mathbf{d_3}$. Hence, the assumption that in equilibrium not all researchers choose frequency **d** leads necessarily to contradictions.

In conclusion, given the type of mutual control the members of a scientific community exert over themselves, we can expect that an equilibrium arises in which the norms are followed with a 'high' frequency. Perhaps this situation is not ideal for scientists, nor for citizens, all of which could get a higher utility if the norms where always obeyed; but surely other situations are possible that would be much worse. In fact, the picture which derives from our model seems to be more realistic than that offered by the 'deconstructionists' referred to at

Figure 1 Determination of the equilibrium rate of compliance with norms

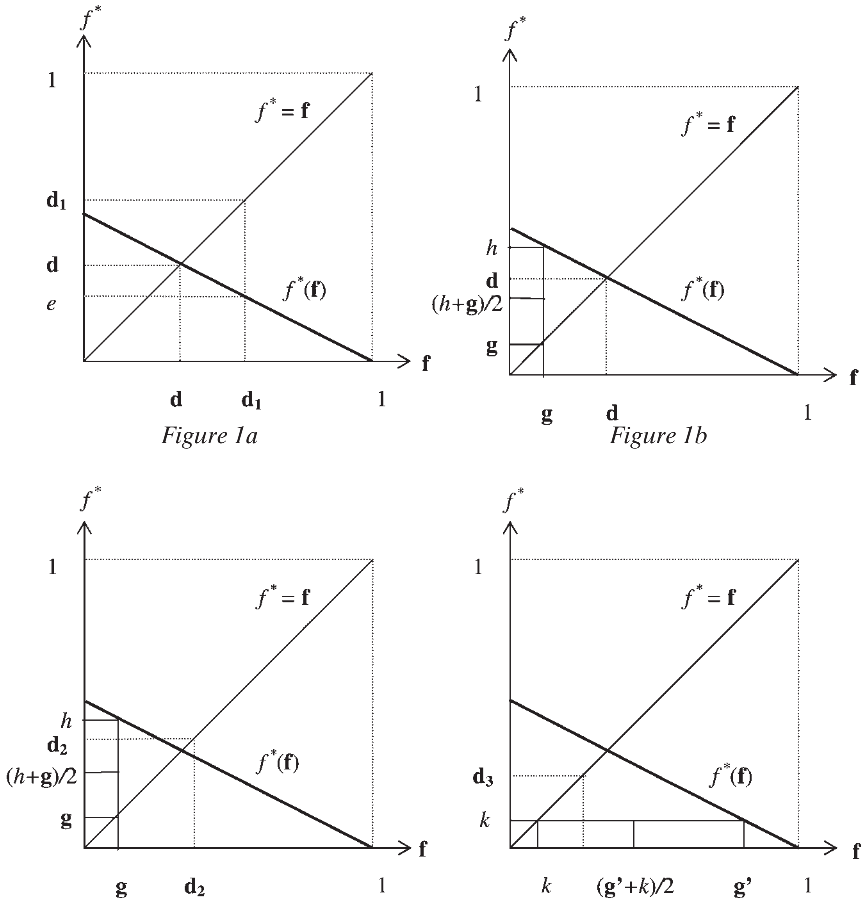

Figure 1a *Figure 1b* *Figure 1c* *Figure 1d*

the beginning of this section, since according to them methodological norms are not designed to be 'obeyed' at all, but just to be used as weapons in scientists' rhetorical tournaments. In contrast to this, researchers seem to follow methodological norms in a very systematic way, though not perfectly, and this is what makes their infringements so salient when they are discovered (either by their colleagues, or by the social students of science). The model presented in this section makes this type of behaviour understandable in the case of people who, as most social studies show, are not exclusively motivated by an altruistic desire for truth.

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Is It a Gang or the Scientific Community?

Comment by

GEBHARD KIRCHGÄSSNER

The paper by Zamora Bonilla intends to show that a game theoretic approach is helpful and perhaps even "unavoidable for understanding many features of science" (p. 263). He interprets science as a game and the outcome of the scientific process as a Nash-equilibrium. To underline his point, he constructs a model of this process and he shows that the equilibrium outcome of this process is unique. Thus, his approach might be seen as being an additional version of economic imperialism. While this was in the past mainly directed towards other social sciences, it is now also directed towards philosophy (sociology) of science.

Despite the fact that this paper is very interesting to read as it provides many interesting historical details which might be interpreted in a game theoretic framework, it is not clear whether this paper reaches its objective (or is at least an important step in this direction). In particular, there are three questions I want to raise:

(i) Does the model really represent the characteristics of the scientific process, or is it just the behaviour of some (special) groups, like criminal gangs? Should it be modified to account more for specific aspects of this process?

(ii) Does the game theoretic approach (the model) add anything to our understanding of the scientific process? Is this approach really 'unavoidable'? Or is it just a new language game?

(iii) Can this paper convince somebody who is not familiar with the game theoretic approach that this approach can add something to our understanding of the scientific process?

In the following, I will mainly discuss the model, its limitations and some extensions. In doing so, I take on the traditional economic perspective and try to give an answer to the first question. Finally, however, I will take on the perspective of a social scientist who is not an economist and try to give answers to the other two questions. As far as I can see, despite some interesting insights in this paper, the model is neither very specific for the scientific process (community) nor will this paper convince other social scientists who do not already follow the rational choice approach that the game theoretic perspective leads to new insights. To reach this, we would need 'new' results, i.e. insights about the scientific process derived by applying the game theoretic approach which at least partially contradict traditional beliefs but are supported by empirical observations.

1 Does the model describe the scientific process?

The model describes a group of identical individuals with an own internal norm system which is different from (or in addition to) the norm system outside this group. Violation of these norms occurs with frequency f, $0 \le f \le 1$, and, if it is not detected, provides a benefit u, which is (for simplicity) assumed to be linear in the violation of the law,

$$u(f, F) = (a + bf)(1 - F), \tag{1}$$

with parameters a, $b > 0$, where F is the average violation of the norm in the rest of the group. The probability that a violation will be detected is also F, and the punishment v is

$$v(f) = cf,\ c > 0 \tag{2}$$

with $c > 0$. Thus, the objective function

$$E[u(f, F)] = (1 - f)(a + bf)(1 - F) - f(cf) \tag{3}$$

is to be maximised, which leads to the first order condition

$$-2f(b(1 - F) + c)) + (b - a) = 0, \tag{4}$$

and to the solution

$$f^*(F) = \frac{(b - a)(1 - F)}{2(b(1 - F) + c)},\ f^* \ge 0. \tag{5}$$

The second order condition is

$$-2(b(1 - F) + c)) < 0 \quad \text{for} \quad F \le 1. \tag{6}$$

If $a \ge b$, the right hand side of (5) is not positive and, therefore, $f^* = 0$. If $0 \le a < b$, then

$$|\,\mathrm{d}f^*/\mathrm{d}F\,| < 1 \tag{7}$$

Thus, $f(\cdot)$ is contractive which – according to the *Banach Contraction Principle* – ensures that the fixed point $f = F$ is unique and stable.[1]

There are several problems connected with this model:

(i) Punishment is assumed to be exogenous, it is without costs, depends on f, but not on F. This does not seem to make much sense, as this assumes that punishment takes its maximum when all individuals violate the norm ($f = F = 1$). This is hardly plausible, as in this case one would rather expect no punishment at all. Thus, let us assume that punishment depends on F, $v = v(f, F)$.

[1] See, e.g., Dugundji and Granas (2003, 9f.). This substitutes the rather cumbersome proof in the paper.

The more a law is violated, the smaller is usually punishment, $\partial v(f, F)/\partial F < 0$. There are many cases where behaviour, which originally was strictly forbidden and, if detected, severely punished, was legalised when it became common. This holds, e.g., for abortion, but in some cases also for consumption of illegal drugs. Thus, a reasonable assumption is $v(f, 1) = 0$, i.e. if all members of a group violate the norm, there is no punishment at all. Assuming a similar relation as in the utility function, the punishment function might be written as

(8) $$v\,(f, F) = cf \cdot (1 - F),$$

with $c \geq 0$. The solution of this model is

(9) $$f^* = \frac{b - a}{2\,(b + c)},$$

which makes the optimal norm-violating behaviour independent of the behaviour of the rest of the group. The reason for this is that the utility as well as the punishment depend in the same way on the frequency with which the others violate the norm. To come to a solution where the norm violating behaviour depends on the behaviour of others, we would need two different (plausible) functional forms of how the utility from violating the norm as well as the punishment if such a behaviour is detected depend on the average frequency of violation.[2]

(ii) Punishment has costs and benefits. Pointing to the norm violation of others might increase the own reputation, but also demands resources and, what may be more important, can produce enemies. But if it reduces their utility, why should people punish others? This, at least, is not consistent with the primitives of neo-classical theory which are the basis of this model.

