CHAPTER 4

The Republic of Science and Its Constitution: Some Reflections on Scientific Methods as Institutions

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INTRODUCTION

One of the main virtues of Ian C. Jarvie’s book The Republic of Science: The Emergence of Popper’s Social View of Science, 1935–1945 (2001) is that it convincingly demonstrates that Popper’s greatest insight in the philosophy of science was to understand scientific rules as social institutions. Exploring the road from Popper’s Logik der Forschung (1934) to The Open Society and Its Enemies (1945), and contrasting Popper’s view of science with his view of social institutions in general, and with the views about these topics of other contemporary authors (mainly Hayek and Polanyi), Jarvie clearly shows the radical novelty of Sir Karl’s approach to the problem of scientific method, as well as why the intrinsic social nature of methodological rules was difficult to discern by most readers of Popper. Another important result of Jarvie’s hermeneutics is that, instead of the traditional interpretation of the young Popper’s intellectual evolution, as if The Open...
Society and The Poverty of Historicism (1944) resulted from an application of his falsificationist methodology to the case of the social sciences and political philosophy in general, Jarvie offers a different and more illuminating perspective: the works of the 1940s would rather be a natural development and generalization of the view of science as a social institution, that is, an exercise in whether society could better be understood on the republican pattern of science advanced in the Logik. I will not say too much in this chapter about this second prevailing element of Jarvie’s book but concentrate instead on the first issue: the essentially conventional and hence social nature of scientific method and the reading of Popper’s falsificationism as an attempted sketch of a constitution of the Republic of Science, as Jarvie magisterially tries to reconstruct, mainly in the first and second chapters of his book.

Sharing Jarvie’s interpretation of the Popperian idea of scientific method, I want to discuss more specifically two concrete aspects of (in Jarvie’s terms) Popper’s proto-constitution of science, one which is closer to traditional debates about methodology and the other one referring to what view of social institutions would be more helpful for a project like the one Jarvie attributes to Sir Karl. The first question is about what should be the supreme rules of the scientific method. I will express some doubts about the idea that being permanently open to criticism has to be taken as such supreme rule, not because it is not extremely important and useful, but because I think that it is more instrumental than substantive, and has to be justified by its presumed efficacy in leading us to other goals whose expression should count as higher-level norms in the constitution of science (which will be the focus of the next section).

The other question has to do with Popper’s view of society and of social sciences, which I will claim excessively determined and limited his vision about what kind of understanding we might reach about the idea of the scientific method as a social institution. Three quarters of a century have passed since Popper elaborated on those ideas, and both the political situation and the social sciences themselves have evolved in a direction that would justify the following conclusion: had Popper devised his social understanding of methodological norms around the end of the century, he could have chosen a very different and more appropriate set of conceptual tools to work it out. I will deal about this possibility below.
In spite of the title of his most important book in philosophy of science, Popper’s fundamental claim about the issue is that the rules of scientific research are not a matter of logic, in the sense that they cannot be derived from logical axioms, nor are they something like applied logic. According to Popper, the logic of scientific discovery is a logic, at most, in the sense in which we can talk about “the logic of chess”:

Methodological rules are here regarded as conventions. They might be described as the rules of the game of empirical science. They differ from the rules of pure logic rather as do the rules of chess, which few would regard as part of pure logic: seeing that the rules of pure logic govern transformations of linguistic formulae, the result of an inquiry into the rules of chess could perhaps be entitled ‘The Logic of Chess’, but hardly ‘Logic’ pure and simple. (Similarly, the result of an inquiry into the rules of the game of science—that is, of scientific discovery—may be entitled ‘The Logic of Scientific Discovery’.) (Popper 2002, p. 32)

