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THE DEVELOPMENT OF POPPER'S CONCEPTION OF THE SOCIAL<sup>1</sup>

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Popper is usually interpreted as first and foremost a student of the logic of the natural sciences; his work on the social sciences is treated as something of an afterthought, or perhaps an application. This paper argues to the contrary that Popper's philosophy of natural science already shifts questions of the methodology of natural science from logic and language to the design and maintenance of social institutions. He is thinking socially from the beginning.

Theses. 1. Popper's methodology of science as expounded in his 1935 book Logik der Forschung,<sup>2</sup> embodies an implicit conception of the social, a conception that was articulated, generalized and applied in his later works "The Poverty of Historicism", of 1944-1945, and The Open Society and Its Enemies, of 1945. From the start, Popper conceives of the social in institutionalist, reformist, piecemeal and, it would also seem, consensualist terms. 2. Treating Popper's philosophy as social all the way down to its roots in the philosophy of science, or all the way back to the first major publication, makes for a fruitful overall interpretation of his philosophy.<sup>3</sup> It also suggests that because most extant characterizations of his philosophy fail to connect with the most innovative aspect of his ideas because they totally overlook this social aspect.

The paper is in three parts. In the first part I extract the implicit view of the social that is to be found in The Logic of Scientific Discovery. In the second part I sketch how this

implicit view was articulated and generalized in “The Poverty of Historicism” and The Open Society and Its Enemies. In the third part I develop some comments and questions towards an assessment, testing Popper’s conception of the social by using it to look at the social aspects of science.

### I. The Social in The Logic of Scientific Discovery

The received view of Popper’s philosophy of science, as expounded by such as Salmon or Grunbaum, is that it is an argument for shifting the role of evidence in science from that of supplying positive reasons for accepting theories to that of providing negative reasons for rejecting theories. Popper is said to have proposed falsifiability as the criterion for the scientific character of theories. Thus presented, Popper can be domesticated as one playing by the rules of contemporary academic philosophy of science. By contrast, my view is that Popper’s position is subversive of the academic approach to the philosophical problems of science. He argues the necessity, if certain objections are to be overcome, of finding a solution to the problem of demarcation at the level of social institutions. My contention is that he saw the inadequacy of all narrowly philosophical approaches to the problems he was working on, and came to realize that if those problems could be solved it could only be by social technology, by decisions to reform institutions. Why he did not make this sharper and clearer in his text is not a matter I want to enter here; but I am convinced that the bold originality of his move does much to explain the woeful failure of so many in the philosophical community even to report his ideas accurately. His work demands that one think outside “disciplinary” boundaries.

In contrast to the received view, I would hold that The Logic of Scientific Discovery (LScD) treats science as a social institution, and re-conceives the problem of method as a problem of institutional reform. For the Popper of 1935 already there is no Crusonian science,<sup>4</sup> and science is not a form of personal knowledge.<sup>5</sup> Cooperation between persons under a régime of institutionalized rules governing procedures are necessary ingredients of science. Let me argue this by putting a few brief passages of this early work under the microscope.

Popper proposes a falsifiability criterion to demarcate empirical science: ‘it must be possible for an empirical scientific system to be refuted by experience’ (LScD, p. 41). But immediately after he proposes his criterion of falsifiability in §7 (pp. 41-42) of L.Sc.D., Popper articulates three criticisms of this view. The first criticism is that it is wrong-headed to confine science to the delivery of negative information. This he answers with the argument that a statement conveys more, the more singular statements it is likely to clash with, so negative does not mean uninformative; on the contrary, information is improbability. These claims have been endorsed by keepers of the received view, yet dissatisfaction with Popper’s view as a “negative” one continues to be expressed. The second objection is that falsifiability is vulnerable to the same objections as is verifiability. This is answered by introducing an important logical fact: there is an asymmetry between verification and falsification, the former being unachievable, the latter logically possible. Endorsing a singular statement does not entail endorsing its generalization, but it does entail rejection of the generalization which is its negation. Accepting a statement such as ‘here is a black swan’ does not compel us to accept ‘all swans are black’, but it does compel us to

reject, on pain of contradicting ourselves, 'all swans are white'. It is quite unclear whether received opinion has come to terms with this simple but decisive logical fact.