On the other hand, modern behavioural economics has shown that there are people who take the costs and punish others, even if it is costly.[3] Their behaviour is essential to ensure that norms are observed within a society which are not sanctioned in a formal way (e.g., by penalties through the judicial system). However, not all individuals behave in this way. There are 'altruists', who behave in this way, but also 'egoists' who follow the classical economic assumptions. Thus, to describe the equilibrium outcome of this game we have to distinguish (at least) two kinds of individuals; a model assuming identical individuals is hardly able to correctly describe the scientific game.

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[2] One might also question why the utility from strictly following the norm, a, depends on the behaviour of others and is zero whenever all violate the law. However, making this utility independent which implies setting $a = 0$ and including a constant into the utility function (3) would not change the character of the solution.

[3] See, e.g., Fehr and Gächter (2002).

(iii) Finally, which are the norms or rules to be observed? And how autonomous are scientists in setting these rules? We can distinguish three kinds of rules:

a) Basic rules, like the rules of logic, or the norm that experimental results should be reported correctly.
b) Rules which constitute the hard core of the paradigm (or of a scientific research programme).
c) Rules which belong to the security belt of the research programme.

The model applies only to the basic rules (a). These are the ones where individuals try to hide their violations and where punishment is to be expected if these violations become public. Rules which belong to the security (c) belt may, on the other hand, be suspended at any time without major consequences. The perhaps most interesting rules are those which constitute the hard core (b). In neoclassical economics, these rules, e.g., demand basing theoretical models on the primitives of this theory. Violations of these rules are punished, but the violators do not try to hide them. Just the contrary is the case: to reach the benefit of the violation it is necessary that the violations become public. If the violators are successful, this might lead to a change of the paradigm in the long-run and to scientific reputation. These violations might be seen as risky investments into the own future scientific reputation.

Though the model in the paper does not apply to these rules, the paper also deals with them. And this is reasonable, as violations of these rules are often a precondition for scientific progress. But this aspect is not discussed in the paper.

Thus, this model covers only part of the norms of the scientific process, perhaps not even the most relevant ones. It is just a model of norm-violating behaviour which could be applied in the same way (or perhaps even better) to any other group with an internal norm system and, therefore, also to a gang of criminals. Insofar, it is of very limited value for understanding the scientific process, and it is at least debatable whether it really covers the essentials of this process. From a perhaps somewhat naïve point of view, the objective of science is the generation of true statements about reality. This holds, despite the fact that we can never be sure that a scientific statement is true. A model which describes the scientific process should – at least in my opinion – give reference to it. This would also distinguish it from a model of a criminal gang.

2 Is game theory unavoidable to understand the scientific process?

Even if this model does meet the essentials of the scientific process, it can be asked whether the game theoretic approach is really "unavoidable for understanding many features of science" (p. 265). Obviously, science is an interactive game played in a community which has a set of rules which the actors more or less obey. Thus, the language of game theory can be used for describing this process. It is true, that the system of rules of the scientific community "will be *stable* if and only if it constitutes a Nash equilibrium, i.e. if under the assumption that the others obey the norms, anyone's best option is also to obey" (p. 272). But is this sufficient to proof that there is an added value in applying game theory? Is then, therefore, each application of the economic model of behaviour to human interaction an application of game theory?

According to my opinion, to be justified, the application of game theoretic concepts should give more and new insights in addition to what we already know. The idea which is behind the definition of an equilibrium given above is much older than the formal treatment by Nash (1950), and has been applied long before by economists.[4] Moreover, one of the basic characteristics of the scientific process is that it is never in equilibrium and that its rules are permanently changed. This raises the question whether the concept of a Nash-equilibrium is well suited to describe the core characteristics of the scientific process. Would not (at least) a model of a dynamic game be necessary?

As mentioned in the introduction, in the first parts the paper provides many interesting details of and insights into the scientific process. However, this is quite independent of the use of the language of game theory. It is to hope that game theory, when used for analysing the scientific process, can provide new insights in the future, and taking this paper as a first attempt, we should perhaps not be too critical. Nevertheless, this paper does not provide such insights; it presents game theory as a new rhetoric or language game of concepts we have known before. Consequently, this paper will hardly convince any non-economist (or non-game-theorist) of the usefulness of the game theoretic language game.

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[4] This concept has, e.g., been applied in the first book of the *General Theory* of Keynes (1936). See for this Tobin (1997, 17).

Distributed Cognition: A Perspective from Social Choice Theory

by

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1 Introduction

'Distributed cognition' refers to processes with two properties. First, they are *cognitive*, i.e. they involve forming certain representations of the world. Second, they are not performed by a single (human) agent, but are *distributed* across multiple (human) agents or (technical) devices. Distributed cognition has attracted interest in several fields, ranging from law (e.g., jury decision making) and sociology (e.g. information processing in organizations) to computer science (e.g., GRID computing) and the philosophy of science (e.g., expert panels).

An influential account of distributed cognition is Hutchins's (1995) study of navigation on a US Navy ship. Hutchins describes the ship's navigation as a process of distributed cognition. It is a cognitive process in that it leads to representations of the ship's position and movements in its environment. It is distributed in that there is no single individual on the ship who performs the complex navigational task alone, but the task is performed through the interaction of many individuals, together with technical instruments. At any given time, no single individual may be fully aware of the navigational process in its entirety. Thus, on Hutchins's account, the ship's navigation is performed not at the level of a single individual – a 'chief navigator' – but at the level of a larger system.

In the philosophy of science, Giere (2002) argues that many scientific practices, especially large-scale collaborative research practices, involve distributed cognition, as these practices are "situation[s] in which one or more individuals reach a cognitive outcome either by combining individual knowledge not initially shared with the others or by interacting with artefacts organized in an appropriate way (or both)" (2002, 641). He distinguishes between 'distributed' and 'collective' cognition, where the first is more gen-

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eral than the second. Distributed cognition includes not only cases of collective cognition, where a cognitive task is distributed across multiple individuals, but also cases where such a task is distributed between a single individual and an artifact, such as a technical instrument.[1] While researchers often compete with one another, collectively distributed cognition is a phenomenon associated with more cooperative practices within research groups or communities.

Knorr Cetina (1999) provides a case study of distributed cognition in science. Studying high-energy physics research at the European Center for Nuclear Research (CERN), she observes that experiments, which lead to cognitive outcomes, involve many researchers and technicians, using complex technical devices, with a substantial division of labour, expertise, and authority. She describes this research practice as "something like distributed cognition" (25, cited in Giere 2002).[2]

Other instances of distributed cognition in science can be found in multi-member expert committees. For example, in 2000, the National Assessment Synthesis Team, an expert committee commissioned by the US Global Change Research Program with members from governments, universities, industry and non-governmental organizations, presented a report on climate change.[3] Such a committee's work is cognitive in that it involves the representation of certain facts about the world; and it is distributed in that it involves a division of labour between multiple committee members and a pooling of different expertise and judgments. Here it may be more plausible to ascribe authorship of the report to the committee as a whole rather than any particular committee member.

In this paper, I discuss collectively distributed cognition from the perspective of social choice theory. Social choice theory can provide a general theory of the aggregation of multiple (individual) inputs into single (collective) outputs, although it is usually applied to the aggregation of preferences. Drawing on social-choice-theoretic models from the emerging theory of judgment aggregation (e.g., List and Pettit 2002, 2004; Pauly and van Hees 2005; Dietrich 2005; Bovens and Rabinowicz 2005; List 2005a, 2005b, 2006), I address two questions. First, how can we model a group of individuals as a

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[1] Collective cognition is "[a] special case of distributed cognition, in which two or more individuals reach a cognitive outcome simply by combining individual knowledge not initially shared with others" (Giere 2002, 641).

[2] Knorr Cetina also studies research practices in molecular biology, but argues that here research is more individualized than in high energy physics and "the person remains the epistemic subject" (217, cited in Giere 2002). Giere (2002, especially 643) responds that, while there may be less *collective* cognition in molecular biology than in high energy physics, there may still be *distributed* cognition, "where the cognition is distributed between an individual person and an instrument".

[3] The title of the report is "Climate Change Impacts on the United States: The Potential Consequences of Climate Variability and Change". See http://www.usgcrp.gov/.

distributed cognitive system? And, second, can a group acting as a distributed cognitive system be rational and track the truth in its cognitive outputs?

I argue that a group's performance as a distributed cognitive system depends crucially on its organizational structure, and a key part of that organizational structure is the group's 'aggregation procedure', as defined in social choice theory. An 'aggregation procedure' is a mechanism a multi-member group can use to combine ('aggregate') the judgments or representations held by the individual group members into judgments or representations endorsed by the group as a whole. I investigate the ways in which a group's aggregation procedure affects its capacity to be rational and to track the truth in the outputs it produces as a distributed cognitive system.

My discussion is structured as follows. I begin with some introductory remarks about modelling a group as a distributed cognitive system in section 2 and introduce the concept of an aggregation procedure in section 3. The core of my discussion consists of sections 4 and 5, in which I discuss a group's capacity to be rational and to track the truth in its cognitive outputs, respectively. In section 6, I draw some conclusions.

2 Modelling a group as a distributed cognitive system

When does it make sense to consider a group of individuals as a distributed cognitive system rather than a mere collection of individuals? First, the group must count as a well-demarcated *system*, and, second, it must count as a system that produces *cognitive outputs*.

