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The Rationality of Science and the Rationality of Scientists

ABSTRACT. The prevalent view in science studies and the philosophy of science is that the presence of individual, non-epistemic interests within the motivations of scientists constitutes an impediment for the attainment of the epistemic goals of science. This paper shows how methodological rules can be seen as a ‘social contract’ between self-interested scientists, in order to create a ‘game’ that is both stimulating to play for individual researchers, and efficient in the production of epistemic value.

KEY WORDS: economics of scientific knowledge, contractarian epistemology, scientific norms, scientific rationality, rational choice

1. Introduction

In this paper I want to present the guiding lines of a research programme into the economics of scientific knowledge, a programme whose ultimate goal is to develop what I would like to call a *contractarian epistemology*. The paper has, for this reason, a merely programmatic nature, in which some of the essential or fundamental aspects of the problems and strategies considered in this approach to (scientific) epistemology are exhibited, though only marginal reference will be given to specific arguments and solutions offered within the approach, for which I shall refer to other papers from mine. The main originality of this view of the epistemology of science is that it gives a fundamental role to the *strategic* (in game theoretical terms) aspects of the decisions that scientists have to make and that have some important effect on the epistemic quality of the outputs of scien-

tific research. Taking into account the academic relevance of those social sciences in which rational choice and game theory are a central element (mainly economic theory, but also, e.g. in recent years, big parts of sociology and political science), as well as the massive work on the philosophy and methodology of these social sciences, it is a disappointing fact that the studies on social epistemology and sociology of science have, with very few exceptions that I shall mention in the paper, tended to ignore the application of game theoretic tools. One important goal of this article would be to encourage other authors to consider this line of work. The structure of the paper is as follows: in the first section I will comment on two conflicting approaches to the topic of rationality in science: the view of the rationality of *scientific knowledge* as deriving from the employment of sound methodological norms, and the view of *scientists* as rational agents pursuing the optimisation of their own personal and professional interests. In section 2, I will try to make both approaches mutually consistent by showing that a competition among rational ‘recognition-seekers’ is only possible if they agree in accepting some system of methodological norms. Section 3 will be devoted to analysing the main kinds and properties of these norms. Finally, in section 4, I will discuss a problem that is far from being easy and innocent: why are scientific norms obeyed by researchers, once they have been established in a scientific discipline?

2. The rationality of science versus the rationality of scientists

According to a traditional view, a view not only common among philosophers but also among the general public, the objectivity and validity of scientific knowledge would be the necessary outcome of scientists’ following an appropriate set of methodological rules. What makes these rules ‘appropriate’ would be their *efficiency* in promoting the cognitive and practical ends of scientific research, but philosophical discussion begun when different authors proposed different conceptions about these goals as well as different ways to assess the efficiency of given rules. Over and over again it has been discussed whether the aim of science is the attainment of

truth *simpliciter*, or that of indubitable truth, or that of probable truth, or that of empirical adequacy, and so forth. Less frequently, and only much more recently, it has also been discussed whether the justification of methodological norms should be given by *a priori* reasoning or by means of empirical research. Usually, epistemologists took for granted that, being methodologically a kind of propedeutic for the attainment of knowledge (particularly of empirical knowledge), it would be circular to try to justify the efficiency of norms on the basis of the kind of knowledge these norms are devoted to justify. So, the only remaining possibility was to approach the study of scientific method as a kind of *a priori* reflection. This ‘rationalist’ conception about scientific method is shared by authors having a few more ideas in common, such as Aristotle, Descartes and Bacon (among classical philosophers), or as Rudolf Carnap and Karl Popper (among XXth century methodologists). Popper, at least, defended the view that the rules of science are *conventional*, in the sense that, being a matter of decision, they cannot be derived from logic alone; but I do not think he approved the idea that the ‘right’ methodological conventions could be *empirically* discovered. The merit of having proposed this view corresponds basically to the philosophical approach known as *scientific naturalism*, and in particular to the work of Larry Laudan, who has defended the idea in several of his books [for example, Laudan, 1984].