The third objection Popper raises against his own preliminary criterion of demarcation is declared to seem more serious: it is easy to evade any refutation with the aid of some ad hoc hypothesis which explains the refutation away. The availability of such ad hoc devices is treated as unproblematic, since one can always introduce some auxiliary hypotheses or narrow down the denotation of some terms in the refuted theory so as to exclude the refuting case. Indeed, no contradiction is involved even in such an extreme manoeuvre as the simple refusal to acknowledge any falsifying experience whatever. The seriousness of this third objection is that it seems to neutralize the value of the proposed falsifiability criterion. If no contradiction is involved in shielding ideas from falsifying experience then any system can be adapted to satisfy this criterion of demarcation.

Popper admits the "justice" of the third objection, but goes on to say that he need not withdraw his falsifiability proposal because he is

going to propose...that the empirical method shall be characterized as a method that excludes precisely those ways of evading falsification which...are logically admissible.

According to my proposal, what characterizes the empirical method is its manner of exposing to falsification, in every conceivable way, the system to be tested (§6, p. 42)

It is notable that Popper here acknowledges the justice of a logical objection by admitting that there is no strictly or purely logical answer to it. His answer is a policy proposal: he suggests that we refuse to license ourselves to rescue hypotheses ad hoc, and choose instead his proposal that we forswear both rescuing operations and ad hoc

hypotheses. He proposes that we adopt a methodological approach which refuses to countenance all ad hoc manoeuvres and all waiving away of falsifying evidence.

This emphasis on policy choices greatly clarifies why Popper earlier distances himself from naturalism, the doctrine that the problems of the logic of science are part of science. Yet his approach is not conventionalist. The conventionalist ascribes to theories truth by convention; the naturalist ascribes to them truth by nature; Popper ascribes to them truth values and suggests the convention of avoiding ascribing truth to them by convention. A name for his approach would be methodological conventionalism - the only way in which Popper is a conventionalist.

My criterion of demarcation will accordingly have to be regarded as a proposal for an agreement or convention. As to the suitability of any such convention opinions may differ; and a reasonable discussion of these questions is only possible between parties having some purpose in common. The choice of that purpose must, of course, be ultimately a matter of decision...<sup>6</sup> (§4, p. 37).

What Popper proposes in this short passage is remarkable: that the problem of demarcation cannot be solved satisfactorily within the logic of statements. It can only be solved by an agreement or a convention. The boundaries of science are like national boundaries; they are man-made social institutions. Popper is a one man Boundary Commission offering a proposal. Opinion may differ on the suitability of a suggested boundary, which will then be open to discussion. In order for this discussion to be fruitful, the parties to it must have some purpose in common. Thus Popper is suggesting that his demarcation criterion is a proposal for institutional reform: reform guided by the aim of

maximizing the impact of experience on hypotheses. The proposed boundary, the guiding aim, and whether the boundary proposal subserves the aim are the obvious points for discussion.

It is easy to overlook the fact that we have here Popper's first published discussion of what he years later defended as piecemeal social engineering. We are faced with a problem, namely, setting the boundaries of science. And we have various boundary proposals, many of which take it for granted that the problem is not institutional but natural. Popper argues that none of these proposals, including his own, will withstand logical scrutiny. The demarcation problem is not solvable within logic and language. This in turn is because science is more than simply a set of statements; it is a set of statements produced in, and governed by, a social context of practices, traditions, institutions, and it is only in that social context that they can become scientific. Outside that social context the self-same statements need not be scientific. Hence, whereas the naturalistic view is not viable, the conventionalist view is viable but, Popper argues, it is objectionable because too defensive.

Popper's analysis has shown him and us that the demarcation problem is social, hence that its solution is social, i. e. social reform, and that in considering the reform of social institutions the very first question to be addressed is, what is the aim common to the reformers? He proposes that the aim of science is to learn from experience, to use, as he says, experience as a method. If experience is to be used as a method, then allowing theories to be protected from falsification by experience is inadvisable. Conventionalists propose ways to avoid refutation, making it possible for any statements to be scientific. Popper offers the contrary convention of welcoming refutation, thus narrowing the range of

statements which can be scientific. Yet he does not explicitly go further and point up the fact that he has treated science as a social activity. He leaves it to us to notice that an agreement or convention is an arrangement between people who share an aim, and to foster that aim they undertake to follow certain agreed-upon rules. In short, they form a social institution.<sup>7</sup> In this way Popper effected a revolution in the philosophy of science: for him science is not a naturally bounded set of statements, but a set of activities the aim of which is the production of statements under the régime of the method of experience. Only statements so produced are admitted as candidates for scientific status.