The first condition is met if and only if the group's collective behaviour is sufficiently integrated. A well organized expert panel, a group of scientific collaborators or the monetary policy committee of a central bank, for example, may have this property, whereas a random crowd of people at London's Leicester Square lacks the required level of integration. And the second condition is met if and only if the group is capable of producing outputs that have representational content; let me call these outputs 'collective judgments'. If a group's organizational structure – e.g. its procedures for generating a joint report – allows the group to make certain joint declarations that count as collective judgments, then the group has this property, whereas a group without any formal or informal organization, such as a random crowd at Leicester Square, lacks the required capacity.

At first sight, we may be reluctant to attribute judgments to groups over and above their individual members. But, as Goldman (2004, 12) has noted, in ordinary language, groups or collective organizations are often treated as subjects for the attribution of judgments. Goldman's example is the recent debate on what the FBI as a collective organization did or did not "know" prior to the terrorist attacks of 9/11. In addition to the literature on distributed cognition, there is now a growing literature in philosophy that considers conditions under which groups are sufficiently integrated to produce outputs that we normally associate with rational agency (e.g., Rovane 1998; Pettit 2003; List and Pettit 2005a, 2005b). Roughly, a sufficient level of integration is given in those cases in which it is pragmatically and explanatorily useful to describe the group's outputs in intentional terms (Dennett 1987), namely as the group's 'beliefs', 'judgments', 'commitments' or 'knowledge'. Arguably, this condition is satisfied by those groups that Hutchins, Giere, Knorr-Cetina and others have described as distributed cognitive systems.

In short, a necessary condition for distributed cognition in a group is the presence of an organizational structure that allows the group to produce collective judgments, i.e., collective outputs with representational content. Once this necessary condition is met, the group's performance as a distributed cognitive system depends on the nature of that organizational structure.

Consequently, to construct a model of a group as a distributed cognitive system, we need to represent not only the individual group members, but also the group's organizational structure. In the next section, I illustrate how we can think about this organizational structure in terms of a simple social-choice-theoretic model.

3 The concept of an aggregation procedure

How can we think about a group's organizational structure? Let me introduce the concept of an 'aggregation procedure' to represent (a key part of) a group's organizational structure. As defined in the theory of judgment aggregation (List and Pettit 2002, 2004; List 2006), an aggregation procedure is a mechanism by which a group can generate collective judgments on the basis of the group members' individual judgments (illustrated in table 1). Formally, an aggregation procedure is a function which assigns to each combination of individual judgments across the group members a corresponding set of collective judgments. A simple example is 'majority voting', whereby a group judges a given proposition to be true whenever a majority of group members judges it to be true. Below I discuss several other aggregation procedures.

Of course, an aggregation procedure captures only part of a group's organizational structure (which may be quite complex), and there are also multiple ways (both formal and informal ones) in which a group might implement such a procedure. Nonetheless, as argued below, aggregation procedures are key factors in determining a group's performance as a distributed cognitive system.

In the next section, I ask what properties a group's aggregation procedure must have for the group to be rational as a distributed cognitive system –

Table 1 An aggregation procedure

Input (individual judgments)

⇓ Aggregation procedure

Output (collective judgments)

specifically, consistent, but also complete, in its collective judgments – and in the subsequent section, I ask what properties it must have for the group to track the truth in these judgments. Both discussions illustrate that a group's performance as a distributed cognitive system depends on its aggregation procedure.

4 Rationality in a distributed cognitive system

Suppose a group is given a cognitive task involving the formation of collective judgments on some propositions. Can the group ensure the consistency of these judgments?

4.1 A majoritarian inconsistency

Consider an expert committee that has to prepare a report on the health consequences of air pollution in a big city, especially pollution by particles smaller than 10 microns in diameter. This is an issue on which there has recently been much debate in Europe. The experts have to make judgments on the following propositions:

p: The average particle pollution level exceeds $50\,\mu gm^{-3}$ (micrograms per cubic meter air).

$p \rightarrow q$: If the average particle pollution level exceeds $50\,\mu gm^{-3}$, then residents have a significantly increased risk of respiratory disease.

q: Residents have a significantly increased risk of respiratory disease.

All three propositions are complex factual propositions on which the experts may disagree.[4] Suppose the group uses majority voting as its aggregation procedure, i.e. the collective judgment on each proposition is the majority

[4] Propositions p and $p \rightarrow q$ can be seen as 'premises' for the 'conclusion' q. Determining whether p is true requires an evaluation of air quality measurements; determining whether $p \rightarrow q$ is true requires an understanding of causal processes in human physiology; finally, determining whether q is true requires a combination of the judgments on p and $p \rightarrow q$.

judgment on that proposition, as defined above. Now suppose the experts' individual judgments are as shown in table 2.

Table 2 A majoritarian inconsistency

	p	$p \to q$	q
Individual 1	True	True	True
Individual 2	True	False	False
Individual 3	False	True	False
Majority	True	True	False

Then a majority of experts judges p to be true, a majority judges $p \to q$ to be true, and yet a majority judges q to be false, an inconsistent collective set of judgments. The expert committee fails to be rational in the collective judgments it produces as a distributed cognitive system.

This problem – sometimes called a 'discursive dilemma' – illustrates that, under the initially plausible aggregation procedure of majority voting, a group acting as a distributed cognitive system may not achieve consistent collective judgments even when all group members hold individually consistent judgments (Pettit 2001; List and Pettit 2002, 2004; List 2006; the problem originally goes back to the so-called 'doctrinal paradox' first identified by Kornhauser and Sager 1986).

Is the present example just an isolated artefact, or can we learn something more general from it?

4.2 An impossibility theorem

Consider again any group of two or more individuals that is given the cognitive task to form collective judgments on a set of non-trivially interconnected propositions, as in the expert committee example.[5] Call an agent's judgments on these propositions 'complete' if, for each proposition-negation pair, the agent judges either the proposition or its negation to be true; and call

[5] Following List [2006], a set of propositions is 'non – trivially interconnected' if it is of one of the following forms (or a superset thereof): (i) it includes $k > 1$ propositions $p_1, ..., p_k$ and either their conjunction 'p_1 and ... and p_k' or their disjunction 'p_1 or p_2 or ... or p_k' or both (and the negations of all these propositions); (ii) it includes $k > 1$ propositions $p_1, ..., p_k$, another proposition q and either the proposition 'q if and only if (p_1 and ... and p_k)' or the proposition 'q if and only if (p_1 or p_2 or ... or p_k)' or both (and negations); (iii) it includes propositions p, q and $p \to q$ (and negations).

these judgments 'consistent' if the set of propositions judged to be true by the agent is a consistent set in the standard sense of propositional logic.[6]

Suppose now that each individual holds complete and consistent judgments on these propositions, and that the collective judgments are also required to be complete and consistent. One can then prove the following impossibility result (for a discussion of parallels and disanalogies between this result and Arrow's (1951) classical theorem, see List and Pettit 2004 and Dietrich and List 2005a).

Theorem (List and Pettit 2002). There exists no aggregation procedure generating complete and consistent collective judgments that satisfies the following three conditions simultaneously:

Universal domain. The procedure accepts as admissible input any logically possible combinations of complete and consistent individual judgments on the propositions.

Anonymity. The judgments of all individuals have equal weight in determining the collective judgments.

Systematicity. The collective judgment on each proposition depends only on the individual judgments on that proposition, and the same pattern of dependence holds for all propositions.

In short, majority voting is not the only aggregation procedure that runs into problems like the one illustrated in table 2 above. Any procedure satisfying universal domain, anonymity and systematicity does so. If these conditions are regarded as indispensable requirements on an aggregation procedure, then one has to conclude that a multi-member group acting as a distributed cognitive system cannot ensure the rationality of its collective judgments. But this conclusion would be too quick. The impossibility theorem should be seen as characterizing the logical space of aggregation procedures (List and Pettit 2002; List 2006). In particular, we can characterize different aggregation procedures in terms of which conditions they meet and which they violate.

If a group acting as a distributed cognitive system seeks to ensure the rationality of its collective judgments, the group must use an aggregation procedure that violates at least one of the conditions of the theorem.

4.3 First solution: relaxing universal domain

If the amount of disagreement in a particular group is limited or if the group has mechanisms in place for reducing disagreement – such as mechanisms of group deliberation – the group might use an aggregation procedure that

[6] This consistency notion is stronger than that in List and Pettit (2002). But when the present consistency notion is used, no deductive closure requirement needs to be added.

violates universal domain. For example, a deliberating group that successfully avoids combinations of individual judgments of the kind in table 2 might use majority voting as its aggregation procedure and yet generate rational collective judgments.

But this solution does not work in general. Even in an expert committee whose task is to make judgments on factual matters without conflicts of interest, disagreement may still be significant and pervasive. Although one can study conditions that make the occurrence of judgment combinations of the kind in table 2 less likely (Dryzek and List 2003; List 2002), I set this issue aside here and assume that groups that are faced with primarily cognitive tasks (as opposed to primarily political ones, for example) should normally use aggregation procedures satisfying universal domain.

