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Attention and the art of scientific publishing

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Abstract As so many other activities nowadays, modern science revolves around the competition for attention. Unlike in so many other attention games, in science those who seek attention are more or less the same people who are giving it. An important characteristic is the skewness of the distribution of scientific attention. We discuss the effect these characteristics have on scientific institutions. An important thesis of ours is that scientists converge in clusters of likeminded scientists. Given the character of scientific organization and communication we expect that the digitalization of scientific communication will not affect the basic scientific institutions as the principles upon which the Internet functions coincide more or less with the way science functions. However, violation of these principles can in principle disrupt science and fundamentally change its character. Diversity, the key element of scientific conversation, may be destroyed.

Keywords: attention, publishing, journals, citations, networks

'Many of you will conjure up reasons why the number of citations should be ignored. There are fads; there are self-citations; there are conspiracies; there are derogatory citations; there are bribes to editors and referees; there are sycophantic students; and there are subjects capable of direct understanding by only a few. But why didn't your paper start fads; why don't you publish more and cite yourself; why did your conspiracies fail; why don't you become an editor; why don't your students care about your welfare; and why don't you insist on writing about obscure issues?' (Ed Leamer 1981)

1 INTRODUCTION

Some will welcome a digital future for scientific communication as a liberation. After all, everyone will be free to publish their findings on the Internet to be read by anyone. Gone will be the tough and ruthless competition for the scarce slots in the limited number of journals that seem to matter, gone will be the narrow-minded and self-congratulatory referees and editors who stand in the way of publication, gone will be the long lags in

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publication, and gone will be the high fees of journals, the trips to the library, and the endless copying. The communication of one's research will be immediate and free, and so will be the reading of that research. Liberation therefore. Think again. Consider the characteristics of the process of scientific communication and you should expect that some institutional changes notwithstanding getting one's ideas recognized will be as tough and arduous a process as ever, if not more.

The key to understanding this paradox is 'attention'. Even though digital technology may make it easier to get one's ideas out into the public realm, say on a personal web-site, this does not mean that they will be read, cited and discussed. Easier access also means more excess. The sheer abundance of texts to be read will render the chance of being noticed at all minimal. The web may expand the space in which we can communicate with each other, but the expanse of that space will confront us with the problem of attention more than ever before. As Herbert Simon once put it: 'A wealth of information creates a poverty of attention.' (1971: 40) The competition for attention, so we will argue here, is a better interpretive device for understanding the various practices in the modern world of science than modeling scientists as truth seeking individuals or by assuming that scientists are driven by monetary incentives.

This paper focuses on the role of attention in the world of science in general and in the world of economics in particular. It shows how problematic attention really is, and how scientists/economists have coped with the problem thus far. The objective is to anticipate the institutions that will help scientists cope with excess on the web.

Since the literature in economics and economic methodology has so far not explicitly identified the problem of attention in the economics literature, we have to develop a theoretical perspective on the role of attention in the production and dissemination of scientific knowledge. In doing so we draw together various approaches and insights not only from economics but especially from neighboring disciplines. The role of attention has been around for quite some time in the sociology of science (see Merton 1968; Gustin 1972; Collins 1975), in marketing and cognitive psychology (Berlyne 1960; Kahneman 1973; Bettman 1979; Payne *et al.* 1993) and in information economics and information science where the bounded rationality of individuals and organizations is stressed (Simon 1971; Van Zandt 2001), but in the economics of the academic publishing industry (see Gans 2000) the equilibrium approach has been the dominant mode to discuss the organization and institutions surrounding academic publishing. Although each of these approaches has their comparative advantage in explaining a particular aspect of science we are concerned with how institutions and organizations in science – the journal or academic publishing industry in particular – have developed to deal with an overload of information and how these institutions not only affect the reward structure in science but how the content and selection of ideas is affected as well. As this is a journal of economic methodology and we are economists, the argument relates most to economics but applies to other sciences as well. We first present a few stylized facts of scientific communication that any theory about the workings of science has to deal with.

2 THE HARSH FACTS OF SCIENTIFIC PUBLISHING

Academic professionals do research (a) to get published and (b) to get cited. That is the picture that emerges from interviews and surveys among economists working inside the ivory tower (Klamer and Colander 1990; and Van Dalen and Klamer 1996, 1997), and with this stance the academic economists are not alone, other scientists reveal the same drive and face similar contexts. There are of course other, more elevated motives involved in the everyday practice but generally speaking 'a successful academic' is 'a publishing academic.' Especially now the budgeting for scholarly research appears to be getting tighter, the 'publish or perish' principle is as true as ever. Promotions, hiring and firing in academia, they are to a large degree related to the citation record of scholars (Hargens and Schuman 1990; Siow 1998). Yet, not any publication will do. In order to get cited it is important to get your work published in the right (hard-copy) journals. Being in the right journal is a necessary but not a sufficient condition for getting the right attention. Being noted is a necessary condition for being persuasive, gaining a reputation, receiving tenure, getting funded and so on. Noting the work of colleagues is a necessary part of partaking in the game of science. Anyone who partakes in this process of seeking attention and paying attention to others who are seeking it, faces the two harsh facts of scientific publication. The first fact is the inflation in number of publications that call for our attention. The second harsh fact is the skewed distribution of attention over all those publications.

The first harsh fact of scientific publishing confronts all people who participate or aspire to participate in scientific discourse and concerns the *inflation of the number of publications.* If *Ulrich's Complete International Database* is of any guidance in this the journal publication numbers must be considered impressive. In 1999 there were 165,000 serials registered by 80,000 publishers world wide covering 969 subjects ranging from anthropology to zoology. In that year 10,000 serials ceased to exist whereas 6,000 serials were born and added to the stock of journals. Of course, not every serial is as important and scholarly as we imagine scientific journals to be. A lot of these journals stick to reporting facts that border on scientific journalism. The more scholarly journals rely on refereeing and this number is considerably smaller as 12,600 journals are registered as 'refereed journals'. And to put this number even more in perspective, the authoritative Institute for Scientific Information which reports on a quarterly basis the citations registered in its set of scholarly journals considers 'only' 7,000 refereed journals as constituting the core of the scientific conversation. But not only do scientists have to cope with competition for attention in publishing their material, it is also a competition that has become fiercer and fiercer with the growing of time. The estimated growth of journals published in the US increased with 62 per cent between 1975 and 1995 (from 4,175 journals in 1975 to 6,771 in 1995). There is a steady growth in number of publications and hence the growing number of articles published. More and more articles ask to be read; no single scholar can possibly cover them all even though scientists are reading slightly more articles than twenty-five years ago (Tenopir and King 1998).

