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The scientific economy of attention: A novel approach to the collective rationality of science

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Science is the core sector of present-day knowledge production. Yet, the mechanisms of science as an industry are poorly understood. The economic theory of science is still in its infancy, and philosophy of science has only sparsely addressed the issue of economic rationality. Research, however, is costly. Inefficient use of resources consumed by the scientific industry is as detrimental to the collective advancement of knowledge as are deficiencies in method. Economic inefficiency encompasses methodological inadequacy. Methods are inadequate if they tend to misallocate time and effort. If one omits the question of how inputs are transformed into outputs in self-organised knowledge production, this means neglecting an essential aspect of the collective rationality of science.

A self-organised tendency towards efficiency comes to the fore as soon as science is described as an economy in which researchers invest their own attention in order to obtain the attention of others. Viewed like this, scientific communication appears to be a market where information is exchanged for attention. Scientific information is measured in terms of the attention it earns. Since scientists demand scientific information as a means of production, the attention that a theory attracts is a measure of its value as a capital good. On the other hand, the attention a scientist earns is capitalised into the asset called reputation. Elaborating the ideas introduced in *Franck* (1998) and (1999), the paper describes science as a highly developed market economy. Science conceived as capital market covers the specific conditions under which scientists, while maximising their reputation, optimise output in the eyes of those competent to judge.

Attention is not just any resource. It is the resource whose efficient use is called intelligence. Science, as an industry transforming attention into cognitive output, is bound to miss the hallmark of rationality if it does not pass a test of collective intelligence. The paper closes with considering the prospective outcome of such a test.

Collective rationality

Science is a collective endeavour. It is not the business of isolated persons or research units, but an industry that combines the work of specialists who collaborate with other specialists in producing inputs for other lines of specialised production. It is only through an efficient division of cognitive labour that science can function rationally

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from a collective point of view. There is, however, no centralised agency planning the distribution of talent and effort over the various lines of investigation. The division of scientific labour must organise itself. This does not mean that there are no central agencies influencing the distribution of talent and effort among the various lines of investigation. Governments fund certain areas more than others, military interests massively influence research in certain areas, industrial funding is easier to get for certain projects rather than others. However, these influences are not indispensable to organising science as distributed production. Nor do they inherently favour the collective advancement of science. They are, at best, neutral with respect to the overall efficiency of public knowledge production. Assuming that science is self-organised means taking the claim of its autonomy seriously. The most clear-cut way of substantiating the autonomy of science is to show that it performs best when it is financed lump sum by the society making use of the knowledge produced.

How does science organise itself? Does it self-organise in a way that allows or even secures optimality? Or does the self-organisation of distributed scientific labour prevent overall efficiency? These are the basic questions in considering the collective rationality of science. As long as the available talents and efforts go into occupations that are socially sub-optimal, resources will be wasted, as hard as the individual scientist may ever be working. Scientific production will miss the mark of collective optimality even if it is optimised from the viewpoint of the individual doing the job.

One may object that the measurement of efficiency, necessary as it may seem for organising science rationally, cannot be applied operationally to scientific work. Efficiency concerns the output into which used resources are transformed. The output of scientific work consists of scientific information. How is one to define scientific information operationally? Scientific information is semantic in nature, which means that it must be understood before any measurement can be attempted. Moreover, the output of scientific work is the input of subsequent stages of knowledge production. The pragmatic value of scientific information thus depends on the productivity it enhances when used as a means of production. Who, however, is competent to understand scientific information? Only those who are themselves working in the field. How do those working in the respective field express the value they attribute to the theorems, hypotheses and scientific facts they understand? The measuring rod of economic value is the preparedness of those interested in a particular item to pay for it. Yet, the output of scientific production is not sold for money, but published and thus delivered free of charge.

If the assessment of efficiency should turn out to be impossible without a comparison of inputs and outputs in terms of money prices, measurement of the economic performance of knowledge production is bound to fail. In this case, the question whether there is something like collective rationality of science would remain without answer. However, there are other ways of assessing efficiency. We shall return to the problem of measuring scientific information below. One approach immediately at hand when assessing economic performance is to consider the social circumstances under which individual agents pursue their interests. Overall efficiency means there is an adequate mix of competition and collaboration among the individuals exchanging the products of distributed labour. Hence, what are the individual scientist's incentives to compete or collaborate with fellow scientists?

When looking into scientists' incentives for competition or collaboration, it is not enough to consider epistemic motivations. Epistemic motivations, such as the joy of discovery or the pleasure of problem solving, are quite neutral regarding the inclination to compete or collaborate with others. Why compete if it is only joy of discovery that counts? In order to come first? The joy of discovery need not diminish by sharing it with others. Why should one collaborate if the sole thing that matters is intrinsic intellectual satisfaction? It may be worthwhile to collaborate in order to exploit complementary skills in solving the problem at hand. However, even though it may be intellectually worthwhile to embark on joint research ventures, the purely intellectual rewards furnish no incentive for collaboration if this means doing what fellow researchers judge to be useful. Yet, it is this sense of collaboration which is crucial for divided scientific labour to self-organise efficiently.

The motivations of scientists for competition or collaboration lie in their quest for success. To be successful in science, making discoveries is not enough. Success in science is tied to priority in communication. It is only the first publication of some advance in knowledge which counts as a scientific discovery. Scientists working for success thus cannot help caring to arrive first. Trying to be first means being in a contest with those who are working on the same subject. Moreover, in order to make your discovery a real success, you have to make other people curious about what you have found. You have to find others who are interested in using or replicating your findings. In short, you have to sell your product. If you must sell your product you are well advised to care for what others find useful.