4.4 Second solution: relaxing anonymity

It can be shown that, if anonymity is relaxed but the other two conditions are retained, the only possible aggregation procedure is a 'dictatorial procedure', whereby the collective judgments are always those of some antecedently fixed group member (the 'dictator') (Pauly and van Hees 2005). Some groups might put one individual – say a committee chair – in charge of forming its collective judgments. But this solution clearly conflicts with the idea of collectively distributed cognition, and as discussed below, a group organized in this dictatorial way loses out on the epistemic advantages of distributed cognition.

However, below I also suggest that a group acting as a distributed cognitive system may sometimes benefit from relaxing anonymity together with systematicity and implementing a division of cognitive labour whereby different components of a complex cognitive task are allocated to different subgroups.

4.5 Third solution: relaxing systematicity

A potentially promising solution lies in relaxing systematicity, i.e., treating different propositions differently in the process of forming collective judgments. For the purposes of a given cognitive task, a group may designate some propositions as 'premises' and others as 'conclusions' and assign epistemic priority either to the premises or to the conclusions (for a more extensive discussion of this process, see List 2006).

If the group assigns priority to the premises, it may use the so-called 'premise – based procedure', whereby the group first makes a collective judgment on each premise by taking a majority vote on that premise and then derives its collective judgments on the conclusions from these collective judgments on the premises. In the expert committee example, propositions p

and $p \rightarrow q$ might be designated as premises (perhaps on the grounds that p and $p \rightarrow q$ are more basic than q), and proposition q might be designated as a conclusion. The committee might then take majority votes on p and $p \rightarrow q$ and derive its judgment on q from its judgments on p and $p \rightarrow q$.[7]

Alternatively, if the group assigns priority to the conclusions, it may use the so-called 'conclusion – based procedure', whereby the group takes a majority vote only on each conclusion and makes no collective judgments on the premises. In addition to violating systematicity, this aggregation procedure fails to produce complete collective judgments. But sometimes a group is required to make judgments only on conclusions, but not on premises, and in such cases incompleteness in the collective judgments on the premises may be defensible.

The premise- and conclusion-based procedures are not the only aggregation procedures violating systematicity. Further important possibilities arise when both systematicity and anonymity are relaxed. The group can then use an aggregation procedure that not only assigns priority to the premises, but also assigns different such premises to different subgroups and thereby implements a particularly clear form of distributed cognition. Specifically, the group may use the so-called 'distributed premise-based procedure'. Here different individuals specialize on different premises and give their individual judgments only on these premises. Now the group makes a collective judgment on each premise by taking a majority vote on that premise among the relevant 'specialists', and then the group derives its collective judgments on the conclusions from these collective judgments on the premises. This procedure is discussed in greater detail below.

For many cognitive tasks performed by groups, giving up systematicity and using a (regular or distributed) premise-based or conclusion-based procedure may be an attractive way to avoid the impossibility result explained above. Each of these procedures allows a group to produce rational collective judgments. Arguably, a premise-based or distributed premise-based procedure makes the group's performance as a unified cognitive system particularly visible. A group using such a procedure acts as a reason-driven system when it derives its collective judgments on conclusions from its collective judgments on relevant premises.

However, giving up systematicity comes with a price. Aggregation procedures that violate systematicity may be vulnerable to manipulation by prioritizing propositions strategically, and strategic agents with agenda-setting influence over the group might exploit these strategic vulnerabilities.

[7] In the present example, the truth-value of q is not always settled by the truth-values of p and $p \rightarrow q$; so the group may need to stengthen its premises in order to make them sufficient to determine its judgment on the conclusion.

For example, in the case of a regular premise-based procedure, the collective judgments may be sensitive to the choice of premises. In the example of table 2, if p and $p \rightarrow q$ are designated as premises, then all three propositions, p, $p \rightarrow q$ and q, are collectively judged to be true. If p and q are designated as premises, then p is judged to be true and both q and $p \rightarrow q$ are judged to be false; finally, if q and $p \rightarrow q$ are designated as premises, then $p \rightarrow q$ is judged to be true, and both p and q are judged to be false. Although there seems to be a natural choice of premises in the present example, namely p and $p \rightarrow q$, this may not generally be the case, and the outcome of a premise-based procedure may therefore depend as much on the choice of premises as it depends on the individual judgments to be aggregated. In the present example, an environmental activist may prefer to prioritize the propositions in such a way as to bring about the collective judgment that proposition q is true, while a transport lobbyist may prefer to prioritize them in such a way as to bring about the opposite judgment on q.

Under the distributed premise-based procedure, an additional sensitivity to the choice of 'specialists' on each premise arises. Likewise, in the case of the conclusion-based procedure, the choice of conclusions obviously matters, since the group makes collective judgments only on these conclusions and on no other propositions.[8]

4.6 Fourth solution: permitting incomplete collective judgments

The first three solutions proposed in response to the impossibility theorem above have required relaxing one of the three minimal conditions on how individual judgments are aggregated into collective judgments. The present solution preserves these minimal conditions, but weakens the requirements on the collective judgments themselves by permitting incompleteness in these judgments (see also List 2006).

If a group acting as an overall cognitive system is prepared to refrain from making a collective judgment on some propositions – namely on those on which there is too much disagreement between the group members – then it may use an aggregation procedure such as the 'unanimity procedure', whereby the group makes a judgment on a proposition if and only if the group members unanimously endorse that judgment. Propositions judged to be true by all members are collectively judged to be true; and ones judged to be false by all members are collectively judged to be false; no collective judgment is made on any other propositions. (Instead of the unanimity procedure, the group

[8] It can be shown that in some important respects, the premise-based procedure is more vulnerable to strategic manipulation than the conclusion-based procedure. See Dietrich and List (2005b).

might also use 'supermajority voting' with a sufficiently large supermajority threshold.)

Groups operating in a strongly consensual manner may well opt for this solution, but in many cases making no judgment on some propositions is simply not an option. For example, when an expert committee is asked to give advice on a particular issue, it is usually expected to take a determinate stance on that issue.

4.7 Lessons to be drawn

I have shown that a group's capacity to form rational collective judgments depends on the group's aggregation procedure: a group acting as a distributed cognitive system can ensure the rationality of its collective judgments on some non-trivially interconnected propositions only if it uses a procedure that violates one of universal domain, anonymity or systematicity or that produces incomplete collective judgments. Moreover, different aggregation procedures may lead to different collective judgments for the same combination of individual judgments. As an illustration, table 3 shows the collective judgments for the individual judgments in table 2 under different aggregation procedures.

If we were to assess a group's performance as a distributed cognitive system solely on the basis of whether the group's collective judgments are rational, this would give us insufficient grounds for selecting a unique aggregation procedure. As I have illustrated, many different aggregation procedures generate consistent collective judgments, and even if we require completeness in addition to consistency, several possible aggregation procedures remain. To

Table 3 Different aggregation procedures applied to the individual judgments in table 2

	p	$p \rightarrow q$	q
Majority voting*	True	True	False
Premise-based procedure with p, $p \rightarrow q$ as premises	True	True	True
Conclusion-based procedure with q as conclusion	No judgment	No judgment	False
Distributed premise-based procedure with individual 1 specializing on p and individual 2 specializing on $p \rightarrow q$	True	False	False
Unanimity procedure	No judgment	No judgment	No judgment
Dictatorship of individual 3	False	True	False

* inconsistent

recommend a suitable aggregation procedure that a group can use for a given cognitive task, the question of whether the group produces rational collective judgments is, by itself, not a sufficient criterion.

5 *Truth-tracking in a distributed cognitive system*

Can a group acting as a distributed cognitive system generate collective judgments that track the truth? Following Nozick (1981), a system 'tracks the truth' on some proposition *p* if two conditions are met. First, if – actually or counterfactually – *p* were true, the system would judge *p* to be true. Second, if – actually or counterfactually – *p* were not true, the system would not judge *p* to be true. These conditions can be applied to any cognitive system, whether it consists just of a single agent or of multiple agents acting together. In particular, if a group's organizational structure allows the group to form collective judgments, then one can ask whether these judgments satisfy Nozick's two conditions.

As a simple measure of how well a system satisfies Nozick's two conditions, I consider two conditional probabilities (List 2006): the probability that the system judges *p* to be true given that *p* is true, and the probability that the system does not judge *p* to be true given that *p* is false. Call these two conditional probabilities the system's 'positive' and 'negative reliability' on *p*, respectively.

By considering a group's positive and negative reliability on various propositions under different aggregation procedures and different scenarios, I now show that it is possible for a group acting as a distributed cognitive system to track the truth, but that, once again, the aggregation procedure affects the group's success.