So when we think of scientific method as Popper envisages it, we would be in error to ask the question whether he is describing it or proposing something normative. Under the pervasive influence of Kant he is, rather, proposing something constitutive: the agreement or convention he wants to put in place will create a particular design of social institution, a pre-condition for realizing the aim of science. Science is envisaged as consensual, because cooperation in operating its rules is necessary; as goal-directed, since it has been constructed (or reconstructed) so as to achieve an aim; it is reformable because both its aim and the effectiveness of the means to the aim can be rationally discussed and proposals brought forward for alteration; and above all it is an institution, a permanent structure to coordinate and direct human activities. It is this institutional and reformist view where Popper shows his methodological conventionalism:

what is to be called 'science' and who is to be called a 'scientist' must always remain a matter of convention or decision (p. 52).

Long before he wrote The Logic of Scientific Discovery Popper had worked in social work and in education and so had much opportunity to reflect on the nature of things social. In thinking about science Popper reveals that he thought of it as a social activity, engaged in by like-minded persons, whose like-mindedness lay not in their agreement in judgement but in their agreement in aim and method. He shows an acute awareness that sociology is about the institutional frameworks within which both individuality and creativity can be nurtured and disciplined. He resembles here a Kantian legislator, or perhaps a constitutional delegate proposing basic law within which social life can be continued on an agreed basis, basic law that is not written in stone, but put forward for certain purposes, and, even after implementation, is open to still further debate and reform. Basic law is not easy to alter, but it must be open to discussion and reform nonetheless.

Before continuing with the articulation and generalizing of these ideas in Part II, I want to make two critical comments that will be developed in Part III. Although in LScD Popper clearly treats the demarcation of science as a social rather than a psychological matter, he says little or nothing about what we might term the internal social structure of scientific institutions. If what constitutes science is submission to a methodological regime, there are many questions to be answered about how that regime is set up, administered, maintained, and reformed. These questions might take one in the first instance to the history of science, and in the second instance to the sociology of science. In The Logic of Scientific Discovery Popper is more or less silent on both, and, moreover, he does not suggest either one as a research programme that might throw some light on what he describes as ‘the choice of methods’ (p. 49):



our manner of dealing with scientific systems: by what we do with them and what we do to them. Thus I shall try to establish the rules, or if you will the norms, by which the scientist is guided when he is engaged in research or in discovery, in the sense here understood (p. 50).

This reticence is puzzling, since it blocks him from pursuing some of the ideas advanced in The Logic of Scientific Discovery. This reticence of his also goes some way to explain why the debates that have surrounded Popper's book have rarely focused on the issues raised here and hence have seldom debated the proposed aim and the regime of rules proposed to foster the aim.<sup>8</sup>

The second comment is about the aim of science. In the early part of LScD Popper indicates that science aims at the presentation and testing of falsifiable statements. This much is straightforward enough. But immediately the further question arises, what is the aim of aiming at falsifiable statements? Popper's answers are internal, that is, he has explained the virtues of systems of falsifiable as opposed to verifiable or merely conventional statements. Thus a falsifiable system is an empirical system, one where our knowledge is subordinated to experience. The values and worth of the whole enterprise Popper takes for granted. So we get no argued alternative to, for example, Bacon's view that the aim of science is to understand nature in order to get power over her. Indeed in 1935 we find Popper explicitly repudiating an evolutionary approach to science as inadequate (he seems to equate it with instrumentalism, see L.Sc.D., p. 278), and closing his book with a powerful passage intimating that science is an end in itself:

Science never pursues the illusory aim of making its answers final, or even probable. Its advance is, rather, towards the infinite yet attainable aim of ever discovering new, deeper, and more general problems, and of subjecting its ever-tentative answers to ever renewed and more rigorous tests (p. 281).

## II. The Elaboration and Generalization of Popper's Conception of the Social

Popper's two principal works on the philosophy of society and of politics were completed in the ten years after The Logic of Scientific Discovery, "The Poverty of Historicism" (1944-45) and The Open Society and Its Enemies (1945).

"The Poverty of Historicism" explicitly portrays social institutions as tools or means for the achievement of social aims, but adds a significant qualifier. Only some institutions have been consciously designed for a purpose; the majority have just 'grown'. Designed institutions have ready-made criteria for assessment of their performance. Institutions that have just grown are more problematic: by what standards should they be judged and, since they are an inheritance, what controls are we entitled to exercise over them? The question of democratic control over, and reform of, institutions that have just grown is not explicitly addressed in "The Poverty", but a related matter is. I refer to two sorts of reform policies, the distinction between piecemeal and utopian social engineering. In his discussion, Popper treats all institutions equally: they are hypotheses the efficacy of which we should test, and he argues that if we reform then we must proceed piecemeal because large-scale social reform is untestable and hence self-defeating.