Does this mean that desire for personal success is the basis of efficiency in scientific production? Both the quest for priority and concern for what others find useful are non-epistemic motivations. Why should individuals led by non-epistemic motivations do what they are supposed to as cognitive agents? In fact, both the quest for priority and

concern for marketing are suspected to distract scientists from what they are supposed to do. Economists are worried about the quest for priority. *Dasgupta* and *David*, for example, suspect that competition in science suffers from inefficiencies because competing for priority means that the winner takes all. Conducted as a contest in which the winner takes all, research assumes the traits of a race. Since it is only the winner's contribution that adds to the social product of an industry, *Dasgupta* and *David* (1994) conjecture that competition in science leads to an excess of parallel research on the same subjects (pp. 506ff).

The incentives introduced by the priority rule seem to be harmless compared with those fuelling the race for recognition. If you are running after recognition, why should you refrain from conspicuously advertising and aggressively marketing your product? If it is public recognition that counts, why should you rely on epistemically virtuous individual reasoning instead of trying to enrol allies who will help to conquer public opinion? In the face of temptations for opportunism and politicking, is it not downright absurd to assume that the run for recognition will act as an invisible hand guiding science towards efficiency? Is it not even naïve to think that scientific controversies are resolvable by appeals to reason and evidence if it is success in impressing the community that counts?

The problem with verifying or refuting this suspicion is that there are no independent ways of testing epistemic productivity. The hallmark of scientific information is truth. The truthfulness of scientific information cannot be immediately tested since we have no immediate access to reality supposed to exist independently of being experienced. There is no scientific experience free of theoretical preconceptions. Nor are there ways of evaluating scientific theories and facts that can dispense with having first understood them. As a rule, however, understanding scientific facts, problems and theories is not trivial. This is why only those personally working in the respective field are competent to judge the value of a piece of scientific information. Accordingly, the test of epistemic productivity depends on the judgement of those who consider such information as useful means in their own production. We are thus caught in a circle. The test of the productivity of scientific information lies in the production of scientific information.

It seems natural to look for an ultimate test of scientific information by its proving useful outside science. A long and lasting discussion has shown, however, that criteria of usefulness are even harder to define that those of truth. Hence, there is no way of circumventing the circle of productivity. We are left with the diagnosis that it is only through accordance of the interests organising scientific production with the interest in truth and understanding that science can function rationally as an industry producing knowledge. Since lack of interference cannot mean that non-epistemic and epistemic

motivations are prevented from mixing, science will work rationally under the sole condition that the researchers who are competent to understand and who are interested in using the information supplied do indeed have good reasons for judging this information carefully and expressing their judgement honestly.

If the social circumstances under which average scientists pursue their interests do not support this condition, heterogeneous interests will shape the doctrines that are accepted and the styles of reasoning that are praised. In this case it would be pointless to ask for the conditions of efficiency in scientific production. The referents of theories shaped this way would in fact be the artefacts which some sociologists of science believe referents of scientific theories to be. Before proceeding to ask about the conditions of economic rationality we should thus try to reaffirm that our task is not hopeless from the beginning.

Setting the stage

The question whether the motives inducing scientists to compete or collaborate are doomed to undermine the collective rationality of science is addressed by *Kitcher* (1993, Chapter 8). *Kitcher* asks whether a philosopher-monarch interested in organising the scientific workforce so as to promote the collective achievement of significant truth should give his blessing to the mixing of epistemic and non-epistemic motives (p. 305). *Kitcher* pursues his argument by reconstructing the strategies that rational researchers will apply when confronted with information challenging established theories. He considers two kinds of researchers: epistemically pure ones and those whom he calls scientific entrepreneurs. The purists are motivated by intrinsic intellectual rewards only. When confronted with heterodox theories, their only concern is to reach an epistemically valuable state. For entrepreneurs, both epistemic value and personal success are important. Moreover, entrepreneurs are successful only if credited with success, where credit is a function of perception by others. Entrepreneurs must both compete for priority and sell their product. By comparing the collective outcome of the strategies applied by purists and entrepreneurs, *Kitcher* is able to show that a population of entrepreneurs is far from being doomed to perform epistemically worse than a population of purists. In contrast, the entrepreneurs promise to fare better than the purists for reasons of cognitive diversity and division of cognitive labour.

Kitcher does not investigate the conditions under which scientific entrepreneurs will judge the information produced by others carefully and express their judgement honestly. His approach is inspired by game theory and population biology (p. 305).

He treats science as a process in which cognitively limited biological entities combine their efforts in a social context (p. 9). This context is not specified as an industry, however, where scarce inputs are transformed into cognitive output. In order to be more specific, let us sharpen the definition of the scientific entrepreneur. Let us define the scientific entrepreneur as the researcher who maximises the recognition received from fellow scientists. The quest for recognition includes that for priority. It also includes the quest for being credited with the discovery. But that is not all. Striving for recognition includes the incentive to market one's product. The desire to maximise recognition includes the thirst for prominence and fame. Thus, the sharpened contours account for the negative undertones perceptible in talk about the business of science.

Recognition, too, is hard to quantify. Linguistic behaviour cannot be taken at face value in this case. Verbal assertions of esteem are a field of conventional lying. Nevertheless, there are ways of verifying the esteem expressed. Since we know that words are cheap, we look at the actions of those who are talking. The desire to be recognised is in search for recognition expressed by attention that is paid. Without a preparedness to pay attention to the recognised person, there is no recognition worth its name.*

Let us assume, thus, that scientific entrepreneurs maximise recognition in terms of the attention paid to them by their peers. Since the attention scientists pay to the work of colleagues is not cheap but scarce, it does not pay for them to lie when expressing interest in this way. Hence, if there are ways of measuring the attention scientists pay to the work of others, there is a way of measuring scientific recognition. Moreover, assuming that scientists maximise attention adds to realism by accounting for cases even worse than those accounted for by the maximisation of recognition. Chasing after attention is prominent among the unpleasant social factors threatening to push the scientific community into arbitrary directions. Scientists trying to maximise attention must not only care about selling their product, they must also care about making it a stir. Encouraging the maximisation of attention engenders the temptation to engage in showmanship and intellectual fraud. Portraying the scientific community as a population of entrepreneurs who maximise attention in the same way that businessmen maximise profit thus highlights the traits that need to be taken into account when asking whether non-epistemic motivations can act as an invisible hand guiding science towards efficiency in knowledge production.