5.1 The first scenario and its lesson: epistemic gains from democratization

Suppose that a group is given the cognitive task of making a collective judgment on a single factual proposition, such as proposition *p* in the expert committee example above. As a baseline scenario (e.g., Grofman, Owen and Feld 1983), suppose that the group members hold individual judgments on proposition *p*, where two conditions are met. First, each group member has the same positive and negative reliability *r* on proposition *p*, where $1 > r > 1/2$ (the 'competence' condition); so individual judgments are noisy but biased towards the truth. Second, the judgments of different group members are mutually independent (the 'independence' condition). (Obviously, it is also important to study scenarios where these conditions are violated, and below I consider some such scenarios.[9])

A group acting as a distributed cognitive system must use an aggregation procedure to make its collective judgment on *p* based on the group members' individual judgments on *p*. What is the group's positive and negative reliability on *p* under different aggregation procedures?

Let me compare three different procedures: first, a dictatorial procedure, where the collective judgment is always determined by the same fixed group member; second, the unanimity procedure, where agreement among all group members is necessary for reaching a collective judgment; and third, majority voting, which perhaps best implements the idea of a democratically organized form of distributed cognition (at least in the case of a single proposition).

Under a dictatorial procedure, the group's positive and negative reliability on *p* equals that of the dictator, which is *r* by assumption.

Under the unanimity procedure, the group's positive reliability on *p* equals r^n, which approaches 0 as the group size increases, but its negative reliability on *p* equals $1 - (1 - r)^n$, which approaches 1 as the group size increases. This means that the unanimity procedure is good at avoiding false positive judgments, but bad at reaching true positive ones. A determinate collective judgment on *p* is reached only if all individuals agree on the truth-value of *p*; if they don't agree, no collective judgment on *p* is made.

Finally, under majority voting, the group's positive and negative reliability on *p* approaches 1 as the group size increases. Why does this result hold? Each individual has a probability $r > 0.5$ of making a correct judgment on *p*; by the law of large numbers, the proportion of individuals who make a correct judgment on *p* approaches $r > 0.5$ as the group size increases and thus constitutes a majority with a probability approaching 1. Informally, majority voting allows the group to extract the signal from the group members' judgments, while filtering out the noise. This is the famous 'Condorcet jury theorem' (e.g., Grofman, Owen and Feld 1983).

Table 4 shows the group's positive and negative reliability on *p* under majority voting and under a dictatorial procedure, and tables 5 and 6 show, respectively, the group's positive and negative reliability on *p* under a dictatorial procedure and under the unanimity procedure. In each case, individual group members are assumed (as an illustration) to have a positive and neg-

[9] Cases where different individuals have different levels of reliability are discussed, for example, in Grofman, Owen and Feld (1983) and Borland (1989). Cases where there are dependencies between different individuals' judgments are discussed, for example, in Ladha (1992), Estlund (1994) and Dietrich and List (2004). Cases where individuals express their judgments strategically rather than truthfully are discussed in Austen-Smith and Banks (1996).

Table 4 The group's positive and negative reliability on p: majority voting (top curve); dictatorship (bottom curve) (setting r = 0.54 as an illustration)

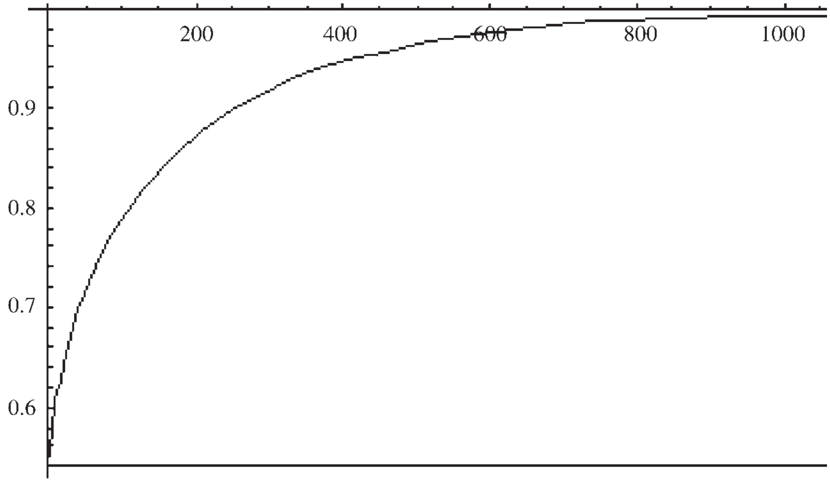


Table 5 The group's positive reliability on p: dictatorship (top curve); unanimity procedure (bottom curve) (setting r = 0.54 as an illustration)

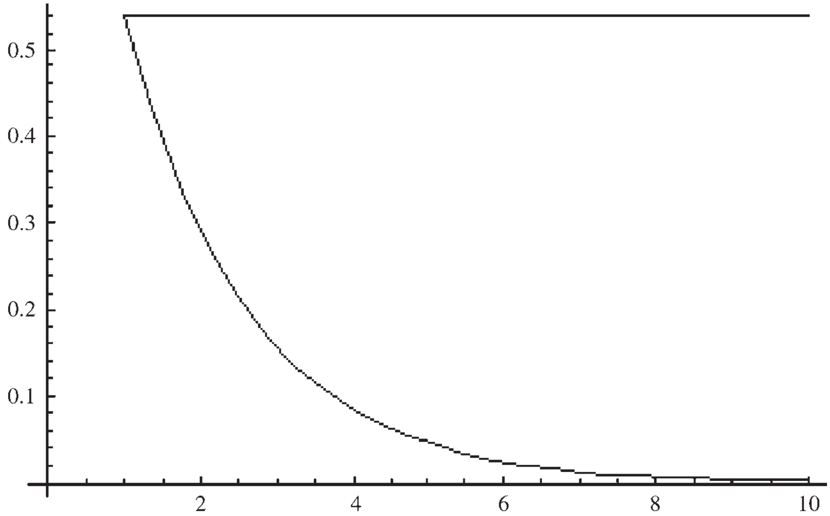
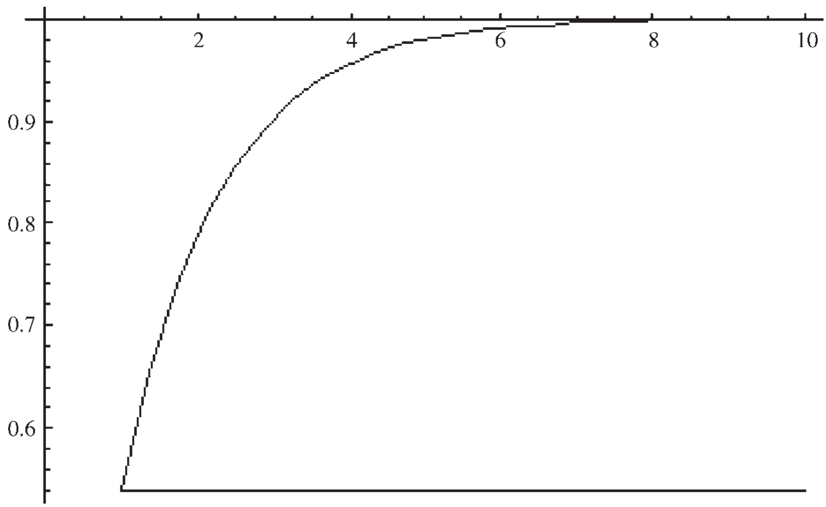


Table 6 The group's negative reliability on p: unanimity procedure (top curve); dictatorship (bottom curve) (setting r = 0.54 as an illustration)



ative reliability of $r = 0.54$ on *p*. In all tables, the group size is on the horizontal axis and the group's reliability on the vertical axis.[10]

What lessons can be drawn from this first scenario? If individuals are independent, fallible, but biased towards the truth, majority voting outperforms both dictatorial and unanimity procedures in terms of maximizing the group's positive and negative reliability on *p*. The unanimity procedure is attractive only in those special cases where the group seeks to minimize the risk of making false positive judgments, such as in some jury decisions. A dictatorial procedure fails to pool the information held by different individuals.

Hence, when a group acting as a distributed cognitive system seeks to track the truth, there may be 'epistemic gains from democratization', i.e. from making a collective judgment on a given proposition democratically by using majority voting. More generally, even when individual reliability differs between individuals, a weighted form of majority voting still outperforms a dictatorship by the most reliable individual: each individual's vote simply

[10] The present curves are the result of averaging between two separate curves for even- and odd-numbered group sizes. When the group size is an even number, the group's reliability may be lower because of the possibility of majority ties.

needs to have a weight proportional to $log(r/(1-r))$, where r is the individual's reliability on the proposition in question (Ben-Yashar and Nitzan 1997).

5.2 The second scenario and its lesson: epistemic gains from disaggregation

Suppose now that a group is given the cognitive task of making a collective judgment not only on a single factual proposition, but on a set of interconnected factual propositions. As an illustration, suppose that there are $k > 1$ premises $p_1, ..., p_k$ and a conclusion q, where q is true if and only if the conjunction of $p_1, ..., p_k$ is true. This structure also allows representing a variant of the expert committee example above. For extensive discussions of the present scenario and other related scenarios, see Bovens and Rabinowicz (2005) and List (2005a, 2006). Analogous points apply to the case where q is true if and only if the disjunction of $p_1, ..., p_k$ is true.