At the same time the ranks of scientists have swollen. The activity of citing each other has increased. Figure 1 shows how over time the number of references used in articles registered by the SSCI has risen steadily from seven references per article to twenty-two references. Part of the explanation for this rise in references is to be found in the increase in article size. Economics papers are, for instance, roughly twice as large as they were twenty-five years ago and have also about twice as many references (Ellison 2000). The average article in 1975 in the *Quarterly Journal of Economics* was sixteen pages long and contained sixteen references, whereas the average article in 1998 consisted of thirty-one pages and made use of thirty-one references. In other neighboring social sciences the case of rapid page and reference increases was not very much different. At the same time the average number of citations received per cited author has increased from 3.4 to 5.0 citations (including self-citations) per year. In other words, for those who get cited the rewards seem to have grown, but with equal force one can say that some debasement of citations has taken place. An article published in 1966 could not count on the same audience size as an article published thirty years later. To take the example

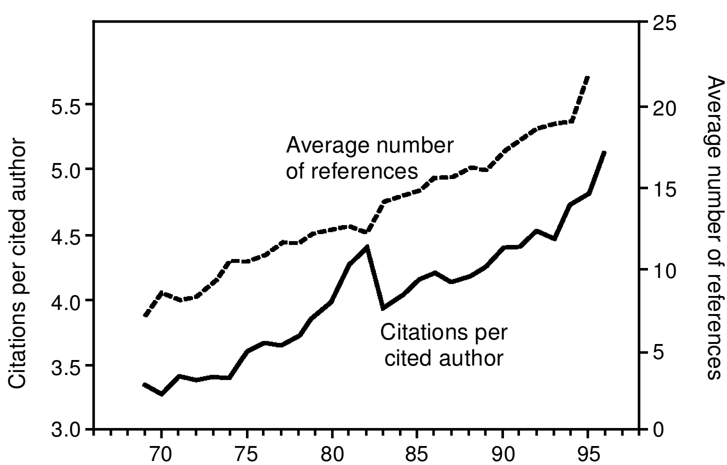

Figure 1 Citation inflation in the social sciences, 1969–1996

of economics, the literature in this particular field has grown in recent decades by an annual rate of approximately 5 per cent and with such high growth rates the potential for being cited increases considerably. But as Diamond (1986) shows for a number of sciences, the marginal monetary reward to a citation in citation-rich disciplines like physics and chemistry is far smaller than the marginal reward in citation-poor discipline like economics and mathematics. In short, the increase in the number of citations is not necessarily a blessing and it certainly does not signify that ideas have become better of more influential.

The growing number of publications has been facilitated by the increasing number of journals which in turn has been inspired by the increasing number of researchers or, to be more precise, the increasing number of specialized researchers. Specialization has become a necessity today. If the Renaissance man would be able to cover a literature across the entire intellectual spectrum, that achievement would be virtually inhuman today. There is simply too much out there for any single individual to take notice and read. And each and every reader has to allocate his scarce reading time to the articles which appear ex ante as the best among a pool of competing articles. And even then he will have to put an effort in to keep abreast of his research field. Holub *et al.* (1991: 324) calculate that experts who read about thirty articles a year and who want to keep up with a field like growth theory will find merely one important article approximately every two years among the ever growing pool of articles.

The sheer numbers evoke the problem of attention. Each year more articles appear in the domain that we scientists are expected to cover. So the question is: which articles to read, which work to take seriously? That is one side of the problem. On the other side each of us faces the daunting prospect that each year our articles compete for attention with ever more other articles. As we know for ourselves, nobody can read all. People have to be selective and so will they select articles to pay attention to.

The poignancy of this question of attention is accentuated by the second harsh fact that concerns the *skewed distribution of attention*. It has long been noted that inequality in attention given to ideas is highly skewed toward ‘giants’ in a particular discipline. The registration of inequality in science essentially started with the work by Lotka (1926) who formulated the following law of scientific productivity: if n_1 is the number of scientists who publish one article, then the number n_k of scientists publishing k articles (for $k > 1$) in the same field can be gauged on the basis of the following equation: $n_k = n_1/k^2$. Hence, if 1,000 scientists publish one article during their lifetime then 250 will have produced two articles, 111 will have written three articles, etc. Later, formulations of inequality distributions in scientific productivity were made by Price (1976) who formulated a variant of Lotka’s Law, viz. one half of the total output of articles published by a population of N scientists will be the work of $\sqrt{N}$ most productive members of the population. Holub *et al.* (1991) made a variant of Price's claim by stating that the number of important publications in a science is the square root of the total number of publications in a research field. Estimation of this iron law turned out to describe their claim rather well. All these statistical theories of inequality distributions share the common trait that they lack a (behavioral) theory of citation, which stands to reason as most inventors of these distributions believed that the making of science was ruled by an iron law. The main task of a 'scientometrician' is believed to be discovering the natural constants of the process of publication and citation. David (1994) has argued that sociologists and economists of science still have some explanatory work to do if they want to understand more clearly why the large majority of articles in social science receives so little attention and why just a small percentage of articles makes the grade in terms of a large number of citations.

To get an idea of how skewed the rewards are distributed just take a look at Figure 2 where the cumulative distribution of the impact factor of all journals registered by the Social Science Citation Index (1,698 journals). The super star SSCI journal has an impact factor of 11.3, which means that the average article in this journal receives 11.3 citations (including self-citations) in the first two years after the publication date. The most visible characteristic of the distribution of journals is however the almost rectangular shape. For 80 per cent of all journals the influence on the scientific community is small if not negligible. The median impact factor for the social sciences journals is 0.5 (which includes self-citations of authors) whereas the top-10 per cent journal has an impact of 1.65. But let us warn you right here and now: even getting published in a major journal will not suffice to gain attention with full certainty. For instance, most economists crave for publishing their work in the *American Economic Review* (AER), but as Durden and Ellis (1993) show only 1.8 per cent of the authors publishing in the AER make the grade and can