* On this point or, more precisely, on the concept of "economy of attention" in general, see *Franck* (1998). The concept was first introduced in *Franck* (1993).

The economy of thought and the market of ideas

By defining scientists as entrepreneurs who allocate time and effort so as to maximise the attention received from other scientists we have adopted definitions of the scientific community and of the scientific industry as well. You gain full membership in the scientific community only by receiving attention from members who are paid attention. The scientific community is thus the community of those who work to earn the attention of those whose work earns the attention of scientists. This definition allows us to dispense with institutional criteria such as formal membership and affiliation. Moreover, defining the scientific community this way anticipates our definition of the science industry. The science industry is the scientific community viewed as a society of producers. The main inputs in scientific production are pre-processed information and fresh attention. A good deal of the attention added is paid to information produced by others. The scientific industry is thus a society of producers who pay attention to the information produced by others as an input to the production of information usable by others.

As is the case in industrialised material production, specialised lines of scientific production communicate through markets where supply meets demand. The market in which the supply of scientific information meets demand is called scientific communication. Effective demand means preparedness to pay for the item demanded. In material industry, factors of production are allocated according to the scarcity of inputs and according to the preparedness of those interested in the output to pay for it. The primary input in scientific production is attention engaged in research. Assuming that scientists strive to maximise the attention received thus implies that scientists allocate their own attention according to the preparedness of fellow scientists to pay attention to the outcome.

If we thus consider science as a closed economy of attention, the immediate task will be to look at the technologies and techniques that have developed to economise attention and to institutionalise scientific exchange. As an industrial way of producing knowledge, scientific production can be expected to imply not only delivery of pre-processed information, but also development of technologies to economise on primary inputs and to facilitate the exchange of intermediate products. Following the analogy of material production, we are led to expect that the technology economising on attention dedicated to problem solving will be of the kind that breaks down complex problems into simpler ones, some of which to be dealt with mechanically. By this analogy we are moreover led to expect that the technologies facilitating the exchange of pre-processed information will turn out to make this exchange safe and rewarding at the same time.

In fact, science is, and has always been, applied thought economy. The resource economised by thought economy is the attention that is at the thinker's command. From its very beginning, scientific research has been tied to the development and application of techniques improving the efficiency of attention in theory building. Be it work in taxonomy and classification, or the specification and refinement of terms through the elaboration of terminologies, be it the definition of symbolisms enabling calculation through mechanical manipulation, or the development of algorithmic languages: the purpose of sharpening and formalising the tools of thinking is to enhance the capacity of the attention doing the job.* Moreover, since Descartes we know that the scientific way of solving problems consists in breaking them down into increasingly simpler ones until, in the final analysis, an elementary level is reached. However, it was not before the advent of digital technology that the economy of thought became susceptible to methods of automation. Now, problems capable of being broken down to an elementary level are solved by, or with the help of, computing machinery. The state of the art in thought economy is characterised by automation of a good deal of the syntactic information from which scientific information is extracted. Automating the processing of information is the most powerful way of enhancing the efficiency of the mind's mechanics.

Progress in thought economy lies at the heart of material progress in science. Since scientific production consists of thinking as well as communicating, and since both communication and theorising consume attention, progress in thought technology should have its counterpart in the technology of scientific exchange. Scientific communication, as we know it, also had to develop gradually in the course of history. Since information is power, there is a constant temptation to monopolise it. Up to the seventeenth century, scholars, even in mathematics and astronomy, were pre-eminently concerned with protecting their claims to priority through secretiveness and mystification. (Cf. *Rescher* (1989, p. 34); on priority conflicts see *Merton* (1957).) Only through the emergence of new means of information sharing, such as academies and learned societies with their meetings and published proceedings, could modern science embark on secure and steady growth. "In particular, the open scientific literature can be seen as an effective and productive system for the authentication and protection of the stake of the creative scientist in the 'intellectual property' created by his innovative efforts."(Cf. *Rescher*

* For an early account of this observation see *Mach* (1883, Chapter IV. 4.). Remarkably, the issue fell into oblivion after his death. Until today, neither the psychology of science nor economics of science are concerned with thought economy. *Mach*, though highly influential as a pioneer of post-classical physics, remained inconsequential regarding his work on the economy of scientific thought.

(1989, p. 34), on this place *Rescher* is referring to *Ravetz* (1971, p. 249).) Publication puts intellectual property at the disposal of the general public under the sole condition that its processing into the user's intellectual property is credited by *citation*.

What are the motives behind this giving away of property? There is only one motive: to earn attention. Only where the desire to earn attention overrides that of earning money or gaining power will markets of scientific publication burgeon. But is money not involved in that business, too? It is, of course. This money, however, is earned by the publisher. Scientific publication flourishes wherever enough money can be earned by publishers and where enough attention can be earned by researchers. From the time these conditions were first met and established, scientific publication became a growth industry. This growth, in turn, fostered organisational development and refinement.* The journals, editions and conferences not only increased in number and degree of specialisation, they also increasingly became ranked according to reputation. Specialisation in contents facilitates overview, ranking according to reputation affords some initial discrimination between insignificant material and presumably valid work. Blind publication would have the disadvantage of a substantial waste of attention in looking for useful information. In order to accumulate and maintain reputation, agencies developed techniques such as the referee system. By thus becoming a dual system of publication and evaluation, scientific communication assumed the dual character of an exchange and pricing system. It developed, in other words, into a fully-fledged market. It was only logical that, with the *Science Citation Index*, a proper accounting system for the attention paid by way of citation was established.**

