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Cooperation, Competition, and the Contractarian View of Scientific Research[1]

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ABSTRACT
Using the approach known as 'Economics of Scientific Knowledge', this paper defends the view of scientific norms as the result of a 'social contract', i.e., as an equilibrium in the (second order) game of selecting the norms under which to proceed to play the (first order) game of scientific research and publication. A categorisation of the relevant types of scientific norms is offered, as well as a discussion about the incentives of the researchers in choosing some or other alternative rules.

KEYWORDS
Economics of scientific knowledge, contractarian epistemology, scientific norms

1. *Introduction*

Science is an essentially collaborative enterprise: it is not only that *most* research projects have unavoidably to be conducted in teams, but that *all* research is necessarily based on results and techniques developed by other researchers. Though it is true that criticising previous ideas is very important for furthering our knowledge of the world, we must not forget that for every idea one scientists criticises during her research, she needs to use in a non-critical (she has to 'take for granted') way hundreds of other ideas. Of course these 'presuppositions' have nothing that intrinsically prevents us from submitting them to thorough criticism on their turn, but in science (as in life) we cannot put between brackets everything at the same time. So, scientist have to *trust* many of the results provided by other colleagues, and this trust is, of course, an (epistemic) variety of *cooperation*. Having said that, we cannot forget,

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however, that scientific research is also a field of intense and perpetual *competition*: research is organised around problems, and scientists struggle for being the first in solving these. There is nothing particularly strange in cooperation and competition coexisting within a social institution like science: most, if not all, aspects of social life consist in some mix of these two things, from markets to sports, from religion to politics, from arts to personal relations. In general, the social mechanism that guarantees a peaceful coexistence of competition and cooperation is simply *norms*. Social norms state what is compulsory or forbidden to do in some circumstances, which means what ways of competing are legitimate and which are not, what ways of cooperating are mandatory and which ones are excluded (yes, many forms of cooperation are –even justly– forbidden; think, for example, of antitrust law).

But norms are themselves a space for cooperation and competition: establishing some norms within a social realm demands at least the collective agreement (if not the active involvement) of most of the people to which the norms will be applied, and this agreement is of course a form of cooperation; but different individuals or groups may have different preferences about what are the best possible norms, and the discussions and negotiations leading to the final agreement will also be a chance for strategic competition. Game theory is the natural formal instrument to examine this type of collective interactions, in which the final point, the agreement, results from the combination of different and conflicting interests in an 'equilibrium' (technically, a Nash equilibrium), i.e., a situation in which no one can improve what she is obtaining from it if the others don't change what they are doing, a situation in which there is no space for unilateral improvement. Applied to the case of the establishment of social norms, game theory obviously suggest to take a *contractarian* approach to the nature of these, and this is the spirit of my previous works on the nature and virtues of scientific norms (see, e.g., Zamora Bonilla, 2002, 2007, 2012, 2013). Taking into account the nature of this journal issue, I will devote this paper not to the analysis of specific norms, nor to their epistemic properties, but to the idea of scientific norms as a kind of 'social contract' (sect. 3), and to the reasons that lead individual scientist to respect those norms to the extent they do (sect. 4). Before that, I will make a short introduction to the 'economics of scientific knowledge' (sect. 2), to which the approach presented here will most naturally belong.

2. *The Economics if Scientific Knowledge*

The term 'economics of scientific knowledge' (ESK) was coined as a reaction to the field known from the seventies as 'sociology of scientific knowledge'. The latter had been defined by the members of the so called 'Strong Programme' in contraposition to the classical notion of a 'sociology of science', having in mind a distinction between the sociological explanation of the institutional, political and cultural aspects of science, on the one hand, and a sociological explanation of the *cognitive* aspects of science, on the other hand. 'Sociology of science' would be devoted to the 'external' (non epistemic) aspects of science, whereas 'sociology of scientific knowledge' would study the 'internal' content of science, i.e., why certain theories, facts or paradigms are accepted or rejected. Sociologists in the 'Strong Programme' derived some radically relativist conclusions from this starting point, in opposition to most traditional views about scientific knowledge. An open question was, hence, whether the application of analytical instruments drawn from the economist's toolkit to the understanding of the process of knowledge generation, i.e., the view of scientists as agents within an economic model, would support the relativist claims of radical sociologists or, on the contrary, would serve to 'save' the intuitive character of scientific knowledge as a paradigm of 'objectivity'. As we shall see, most contributions to ESK fall under the second of these options.

Obviously, in retrospect we can realize that many antecedent works can be identified under the ESK label, though the main production of papers and books on the topic has been made in the last two decades. One possible way of classifying all these works is according to the type of economic models or metaphors they attempt to apply to the study of the creation of scientific knowledge. From this point of view, we can distinguish, first, formal (or 'mathematical') from non-formal (or 'institutional') approaches.