I think that the attitude of real scientists to ‘methodological norms’ is much closer to Laudan’s theory than to the traditional philosophical conception. In fact, when scientists talk or write about ‘methodology’, they usually refer to the *practical procedures* they follow when performing an experiment, for example; and the efficiency of those procedures (a process for analysing chemical substances, a way of dissecting animals for observation, the use of certain ways of calibrating measuring instruments, etcetera) is obviously the outcome of ‘dirty-hands’ empirical research, not of anything similar to philosophical reflection. Hence, if most scientific *methods* are empirically established, or, in other words, if the appropriateness of these methods is always recognised within a scientific community by means of scientific research, then the idea that methodology should not be seen as a kind of *a priori* reflection ceases to be surprising.

Nevertheless, this *naturalist* idea still corresponds to the more general traditional view I mentioned at the beginning: the view according to which the rationality of *science* is grounded on the *soundness of methodological norms*. To this view many sociologists of science (and some philosophers as well) have posited in recent times a striking opposing vision of the process of scientific research. These sociologists, the so called ‘constructivists’, assert that scientists are usually *not* motivated by ‘a disinterested pursuit of truth’, but, instead, by the pursuit of personal and professional goals, especially the attainment of *public recognition* from their peers. They also assert that the production of scientific knowledge is usually *not* governed by methodological rules, but by the strategic choices of competing individual scientists, who only ‘obey’ a rule, or employ it, to defend a particular choice when doing so is in their own interest. Constructivism, hence, opposes to the *epistemic* rationality of scientific *knowledge* and scientific *method* the *instrumental* rationality of *scientists* considered as agents, whose only goal is optimising their own personal situation, either within the restricted game of science or within society at large.

This view of scientists is by no means an arbitrary fiction of some sociologists: it is certainly well supported by an impressive amount of empirical case studies,¹ and I will take it here as an undisputed assumption. What I will ask in the first place is whether it is necessary to conclude that both conceptions about the rationality of science are really contradictory, or if they can be made mutually consistent?

3. Personal interests and methodological norms

What I am going to defend in this section is that the view of scientists as ‘recognition-seekers’ is not only consistent with the existence of methodological norms. My thesis is still stronger: I affirm that, given the high degree of consensus we observe in many areas of natural science, this assumption about the motivations of researchers *entails* that research processes *must be governed by some methodological norms*. In order to prove

¹ One of the first was Latour and Woolgar, 1979.

this thesis, we have to take seriously the idea that scientists are *rational agents*, that is, we have to look at them through the lens of economic theory, particularly through the lens of *game theory*. The fundamental game theoretical concept is that of *equilibrium*: when what an agent obtains depends not only on what decision (or ‘strategy’) is taken by him, but also of what decisions are taken by the other participants in the ‘game’, then the only combinations of decisions that can take place are those in which the decision of every agent is *the best possible response* to the strategies of the others. i. e., we will hardly observe a social situation corresponding to a combination of decisions which is *not* an equilibrium.

So, imagine the following situation: 1) you are a scientist and you have to decide whether to enter a ‘race’ for the solution of an unsolved scientific problem or not; 2) you are a ‘recognition-seeker’, that is, your fundamental goal is having your own solution to the problem explicitly accepted by your colleagues; and 3) the other participants in the ‘race’ have exactly the same kind of motivation as you. Besides this, imagine you have read or heard something about the contemporary philosophy of science, and so you know that no scientific hypothesis can be conclusively confirmed (I actually think that most researchers are aware of this possibility without knowing anything about philosophical epistemology). So, to the purported solutions presented by your competitors, you could always respond that they are not still ‘supported enough’ by the facts; this entails that you will never be *forced* to accept a solution advanced by a colleague. Your problem is that *all your rivals* have also the same option regarding the solutions presented by everybody else! Obviously, the conclusion is that the game of research is absolutely pointless for you, because you know *a priori* that *you will never have a chance of ‘winning’ the game*, that there is nothing you can do in order to force your competitors to recognise that your solution is the right one. Expressed in game theoretical terms, the equilibrium (or at least one of the possible equilibria) of the ‘persuasion game’ is that each researcher would adopt the strategy of never accepting publicly a solution proposed by another researcher; this would be a kind of epistemic ‘prisoner dilemma’, in which the community of scientists reach a suboptimum result, whereas each member is trying to get as much as they want.