In remarkable analogy to industrialisation, the technologies improving scientific production and those improving scientific exchange have developed in unison. It is beyond the scope of this paper to follow up the analogy in detail. (For further information see *Ben-David* (1971) and *Ravetz* (1971).) What can easily be seen, however, is the mechanism thus put into operation. As soon as publication becomes the regular way of scientific communication, exchange of information and exchange of attention intertwine systematically. In publishing, researchers supply information in order to in turn obtain attention. Since the main input of scientific production is the researchers' attention, one can also say that scientists, in working for publication, spend

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* It shaped, moreover, the literary style of presenting scientific theories and facts. See *Gross* and *Harmon* (1999) for an account of the forces shaping the scientific paper as a literary genus.

** The SCI was founded by Eugene Garfied. Garfield's work is comparable to the achievement of the founding fathers of the banking industry in medieval Italy. As an account of this work see *Garfield* (1977). For a concise description see *Bonitz* (1990).

their own attention in order to obtain the attention of others. Since attention, both in the form of an input to production and in the form of a means of gratification, is scarce, one can even say that the allocation and exchange of attention mutually control each other.

Attention as a measure of scientific value

We are now equipped to consider the scientific economy of attention's performance. Do the conditions under which scientific information is exchanged for expert attention lead to an efficient allocation of the attention engaged in research? Is it conceivable that maximising one's income of attention is the appropriate motive on the part of individual scientists that will maximise the collective advancement of science? Might it even be that the individually contemptuous form of longing for attention, known as vanity, can turn out to be a social virtue?

The attention one can earn through publishing one's findings is maximised when supply meets demand. Demand is effective when expressed in preparedness to pay. The motive behind self-interested scientists' preparedness to pay attention to the work done by others lies in enhancement of their own productivity. Being maximisers of earned attention, scientists strive to use their own attention most productively. It does not pay for them to find things out anew that have been discovered already. Nor is re-invention rewarding in terms of the attention paid. It would thus be a waste of time and effort not to pay attention to the work done by others. When paying attention to the work done by others, those demanding scientific information are effectively collaborating with the suppliers. Conversely, suppliers of scientific information are also collaborating with those demanding it, since to compete for attention not only means pursuing one's own interest but also being concerned with what is useful for others. Motivated concern for what is useful to others is the first and most crucial condition for self-interested action self-organising into an efficient division of labour.

Does this mean that scientists do what they are committed to doing when they pursue professionally what other people are seduced to pursue out of vanity? Regrettably, things are not so simple. Even though the income of attention is a capable means of motivating scientists to optimise output in the eyes of those competent to judge its value, maximising the attention received from fellow scientists is not the same as optimising cognitive output. Maximisation of the income of attention automatically leads to efficiency only under the condition that the attention earned measures the scientific value of the information supplied unambiguously. However, the properties making a theory attractive may be quite different from criteria of scientific value such as those established by the 'logic of scientific discovery'. The attractiveness of a theory depends

on its looking suggestive, on its being an intriguing mix of clarity and obscurity, on its matching the *Zeitgeist*, on its referring to, or rebelling against, commanding authorities. A theory's ability to attract attention grows if it is published by a renowned publisher, or presented in the proper surroundings to the right public, or if it is reviewed by influential reviewers. The best way to promote a theory is to turn its keywords into slogans. All these features are inessential if not detrimental from an epistemic point of view. According to the logic of scientific discovery, only criteria such as consistency, correspondence to facts, range or productiveness are legitimate measures of scientific value (see *Popper* (1959)).

Looking at the scientific economy of attention's performance means investigating the conditions that align the attractive power of a theory with its value measured according to the above criteria. In order to uncover those conditions, let us translate the legitimate measures of scientific value into terms of productivity. Consistency, correspondence to facts, range and productiveness are measures of the value that a theory assumes when used as a means of theory production. These measures, to put it differently, are measures of the pragmatic value of a piece of information. The pragmatic value of a theory lies in its power to enhance the productivity of its user. In economic terms: the pragmatic value of scientific information is the value it assumes when used as a capital good.

A good deal of the difference between the attractive power and the scientific value of a theory can be explained by the fact that theories earn attention not only as capital goods, but also as consumer goods. Scientific communication is far from being the only market for scientific information. Rather, the science industry's final products are information exported to society, which in turn is financing this industry. The main export market is that of technology. However, scientific information is also imported by the entertainment industry. Moreover, the borderline between scientific publishing and entertainment is blurred. By making use of the leakage, one and the same product can be made to earn attention on both markets.

The first and foremost condition for turning attention earned into a measure of scientific value is that attention paid to information performing as a capital good must be separated from the attention this information attracts as a consumer good. This separation, difficult as it may seem, does in fact take place. It is a by-product of the rule that publication does not imply permission to use the information communicated commercially free of charge. Publication, to repeat it, establishes intellectual property. Property rights on the communicated information mean that it must not be used as a capital good without acquiring a licence to do so. The legally prescribed way of acquiring the licence to use somebody else's information as a means of production is

citation. Citation is not equivalent to being free of charge. Citation means to owe payment of a fee. The fee is discharged by the transfer of some of the attention earned by the citing author over to the cited author. Acquiring the licence of using information supplied as a capital good thus includes payment of a regular licensing fee.

The attention a piece of information earns by being cited in scientific publications is attention it earns as a capital good. Hence, it is possible in principle to discriminate between attention capable of measuring scientific productivity and attention that has nothing to do with scientific value. Asking which conditions will turn maximisers of attention into optimisers of cognitive output thus splits into two questions. First, under which conditions will scientists be prepared to discriminate among the sources of attention income? Second, which conditions will make attention paid for by way of citation a reliable measure of scientific value?