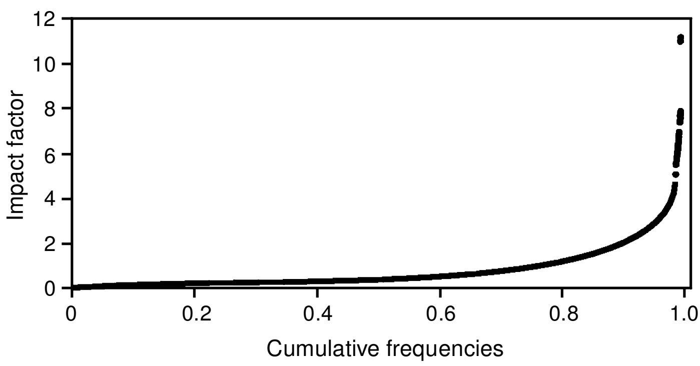

Figure 2 Cumulative distributions of impact scores of SSCI journals, 1999
Source: ISI, *Journal Citation Reports* (2000)

say that they have written a 'classic'. Naturally, gaining attention with an AER paper is easier than publishing your work in some journal outside the core as the AER can count on roughly 27,000 subscribers, whereas specialized journals reach only a fraction of this circulation number. The percentage of 'classics' of the AER can therefore be seen as an upper bound of what the odds are in gaining attention in general. If one takes into account all the second-tier journals the average chance of becoming a classic in the economics literature is extremely small. In the end only the top 0.1 per cent of the hundred of thousands articles and books published each year receive a great deal of attention (see Laband 1986; Garfield 1990). The majority of the research done, published or not, passes by unnoticed. And things have not become easier over time for economists: while the median number of articles published has remained more or less constant for different cohorts of PhDs, the chances of publishing an article in the top journals of the field (*JPE*, *AER*, *QJE* and *Econometrica*), have dropped steadily over the past three decades (Coupé 2000).

3 HOW TO ACCOUNT FOR THE HARSH FACTS: THE FACTOR OF ATTENTION

The big question surrounding these harsh facts is *not* whether this state of affairs is fair, because fairness does not play an overriding part in superstar professions. As the Leamer quotation at the very beginning of this article made clear, it is part of the academic game to get attention. The facts tell us that this is a complicated game. One question we want to ask concerns the institutions that structure this game and help producers and consumers of science cope with the excess. In the back of our mind is the question whether these institutions may transfer to the world of digital communication. By focusing on the factor of attention, we downplay the role of truth as a discriminatory and allocation device.

When many claims to the truth circulate, attention is the *sine qua non* for a claim to be shared and become part of the 'stock of knowledge'. Attention stems from the Latin *attendere*, and connotes the act of attending one's mind or consciousness to a phenomenon. In this cognitive characterization attention is the mental activity of selecting and focusing (see for instance Berlyne 1960; and Kahneman 1973). The question here is how individuals cope with the excess of sense-data and how their selection mechanisms work. Kahneman observes that attention did not play an important role in behaviorism, Gestalt theory and psychoanalysis (p. 2). His explanation is that the notion of attention makes the connection between stimuli that a human organism receives and its mental state problematic. When there is a straight connection a stimulus has a predictable response. However, when the connection is loose and overdetermined, the response becomes harder to predict. A red flag will get a reaction in one case and will go unnoticed in another. Crying will work in the morning but not in the late afternoon. The response will be especially hard to predict when there is an abundance of stimuli. With a multitude of scientists presenting their work, how do we determine whose work to pay attention to? Psychological research will focus on specific mental activities (like cognition and arousal) to determine the factors that influence the quality of attention that we give to any particular stimulus. The theory has to allow for some degree of spontaneity and unpredictability to make sense of the factor attention.

In general, psychological theorizing seems preoccupied with the type of limitations that restrict the attention span and scope of humans. Kahneman points at the importance of effort as an input next to the input of information or stimuli to the process of paying attention theories. The presumption is that humans have a limited capacity at any time for the total amount of attention that they can give. Effort can influence the intensity of attention. Study a paper before going to the seminar and you will intensify your attention during the discussion, probably picking up details that a less well prepared visitor will miss. Although this cognitive perspective is of interest to students of scientific processes – think for example of the cognitive impact of rhetorical devices like titles – it has the disadvantage that it focuses on the individual whereas we are dealing with interactions and communications among individuals.

We therefore focus on the attention as a social phenomenon that is to be detected not inside the head of individuals but in their interactions. Attention in this sense is the density of the signals that relate to a particular argument, idea, article, research program, scientist, discipline, or science as such. No attention for an article means that nobody talks, writes or communicates in any way about it. This is virtually impossible as the article will most likely appear in a table of contents and in some lists of publications (of the department of the author, for instance); any such signal represents attention. Absolutely no attention for an idea is easier to conceive: the person with the idea may simply abstain from any communicative action whatsoever to guarantee that no-one else is able to pay it any attention. When people talk about an article, they give it attention; when they cite it in their own articles they do not only give it attention but also increase the chance for the article to receive more attention like when other people begin talking about the cited idea. Attention appears in numerous forms: the frequency in which a work is talked about within the scientific community or a subset thereof, the haggling over the order of names on articles, the number of citations a paper receives and positions and prizes awarded because of original work. Accordingly, in our usage 'attention' stands for the intensity with which any product of scientific imagination (an argument, a idea, an article, a scientist, a research program, an *oeuvre*, a discipline) is communicated in the scientific community.

Knowledge about how attention is formed and distributed in the community of scientists is in our opinion the key to understanding the creation and diffusion of ideas in science. Let us first see how far we get when we consider the stylized facts in the light of an ordinary economist's perspective who looks at science as just another social phenomenon that can become part of the Marshallian empire of supply and demand curves. We first discuss how much mileage the most rudimentary market approach gives us (Section 4), to be followed by Section 5, which tries to understand science as a winner-take-all market. Of course, no model is perfect (even a model of perfect competition) and the imperfections that are not dealt with by the market equilibrium approach are an inspiration for the rest of the paper, a section that deals with issues that are of particular concern to the latest vintage of economists of science (see for an overview: Sent 1999).

4 SCIENCE AS A MARKET

Economists ordinarily refer to the metaphor of the 'market place of ideas' and apply the logic of the market to the way science works. Most economists focus on issues that are close to the ones examined by the Mertonian school of thought (Merton 1968; Cole and Cole 1973 and Zuckerman 1977). This particular brand of sociology focuses on the reward and communication systems in science, topics which economists can relate to. However, where sociologists stress social norms in the process of giving and receiving credit, economists are in the habit of looking at science as 'getting the prices right'. As early as Adam Smith we see this preoccupation as he focussed on the inefficiencies in university education due to the reward structure imposed by the university's non-profit organization structure (see Smith 1776: 245–70). Within the boundaries of economic science the element of attention is rarely discussed, but when it does happen economists stay close to home and use equilibrium analysis to explain scientific practices. The work by Levy (1988) and Franck (1999) falls squarely in that tradition and they are a perfect example of how far the market approach takes us in explaining scientific publishing. Levy views the strive for fame as an ordinary market activity and models fame and lifetime income as a trade-off. In order to attain fame later you have to sacrifice income and time. He shows that in the days of Jevons and Marshall fame would be cheaper to maintain than in the days of the Academic Professional like Samuelson and Friedman. Of course, the difference is easy to explain as knowledge diffusion was very slow in the 'amateur' days, books were the embodiment of the final statement on a subject contrary to the day of the professionals who use journals as means of communication. Levy's treatment is however rudimentary and hardly reflects the trade-off for the common scientist whose work has a slight chance of being noticed.