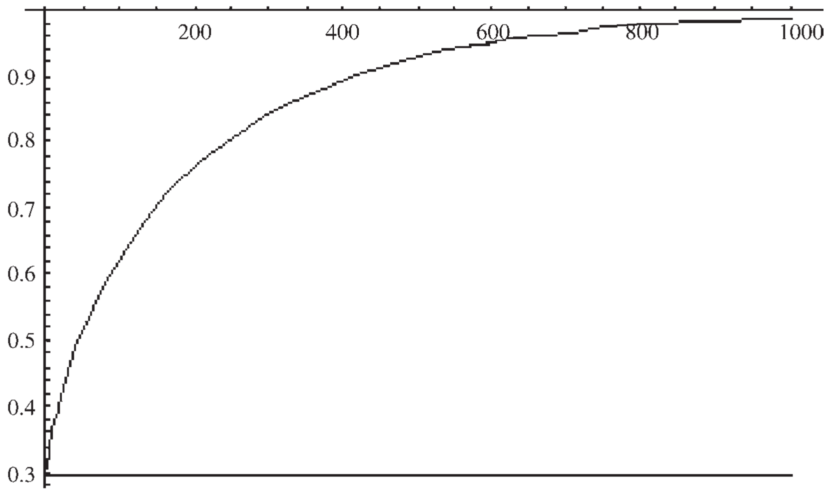
In this case of multiple interconnected propositions, individuals cannot generally have the same reliability on all propositions. Suppose, as an illustration, that each individual has the same positive and negative reliability r on each premise $p_1, ..., p_k$ and makes independent judgments on different premises. Then each individual's positive reliability on the conclusion q is r^k, which is below r and often below 0.5 (whenever $r <$ k-th root of 0.5), while his or her negative reliability on q is above r. Here individuals are much worse at detecting the truth of the conclusion than the truth of each premise, but much better at detecting the falsehood of the conclusion than the falsehood of each premise. In the expert committee example, it might be easier to make correct judgments on propositions p and $p \rightarrow q$ than on proposition q. Of course, other scenarios can also be constructed, but the point remains that individuals typically have different levels of reliability on different propositions (List 2006).

What is the group's positive and negative reliability on the various propositions under different aggregation procedures? As before, suppose the judgments of different group members are mutually independent.

Majority voting performs well only on those propositions on which individuals have a positive and negative reliability above 0.5. As just argued, individuals may not meet this condition on all propositions. Moreover, majority voting does not generally produce consistent collective judgments (on the probability of majority inconsistencies, see List 2005a). Let me now compare dictatorial, conclusion-based and premise-based procedures.

Under a dictatorial procedure, the group's positive and negative reliability on each proposition equals that of the dictator; in particular, the probability that *all* propositions are judged correctly is r^k, which may be very low, especially when the number of premises k is large.

Table 7 The group's probability of judging all propositions correctly: premise-based procedure (top curve); dictatorship (bottom curve) (setting r = 0.54 as an illustration)



Under the conclusion-based procedure, unless individuals have a high reliability on each premise, namely $r > k$-th root of 0.5 (e.g. 0.71 when $k = 2$, or 0.79 when $k = 3$), the group's positive reliability on the conclusion q approaches 0 as the group size increases. Its negative reliability on q approaches 1. Like the unanimity procedure in the single-proposition case, the conclusion-based procedure is good at avoiding false positive judgments on the conclusion, but (typically) bad at reaching true positive ones (see also Bovens and Rabinowicz 2005).

Under the premise-based procedure, the group's positive and negative reliability on every proposition approaches 1 as the group size increases. This result holds because, by the Condorcet jury theorem as stated above, the group's positive and negative reliability on each premise $p_1, ..., p_k$ approaches 1 with increasing group size, and therefore the probability that the group derives a correct judgment on the conclusion also approaches 1 with increasing group size.

As illustration, suppose that there are $k = 2$ premises and individuals have a positive and negative reliability of $r = 0.54$ on each premise. Table 7 shows the group's probability of judging *all* propositions correctly under the premise-based procedure and under a dictatorial procedure. Tables 8 and 9 show, respectively, the group's positive and negative reliability on the conclusion q under a dictatorial procedure and under the conclusion-based procedure.

Table 8 The group's positive reliability on the conclusion q: dictatorship (top curve); conclusion-based procedure (bottom curve) (setting r = 0.54 as an illustration)

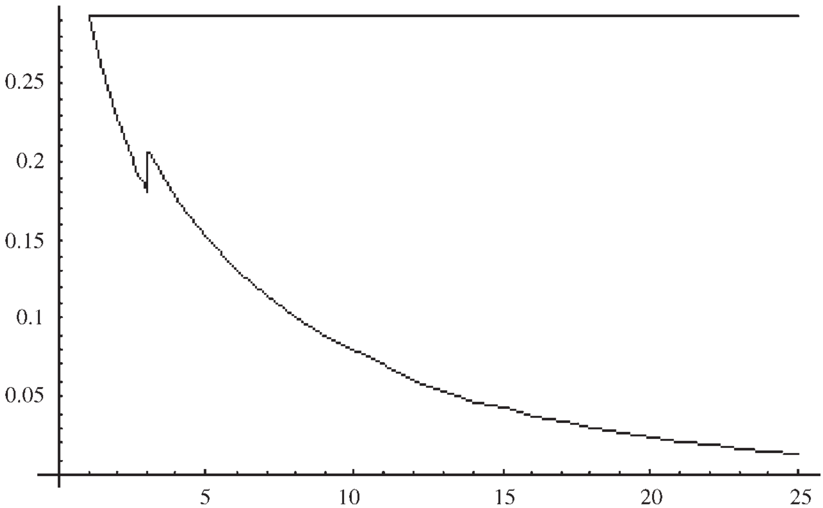
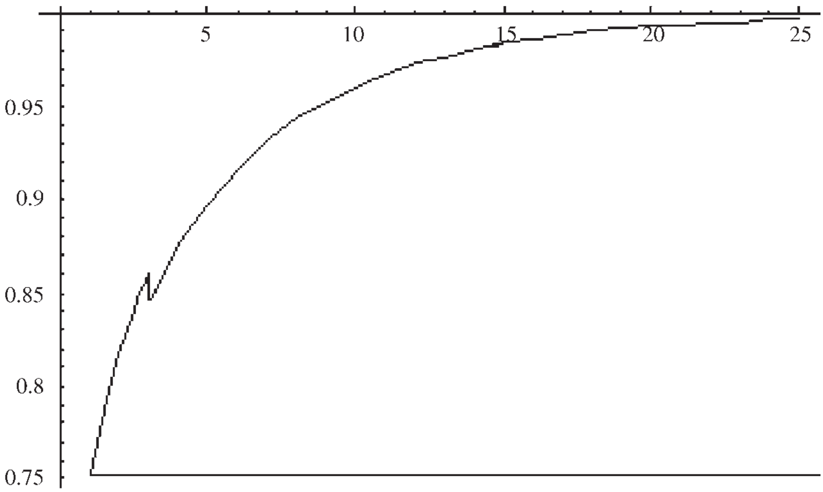


Table 9 The group's negative reliability on the conclusion q: conclusion-based procedure (top curve); dictatorship (bottom curve) (setting r = 0.54 as an illustration)



What lessons can be drawn from this second scenario? Under the present assumptions, the premise-based procedure outperforms both dictatorial and conclusion-based procedures in terms of simultaneously maximizing the group's positive and negative reliability on every proposition. Like the unanimity procedure before, the conclusion-based procedure is attractive only when the group seeks to minimize the risk of making false positive judgments on the conclusion; again, a dictatorial procedure is bad at information pooling.

Hence, if a larger cognitive task such as making a judgment on some conclusion can be disaggregated into several smaller cognitive tasks such as making judgments on relevant premises, then there may be 'epistemic gains from disaggregation', i.e. from making collective judgments on that conclusion on the basis of separate collective judgments on those premises. (For further results and a discussion of different scenarios, see Bovens and Rabinowicz 2005 and List 2006.)

5.3 The third scenario and its lesson: epistemic gains from distribution

When a group is faced with a complex cognitive task that requires making judgments on several propositions, different members of the group may have different levels of expertise on different propositions. This is an important characteristic of many committees, groups of scientific collaborators, large organizations, and so on. Moreover, each individual may lack the temporal, computational and informational resources to become sufficiently reliable on every proposition. If we take this problem into account, can we improve on the premise-based procedure?

Suppose, as before, that a group has to make collective judgments on $k > 1$ premises $p_1, ..., p_k$ and a conclusion q, where q is true if and only if the conjunction of $p_1, ..., p_k$ is true. Instead of requiring every group member to make a judgment on every premise, we might partition the group into k subgroups (for simplicity, of approximately equal size), where the members of each subgroup specialize on one premise and make a judgment on that premise alone. Instead of a using a regular premise-based procedure as in the previous scenario, the group might now use a distributed premise-based procedure: the collective judgment on each premise is made by taking a majority vote within the subgroup specializing on that premise, and the collective judgment on the conclusion is then derived from these collective judgments on the premises.