There are two reasons why scientists should discriminate between attention they receive via citation in learned journals and attention they earn by appearing in other publications or the mass media. The first is that attention received through citation is congenial with one's own. It is attention dedicated to the same or some related problem, it is attention sharing the same interest, education and intellectual passion. Since only those working in the same field are really competent to understand the problems one copes with, only the attention paid by colleagues will count as confirmation that one has accomplished a difficult task. In short, the attention received by being cited or mentioned in the context of scientific communication is of superior value for those whose self-esteem depends on being recognised as scientists. However, even for those who lack this distinction the attention received by way of citation can be of superior value. The second reason why scientists are obliged to discriminate between the sources of received attention has to do with their careers. It is not uncommon, to put it mildly, that those deciding on scientific careers pay attention to the impact the candidate's work has in terms of the citations it collects. There are certain disciplines of learning in which a scholar's career decisively depends on her or his SCI account. These are the disciplines, moreover, to which science owes its enormous prestige. Hence, for scientists who are both ambitious and capable of making a scientific career there are good reasons for discriminating between the sources and to focus on attention they receive by way of citation. This, of course, does not mean that the temptations to serve both markets are negligible.

Let us assume, for the sake of the argument, that scientists when maximising attention are maximisers of citations above all. The condition still lacking to turn them into optimisers of cognitive output is that citations should be a reliable measure of scientific value. The only way of measuring scientific value lies in accounting for the

judgement of those who are competent to understand and willing to assess the respective information. In order to measure scientific value, these judgements have to be brought together and to be made commensurable. But that is not all. Only those judgements count that are expressed honestly or, to put it differently, that reveal the value attributed. The condition that renders it rational to reveal one's judgement of value is that it is uttered in terms of preparedness to pay for the item. Citation in fact addresses the preparedness to pay on the part of those who are competent to understand and interested in using the respective information productively. Citing means crediting the productive impact that the cited information has on one's own work. It is not irrational, to say the least, to be honest in crediting this impact to somebody else's account by citation. Since citing is costly in terms of attention lost, it does not pay to exaggerate the impact credited. In so far as plagiarising is prohibitively costly when detected, it does not pay to understate the impact. Hence, counting citations is a way of measuring the productive impact of scientific information according to the preparedness of those competent to understand it to pay for its use. Again, however, a proviso is in place. Saying that counting citations is a measure of scientific value does not mean that everything is fine as soon as the account of citations becomes a generally accepted measure of scientific value.

The reign of competition

Without relying on some kind of measurement of scientific value, the scientific industry cannot work efficiently. If no measurement of scientific value is available, the individual scientist lacks the information needed to invest her or his attention in the most promising way from the collective point of view. Without this kind of feedback it will be pure chance if allocated attention is in line with the optimal advancement of science.

We have identified the conditions under which scientific value is measured and under which the value communicated becomes effective. Scientific value will be measured where it is rational for those using information produced by others to cite honestly. This measure becomes effective where it is the goal of the rational scientist to maximise the attention she or he is paid in terms of citations. Revealing those conditions, of course, does not mean that they prevail. Rather, it is easy to see that there are further conditions on which it depends whether the process of citation will work reliably and whether maximising citations is of overriding importance for the working scientist.

The process of citation is part and parcel of the process of scientific communication. Scientific communication is a market where information is exchanged for attention. Wherever there is a market there is a shadow market. It should thus not come as a surprise that there are ways and means of accumulating citations that have little to do with scientific value. The simplest way of circumventing the hurdle of productivity enhancement lies in the formation of citation cartels. Why not agree on citing one another mutually just to make the counting machine click? Another, more subtle but perhaps not less widespread way of augmenting one's account of citations without enhancing one's productivity is to use one's power as an editor or referee. Why not suppress papers submitted for publication until their authors understand whom they owe a citation?

In fact, the scientists calling the largest citation accounts their own will not always be top of the faculty. Nor is populism alien to all those who have made their way in science. Rather, cheating and politicking are to be reckoned with wherever social control is not watertight. The only form of social control fully compatible with the working of markets and with unrestricted freedom of thought is that which works through competition. Asking about the conditions making the process of citation work reliably as well as authoritatively thus means asking whether competition within scientific communication is enough to control the process.

Social control is watertight under the conditions of so-called perfect competition. I leave it to the interested reader to consult any textbook on economics for a detailed list of conditions defining what 'perfect competition' means. In the present context, two conditions are of immediate importance. The first is that the number of both suppliers and consumers should be large enough to prevent monopolies and monopsonies. Forming cartels is a typical case of building up monopoly power. Suppressing papers worthy of publication is a typical case of exerting monopsony power. The existence of such practices indicates that competition on the scientific market is not perfect. The second condition is that of symmetrical information. Perfect competition implies that those on the demand side are fully informed about the goods in supply. In order to fulfil this condition, researchers looking for useful information would have to scrutinise the information supplied in every detail. In the meantime, this condition has become unrealistic even in minor disciplines. Even there, scientists primarily devoted to their own production have to cope with a constant information oversupply. Under this condition it pays, or even becomes unavoidable, to just browse superficially through part of the supply. That is, it becomes rational to resort to guessing to a certain extent. Guessing is susceptible to eye-catching, showmanship and hard selling. The techniques

of advertising, PR and marketing are methods utilising a gap in rationality that is due to scarcity of attention. Rational choice is not only needed to use one's attention rationally, it is also an activity which consumes attention in its own turn. As soon as attention becomes scarce, choosing rationally how to use one's attention leads to a regress. Utilisation of this regress is the business of advertisers and sales managers. For a fuller account of the regress see *Franck* (1998, pp. 69ff).