Conventions can be justified, argued in favour or against, but they cannot be proved as logical or mathematical theorems, nor can they be demonstrated or confirmed as some (other than Popperians) think the laws of empirical science can. There is a conventional element in the choice of the rules of any practice: one might decide not to play the game according to those rules, though in that case probably she will not be playing that game but a different one. Popper, hence, responds to the different forms of conventionalism (i.e., philosophical understandings of science according to which the truth of scientific claims is “conventional”), not by proving that conventionalism is self-consistent but by admitting that science contains an indispensable conventional component built into it; this component, however, is not the truth (or the accepting-as-true) of the theories and laws but the choice of the rules according to which scientists carry out the process. Nevertheless, if we want this choice not to be purely arbitrary, it is necessary that we engage in a rational discussion about the reasons why it could be better to play that game according to those rules instead of others, or instead of a very different game. And these reasons are necessarily related to whether the proposed rules will actually lead us to satisfy the goals of the game. So, before studying what the rules of science should be, we need to have an idea of why we want to do science rather than some different kind of activity. Just a few paragraphs after the text just quoted,
Popper clearly formulates the goal he has in mind: “the rules are constructed with the aim of ensuring the applicability of our criterion of demarcation” (Ibid., p. 33).

Popper’s criterion of demarcation is falsifiability: “it must be possible for an empirical scientific system to be refuted by experience” (Ibid., p. 19; italics in the original). Or, a little bit more explicitly:

In other words: I shall not require of a scientific system that it shall be capable of being singled out, once and for all, in a positive sense; but I shall require that its logical form shall be such that it can be singled out, by means of empirical tests, in a negative sense: it must be possible for an empirical scientific system to be refuted by experience. (Ibid., p. 18; italics in the original)

Hence, the rules of science, according to Popper, must be designed in order that all scientific systems (theories? models? hypotheses? theoretical frameworks?) are not only falsifiable through empirical testing but also as much falsifiable as possible (as it is clear from rule R5 below, and from Popper’s insistence on degrees of testability throughout the book, especially Chap. 6). Before starting to discuss to what extent this is a sufficient goal on which to set a system of methodological rules, let’s see what norms Popper had in mind. One discouraging aspect of *The Logic of Scientific Discovery* from a contemporary point of view is that, once its main goal of offering a defence of the norms of science has been stated, the book does not proceed to systematically formulate such rules but enters into some discussions characteristic of the philosophical concerns of the time. The lack of systematic analysis led Jarvie to gather the rules that Popper randomly formulated throughout the book. I will present here just the most important ones, starting by what Popper and Jarvie call the “supreme rule”:

(R5) The other rules of scientific procedure must be designed in such a way that they do not protect any statement in science against falsification.

(R1) The game of science is, in principle, without end. He who decides one day that scientific statements do not call for any further test, and that they can be regarded as finally verified, retires from the game.

(R2) Once a hypothesis has been proposed and tested, and has proved its mettle, it may not be allowed to drop out without “good reason.”

(R3) We are not to abandon the search for universal laws and for a coherent theoretical system, nor ever give up our attempts to explain causally any kind of event we can describe.
(R4) (Don’t) use undefined concepts as if they were implicitly defined.
(R5) Only those (auxiliary hypotheses) are acceptable whose introduction does not diminish the degree of falsifiability or testability of the system in question, but, on the contrary, increases it.
(R7) Inter-subjectively testable experiments are either to be accepted, or to be rejected in the light of counter-experiments.
(R9) After having produced some criticism of a rival theory, we should always make a serious attempt to apply this criticism to our own theory.
(R10) We should not accept stray basic statements –i.e., logically disconnected ones– but … we should accept basic statements in the course of testing theories; or raising searching questions about these theories, to be answered by the acceptance of basic statements.
(R11) Those theories should be given preference which can be most severely tested … (i.e., favour) theories with the highest possible empirical content.

(Jarvie 2001, pp. 31 ff. Of a total of fifteen, the above are taken verbatim from The Logic of Scientific Discovery.)