Franck (1999) corrects this biased view of science by explicitly considering attention in the context of knowledge production. He claims attention to be the main input to knowledge production and recognition, or attention income, as the factor that motivates scientists most (Franck 2000: 1–6).

Attention, or recognition of one's efforts, must compensate for the pecuniary income foregone. It does well for our self-esteem and sense of satisfaction. Accordingly the assumption is that individual scientists will try to maximize the attention to their own work. They have an incentive to pay attention to the work of other scientists insofar as that work will enhance their own productivity and hence the chance for getting more attention. He characterizes the world of science as a closed system of attention: the assumption here is presumably that scientists operate in more or less fixed pools of people and papers calling for and paying attention. In that regard this world differs from the world of musicians, say, as musicians have to seek attention for their work outside their own circle. Scientists most of all pay attention to each other.

The economic question concerns the efficiency of this exchange of attention. According to Franck the outcome is efficient if the intensity of the attention that papers receive corresponds with their scientific value. We thus have the classic Smithian problem: does a 'free' economy of attention, in which individual scientists try to maximize attention for their own work, generate optimal outcomes? Franck does not believe this to be the case. All is well according to Franck as long as scientists are citation maximizers and citations measure scientific value accurately. The attention they can muster for the work of fellow scientists is limited in its intensity and extensity. In other words, attention is scarce and because of this scarcity they have developed a variety of practices to deal with the factor attention. It is, of course, not the only motive which makes scientists tick, but it certainly is nowadays the dominant driving force. Science involves communication and communication happens because people not only ask for attention but also are willing to give it. Getting published is a *sine qua non* for gaining attention beyond one's friends and immediate colleagues.

Franck's analysis is designed to deal with the efficiency question. It does not account for the stylized facts of inflation in citations and the skewed distribution of citations. The competitive market model is of no use for explaining these as it presupposes that the market for ideas works like a spot market: everyone receives his marginal productivity or put differently, the price paid for an idea (e.g. by citing an article) reflects its marginal usefulness. Spot markets fail however to explain why reward and productivity seem to be distributed by a wrong-headed Robin Hood character who takes from the poor and gives to the rich. The phenomenon of the skewed distribution of recognition and the shifting of attention from the 'have nots' to the 'haves' has been aptly coined the Matthew effect in science by Merton (1968). According to the gospel of Matthew, 'For unto everyone that hath shall be given, and he shall have abundance: but from him that have not shall be taken away even that which he hath.' Merton made the case that this particular effect appears principally in either cases of collaboration or in cases of independent multiple discoveries made by scientists of distinctly different rank or status. In the case of economics one can think of Friedman and Phelps simultaneously popping up with the idea of the natural rate hypothesis. The credits however went to Friedman whose status exceeds that of Phelps by far. The unfair treatment in the allocation of attention can even materialize in eponyms like 'the Solow residual' in which all the credits go to one man even though the idea may have been posited earlier by others (Griliches 1996). In short, this is Stephen Stigler's Law of Eponymy (1980): fundamental ideas are never named after their *true* inventor.

5 SCIENCE AS A WINNER-TAKE-ALL MARKET

A possible starting point for putting the heavily skewed distribution of attention in science in perspective can be found in Rosen's theory of superstars (Rosen 1981), a theory which claims that initial endowment differences, however small, tend to be magnified by the market size or the audience for a service or idea. In science this phenomenon is clearly visible as an extremely small number of researchers receives a disproportionate amount of attention. Cox *et al.* (1997) show how the economics publishing industry conforms remarkably well to the laws of superstardom. In order to test the theory of superstars for science two elements of science could possibly explain the extremely skewed distribution of attention across social scientists, viz. reputation and the size of markets. Rosen focussed on the role of market sizes in determining the rewards on labor markets for creative professions. Rosen (1981: p. 845) distinguishes two characteristics of the superstar: 'first, a close connection between personal reward and the size of one' s own market; and second, a strong tendency for both market size and reward to be skewed toward the most talented people in the activity.' The result of it all is that 'small differences in talent become magnified in larger earnings differences, with greater magnification of the earnings–talent gradient increases sharply near the top of the scale.'

The skewed distribution of returns can be explained by the convexity of the sellers' revenue functions which implies that the rewards are more skewed than the distribution of talent. Apparently superstars have something extra, the gift of the gab or perhaps they are simply better salesmen. In writing on the technique of persuasion George Stigler (1955) once stated that new ideas are even harder to sell than new products: 'Wares must be shouted – the human mind is not a divining rod that quivers over truth.' General repetition, inflated claims and disproportionate emphases are according to Stigler the strategies that accompany the adoption of every new idea in economic theory. This element of Rosen's theory may perhaps be one of its weak points as the skills to sell an idea may in fact be a necessary part of talent. It is very easy to condemn the work by superstars as being something which the average graduate student down the hall could have done just as well, or even better. The rewards of superstardom are captured by imperfect substitution among authors. Authors of lesser talents are often a poor substitute for the authors who think or sell great thoughts, or to put it less bluntly from the consumer's side, reading ten mediocre articles do not make up for the read of one excellent article. There was nothing essentially new in Paul Romer's increasing returns article in the *Journal of Political Economy* of 1986 but still you have to admire his salesmanship. Apparently persuasion and style carry a substantial premium in science. A distinguishing characteristic of the superstar is therefore that there is a 'marriage' of buyers and sellers: audiences are assigned to authors and if the authors plays their cards right they acquire by means of the workings of the pen a top quality audience. High quality audiences are important as those audiences can reciprocate the author's efforts by citing the work in question. Furthermore, the costs of production (e.g., writing an article) do not rise in proportion to the size of a seller's market. The 'only' hurdle one has to take is the peer review. Managing editors, editorial board members and external referees are all involved in evaluating manuscripts and ideas.