In order to rule out these sources of unreliability, competition would have to become perfect in the textbook sense. However, no market is perfect in real life. So, the market for scientific information cannot be expected to work under conditions of perfect competition, either. The list of conditions defining perfect competition includes, among other things, perfectly symmetrical information, lack of any kind of scale economies, and reducibility of uncertainty to calculable risk. These assumptions are downright absurd in the context of scientific communication. The only reasonable condition to assume when considering the science industry's performance is that competition in scientific communication is effective.

In scientific communication a vast number of sellers meets a vast number of buyers. Even though monopolistic and monopsonistic positions are initially not prevented, they are exposed to notoriously strong forces of dissolution. In addition, it seems safe to assume that citations matter. In order to grow big in science you have to be cited. It is certainly possible to call a considerable account of citations one's own without being of highest rank. The converse, though, is not true. Nobody will be ranked highly as a scientist without calling a considerable collection of citations one's own. Hence, the assumptions that (1) competition works within scientific communication and that (2) those competing work to earn the attention of fellow scientists should not be rated as downright unrealistic.

It would be unreasonable to expect that assumptions (1) and (2) are strong enough to show an invisible hand that leads science up to the point of efficiency. The best one may expect is evidence for an invisible hand that establishes a tendency towards efficiency. Even here it would be hopeless, however, to look for necessary and sufficient conditions. We should be happy if we are capable of answering the question whether competition is in fact necessary for establishing a tendency towards efficiency.

Even the answer to this much more modest question is far from being obvious. To be sure, there are strong reasons in favour of an affirmative answer. The strongest is lack of a working alternative to competition. Competition, to repeat it, is the only way of anonymous social control that is fully compatible with unrestricted freedom of thought. However, there are details that matter. Two of them are immediately at hand. The first is

the suspicion, mentioned above, that the priority rule leads to an excess of parallel research on the same subjects. The second is that the capitalistic nature of the scientific economy of attention cannot be assumed to be totally harmless.

The priority rule establishes a contest in scientific discovery. The prize to be won consists of the attention that the prospective discovery promises to raise. The attention to be won depends on the importance that the scientific community attributes to the discovery. The more attention is at stake, the more candidates will enter the contest. Hence, the number of competitors increases with the importance of the discovery. However, it is only the winner's contribution that adds to the social product of the industry. There is no value added when the same discovery is made a second or third time. Typically, important discoveries are made several times. The difference in value between the first and the second discovery lies in the winner's temporal lead. As a rule, the social benefit of this lead is negligible. Therefore, the contest leads to a waste of resources. From the collective point of view, the competitors who have lost the contest would have done better to invest in other projects.

This does not mean that the losers' decision had been wrong from their individual point of view. It is perfectly rational for a scientific entrepreneur to enter a contest as long as the prize times the probability of winning exceeds the prospective gain of alternative projects. The probability of winning depends on the number of competitors. Even though this probability decreases with every entrant, it pays to enter if the prize is high enough. Hence, does the priority rule have the effect of making individual and collective rationality fall apart?

In order to answer this, let us turn the tables. When looking at the contest the other way round, we see that competition grows the fiercer the more importance the scientific community attributes to the discovery. Isn't this as it should be? The greater the importance attributed, the more attention is at stake. The more attention at stake, the greater the temptation for presumption and downright fraud. The only capable means of social control in science is competition. Hence, why should we not welcome the increase in competitors as a self-organised means of social control which emerges where it is needed most?

We see there is no simple answer to the question of the priority rule as a source of inefficiency. Before concluding that this rule is a source of serious malfunctioning, we must compare the social cost of the resources wasted on parallel research with the social benefit of heightened competition. Since competition is of such overriding importance, it is far from clear that its cost exceeds its benefit. Above all, it would be unfounded to conclude that competition is not capable of guiding self-interested action towards

efficiency. Rather, we have to be aware of the trade-off which prevents individual and collective rationality being aligned free of charge. As emphasised elsewhere (see, e.g., *Stephan* (1996, p. 1230)), this point deserves further investigation.

Mental capitalism

More serious doubts are in place regarding the capitalistic nature of the scientific economy of attention. Scientists, when competing for the attention of other scientists do so as suppliers of a capital good. The output offered and transacted turns into the input of subsequent production. But that is not all. The attention received is accumulated and eventually turned into a form of capital as well. The form of capital a scientist eventually builds up through the attention she or he earns is called reputation. Scientific reputation is to scientific information as financial assets are to real capital. Scientific information, like tangible assets, consists of a heterogeneous collection of means of production. This information is sold for attention in the same way as real capital goods are sold for money. Attention, like money, can be accumulated and capitalised into an income-generating asset. The matching goes as far as implying that reputation, as an asset, bears different kinds of profit. Reputation bears 'normal' profit due to the productivity of its owner and surplus profit due to the amount of wealth accumulated. The larger your account of citations, the more attention you earn not only for having been productive, but also for being a big earner of attention.

In the scientific economy of attention we are confronted with a fully-fledged form of capitalism. Accordingly, we may expect that the same kind of unequal distribution and even class rule prevails in the scientific community as is characteristic of money-driven capitalism. In fact, there is a remarkably uneven distribution of citations. There are few who receive many citations and many who get only a few. The distribution curve is so clearly skewed that the phenomenon captured the attention of sociologists of science since the beginnings of that discipline.[*] *Merton* (1968) called this conspicuously uneven distribution the 'Matthew effect in science'. The Matthew effect refers to the Biblical parable of being entrusted with talents, the text reading that "those who have will be given and those who have not will be taken away" (Matthew, 25, 14-30).