The complete list of these rules is what Jarvie calls Popper’s proto-constitution of science. I shall devote the rest of this section to discuss several important problems that can be recognized in this constitution, its evident lack of systemization not being one of them. A problematic question in Popper’s list is whether he proposes these norms as just a suggestion and “invitation to play science” according to these rules or if he thinks that it is the “essence” of science to be played in exactly this way, so that those that are not following these norms are just not doing science, properly speaking. Another way of putting this question would be to ask if, for Popper, someone not following these norms would be a bad scientist or no scientist at all. Having here a set of norms (rather than a single one), and taking into account that many of them could in principle be disobeyed to a higher or lower extent (something to which I will come back later), it is conceivable that the norms were followed more or less, so that one could be a better or worse practitioner of Popperian science. The Logic of Scientific Discovery, with its insistence on the problem of demarcation, is written in such a way that a categorical reading of the methodological norms seems closer to Popper’s intentions: if you don’t strictly follow the falsificationist rules, you are simply not doing science, but metaphysics. But this seems too strict: one can prefer sometimes a theory with a little lower degree of testability, say, and still be a scientist, even a falsificationist scientist, as one can be a professional athlete and skip some few training days occasionally.
Perhaps it is better and more coherent with Popper’s thought to interpret this “methodological constitution” as an (Weberian) *ideal type* or as a kind of (Kantian) *regulative idea*. This, however, does not totally solve the problem of whether these norms constitute a definition of science or have just to be taken as suggestions to carry out research in a certain way. As we have seen above, that methodological norms are *conventions* (and not truths) invites us to take more seriously the latter option. But this option would obviously lead to a more fundamental question: why should I follow those norms instead of other possible ones (perhaps slightly different, or perhaps more disparate)? Popper’s immediate answer is, of course, that you should follow these norms in order to attain the goal of keeping your theories (or systems) as testable as possible. And this leads us to ask the following questions: first, would following Popper’s rules, such as they are formulated, actually lead us to maximize the degree of testability of our theories? Wouldn’t it be possible to attain this goal by some different means? And second, why should be testability the most important goal and perhaps the only goal of scientific practice? After all, testability sounds like something that is most naturally a means to attain other valuable things, something that has an instrumental value, not something that is valuable in itself. Popper often speaks in *The Logic of Scientific Discovery* of “the progress of science,” “the progress of knowledge,” or just “progress” as the prize attained by his recipes. Hence, why is the maximization of testability (and just testability) the only means to further the progress of science? What if the goals of science (and of scientists) are more diverse and complex and are accompanied by more labyrinthine ways of approaching them than through an obtuse fixation with testability?

Regarding the first question, a simple way in which Popperian rules could be inefficient enough is if the maximization of testability within a particular segment of the complex network of statements involved in any realistic research process could sometimes entail a significant reduction of testability in other segments. Perhaps in order to keep a theory highly testable we should sometimes accept other theoretical hypotheses, either more general or profound (having to do with general presuppositions of the field) or more technical ones (like those about the use of measurement devices or statistical assumptions). I doubt that the Popperian system could offer a global measure of testability (if developing a workable local measure were not utopian enough) with a clear recommendation. Perhaps Popper would insist that his norms are recommendations for the individual scientist, which has the obligation of maximizing the testability of her
portion of research. But even in this case, it is an open question whether a high number of individual researchers proceeding in this way would lead science to globally maximize its degree of testability (and progress), or if there could exist some complex, negative feedback mechanisms that would better be served in a Mandevillian way (when some scientists behaved less honestly). Imagine that individual scientists could choose between developing their preferred theories following in the strictest possible way the falsificationist rules and following a more verificationist way, protecting those theories from falsification. Is there a guarantee that the first strategy would necessarily lead to a higher degree of global progress than the second one (which, by the way, is reminiscent of the Lakatosian methodology of research programmes)? Perhaps in the case of science, private vices also transform at times into public virtues.