There are, however, two sides to the coin of success. A superstar's gain is the loss of the researcher working in the backwaters of science. In the competition for attention contributions from new authors have to stand out markedly if they want to catch the eye of authors of some reputation. What's missing from the Rosen story is the rise to fame. Even superstars started out as rookies, so what made them so famous and why did other starlets of equal talent not make the grade? Much of this story revolves around selection and as demonstrated by MacDonald (1988) the type of reward schedule as described by Merton (1968) and Rosen (1981) is optimal as a selection device as it selects the young and promising individuals in a profession or a trade in which creativity dominates the product or service produced. This selection feature fits the labor market for academics well. MacDonald's model is based on a dynamic 'information accumulation process' with multiple market levels. To translate his model to suit the circumstances of science, researchers begin in the entry-level market where they compete mostly as unknowns. They receive the attention from lower ranked researchers who also have lower opportunity costs of time and who therefore can 'afford' to read the writings of these 'nobodies'. However, some of these nobodies in the entry-level market exhibit above average talent in their writings and they also receive above average attention as their work is cited on a modest but given their standing at an exceptional rate. These exceptional researchers are then given a chance to compete in more select or high quality markets where consumers of knowledge have more human capital than the average consumer in the entry-market and whose time is more valuable than those lower ranked consumers. In each higher market level 'consumers' are willing to pay a premium to enjoy or read the lectures and writings of the higher quality scientists. Furthermore, MacDonald (p. 162) made the observation that in occupations in which performance is stochastic and in which past performance is a poor predictor of current performance, the chance of leaving such an occupation will be slight. The evidence of the tolerance for below average work is ample in science. Siow (1991) showed that for scientists working in mathematics and economics first impressions, as displayed in the quality of their first publication, are in practice not as important as they appear in theory. And on a more general note, both editors and referees are noted to err in judging quality and the type I-errors of judgement (low quality work is accepted for publication) are a good candidate for explaining the high uncitedness rates in science (see Hamilton 1991).

6 SCIENCE AS A NETWORK

Scarcity, incentives, selection, the size of the 'market', a customary set of economic concepts appear to help us account for the skewed distribution of citations and citation inflation. However, what is missing from the picture of science is the aspect of community and social interaction. Publishing an idea in the AER is not necessarily a one-way ticket to eternal fame, there is more to giving birth to an idea. The giving and receiving of attention is by definition a social phenomenon and this fact impinges directly on questions of theory choice in science, something which the neo-classical view can not deliver or provide. The equilibrium view of science revolves very much around deriving rational choices of individual optimizers and aggregate level states of the economy that satisfy some (aggregate) consistency condition. An economist who throughout his career has paid close attention to social interaction is Thomas Schelling (1978). His claim is that the equilibrium analysis of markets is a large and important special case: 'Equilibrium is simply a result. It is what is there after something has settled down, if something ever does settle down' (1978: 26). But . . . to understand all social phenomena with this simple model would be something of a miracle and Schelling makes the point that interactive behavior – 'What people do affects what other people do' (p. 27) – is the key to understanding truly social behavior. Micromotives imply macrobehavior. Slowly but gradually this point has been catching on and is nowadays known as the complexity or 'Sante Fe' approach to economics (see Arthur *et al.* 1997 and Durlauf 2000), an approach which stresses the interaction among individuals and tries to incorporate empirical insights from sociology, economics and anthropology. Interaction based theories are, however, not sufficient to understand science, one also has to explain how institutions – the rules by which the game of science is played – come about and change. Furthermore, in understanding science one has to deal with the aspect of creativity. The equilibrium approach put forward in the previous sections has its merits but in matters of creativity the tacit principle of plenitude ('every conceivable entity already exists'; see for a discussion Romer 1994) becomes a straightjacket in thinking about science. One could arrive at the rather bold conclusion of the head of the patent office who recommended at the turn of the nineteenth century to abolish the patent system because everything had already been invented.

To those ends we now first turn to the institutions that shape and characterize the worlds of the sciences. Our claim is that the main institution that helps scientists cope with the problem of attention (i.e. the channeling and distribution of attention) is the clustering in groups and discursive entities. Scientists cluster in universities, set up barriers to entry, organize professional associations in order to organize conferences and issue journals, constitute schools, subscribe to research programs, develop specialized research communities which will organize specialized conferences and issue specialized journals, and form networks of like-minded souls. All these institutions help to define, bolster or protect a space of attention, that is, a concentration and intensification of signals interchanged. It helps explain why innovations in science are geographically localized and not evenly dispersed throughout the world. In order to make an intense conversation possible face-to-face communication with like-minded colleagues appears to be essential. The University of Chicago is perhaps one of the most outstanding examples in economics (Van Dalen 1999) but the importance of geographic proximity runs throughout the history of other sciences as well (Zuckerman 1977).

There are good epistemological reasons why scientists operate this way. It is not just the excess that forces them to be selective and concentrate on a limited domain of scientific production. They also form and join communities of fellow scientists because it is the way to develop, share and sustain whatever knowledge they have. Knowledge is not a thing that is lodged in a human mind and can be communicated by means of bits of information to another mind (Maturana and Varela 1980, Van de Velde 1999). Knowledge is not a product that can be stocked and transferred. It is rather an activity that people engage in. Maturana has introduced the notion of autopoietic system (self-creation) to express the idea that each one of us operates in a more or less autonomous system that continues to reproduce itself. 'From an epistemological point of view, autopoiesis implies that agents are open to data but close to knowledge – new data is only *potential* knowledge' (Van de Velde 1999: 5). When new data affect the system (i.e. get attention), the system will have to deal with them in one way of another. The important point is that people can not know what other people know; they can only process signals that others send. Most signals will bounce off (fail to get attention); others will be seriously distorted in the processing. To contain the potential chaos and to restrict the uncertainty of this process scientists form groups or communities. A group allows for, and stimulates a frequent and intensive interchange of signals. The members will never fully know what the others know[1] but because of their systems may have to process so many similar signals and hence share so many codes and the like that they begin to develop similar responses to new signals. Matunara calls such a process 'structural coupling' and the outcome a 'consensual domain' (see Maturana and Varela 1980). We see in a consensual domain an attention space in the sense that it concentrates the attention of all those connected with it.

Accordingly, we would expect that scientists will benefit from an association with other scientists. Support for this expectation we find in Collins' impressive study *The Sociology of Philosophies* (1998). Collins depicts a 'dynamics of conflict and alliance' in the formation of important philosophical 'schools' or 'networks' such as the German idealists (Kant cs), the Vienna Circle and the neo-Confucians. His extensive research brings out a few remarkable patters: important philosophical schools (i.e. those that survive their times and make it into the textbooks) come about within a restricted time span (30 years or less), within a restricted network – just as we would have expected. According to Collins '[t]he social structure of the intellectual world, the topic of this book, is an ongoing struggle among chains of persons charged up with emotional energy and cultural capital, to fill a small number of *centers of attention*' (ibid.: 14, italics added).