When does the distributed premise-based procedure outperform the regular premise-based procedure at maximizing the group's probability of making correct judgments on the propositions?

Intuitively, there are two effects here that pull in opposite directions. First, there may be 'epistemic gains from specialization': individuals may become more reliable on the proposition on which they specialize. But, second, there may also be 'epistemic losses from lower numbers': each subgroup voting on a particular proposition is smaller than the original group (it is only approximately $1/k$ the size of the original group when there are k premises), which may reduce the benefits from majoritarian judgment aggregation on that proposition.

Whether or not the distributed premise-based procedure outperforms the regular premise-based procedure depends on which of these two opposite effects is stronger. Obviously, if there were no epistemic gains from specialization, then the distributed premise-based procedure would suffer only from losses from lower numbers on each premise and would therefore perform worse than the regular premise-based procedure. On the other hand, if the epistemic losses from lower numbers were relatively small compared to the epistemic gains from specialization, then the distributed premise-based procedure would outperform the regular one. The following result holds:

Theorem. For any group size n (divisible by k), there exists an individual (positive and negative) reliability level $r^* > r$ such that the following holds: if, by specializing on some proposition p, individuals achieve a reliability above r^* on p, then the majority judgment on p in a subgroup of n/k specialists (each with reliability r^* on p) is more reliable than the majority judgment on p in the original group of n non-specialists (each with reliability r on p).

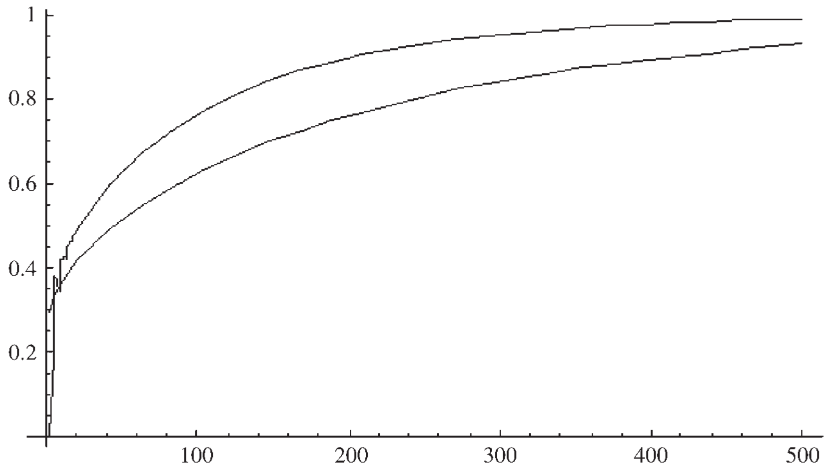
Hence, if by specializing on one premise, individuals achieve a reliability above r^* on that premise, then the distributed premise-based procedure outperforms the regular premise-based procedure. How great must the reliability increase from r to r^* be to have this effect? Strikingly, a small reliability increase typically suffices. Table 10 shows some sample calculations. For example, when there are $k = 2$ premises, if the original individual reliability was $r = 0.52$, then a reliability above $r^* = 0.5281$ after specialization suffices; it was $r = 0.6$, then a reliability above $r^* = 0.6393$ after specialization suffices.

Table 10 Reliability increase from r to r required to outweigh the loss from lower numbers*

	$k = 2, n = 50$			$k = 3, n = 51$			$k = 4, n = 52$		
$r =$	0.52	0.6	0.75	0.52	0.6	0.75	0.52	0.6	0.75
$r^* =$	0.5281	0.6393	0.8315	0.5343	0.6682	0.8776	0.5394	0.6915	0.9098

Table 11 shows the group's probability of judging *all* propositions correctly under regular and distributed premise-based procedures, where there are $k = 2$ premises and where individuals have positive and negative reliabilities of $r = 0.54$ and $r^* = 0.58$ before and after specialization, respectively.

Table 11 The group's probability of judging all propositions correctly: distributed (top curve) and regular premise-based procedure (bottom curve) (setting r = 0.54 and r = 0.58 as an illustration)*



What lessons can be drawn from this third scenario? Even when there are only relatively modest gains from specialization, the distributed premise-based procedure may outperform the regular premise-based procedure in terms of maximizing the group's positive and negative reliability on every proposition.

Hence there may be 'epistemic gains from distribution': if a group has to perform a complex cognitive task, the group may benefit from subdividing the task into several smaller tasks and distributing these smaller tasks across multiple subgroups. Plausibly, such division of cognitive labour is the mechanism underlying the successes of collectively distributed cognition in science, as investigated by Knorr Cetina (1999), Giere (2002) and others. The research practices in large-scale collaborative research projects, such as those in high-energy physics or in other large expert teams as mentioned above, rely on mechanisms similar to those represented, in a stylized form, by the distributed premise-based procedure.

In conclusion, a group acting as a distributed cognitive system can succeed at tracking the truth, but the group's aggregation procedure plays an important role in determining the group's success.

6 Concluding remarks

I have discussed collectively distributed cognition from a social-choice-theoretic perspective. In particular, I have introduced the emerging theory of judgment aggregation to propose a way of modelling a group as a distributed cognitive system, i.e. as a system that can generate collective judgments. Within this framework, I have asked whether such a group can be rational and track the truth in its collective judgments. My main finding is that a group's performance as a distributed cognitive system depends crucially on its aggregation procedure, and I have investigated *how* the aggregation procedure matters.

With regard to a group's rationality as a distributed cognitive system, I have discussed an impossibility theorem by which we can characterize the logical space of aggregation procedures that a group can use to generate rational collective judgments. No aggregation procedure generating consistent and complete collective judgments can simultaneously satisfy universal domain, anonymity and systematicity. To find an aggregation procedure that produces rational collective judgments, it is therefore necessary to relax one of universal domain, anonymity or systematicity, or to weaken the requirement of rationality itself by permitting incomplete collective judgments. Which relaxation is most defensible depends on the group and cognitive task in question.

With regard to a group's capacity to track the truth as a distributed cognitive system, I have identified three effects that are relevant to the design of a good aggregation procedure: there may be epistemic gains from democratization, disaggregation and distribution. Again, the applicability and magnitude of each effect depends on the group and cognitive task in question, and there may not be a 'one size fits all' aggregation procedure that is best for all groups and all cognitive tasks. But the fact that a group may sometimes benefit from the identified effects reinforces the potential of epistemic gains through collectively distributed cognition.

The present results give a fairly optimistic picture of a group's capacity to perform as a distributed cognitive system. I have thereby focused on cooperative rather than competitive practices within groups or communities. It is an important empirical question how pervasive such cooperative practices are and how often the favourable conditions such practices require are met. Clearly, scientific communities are characterized by both competitive and cooperative practices. Much research in the sociology and economics of science has focused on competitive practices (as evidenced by the theme of the 2005 Conference on New Political Economy). There has also been much research on rationality failures and inefficiencies that can arise in groups trying to perform certain tasks at a collective level. Public choice theorists, in particular, have highlighted the impossibility results on democratic aggregation and the pervasiveness of suboptimal equilibria in various collective interactions.

Clearly, the details of my rather more optimistic results depend on various assumptions and may change with changes in these assumptions. But my aim has not been to argue that *all* groups acting as distributed cognitive systems perform well; indeed, this claim is likely to be false. Rather, my aim has been to show that successful distributed cognition in a group is *possible* and to illustrate the usefulness of the theory of judgment aggregation for investigating how it is possible and under what conditions.

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Distributed Cognition

Comment by
Siegfried K. Berninghaus

1 Brief summary of List's contribution

From a general point of view the contribution by Christian List is concerned with such types of social processes which can be called *cognitive* and *distributed*. Distributed cognition is not a new field which is applied for the first time to problems in social science in this paper. However, more often we observe ideas of distributed cognition in various and different fields of research, for example in the field of artificial intelligence or distributed computing.

To learn some basic facts about distributed cognition, we need not even go deeply into the artificial intelligence literature. In a nutshell, we can observe most aspects and problems of distributed cognition in our university system itself. In some sense the numerous committees established in universities, for example, committees on allocating the university budget, or committees on establishing new studies, can be regarded as expert panels representing distributive intelligence. There is a strong connection of List's theoretical paper with practical problems of scientific competition which is the main topic of this conference.

In his paper, Christian List discusses distributed cognition from the particular perspective of Social Choice Theory. He basically refers to results on group aggregation procedures in *judgement aggregation*. Such procedures are used to combine individual beliefs and judgements of the members of a group into collective beliefs and judgements. We know from the pioneering work by Christian List himself and other authors (see, for example, List and Pettit (2002), List and Pettit (2004), Dietrich and List (2004) that judgement aggregation procedures may suffer from serious inconsistencies in the collective decisions, i.e., that a group may not achieve consistent judgements although all group members individually hold consistent beliefs. A famous impossibility result shows that consistent collective judgements are not possible provided the aggregation procedure satisfies some mild conditions.[1] These conditions are *universal domain* (any logically possible combination of personal sets of judgements is admissible as aggregation input), *anonymity* (collective set of judgements are invariant under any permutation of the individuals) and *systematicity* (collective judgement depends exclusively on

[1] There exist some analogies to Arrow's famous impossibility theorem on the aggregation of individual preferences (for details see List and Pettit 2004).

the pattern of individual judgements). In the literature on judgement aggregation one can find various strategies to escape from this impossibility result. One strategy could be to give up some of the basic requirements on the aggregation rule or to consider incomplete judgements.