The history and sociology of science show us that this situation happens very frequently: no agreement about the appropriate methods or acceptable theories is reached in many research processes. But my argument is that, if scientists are ‘recognition-seekers’, and if they have always open the possibility of rejecting a rival’s solution for not being ‘confirmed enough’, then we could *never* observe a general agreement about a scientific fact, theory or law. Actually we could never observe anything like scientific research, because it would be a game that nobody would like to play. Hence, if science does exist, and if agreements about facts, laws or theories do exist, then it is necessary to conclude that *the game of scientific research is organised in such a way that scientists do not have permanently open the possibility of rejecting the solutions presented by their rivals*. There must be some circumstances where the acceptance of a proposed solution becomes *compulsory*. Or, stated differently, we can conclude that: 1) every game of scientific research must be subjected to some rules, and 2) researchers must know that their colleagues usually comply with these norms. If these conditions are not met, recognition-seeking researchers will simply have no interest at all in playing the ‘persuasion game’.

So, we can see methodological norms as *mutual constraints* collectively adopted by the competitors in a research process. These constraints are simply the rules of the game, the rules that make the game both exciting and profitable. Given certain rules, every researcher will have to decide whether it is interesting for him to enter the game or not. And analogously, given a set of people having entered the game, they obviously can ‘negotiate’ whether to keep the prevailing rules or to modify them, a possibility that I will discuss with more detail in the next section.

4. Negotiating the rules of the persuasion game

Seeing methodological norms as rules defining how the research game is to be played and how researchers can decide who of them is the winner, allows to understand where and why the naturalist approach mentioned in

section A is, in general, more acceptable than its rationalist and constructivist counterparts, though our own approach preserves also some appealing aspects of these other views. Regarding the merits of naturalism, the notion of scientific methods as conventional rules of a game makes it evident, in the first place, that these norms need not be universal: different research processes, or even similar processes played by different scientists, may have *different rules*, since these will usually depend on the interests of the competitors, and even on accidental historical facts. Methodological rules may *change*, as well, even within the same research process. In the second place, any particular rule or procedure will only be adopted after a public process of negotiation, during which the flaws and virtues of the rule must become as clear as possible for everyone, and this will generally demand the development of new research processes, usually *empirical* ones.

In connection with this, constructivism has still a point (and certainly a strong one) because, in the process of attaching normative force to a given procedure, what is relevant in principle is not that it is ‘empirically supported’ in the *rationalist* sense of ‘supporting’, but that the empirical information each researcher has about the working of the new rule makes it *interesting for him* to adopt it. But we can also save some of the insights of the rationalist approach, for, although *particular* norms are determined by the interests of scientists and grounded on empirical information, the possible *kinds* of norms and the *essential aspects* of their operation can be analysed in a more or less ‘transcendental’ sense, *i. e.*, we can study the ‘necessary conditions of possibility’ of a game as the one described in the past section², and it can be shown that, under some reasonable assumptions, the adopted methodological norms will usually be sensibly sound from the epistemic point of view. Let us consider these ‘rationalist’ aspects.

Firstly, what *kinds* of norms can be expected to arise in a negotiation among ‘recognition-seeking’ researchers. It seems that three types of them are needed, at least:

² Obviously, this is just a *façon de parler*. I have presented a more detailed argument in Zamora Bonilla, 2002 and 2010.