Two other innovations introduced in "The Poverty" were a methodological rule for social explanation and a stress on the unintended consequences of institutional innovation

and reform. The rule of methodological individualism says that to avoid essentialism towards the social entities used in our explanatory models, we do better to think of our models in descriptive or nominalist terms, “in terms of individuals, of their attitudes, expectation, relations, etc.”<sup>9</sup> . Although some institutions are purposely designed both they and grown institutions are part of a large network that interacts in ways too complex to predict, even in the short term. Thus institutions require maintenance, reform and sometimes dismantling, depending on how they are performing. Much of this is generalization of what was implicit but particular in The Logic of Scientific Discovery.<sup>10</sup> Science was there assessed against an aim, and its deficiencies remedied with proposals for a reformed set of values. There was no essentialist appeal to the ethos of science or anything similar; the proposed methodological rules are to be evaluated by their consequences, and the rules are intended to create an institutional situation that will promote a certain social outcome.

The Open Society and Its Enemies is much longer than “The Poverty” and much richer in its discussions of these matters. What are described as the social aspects of scientific method are explicitly addressed, and a strong parallel is drawn between the community of scientists united in cooperative rational pursuit of the truth, and the enlightened approach to the reform of society and social institutions in general. The specialized rationality of scientific institutions is treated as a model for democratic politics: institutional structures that enjoin open-mindedness and the critical attitude are recommended, the aim being applied or useful knowledge about the way to achieve social aims.

Popper's conception of social institutions is quite a bit more explicit and developed in The Open Society and Its Enemies. Hypothetical knowledge itself is seen as a social institution; and institutions embody hypothetical knowledge. This is because institutions are experiments at reform or change to be implemented and tested in practice. Society accumulates knowledge in its institutions, society is thus a knowledge-accumulating entity. This boldly generalizes the idea that scientific knowledge is social and that scientific institutions need to be constructed in such a way as to maximize their potential to foster knowledge.

Two further innovations in The Open Society and Its Enemies are extensions of L.Sc.D. First is Popper's general characterization of societies as open or closed and his historical idealization of the direction of social change from concrete or face-to-face societies towards abstract and anonymous ones. Second is the emphasis on the autonomy of sociology and the recommendation that social problems should be approached sociologically, not psychologically.

Science as a social activity is the model for the open society, especially as the prototype of science is Socratic dialogue. This is a social, intellectual and moral ideal in The Open Society, where it is strongly contrasted with the hierarchy, dogmatism and irrationality of the closed or tribal society. Because there is no science without social institutions, Robinson Crusoe's lonely scientific effort is not science.<sup>11</sup>

The anti-psychologism of The Open Society is independent, but dovetails very nicely with the anti-psychologism of The Logic of Scientific Discovery. In that latter book intersubjectivity is substituted for detachment, institutions for mental preparation, and

methodology for epistemology, where methodology is agreed rather than natural rules. It is remarkable that Popper, steeped in psychology as he was in the 1920s, is one of the sharpest critics of any tendency to psychologize social matters.<sup>12</sup>

### III. Some Comments and Questions

We find that in Popper's conception of the social there is no science without scientific institutions; there is no objectivity to science without its institutions; and the empirical method is necessarily piecemeal because it is impossible to test and try to eliminate all errors in one go. Science is a model for how we use reason to learn, and hence a model for the moral unity of mankind. It is only by cooperating in social institutions, while reducing the traditional (social and cultural) barriers to cooperation, that knowledge is obtainable at all. We are thus mutually interdependent and our mutuality has to do with a recognition that we are all in the same boat and in need of one another to accomplish such projects as science.

Why, then, did Popper not more clearly proclaim and develop his turn towards the social embodied in The Logic of Scientific Discovery? I raise the question, but do not wish to speculate upon an answer.

There are other specific questions for the answers to which one searches his work in vain. Are scientific institutions grown or designed; open or closed; abstract or concrete; and do they change piecemeal or holistically? The key question here is whether science as institution is grown or designed. Let me distinguish Science in general from science in particular. Science with a capital S, the worldwide invisible college, has, it seems to me, just grown. (This is consistent with many of the elements of the aggregate, such as

international scientific societies, being designed.) Science in its particular institutional embodiments, from the Royal Society to the Manhattan Project, is largely contained within designed institutions. Only designed institutions have aims, although grown institutions can have aims attributed to them. Unless science is designed, there is something puzzling about discussing its aim. Furthermore, both designed and grown institutions have functions as well as aims, and the functions may not be congruent with the aims. The Royal Society was not founded with any anticipation of the function it would perform in setting worldwide standards for Science, or even of being a worldwide model for scientific societies. The creators of the Manhattan Project (to develop the atomic bomb) did not aim to create a model for all future “big science”, still less for its colossal and normative impact on science proper.