The Matthew effect still awaits economic explanation. The explanation suggesting itself in the context of the scientific economy of attention lies in the quality of reputation as an income-generating asset. To receive attention not only for being productive, but also for being renowned, means to be given because one is rich in attention. Receiving

[*] The pertinent names are Alfred Lotka, Derek de Solla Price and Robert Merton.

attention for being rich in attention is the epitome of 'unearned' income. In economic terms, this kind of income is called rent. In the economy of attention, the class enjoying this rental income are those who are prominent. The prominent are the class of people who are really wealthy in the economy of attention. They are wealthier than is due to their merit. Actually, they have grown so rich because they have become celebrities.

The commotion around celebrities is detrimental to the collective progress of knowledge. The accrual of rent, to begin with, interferes with the ability of realised income in attention to serve as measure for the earner's productivity. The attention prominent scientists receive bears no clear-cut relation to their productivity. In fact, it may get into conflict with the attention their current production deserves. The homage paid to prominence is one of the major conservative elements in science. And it is a most questionable one. Science is a forward-oriented activity. Its strength lies in the productive destruction of outdated wisdom. The destruction of such wisdom is all too easily blocked by the prominent names attached to it. Why on earth do prominent names play such an eminent role in science, then? The answer is simple: because vanity is so common among scientists. It is vanity that seduces us to valuing the attention we receive according to the income class to which the payer belongs. If we find ourselves esteemed by someone prominent we feel better than if we are paid attention by just anybody. In order to obtain attention that incorporates renown, we tend to discriminate in our own payment of attention in a way that diminishes the power of the amount paid to measure the productivity of the earner.

Hence, vanity may indeed become a threat to the collective rationality of science. However, how serious a threat is this? Economic rationality is a matter of degree. Humans are never perfect. Within imperfection, trade-offs have to be taken into account. The trade-off in the role played by rental attention within scientific communication is that between orientation enjoyed and disorientation suffered. Ranking authors according to prominence is a way of facilitating overview, as is the ranking of the agencies of publication according to their reputation. In either case, the market value of the capital involved enables readers to make use of the value judgements which former readers have expressed and confirmed.[*] In addition, rent accruing from wealth of attention is what makes a scientific career especially attractive for the highly talented and the highly ambitious. It is the wage of fame. This wage is worthy each and every effort for those who believe to have a chance of gaining it. Science might be a good deal poorer in geniuses without offering the prospect of this superior reward.

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[*] On this point see *Merton* (1973, pp. 439-459).

Yet, this trait remains dubious. The social benefits connected with the prospect of fame have their counterpart in the social costs of abysmal differences in income and wealth of attention. The capitalist economy of attention is thus not immune to the classical objections against capitalism. What, therefore, are the social costs of the capitalisation of accumulated attentive wealth? Must we face similar forms of exploitation and social conflict as in the case of money capitalism? Or is this mental capitalism of a more agreeable nature?

In money capitalism, competition among persons is embedded in competition among organisations. Differences in personal wealth and income are overshadowed by the differences in power and influence exerted by firms. The same applies to mental capitalism. Science is not only the business of individual persons, but also of organisations. As a rule, individuals participate in the market of scientific information as members of a university or research establishment. When competing for attention, they do so in their own name as well as in the name of the organisation they represent. The organisation participates in the income earned by its members. The share accruing to the company is accumulated and capitalised in its own turn. The capital thus generated is the renown enjoyed by the organisation itself. The same applies at the level of nations. *Bonitz* and his collaborators have shown that the Matthew effect also operates with respect to countries. See *Bonitz* et al. (1997).

It is here where forms of class rule enter the scientific business. The renown owned by the organisation is an asset its members are free to exploit when competing with members of other research institutions. You are privileged from the outset when entering the competition as a member of a famous university or awe-inspiring research establishment. Conversely, you are handicapped from the outset if you are without recognised affiliation. However, whether or not you have access to such a privileged position does not only depend on your talent and merit, but also on the place where you were born and on the social class your family happened to belong to.

The existence of a class ruling through institutional renown tends to impair distributive justice and efficient allocation at the same time. It leads to an allocation of talent and effort that is different from the one that would maximise the overall advancement of science. But does this misallocation suffice to undermine a presumed tendency towards efficiency? The answer could perhaps be affirmative if the reputation a person acquires through privileged access were inheritable. The answer would probably be affirmative if the class monopolising institutional renown were capable of monopolising ownership of knowledge capital as well. Neither is the case. Scholarly reputation, as personal property, cannot be transferred to other persons. Nor can authorship of scientific information be sold off. Accordingly, there is no inheritance of

scholarly reputation, prominence and fame. Each scientist must start her or his career from scratch. Even the attention a person eventually receives for being known to be known has the economic form of interest accruing from personal savings. Intellectual property, on the other hand, comes into existence by way of publication. The only way in which organisations may monopolise ownership of knowledge capital is by suppressing publication and taking out patents. Patents, however, are property rights withdrawn from scientific communication altogether.

In science there are revolutions, but there is no class struggle. In scientific business, there are vested interests, but there is no propertied class monopolising ownership of productive capital and personal wealth. There are forms of exploitation and social conflict, to be sure. These forms, however, are of a more harmless nature than the ones characteristic of money capitalism. Without playing down the blemish of the unequal distribution of opportunities it is safe to state that mental capitalism is superior to money capitalism in terms of distributive justice as well as in terms of the allocation of scarce resources. Even though the scientific economy of attention is far from being perfect, it comes close to being a model economy when compared with money-driven market economy.

The scientific economy of attention: a case of collective intelligence?

Economic efficiency is not a question of all or nothing, but a matter of degree. Accordingly, examining the scientific industry's economic performance does not mean asking whether scientific knowledge production is performing perfectly. It rather means asking whether there is a tendency in scientific production that will allocate the resources disposed of in such a way that this optimises the collective advancement of knowledge. Such a tendency does indeed seem to exist, even though the evidence rests on nothing more than the plausibility of the conjecture.