In his paper, List deals with the problem whether collective beliefs or judgements constitute group knowledge. He considers the *positive* and *negative reliability* on propositions under various different aggregation procedures and scenarios. Suppose that each group member has a certain degree of reliability on a factual proposition *p*.

1. In scenario 1, the effect of three different aggregation rules on the groups reliability on the same factual proposition is investigated.
2. In scenario 2, the focus is on the collective judgement on a collection of interconnected factual propositions.
3. Scenario 3 is concerned with gains from specialization which may arise from splitting up the original group into expert panels who are responsible for making judgements only on a small subset of interconnected propositions.

2 *Questions and comments*

List's approach to judgement aggregation is an interesting and innovative contribution to Social Choice. Compared with most papers presented at this conference, this contribution is rather abstract. However, this may not be a disadvantage at all. Quite to the contrary, this paper contributes substantially to basic research in group rationality and, therefore, to basic research in the theory of scientific competition, too.

2.1 General comments

1. In the paper, *institutional design* is more or less identified with the judgement aggregation rule. I think this is a rather narrow interpretation of the processes taking place in institutions. Of course, aggregation rules are the core part of institutional design but important *strategic aspects* are missing. Why don't group members try to manipulate either the aggregation procedure by itself or why don't they communicate with other group members to make bargained arrangements?

2. In the logical framework of List and Pettit, the individuals do not express preferences (like in Arrow's Social Choice framework) but make statements about their beliefs in the truth of propositions. Can we interpret the individual reliability probabilities *p* as *degrees of confirmation* (in the sense of Carnap's

Inductive Logic[2])? Then the *p*'s could differ even in a group of homogenous individuals because of different individually accumulated empirical evidence (for the truth of a proposition). Therefore, the judgement aggregation problem could be transformed into a belief aggregation problem.

Belief aggregation problems were considered, for example, by DeGroot (1974), who proved in an elegant approach (via Markovian process arguments) that the weights, which each group member attaches to the subjective beliefs of the remaining group members converge to a common weight scheme which may generate common subjective beliefs in the group.

2.2 Specific comments

1. First comment on scenario 1: The most important conclusion of this scenario is that there exist highest epistemic gains from democratization (majority voting) when compared with two alternative aggregation procedures (dictatorial and unanimity).

Is a set of (two) alternatives really large enough to draw definitive conclusions on the superiority of majority voting? Don't there exist many more voting procedures?

2. Second comment on scenario 1: Results in this scenario are derived from rather restrictive formal assumptions on the majority voting aggregation procedure. More concretely, group members must have identical reliability in judging the truth of a proposition and, moreover, they should act *independently* from each other.

The results on majority voting are based on the simple relation

$$r_{group} := Prob\left(\left\{\sum_i X_i \geq (m+1)\right\}\right)$$

where $X_i \in \{0,1\}$ denotes the random variable that group member i judges the proposition in question as being true and $n = 2m+1$ is the group size which is supposed to be odd. The group's reliability r_{group} on a proposition can then be calculated explicitly when the X_i are stochastically independent and its limit can be determined when the group size increases.

List himself mentions that these restrictive assumptions on judgement aggregation can be relaxed without changing his results. Boland (1989), for example, shows that the aggregation results on group reliability still hold when the group members' independence assumption is substituted by a less restrictive assumption postulating the existence of an opinion leader in the

[2] That is, we define the reliability p of group member to judge a proposition to be true as the degree of confirmation $c(h\,|\,e)$ of proposition h supported by accumulated empirical evidence e (see Carnap and Jeffrey 1971).

group. There is not a unique way to escape from the independence assumption. Many alternative assumptions modelling dependence between group member in judgement aggregation exist. Some may be more and some may be less reasonably be applied to this particular Social Choice framework. In the following, I would like to suggest two interesting extensions of the independence assumptions.

a) Suppose the $\{X_i\}_i$ are *exchangeable random variables,* i.e.

$$Prob\,(\{X_1 = x_1, ..., X_n = x_n\}) = Prob\,(\{X_{\pi(1)} = x_{(1)}, ..., X_{\pi(n)}\})$$

for any permutation $\pi : \{1, ..., n\} \rightarrow \{1, ..., n\}$ and $x_i \in \{0, 1\}$.

Exchangeability has an interesting interpretation: According to *De Finetti's* theorem (combined with Aldous's results 1985) on finite exchangeable random variables the reliability of the judgements of the group members

$$Prob\ (\{X_1 = x_1,\ ...,\ X_n = x_n\})$$

can be (approximately) regarded as a "mixture" of the judgements of independently judging group members, where the weights are determined by "collective events" which concern the whole group. In other words, we still assume some type of independence in judgement making on the individual level which, however, has to be conditioned on collective events.[3]

I am not sure how the results of majority voting in the modified model with exchangeable agents will change. Because of the equivalence of exchangeability and conditional independence, I would conjecture that List's results will remain valid conditionally on the occurrence of specific collective events.

b) It is reasonable to assume that all human groups are composed of individuals having many social interactions with their neighbors. In other words, each group can be characterized by a *social network structure* which has an important impact on individual judgements. As an illustrative example of a simple network structure see Figure 1, where each group member is connected to 4 other group members (one neighbor on the right, one on the left, one above, and one below).

Being connected to some group members can be interpreted in this framework as being influenced by the judgement of the neighbors.

Formally, a group's judgement configuration in period t can be defined as a mapping

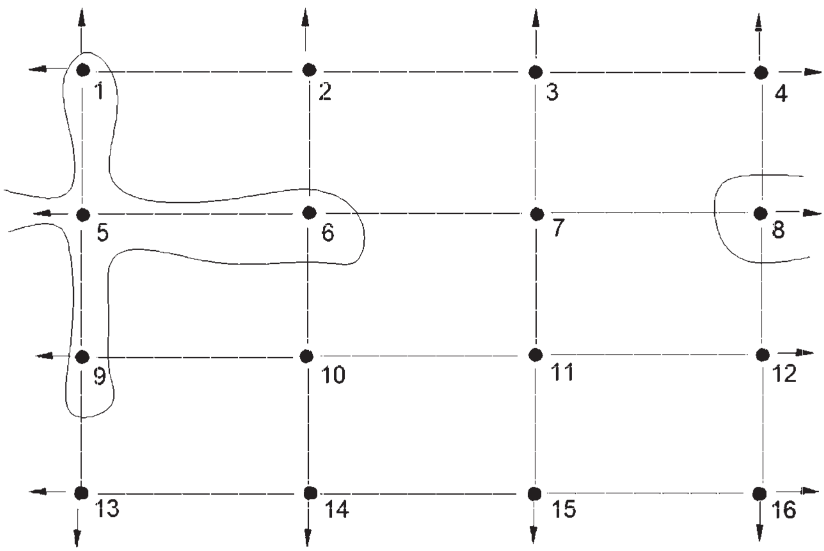
$$\eta_t : S \rightarrow \{0, 1\},$$

where S denotes the sites of a graph which represents the local interaction structure imposed on the whole group.[4] There exist many models in the lit-

[3] In technical terms, collective events are elements of the *terminal σ-algebra* generated by the random variables $\{X_i\}_i$

[4] In the example presented in Figure 1, sites of the interaction graph are the points 1–9.

Figure 1 2-dimensional local interaction



erature (for example, "voter models", "contagion models", see Liggett 1985, Morris 2000) dealing with the evolution of decision making in groups with social network structures. In a voter model, for example, it is assumed that the rate at which each group member $x \in S$ flips from judging a proposition being true to being false is given by $\frac{1}{4} \sum_{\{y||y-x|=1\}} 1_{\eta(y) \neq \eta(x)}$. In the voter model, one is interested in the temporal evolution of the probabilities of judgement configurations η_t when t increases. In our simple 2-dimensional social network the process $\{\eta_t\}_t$ has two limit distributions. Either we have $\eta^*(\cdot) = 1$ with probability to one or we have $\eta^*(\cdot) = 0$ with probability equal to one. In higher-dimensional social network structures some non-trivial results (with $\eta^*(\cdot) \neq 0$ or $= 1$) hold.[5]

In the voter model, emphasis is laid on the temporal evolution of judgements in a group of infinitely many members. Another view on the impact of social interaction structures would be to start from a finite group with a given social interaction structure and let the number of participants go to infinity. However, we cannot go into the details here.

Summarizing, I believe that there exist a lot of ways to get rid of the independence assumption in List's aggregation procedure. It would be inter-

[5] Note that in order to derive these results, one has to assume that the group size is infinite.

esting to see how these alternative assumptions would change the results in List's paper.

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