Collins conceives scientists as being involved in interaction rituals (cf. Goffman 1967). Crucial ingredients of each interaction ritual are, for instance, the physical assembly of a group of at least two people, focused attention on the same object or action, and a common mood or emotion. Intellectuals differ from most lay groups in the self-consciousness and reflexivity of their interaction rituals; they usually receive attention only from each other. Although they may think of themselves as individuals, philosophers tend to operate in groups; successful philosophers invariably have done so. Collins has found that to be the case long before the invention of print as well as in contemporary times. Personal meetings, professional meetings, lectures, debates, seminars, departments in universities all help them to focus their attention and develop a common mood, motion, or intellectual energy.

> Without face-to-face rituals, writings and ideas would never be charged up with emotional energy; they would be Durkheimian emblems of a dead religion, whose worshippers never came to the ceremonies. Texts do not merely transcend the immediate particulars of the here-and-now and push towards abstraction and generality. To be oriented toward the writings of intellectuals is to be conscious of the community itself, stretching both backward and forwards in time.
>
> (ibid: 27)

Thus intellectuals overcome the problem of abundance – too many others calling attention for too many ideas. They form distinctive conversations to generate an inside world. The conversation compels insiders to focus on contributions of other insiders and to ignore all others. Collins simply observes that for any conversation to be vital and have a lasting impact, personal interactions are a necessary condition.

His investigation of 'centers of attention' through time adds another interesting insight: important philosophical schools invariably are in competition with other schools, but never with more then two others. The latter finding Collins calls the 'law of small number'. It appears that three rival schools are all that the philosophers can handle. More rival positions would scatter the attention too much and dilute the focus of the conversation to the point of fragmentation and subsequent disintegration.

Accordingly, the most enduring strategy to make the process of gaining, distributing and sharing attention manageable is to create clusters which we also might call attention spaces. Scientists specialize and form clusters in their specialization, each with its own 'discursive practice' or 'conversation', its own journal, association. When the chance of being noticed is minimal in the field at large, the solution is to organize or partake in a smaller sub-field where the chance of being noticed is much greater. Take the example of cultural economists. Even though a few prominent economists had published on the economics of the arts in well-known journals, less prominent economists failed to gain the attention of the core economics journals. So they formed their own association that issues a newsletter, organizes biannual conferences, started up a new journal and tried to be included in the *Social Science Citation Index* (*SSCI*) and have the abstracts published in the *Journal of Economic Literature*. The advantage of the association, the journal and the conferences was the attention by a reasonably well-defined group for work on the economics of the arts. The attempt to get included in the *SSCI* and the *JEL* represents an attempt to gain legitimacy for the sub-field and thus to secure its sustainability. Failure to do so would have a negative effect on the incentive to publish in the *Journal of Cultural Economics* because such a publication would not count for much in tenure and promotion decisions. The example indicates that the organization of a space of attention is not only important for the satisfaction of those working in the field of cultural economics, but also serves the career interests of those participating.

Clustering is a condition for making the process of attention seeking and getting more manageable. As we saw before, stars emerge when there are lots of people who want to pay attention and want to share their attention. Stars, therefore, are a phenomenon of large fields, like the field of economics in general. Small clusters tolerate stars less well and will more equally distribute the attention among the 'members'. The downsizing of clusters, therefore, is an effective response to the skewed distribution of attention.

The clustering also accounts for the inflation of publications and citations (cf. Van Raan 2000). Modern science displays a fractal-like structure, that is: each cluster generates its own publications and forms a mutual citation society and as time goes by this cluster generates a more refined cluster which again generates 'offspring'. In large part, the citation inflation derives much from intensified debates within the cluster. Those who write in the *Journal of Cultural Economics* cite other articles in the same journal (journal self-citation rates are generally high). So even if these articles are not cited elsewhere, their citations add to the total (provided they are included in the SSCI). The argument could also be put another way: the inflation of citations indicates a rapid expansion of the number of clusters in the world of the sciences.

In short, when you as a scientist seek recognition for your ideas, you do better by joining a conversation within a cluster. That means you will have to respect its rhetoric, cite its often cited texts, attend its conferences and so on. Yet, joining a cluster does not guarantee attention. Clusters can be closed-minded. When a few of the distinctive 'members' of the cluster dismiss a new idea there is the chance that the scientific community will stick to the old idea as the negative experience of a few will tip locally gathered positive experience of everyone. The interaction structure can therefore be quite powerful in its consequences. Goyal (1999), for instance, argues that locally independent individuals may be better off when novel ideas are at stake. Local independence, i.e. individuals who do not have direct neighbors in common, allows individuals to choose different actions and learn about the potential of these actions, before information about the potential of other actions reaches them. In other words, a loosely connected society appears to be more tolerant towards novelties as the amount of experimentation with new ideas is 'optimal'.

Things become a bit more complicated if the ideas or technologies exhibit network externalities, i.e. the pay-off to an idea increases with the number of people using the good. The network externality idea closely resembles the character of a conversation topic: if everyone has read a different article a conversation would be hard to maintain. However, if everyone has read the latest article by Deirdre McCloskey a lively conversation is a real option. For the generation of attention this is an important insight, a concentration of attention functions like a focal point. In case individuals have to choose between two or more new ideas, and it is assumed that individuals are indifferent with respect to the ideas *per se*, then it becomes important to coordinate on some idea. The market place for ideas is in that respect a competition for networks or standards. Expectations, coordination and compatibility of standards are then of paramount importance. Various institutional arrangements try to cover these so-called market failures and most of the issues touched upon are of relevance to the journals market as, of course, any journal would like to be the journal which 'dictates' what the norm of discussing economics and policy will be. Presently, US-based journals dominate mainstream of economics to the chagrin of some (Colander 1994, and Hodgson and Rothman 1999).[2]

Now which network models have been in place or are pushed forward as models for understanding science. The model of Lone Wolves, corresponding with situation (a) in Figure 3, is clearly a much depicted extreme system for those who think isolated individuals can make contributions to science. Perhaps for the extremely gifted like Mendel or Srinivasa Ramanujan this