Regarding the second question, it is well known why Popper said that he refused to mention truth as the goal of science, as he attempted to do some decades later thanks to Alfred Tarski’s rehabilitation of the notion of truth without abandoning the idea that a more abstract notion of progress is what distinguishes science from other activities. One may wonder how Popper’s book would have looked like if he had opted for explicitly asserting that progress towards (knowledge of) the truth is the ultimate goal of science. Probably, his views on methodology would not have changed substantially, for the concept of verisimilitude, or approximation to the truth, was later defined by him in such a way that a falsificationist methodology would naturally derive from or be coherent with it, in the following sense: theory X is at least as close to the truth as theory Y (according to Popper’s qualitative definition) if and only if every true statement following from Y also follows from X, and every false statement following from X also follows from Y; hence, under the hypothesis that X is at least as verisimilar as Y, it follows that all verified predictions of Y will be correct predictions of X and all verified mistakes of X will also be mistakes of Y. (I am using the term “verified” just in the sense that these predictions have been tested and accepted on empirical grounds.) Stated differently, the hypothesis that X is at least as close to the truth as Y allows to make the second-order prediction that X’s tested predictions would always be at least as good as those of Y. Hence, the provisional corroboration of this second-order prediction (that all empirical successes of Y have been to this date matched by X, and all empirical failures of X known to this date are also failures of Y) would count as a corroboration of the second-order conjecture that asserts that X is closer to the truth than Y. In Popper’s own words:
I do not suggest that the explicit introduction of the idea of verisimilitude will lead to any changes in the theory of method. On the contrary, I think that my theory of testability or corroboration by empirical tests is the proper methodological counterpart to this new metalogical idea. The only improvement is one of clarification. Thus I have often said that we prefer the theory \( t_2 \) which has passed certain severe tests to the theory \( t_1 \) which has failed these tests, because a false theory is certainly worse than one which, for all we know, may be true.

To this we can now add that even after \( t_2 \) has been refuted in its turn, we can still say that it is better than \( t_1 \); for although both have been shown to be false, the fact that \( t_2 \) has withstood tests which \( t_1 \) did not pass may be a good indication that the falsity-content of \( t_1 \) exceeds that of \( t_2 \) while its truth-content does not. Thus we may still give preference to \( t_2 \), even after its falsification, because we have reason to think that it agrees better with the facts than did \( t_1 \). (Popper 1963, p. 235)

As it is well known, Popper’s dream of supporting his falsificationist method with a formal theory of approximation to the truth received a fatal blow when it was proved that no false theory could be logically related to any other theory in exactly the way Popper’s definition of verisimilitude demanded for the former being more verisimilar than the latter, and hence, the idea that we can give preference to a falsified theory over another because of that reason became logically untenable (more on the debates regarding verisimilitude, see Niiniluto 1998). Perhaps we could explicate the notion of a theory being closer to the truth than another in different terms than Popper’s definition of verisimilitude. According to alternative definitions of truthlikeness, would the methodological norms selecting more truth-like theories be the norms Popper defended in his proto-constitution?

Instead of starting by proposing a philosophical argument about what the goal of science should be and logically deriving from that goal the methodological norms that could better serve to its attainment, we might perhaps follow a more empiricist or abductive procedure, examining in the first place what methodological norms scientists actually follow and conjecturing afterwards which goal or goals could they be trying to pursue such that the norms actually followed in scientific practice are efficient procedures for the attainment of those goals. I leave the details about this possibility for the next section; I will only mention now with respect to this question that we might find that scientists actually follow under some circumstances verificationist rules, and that this can be explained not
because they are irrational or dogmatic but just because this is the best science game they can play, taking into account both their goals and the limited resources at their disposal.

The latter discussion can also be connected with a different problem. Science is (mostly) not done by algorithms, but by humans, and every person has usually more than one goal in everything she does. Even if the maximization of testability and the pursuit of truth (of deep, useful and interesting truths) were the only substantial goals of science as an institution, the people that carry out the activities in which science consists may and usually will have other goals and values as well. These range from diverse preferences about the epistemic qualities of the theories, models, empirical procedures, and styles of thought to more practical things, like good work conditions, access to facilities, and, most importantly in the case of many scientists, prestige and intellectual influence. Perhaps the institutional arrangement that would maximize the testability of theories is to publish everything in a strictly anonymous way, for this would probably avoid serious biases due to the publish or perish tendency of current science. But, under such institutional arrangement, it would be likely that not enough people would pursue a science career. Many scientists’ thirst of glory would be hardly satisfied if science worked in such a way that no result could ever be declared definitively established beyond all reasonable doubt: for a seeker of scientific glory, provisional truth and fallibility are very often something we have to pay lip service to, and firm verification of a theory or law is an institutional goal more valuable than having passed till now all tests but who knows what will happen tomorrow. If scientists’ motivations are close to this description, then establishing methodological rules that at least have a little verificationist flavour could be seen as a necessary price for having a powerful science system, an unavoidable element in any realistic constitution of the republic of science.