1) *Inferential norms*: these tell that if a researcher has accepted certain propositions, and if another proposition stands in a certain specified relation with the former ones, then that researcher will be forced to accept also the later proposition. For example, norms of this type will establish when is a hypothesis ‘well supported enough’ to make its acceptance compulsory. These rules are useful for a ‘recognition-seeking’ researcher because they indicate what statements you have to persuade your colleagues about, *before* attaining the public acceptance of your hypothesis.

2) *Observational norms*: in order to prevent the strategic denial to accept *any* statement that can ‘trigger’ the undesired acceptance of a rival’s theory through the rules of the first type, it is necessary that the commitment about some kinds of propositions is compulsory for reasons different from the previous acceptance of other statements. Typically, observations and experiments (or specific parts of them) are the natural locus of this type of norms, though probably not the only one.

3) *Distributional norms*: these norms govern the allocation of the power to control the resources needed for making research and communicating their results. Obviously, this power is interesting for scientists not only for the ability they confer to increase the probability of getting their theories accepted, but also because many other ‘private benefits’ accrue to them together with that power (Perhaps these rules are less appropriately called ‘methodological’).

Secondly, it is perhaps more important to notice some properties that any ‘reasonable’ system of rules must have. These properties are grounded on the very nature of the negotiation process through which the rules are established:

1) Norms are usually chosen ‘*under the veil of ignorance*’ (to use a Rawlsian expression). It is certainly possible that accepting a norm may be interesting for you on a particular occasion because that norm ‘supports’ the theory you are proposing; but committing to a norm *today* forces you to be committed to it also in the future, and perhaps the same rule makes it that the facts discovered *tomorrow* support some of your rivals’ theories more than yours. In general, it is very difficult for you to predict exactly

what theories or hypotheses will you be proposing in the future, and what will its connection will be with the accepted facts. So, as long as methodological rules operate as real (and more or less durable) commitments, it is not necessarily a wise strategy to 'vote' for the rules that happen to favour your 'current' theory.

2) As long as the decision of belonging to a scientific community or exiting and constituting a different one is open for researchers, it makes no sense to talk about 'imposing' a rule. A norm is a norm *within a scientific discipline* because it is interesting *for all its members* to adopt it. So, a rule will only be established if it promotes reasonably well the prospects for recognition of every researcher. This does not entail that everyone will have exactly the same probability of success, for scientists less talented and poorly equipped will be content with a lesser probability than their more fortunate colleagues.

3) The two previous properties entail that scientific norms will tend to be *impartial*, because they must offer a fair opportunity to *rival* approaches and theories. If a particular approach is seen as 'promising' by the members of a scientific discipline, and some existing norms tend to diminish the chances of success of those following that approach, researchers will be interested in negotiating a change in the norms and will begin to explore the new ideas according to the new rules. On the other hand, it also is true that norms may have some 'inertia', and this can slow down the negotiation process.

4) In many cases, the real effects of a norm on the prospects of getting public recognition will be so uncertain that scientists will tend to be indifferent between several alternative rules *as long as only recognition is considered*. Think, for example, in a norm indicating that '*ceteris paribus*, the theories with a higher predictive success have to be preferred', and contrast it with alternative norms, as '*ceteris paribus*, the theories with a lower predictive success have to be preferred', or '*ceteris paribus*, the theories which have been formulated in Latin verses have to be preferred'. Imagine now that you could negotiate with your colleagues which of these three rules to adopt. It is by no means clear which one of the three maximises the

probability of *your* winning a game of research; perhaps you are much better at Latin than the rest, but in this case it is just this differential ability that will make your competitors abstain from accepting a norm so clearly benefiting you. In any case, it is difficult, if not impossible, to ground your decision about which norm to accept on an estimation of your probability of success. What other criteria will you employ, then? It seems to be a benevolent assumption that, *ceteris paribus*, researchers will prefer methodological norms which are consistent with the maximisation of the *epistemic value* of the theories which happen to win in the game of persuasion. After all, why would they have chosen a *scientific* career as a means of getting public recognition, instead of other kinds of activities, as pop music, sports, or politics, if they *did not worry at all* about the attainment of ‘knowledge’?