One of the most important contributions in the first group is Philip Kitcher's paper entitled "The Division of Cognitive Labour" (1990), in which he develops a set of models based on the assumption of interacting rational self-interested scientists. According to Kitcher (1993, p. 303), the aim of his models is "to identify the properties of epistemically well-designed social systems, that is, to specify the

conditions under which a group of individuals, operating according to various rules for modifying their individual practices, succeed, through their interactions, in generating a progressive sequence of consensus practices". Other interesting mathematical models of scientific activity that have been developed during the last decades refer to the 'game' between researchers and journal editors, or the way in which researchers try to change the subjective probabilities of their colleagues), or about the decision whether to replicate another resarcher's experiments, or on the decision of accepting a more 'popular' theory or defending a more 'heterodox' one, on the basis of the different information about both theories each individual scientist has. The last two cases show the possible existence of more than one equilibrium in the 'cognitive state' of the scientific community, what can lead to phenomena of path-dependence, inefficiency, and sudden 'revolutions'. Some more recent contributions have analysed the properties of the priority rule, the choice of methodological rules, and the negotiation about the interpretation of empirical findings. In the last times, due surely to the availability of more powerful software, it has become relatively common the use of simulation models to study Kitcherian 'division of epistemic labour' problems, especially in cases of great complexity. Many of these papers are grounded on the simulation models of Reiner Hegselmann and Ulrich Krause, which in turn are inspired by the work of Keith Lehrer and Carl Wagner on 'rational' belief aggregation.[2]

3. *The Scientific Method as a Social Contract*

Though there is obviously a big amount of literature on the social structure of science, and on the application of economic or game-theoretic categories to its study, I think that the academic analysis of the social structure of science and of the relations between science and society from a contractarian point of view is still a project for the future, and this paper attempts to be an invitation for the development of such a project. I shall try to apply the contractarian view to a particular aspect of the 'governance of science' which has often been considered as totally alien to the introduction of 'sociological' or 'political'

2 See Zamora Bonilla (2012) for a comprehensive study and bibliography on the economic approach to the social structure of scientific knowledge production.

considerations: i.e., that set of procedures usually referred to under the rubric of 'scientific methodology'.

Our starting point would be to assume that scientists are players in a competitive game whose goal is the attainment of a particular kind of social and epistemic reward: being acknowledged as the author of an important discovery. The basic idea is to consider that this game is played according to a system of rules or norms that are not absolutely imposed by a transcendental logic or something of the like, nor by another external authority, but that can be negotiated by the very same players of the game (i.e., the researchers). The question is, hence, the following: if you were a scientists playing that game, i.e., pursuing recognition for discoveries, 'against' other scientists attempting to do exactly the same, *what scientific norms would you like the game were played by?*

Firstly, let's see 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:

1) *Inferential norms*: these tell that, if a researcher has accepted certain propositions, and if another proposition stands in 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 can we take a hypothesis as '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 nor the only one.

3) *Distributional norms*: these norms govern the allocation of the power to control the resources needed for making research and communicating its results. Obviously, this power is interesting for scientists not only because it increases the probability of getting their theories accepted, but also because many other 'private benefits' accrue

to them together with that power (I admit that 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 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 better *for everyone of its members* to adopt it as long as it is adopted by the other members. So, a rule will only be established if it promotes reasonably well the prospects for recognition of most of the members of the discipline (for, if there is another rule they think it were more beneficial for them, they could simply impose it). 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 of success 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 is also 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*. Let consider, for example, 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 what 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 cannot 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, this 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.

4. *Do Researchers Obey the Norms? And Why?*

The past section has 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. 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 something like an institutionalised 'science police' or 'science tribunals': scientific research seems to be 'self-policing', or at least more 'self-policing' than other kinds of practices. It is true that a large amount of case studies in history and 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' (Pera and Shea 1991). 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 here 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 to his own theory. In the second place, usually not all methodological rules are violated simultaneously by a researcher; rather on the contrary, she

must employ some rules in order to *justify* why she has broken others; otherwise, her 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 we are referring to.

The main reason why these patterns are chosen *and followed* is probably 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 it, 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 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 be seen as the outcomes of collective negotiations on a 'methodological contract'.[3]

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,

3 See Zamora Bonilla (2007) for a more systematic game theoretic analysis.

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 guaranteeing 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 which offers a promising avenue of research for students of the economics of science.

In conclusion, 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 the information. On the contrary, the actual norms of distribution of resources within science will probably be more subject to criticism, in the sense that the interests of many people outside science may be strongly affected by the establishing (and enforcing) of some system of norms instead of another.

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