**THE CONSTITUTIONAL POLITICAL ECONOMY OF SCIENTIFIC INSTITUTIONS**

In the preceding section I have basically considered the content of Popper’s methodological proto-constitution of science, shedding some light on the question whether testability (and the maximization of degrees of testability) can be taken as the sole regulatory ideal for the normative architecture of the scientific method. In a nutshell, if testability is not seen as the only
constitutive goal of science (nor as the only goal of scientists), then it is conceivable that, under some circumstances, a trade-off between the maximization of several different goals can arise, and so it may be rational to be satisfied with a little less testability in exchange for a higher level of satisfaction of the other aims. I plan to deal now with a different question, which does not refer directly to the content of the norms of science, but to what are the best theoretical tools to illuminate the process of choice or the emergence of those norms. Popper, in general, was deeply suspicious of the state of a big part of the social sciences of his time, both because of the lack of testability of many of the most popular theories and because of the ease with which many of those theories were put into ideological use.

It is a pity that Popper hardly ever bothered to take into consideration the work of people like Robert Merton and his followers which might have offered an enlightening way of discussing the normative and institutional structure of science. Instead, Sir Karl seemed to have more clearly in view other approaches to the topic, like those of Karl Mannheim’s sociology of knowledge (with its Marxist inspiration). We shall never know if Popper’s ideas about a constitution of science would have been more explicitly developed or would have changed to some significant degree had he devoted more time to contrast his view on the topic with that of Merton’s “ethos of science” (Merton 1942).

As Jarvie explains in the third chapter of _The Republic of Science_ (2001), Popper’s view of society, institutions, and social science is deeply influenced by the work of liberal thinkers and economists, especially his friend, and later colleague at the London School of Economics, Friedrich Hayek. As it has been argued, the influence was surely bidirectional (Caldwell 2006), but most probably it was Hayek’s ideas on the structure and development of society that basically influenced Popper’s view about historicism and the open society around the end of the 1930s and 1940s, while Popper’s general methodology strongly influenced Hayek’s epistemological views in the 1950s. In spite of Popper’s subsequent deep influence on several generations of economists who tried to elaborate their theories according to falsificationist guidance (Richard Lipsey 1963), often mixed with Milton Friedman’s views on positive economics, Popper’s ideas about the fundamental nature and method of the social sciences didn’t seem to have gone very further than what he sketched about “situational analysis” in _The Poverty of Historicism_ (1944), and, in this sense, he shared Hayek’s Austrian reservations about the possibility of developing a mathematical science of society. This might have been more justifiable during the 1940s...
when even the most well-known formalized branches of economics of the
time (Hicks’ or Tinbergen’s macroeconomic models) were far less com-
plex and diversified than what they turned into a decade later thanks to the
work of people like Samuelson, Arrow, and Nash. In particular, the rise of
game theory could have been taken as an opportunity to put more flesh on
Popper’s scheme of situational analysis, for it is clear (at least for some
authors) that situational logic could be seen as a minimal description of
the very idea of analysing social interactions by means of game-theoretic
tools (see Hands 1992 for a positive view about this question and Morgan
2012 for a slightly more sceptical one). One possible reason of Popper’s
reluctance to advance a more formal development of his situational analy-
sis may have been his deep scepticism about the existence of laws within
the social sciences. But the more or less successful application of game-
thoretic thinking to nearly all fields of social science in the last decades
has not been driven by the hope of finding deep universal laws but rather
by the interest in constructing a battery of alternative, useful, piecemeal
models of specific types of situations, something that parallels Popper’s
(and Hayek’s) praise of “piecemeal social engineering” over conceited
attempts to manage a whole national economy, for example. Actually, a
whole branch of applied game theory, known as mechanism design, could
be considered as the most accomplished, state-of-the-art version of a
piecemeal social engineering.