A last important point in connection with this is that although the contractarian approach to scientific norms leaves some space to the influence of epistemic factors in the choice of the rules (and hence in the justification of scientific knowledge), we can not interpret this result as a return of the classical view of epistemologists as deciding *a priori* how the pursuit of knowledge has to be. Because it is essential to recall that even if epistemic values enter into the negotiation of scientific norms, these values *are those of the researchers* who are taking part in it, not those of the philosopher or the ‘science student’ who are observing the process from outside. This is again something that our approach shares with that of many scientific naturalists, though I want to point towards an aspect more specific of the contractarian view: the assumption that an explicit or implicit *agreement* between the members of a scientific discipline is the only legitimate way of ‘aggregating’ the epistemic preferences of all these individual scientists. Nevertheless, it is true that other agents outside the research field or even outside science may have an interest in negotiating the norms according to which the game of research is played, and the study of this interaction can also be an interesting point of contact between the approach defended here and other approaches in the field of social epistemology.

5. Do researchers obey the norms? And why?

The past two sections have been devoted to show why recognition-seeking researchers are interested in establishing a set of methodological norms and what are the fundamental types and properties of these norms. But it is legitimate to ask still a further question, which is whether a scientist basically motivated by the attainment of public recognition will have an interest in *obeying* the rules he has approved. We must take into account that, both in the case of science and in other norm-regulated activities, individuals benefit from the fact that *other people* comply with the rules, but it can be very costly *for oneself* to behave accordingly. For example, *my* paying taxes is not advantageous *for me* (rather on the contrary!), but my life is much better because people pay taxes regularly.³ This is obviously the reason why such an impressive amount of resources are spent just in making people comply with the norms. Curiously enough, we do not observe that there exists something like an institutionalised ‘science police’ or ‘science tribunals’: scientific research seems to be ‘self-policing’, at least to a higher degree than other kinds of practices.

It is true that a large amount of case studies in history and the sociology of science have been devoted to showing that scientists are far from being mechanical and systematic in their application of methodological norms, and that they tend to use the existing rules ‘strategically’ or ‘rhetorically’. But I do not think that this may serve to prove that scientific research is not regulated by those norms. In the first place, the vision of scientific method suggested in the preceding sections is *not* that of a logico-mathematical algorithm: actual methodological rules are usually ambiguous in their application to concrete cases, and they are frequently contradictory in their practical suggestions. So, it is natural that each scientist tries to interpret each norm in the way which is most favourable for his own theory. In the second place, usually not all methodological rules are violated simultaneously by a researcher; rather on the contrary, he must employ some rules in order to *justify* why he has broken others; otherwise,

³ At least if the collected money is wisely administered.

his colleagues will simply not take into account what the former scientist is asserting. In the third place, and more importantly, a ‘rhetorical’ use of a norm only makes sense if one expects that others are going to be persuaded by such a move: if *everybody* employed ‘just rhetorically’ the norms *every time*, no one would have a reason to do it. Appealing *successfully* to rhetorical strategies shows that your audience act according to some *predictable patterns* (at least within certain limits), and these *regular patterns of decision making* are just the *real* methodological norms I am referring to.⁴

I want to suggest that the main reason why these patterns are chosen *and followed* is because of the nature of the reward pursued by scientists, *i. e.*, recognition. Since what you want is that *others* express a public approbation of your own work, you do not obtain anything directly from *your own* decision about what facts or theories to accept; it only matters to you what facts or theories are accepted *by your colleagues*. So, the only question relevant for you is whether *your colleagues* obey the rules or not: if they do, you will be rewarded for doing ‘good research’ (‘good’ according to the accepted norms), and you will get nothing otherwise; if they do not obey the rules, you will get nothing no matter what you do, because they are not going to accept your own theory however much effort you might put in to defending it. So, the game of persuasion has two possible equilibria in general: either no one obeys the rules of the game (and this means that no research is done, save perhaps by isolated people), or everybody does (though, in this case, further problems arise when deciding *which* norms to institute). Under the contractarian vision of scientific method I am defending here, the first of these two equilibria would represent something like the ‘state of nature’, or, to express it in popular Kuhnian terms, perhaps the state of scientific disciplines in their ‘pre-paradigmatic period’. The emergence of a ‘paradigm’, as well as its subsequent changes, can then