It would be difficult if not impossible to state the necessary and sufficient conditions of efficiency in scientific production. Accordingly, it would be asking too much of the foundation of the economic rationality of science to give a full account of those conditions. The reasons for the difficulty, however, are theoretical ones. So far, we do not dispose of any description of scientific production that would allow us to deduce the full set of conditions. It even seems unreasonable to hope for such a description. For the time being, investigating the economic rationality of science means looking for conditions which, on the one hand, are not unrealistic and which, on the other hand, do not render it implausible to assume that a tendency towards efficiency prevails.

Conditions such as these come to the fore when describing science at the level of an economy of attention. As soon as we assume that competition in scientific communication works and that the competitors work both for intrinsic intellectual rewards and to earn the attention of fellow scientists, a tendency towards efficiency is waiting to self-organise. Moreover, this tendency, when free to self-organise, proves to be quite robust. The scientific economy of attention, from whichever angle we consider it, presents itself as remarkably sound.

Of course, there are sources of inefficiency. These sources, moreover, have to do with the mixing of epistemic and non-epistemic motivations. It would be the wrong remedy, however, to suppress the non-epistemic motivations. The run for attention is a major driving force in the astonishing dynamics of science. The mix of intrinsic intellectual rewards with payment in attention constitutes the strength of the reward structure in science. Even vanity is far from purely being a threat to efficiency. It is, at the same time, an invaluable, because inexhaustible, source of energy. Above all, the reward of attention is indispensable as incentive for researchers to compete and collaborate. The attention to be earned is what makes it rational for individual scientists to optimise output not only in their own eyes, but also in the eyes of those who share the expertise. It is the reward of attention that renders it rational to pay attention to the work of peers, to judge the information supplied to the best of one's knowledge, and to express one's judgement honestly. It would be fatal for efficiency not to make use of this incentive. When looking for ways of improving efficiency, the focus should be on the mechanism of social control.

Competition, though effective in general, is not strong throughout in science. Rather, there are niches that are well protected against competition. These niches, however, have much more to do with the institutional background of science and the ways it is funded than with the logic of the exchange of information for attention. Moreover, social control need not be watertight to be efficient. On the contrary, it would be prohibitively costly to intensify control up to the point where illegal behaviour is eradicated. Under realistic conditions it is the trade-off between the possible cost incurred and benefit to be expected that matters.

Thus, it is reasonable to assume that a self-organised tendency towards efficiency exists within the scientific economy of attention. Assuming that such a tendency prevails does not mean to suppose that the overall efficiency of scientific production is remarkably high. It rather means that cases of serious malfunctioning and gross misallocation are probably not due the search for attention. Instead of being suspicious

of the self-organised exchange of information for attention, we should focus on the ways science is financed and organised from outside when looking for remedies against its major defects.

The findings discussed so far are preliminary and tentative. However, if substantiated they afford an explanation of the strength of science to gain cultural and even economic leadership. In the exchange of information for attention an incentive system is operative that links the collectively most rewarding allocation of the attention doing research with the maximum value of the attention the individual producer can earn. The efficiency enabled by this incentive system is of a higher order than the efficiency scientists working in isolation can attain. Are we thus justified to speculate that science could pass the test of collective intelligence?

Intelligence, in whatever way it may be defined elsewhere, means to be capable of efficiently using the attention at one's command. Efficiency is contingent on an objective function. The objective function a sentient being is supposed to maximise concerns the quality of her or his life. The objective function that science is supposed to maximise is the advancement of knowledge (or, more precisely, the advancement of knowledge according to what the members of the scientific community consider relevant). If a mechanism is thus operative in the scientific economy of attention that allocates the attention at service in a way which maximises the advancement of knowledge, science, as a social system, deserves to be called intelligent.

Granted that the efficient use of attention is a necessary condition of intelligence, is it sufficient also? The meaning of intelligence has wide ramifications. These ramifications include, or at least touch upon, self-assurance. Self-confidence concerning the use of one's attention includes, or at least touches upon, awareness of the subjective nature of attention. The subjective nature of attention lies in its being tied to consciousness. Consciousness must not be confused with the capacity of selectively processing information. The capacity of processing information selectively can be technically reproduced (at least in principle), the quality of being conscious cannot. Being conscious denotes a 'how it feels' quality to which only its own subject has access. This 'how it feels' or, as it is called, *phenomenal* quality of consciousness does not exist in the perspective of the third person. Accordingly, phenomenal consciousness has no place in the scientific view of the world.

The question of sufficient reason is this: How much ignorance about oneself is compatible with being intelligent? By being ignorant of the phenomenal quality of consciousness, science is ignorant of an essential property of its main productive factor. Being conscious is a regular if not necessary concomitant of self-assured cognitive and creative functions. But that is not all. The supposedly phenomenal quality of foreign

consciousness is what makes the exchange of attention so attractive. When striving to obtain the attention of others it is not the processing of data in foreign nervous systems we have in mind. What we are looking for is entering another sphere of conscious experience. The wish to earn attention is the wish to play a role in other people's consciousness. What we are addicted to are the feelings welcoming us over there. We are definitely not satisfied with unconscious or half-conscious data processing having to do with us. It would be even one of the hardest offences to our vanity if we found ourselves fooled by an automaton that only mimics conscious behaviour.

Without notion of the quality of subjective awareness, science is poorly equipped to understand its own strength in making efficient use of the attention being at its service. This mild form of schizophrenia could be overlooked if it had no impact on the role that science plays as a social system. Yet, its being oblivious to the nature of subjectivity not only blurs the image science entertains of itself, it also taints the view which the scientific community entertains of the world out there. Whether a world view implies or excludes phenomenal consciousness makes a big difference concerning the value attributed to certain research projects and to the ethical questions involved. A collective kind of intelligence that is blind to the needs and wants of subjectivity is without soul and void of morality. It is due to this blind spot that people intuitively hesitate to award science the mark of collective intelligence.

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