In particular, I have defended in other papers (Zamora Bonilla 2002
and 2008) that one specific branch of economics, constitutional political
economy, could be particularly useful to apply the Popperian idea of a con-
stitution of science. Constitutional political economy was inaugurated by
Buchanan and Tullock’s book on The Calculus of Consent (1962), and its
fundamental idea is that we can apply rational choice microeconomic,
game-theoretic thinking, to the cases in which a group of people are not
just making some choices under a situation defined by the applicability of
some specific norms but to those cases in which the members of the group
choose the norms themselves. Buchanan and his collaborators and follow-
ers have applied this type of work mainly to study the choice of voting
procedures, or of other political representation systems, and how their pre-
dicted impact on economic performance and distribution affect these types
of choices. But this methodology can be applied to the choice of any kind
of norms whatsoever. Why not, then, to the norms of scientific research?

I think an important (though unconscious) obstacle for Popper and his
disciples to consider seriously this possibility was a certain distrust of the
scientific community itself, particularly after the big debate of the 1960s around Thomas Kuhn’s work on scientific revolutions and “normal science.” If normal scientists were to choose the actual rules of scientific practice, they would probably prefer norms that made life easier for them, so to say, instead of norms enforcing the boldest and most austere search of highly testable theories and their possible falsifiers. Perhaps this possibility is neither too unrealistic nor too undesirable when we take into account that science, after all, must be practised by people organized in communities and institutions and not only by heroic geniuses. It is important to take into account that the choice of norms is very different from choices made under norms. One essential aspect of the former is that, if not only because norms are expected to be in force during a very long period and are going to affect individuals under circumstances whose specificities are difficult to foresee, norms tend to be chosen under a “veil of ignorance” (to use the Rawlsian metaphor): the choice of norms tends to be more impartial, and less subject to strategic manoeuvres, than decisions more directly related to specific circumstances, and the more general the norm, the less space for manoeuvring, in general (except in circumstances where an individual or a subset of the group occupies or expect to occupy a powerful role in the field during a long time). Hence, a constitutional choice of the most general norms of science, made by a community composed of normal scientists, would perhaps be not very different from the choice that a group of heroic or revolutionary scientists would make.

Another important aspect of the choice of scientific norms that constitutional political economy would force the analyst to justify is what the communities in charge of making the choice of the norms actually are: What is the relevant constituency? Is it the scientific community writ large? Would different scientific specialities or subcommunities have different methodological constitutions, depending on the availability of data, techniques, and so on? Or should the most general constitution of science be decided by the informed choice of any citizen, for, after all, the outputs of scientific research are going to directly or indirectly affect their lives, especially because of the funding of science? Why should the philosopher have a prominent role in the choice of these norms? If they should, would it be reasonable that the constitution of science was written and voted by an assembly of philosophers representing all the relevant methodological schools? Some of these questions are obviously wiser than others, but the very possibility of launching them opens interesting perspectives from which to consider problems not only within methodology of science but
also about the social, economic, and political relevance of methodological questions, something that, unfortunately, has been absent in most of the core literature on philosophy of science during the last century.

Many of the more severe problems that science has been suffering during the last decades could be profitably approached from this constitutional point of view, for example, the replication crisis denounced in many sciences, the hardening of academic careers, the choice of open or proprietary systems of publication, and the contested role of universities in an increasingly privately funding research systems. These are very likely problems that have to do with a dysfunction of norms that perhaps had been working in an acceptable way till a few decades ago but that now should be modified if we agreed on who are the agents directly responsible for choosing the right norms. If we do not do it this way, then the invisible tentacles of a Hayekian mangle of self-organizing, emergent institutions will certainly make the choice of norms instead of us, and these norms will likely not necessarily be in our best interests.

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