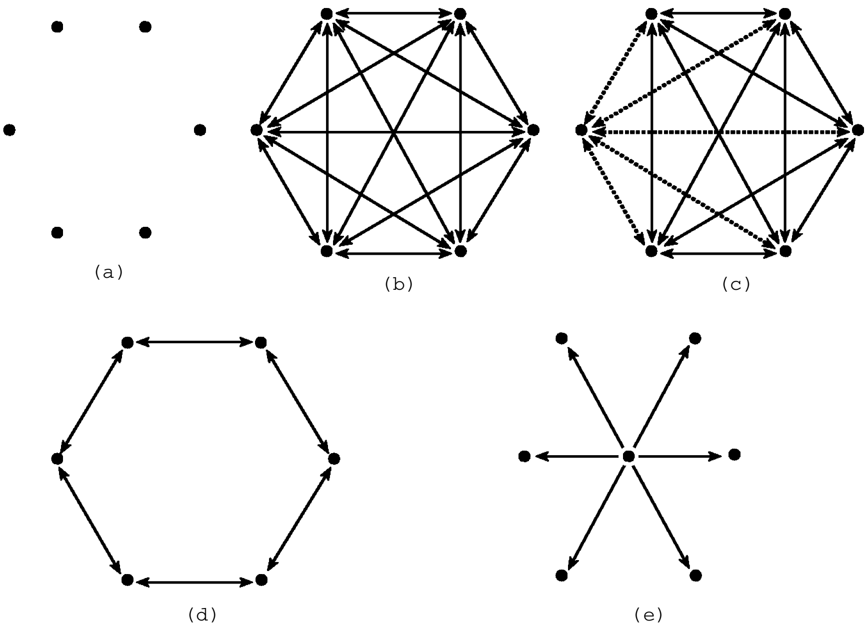

Figure 3 Network interaction structures: (a) Lone wolves – no interaction; (b) The Science Ideal – full interaction; (c) Technology leader sets the standard (of language, methods, issues); (d) Learning from neighbors; (e) Minimal network structure with a core.

model could be made to work, but for the large majority science is made in communities and without the interaction among individuals science would break down or produce an excessive amount of duplication. A less extreme version of the lone wolves model is to focus on the number of authors publishing a paper. Certainly in the modern world of academic professionalism team work has become the rule and solo production – written by lone wolves – has become the exception.

The other extreme network structure, that of the fully integrated scientific community, corresponding with Figure (b) is also not much help in understanding science as it lacks plausibility. In a way it corresponds to the views of science as a neoclassical market of supply and demand as depicted by Franck (1999): the world of science is fully integrated and everyone has an equal chance of meeting each and every participant. And in a way it also reflects Polanyi's (1962) description of the Republic of Science a system in which science is not a solitary affair, but an activity done in 'overlapping neighborhoods'. In solving a gigantic jigsaw puzzle science is at its best when each and every participant solves the puzzle simultaneously but in close contact with the rest of the community. That is the ideal picture of science, a spontaneous order. The perfect science model is often seen as a role model for other organizations, in particular open source code projects. The most direct evidence on the workings of such an organization model is to be found in Lerner and Tirole (1999) who argue that appearances may deceive. Open source projects like *Linux* and *Sendmail* are far from spontaneous. Most of the programming work *seems* to be done in an anarchistic way, but in actual fact the spontaneity is of an organized manner, often led by a charismatic leader, who divides up the work in smaller and well-defined tasks, attracts other programmers and last but not least 'who keeps the project together'. Translate this type of organization to the surroundings of a university department and one must agree that this description has a far more realistic ring to it than the perfect market model.

One of the harsh facts that science has to deal with is an information overload. In order to make the scientific communication possible the scarcity of attention needs to be allocated and other, more plausible network structures have arisen. Model (c) in which a leader sets the standard adds some plausibility to modeling the world of scientific publication. Indeed Frey and Eichenberger (1997) note how the US universities and journals set standards for the rest of the economic community.

Still, the star model (e), which presents the most efficient network, and the learning-from-neighbors model (d) are both closer to home. The star model comes close to how each discipline operates, with a set of core journals to which minor, more specialized journals are connected (see Stigler 1994; Stigler *et al.* 1995; Van Dalen and Henkens 1999). Notice however how the communication in this model generally moves in one direction. The intellectual triad between journals is generally one in which core journals export knowledge to specialized journals and not the other way around. The reason why this happens is easily provided: core journals generally reach a large number of readers and practitioners contrary to specialized journals which reach more targeted and smaller audiences and because of their size the price of specialized journals are generally higher than core journals. Now, of course, we all know exceptions to this rule but considering the preference of scientists in getting maximum attention they will submit their key articles initially to the core journals as these journals guarantee the largest audiences. Upon rejection they will consider second-tier or more specialized journals.[3]

The model in which we learn from others or in which we conform to opinions of neighboring colleagues or from neighboring disciplines is clearly a realistic one as the research by Garfield (1998) shows when he models the entire world of science as an chain-like system as depicted in (d). Of course, whether the transfer of knowledge comes full circle is debatable, but it remains a distinct possibility in closely connected environments. The basic feature of these models of learning is that people learn not only from their own experience, but as most experiments are time consuming people also learn from the experience of their peers. The central insight of the learning literature is that the interaction structure of individuals or groups of individuals matters a lot (see Bala and Goyal 1998).

The learning from neighbors models is not only a plausible model at the micro-level, it is particularly powerful in its explanatory power at the macro-level. For instance, in theory it does not take much effort to start an informational cascade when individuals learn by observing others (Bikhchandani *et al*. 1998). The role of opinion leaders or leading journals is critical in bringing about fads and conformity. It is in this respect that the earlier mentioned Matthew effect in science becomes dysfunctional if the ideas that are accepted are not entirely foolproof. Under such circumstances one can arrive at the case that behavior of a star, let's say, Robert Barro running numerous economic growth regressions, is imitated because Barro makes it legitimate to do such simple and perhaps research (and perhaps because it is so simplistic it is easy and inexpensive to copy such behavior). There are two responses possible in putting fad-prone scientists in perspective, a theoretical and an empirical one.

The theoretical answer can be found in some detail in Brock and Durlauf (1999) who claim that the role of social factors in science is far more complex than is often recognized. They demonstrate that under some plausible interaction conditions social factors may not hinder the development of science but *increase* the degree of consensus around a superior idea. The intuition behind this finding may seem counterintuitive but it makes perfect sense because once the consensus of the community focuses its attention on the superior theory, this consensus will speed up its rapid acceptance. Of course, one can still claim that judging theories to be superior to others – a key assumption made by Brock and Durlauf – is a questionable assumption and a speedy convergence to only one theory may just as well be seen as a bad thing. The diversity of science which is central in Callon's (1994) muddled but nevertheless plausible argument for defending science as a public good becomes a relevant issue at this point. Diversity circumvents science and in the end also society from becoming stale or as Callon puts it : 'without this source of diversity, the market – with its natural propensity to transform science into a commodity – would be ever doomed to convergence and irreversibility' (p. 418). What Callon does not deal with is the question of the optimal amount of diversity. Diversity as such is no great quality if each and every scientist has a different idea and operates as a lone wolf. Perhaps one of the reasons why European economics departments until quite recently have been such a stale territory for economists may be pin-pointed to this quality of 'isolated' diversity (see Coats 2000). A complete consensus of opinion (which is behind the worry about Americanization of science) may also not be wholesome as it would destroy original insights outside the false state of consensus.