⁴ Besides this argument, ‘rhetorical’ norms have not a mere ornamental or ‘marketing’ role, but are a constitutive aspect of the game of scientific research [Cf. Pera, Shea, 1991]. I have dealt more extensively on the economic reconstruction of scientific rhetoric in Zamora Bonilla [2006].

be seen as the outcomes of collective negotiations on a ‘methodological contract’.

Unfortunately, the argument of the preceding paragraph does not entirely solve the problem stated in this section, for it only works properly with inferential and observational norms, *i. e.*, the rules governing what propositions have to be accepted. Distributional norms, instead, open the possibility of enjoying other types of benefits (income, travels, power, relief from boring activities, and so on), and people who have control over this kind of resources will surely be tempted to use them to their own advantage. It seems that, ‘under the veil of ignorance’, scientists will prefer that an institutional mechanism is established that guarantees that a closer relation exists between the level of recognition one has reached and the resources and advantages that one can enjoy. Anyway, the design of such a self-enforcing, self-policing mechanism (if actual institutions are not satisfactory) is a difficult problem that offers a promising avenue of research for students of the economics of science.⁵

If we desired something like a ‘moral’ from this section, we could affirm that the norms for accepting facts, theories and laws prevailing in a scientific discipline are very probably ‘right’, in the sense that everybody trying to enter into the discipline to make a ‘critical examination’ of the knowledge produced by its members would conclude that those norms are acceptable, given all the available information. On the contrary, the actual norms of the distribution of resources within science will probably be more subject to criticism, in the sense that the interests of many people outside

⁵ See Zamora Bonilla, 2007, section 1, for a simple account. One referee of this paper has suggested that there is an interesting distinction between ‘defining’ the game according to which a scientific contest is carried out, and ‘governing’ that very same game. From the point of view of game theory, the distinction would consist in showing that the structure of the game, as established or described by that ‘definition’, allows that there is a sustainable equilibrium which offers a reasonable outcome for every or most participants in the game; *i. e.*, if the norms defining the game are, at least under reasonable and practical assumptions, self-enforcing ones. This is exactly a result that is offered at the end of section 1 of the paper cited at the beginning of this note.

science may be strongly affected by the establishing (and enforcing) of some system of norms instead of others.

6. Conclusion

I want finally to say that the contractarian approach to scientific norms, whose fundamental lines I have tried to sketch in this short paper, is able to offer a coherent explanation of many features of science that have usually been remarked upon by conflicting philosophical and sociological schools. The assumption that researchers are rational agents (in a sense which is more usual in economic theory than in sociology) allows to understand why the pursuit of private interests demands the constitution of a methodological order, whereas ideas such as that of ‘choosing norms under the veil of ignorance’ allows to reserve still an important place for epistemic values in the construction of scientific knowledge by selfish people. I think that the contractarian approach can be particularly fruitful because it suggests many ways of modelling the interactions between researchers (as well as between them and other agents) using the powerful instruments of game theory.⁶ The need of using the techniques and skills of philosophers, economists, sociologists, historians, and probably experimental psychologists, in order to develop an approach which is both theoretically and empirically well grounded, may also enhance the cooperation between several species of students of science.

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⁶ See Kitcher, 1993, ch. 8, for an interesting example, though he does not follow a strictly contractarian approach. I have developed some graphic models of the process of ‘theory choice’ within a scientific community in Zamora Bonilla, 1999.

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