The empirical response to the possibility of fads and ending up in a 'bad' state is that the evidence is mostly anecdotal and is not thoroughly scrutinized. A priori one would expect this reputation effect in bringing about cascades to reflect the property of increasing returns to the scale of an individual's reputation. In examining the elements which might make a scientific research article influential, Van Dalen and Henkens (2001) show that the characteristics of a journal (reputation of the journal and editors) overwhelms the reputation of an individual in getting ideas accepted. So the reputation of journals outranks by far the reputation of the author, a message that also comes across in a refined network-analysis by Baldi (1998) who demonstrates that the reputation of the author of an article does not affect the reception of published ideas, whereas writing in a widely disseminated journal generates a distinct attention bonus.

In short, personal reputations do not seem to be as important as the reputations of journals. Journals therefore have not only in a formal sense a great responsibility as gatekeepers of science, they also seem to function as monetary institutions which regulate the circular flow of the academic's only coin worth having: attention. The academic journal and all the people involved in constructing and maintaining this institution should be particularly cautious with their 'monetary' power as it could destroy diversity of opinion.

7 THE FUTURE OF SCIENTIFIC PUBLISHING

No matter how truthful scientific claims may be, they need to be read, talked about, and cited in order to exist. Scientists need to persuade each other of the merit of their work. Yet in order to persuade they need the attention of their colleagues first. As the ranks of scientists are swelling, the number of articles competing for attention is exploding. With so many scientists around willing and needing to give attention, the possibility of enormous attention bonuses in case one strikes the mother lode in science increases, in other words the fortune of stardom will keep on rising. At the same time the chance of being crowded out in the attention game is also getting bigger. In order to deal with the excess, the phenomenon of stars as well as the lack of attention for most, scientists will converge in what we have called clusters: consensual domains and centers of attention. The information overload will make targeted communication more important than broadcast communication. The clusters will be part of larger fields but will be sufficiently distinctive to constitute a recognizable space of attention. In order to guard the quality of the attention members of cluster will screen contributions. They will expect contributors to have a relevant academic degree, an academic affiliation, and will referee the submitted texts. The evaluation will involve an assessment of rhetorical fit, that is, the overlap with the conversation that constitutes the cluster.

The digitilization of academic journals has the potential of changing the way scientists interact. The information and communication technologies of today and the near future allow for a more direct and interactive refereeing process and it will make rankings based on citations, hits, and downloaded papers easier. The intensity and speed of interactions will increase, thereby reinforcing the winner-take-all character of science but also making journals truly a reflection of conversations. Furthermore, the tracking of linkages between papers will make communication more intense, the linkage of databases to papers will enable more experimentation, replication can in principle yield more reliable information and the appearance of intelligent software agents who search the net in a systematic and intelligent manner will make that all relevant knowledge will be used in the production of new knowledge.

Screening will remain an essential part of science although the information overload will make it necessary to shift the responsibility of screening from the senders to the receivers of information (Van Zandt 2001). One way of doing this is by shifting from primarily *ex ante* peer review systems to primarily *ex post* review systems. Being an ex post evaluator might become a separate and valued quality within academia just like in open source projects (Lerner and Tirole 1999). The Internet clearly offers the technology to exploit alternative peer review systems. Possible suggestions for electronic publishing could amount to editors who initially grade papers by relevance, if authors find the grade too low, they can withdraw their paper, otherwise it is put on the web and correctness and real relevance are tested ex post (Varian 1997). Other *ex post* strategies which can help to the evaluation of science a spontaneous order in the new economy: (1) put all working papers on the net; (2) depending on how much times the paper was downloaded from outside the university/institute/country it can go on to the next stage, the electronic journal stage, where the paper is 'printed' with the (editorially refereed) reviews of readers (just like on Amazon.com), possibly with comments of the authors; (3) depending on the number of citations in the SSCI/SCI the paper – optionally adjusted to take account of the comments – is printed in a yearly volume of 'citation classics'.

However, the attention factor may compel the scientific community to stick to its well proved method of clustering. The Internet will not alter the fundamental principles of science as the internet itself is based on the same leading principles on which science is based: openness, communality and universality. Because it may open up more space, the need for clustering will only get stronger. Scientists want to move in groups they can manage, within which they can noticed by as well as pay notice to the other members. With the increase of specialized clusters in science we will see the appearance of more knowledge-broking journals (like the *Journal of Economic Perspectives* and the new abstract journal *Economic Intuition*). Of course, to fill this journal type authors will have to develop more knowledge brokering qualities or in case authors lack this marketing quality the brokerage function can be performed by true knowledge brokers.

Still not all is fine in the New Republic of Science as much of the future of publishing depends on how the character of Internet will develop. If it becomes a purely commercial affair in which access and distribution is restricted by private interests the character of science and therefore of journals will be debased. Intrinsic motivation is crucial when tacit knowledge must be transferred and the possible translation of indicators of 'success' (number of downloads, print outs, time spent reading) in science into extrinsic and explicit incentives will disrupt the internal organization of science. Furthermore it can diminish the much praised diversity of ideas within science as the competition for attention might generate too much consensus and too little diversity. Journals and their editors carry much responsibility in maintaining this diversity property. The social sciences, economics in particular, have to be careful in keeping the scientific debate alive as the audiences of economists range from the 'low brow' audience of local policy makers and citizens interested in 'what to make of it' to the 'high brow' audience of Academic Professionals reading only pure theory journals (Van Dalen 1998). The risk is that the distinction between the cultures of Science – norm-driven – and Technology – market-driven (Dasgupta and David 1994) – will disappear, thereby making public disclosure of results more and more susceptible to market valuation. Of course, without the Internet this process was already under way as journals and citations were already being used as discriminating factors in decision making, but the intensity of use under the Internet regime will differ considerably as the information overload will make science more and more an art of persuasion. The scientific community may resist the development by holding to their organization in the form of associations and by keeping control of the screening and editing of the common digital space themselves instead of surrendering those tasks by those who have commercial interests.

Summing up, the coming age of electronic journals can in principle circumvent much of the inefficiencies of hard-copy journals, but new distortions lurk. The basic principles of scientific communication coincide to a large degree with the principles of the Internet and open source code projects. The future of electronic journals depends to a large degree on which norms and values will prevail on the net: those of the market or those of science.

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NOTES

1 Even partners in a marriage will fail to know fully what the other knows, their (hopefully) frequent and intensive interactions notwithstanding.
2 However, Smart and Waldfogel (1996) show that manuscripts by non-top authors receive editorial treatment more favourable than their status warrants.
3 And even those rejected articles that have become core ideas in economics have generally ended up within the circle of core journals (see Gans and Shepherd 1994: 167).

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