Are methodological rules an incidental part of science or constitutive, as Popper’s writing seems to suggest? And, more important, how did the extant body of rules come into being? And, still more important, how are they to be reformed? In which forum, by whom, and how are any changes promulgated? Despite his institutionalism, Popper does not follow through on the consequences of his ideas in their social application; he persisted with an amorphous model of science. When we are invited to consider a model for the learning society in The Open Society, the invitation is to admire the critical rationalism of Socrates; and it is quite apparent that this is also Popper’s model for science. Socrates conducted philosophy with a small circle of friends and hangers on, in a group that appears to have had no internal structure at all.<sup>13</sup> Popper must have known very well that as a model for the actual practice of science this was an idealization - to say the least. The debates surrounding

the ideas of Einstein and Bohr, being largely theoretical, may be thought to have constituted a transnational Socratic seminar. The social ramifications of that seminar bore no resemblance to the practice of Socrates - they included the Manhattan Project. Laboratory science, as pursued in industry, follows the model of the Manhattan Project: in it large teams, large resources, and division of labor are coordinated in institutions very different from Socratic seminars. This kind of science is conducted in institutions with clear internal structure, such as hierarchy, specialization, scale, compartmentalization, division of labor, and barriers to entry. The transnational institution of Science as such, the widest construal of Robert Boyle's invisible college, is by interesting contrast much more diffuse, organized more like the Internet than like a hierarchy. Be it noted that in their institutionalization, neither concrete science nor Science in its worldwide sense resembles Popper's Socratic model.<sup>14</sup>

Popper's devotion to Socratic face-to-face critical discussion in small undifferentiated groups is longstanding; it is a model first for science (L.Sc.D. 1935), then for philosophy, then for the open society at large (The Open Society 1945). Given that he is a bold critic of other social institutions, such as the market or the educational system, his not examining the actual workings and actual shortcomings of the institutions of science is difficult to understand. From the Royal Society to the German chemical industry there is a real history and a real sociology of the institutional embodiments of science, containing no doubt many dangers and mistakes that we would do well to learn from and avoid. And the most ticklish point is what one might call the politics of science: the formation of schools and parties and client-relationships and laboratory cultures and Internet groups that can be only partly

explained by divisions over what is the truth of the matter and how we should advance towards it.<sup>15</sup>

Popper had a golden opportunity to discuss the actual institutions of science when he and Kuhn were brought together for a debate in 1965. Instead, he made the jarring claim<sup>16</sup> that Kuhn was his “most interesting” critic, one who understood him better than most, and had seen clearly and named a kind of scientific activity - normal science - that was inimical to Popper’s idealization. Yet Kuhn’s sociology was meager and he said little about the politics of science and did not analyze major scientific institutions. Fair enough; Kuhn’s sociological ideas are thin and rather quickly run off into the sands of psychologistic reductionism.<sup>17</sup> Not so Popper, who we have seen to have a quite distinct sociology, with a theory of individuals and a theory of institutions and traditions and a theory of how individuals reform institutions - all of which could have yielded a rich discussion of Kuhn’s historical materials about teaching, socialization, textbooks, and leadership. These remarks concerning Popper’s omissions hardly amount to criticism; they are more expressions of disappointment at consequences not thought through.

To illustrate, consider one very fruitful idea. Popper says in The Open Society that institutions should always be framed on the assumption that anti-democratic tendencies are ubiquitous, whether latent or overt, in leaders and in followers. The application of this warning to science could explain why science has so proliferated institutions - proliferation is one way to end-run anti-democratic tendencies in extant institutions - and also why science is so riven with politics, which may be connected as much with populist anti-democratic tendencies as with divisions over the truth. Politics and proliferation are means



to mediate the allocation of power. In idealized science the only power should be the power of truth - the power of a theory, the power of a critical argument. But as soon as there is an organized structure there are other kinds of power that do not harmoniously correlate with truth and argument. For example, there is power in establishing the constitution of the organization; power in administering it; power in setting its agenda; all of these raise questions difficult to settle by appeal to the power of current theories or of arguments, especially as the author of a theory or argument may espouse views on the constitution, administration and agenda of the institution that are not congruent with the aims of science.

Thus the client system - you take my students, I take yours - which is particularly important in science recruitment, is not seen by its operators as a method of perpetuating and entrenching mediocrity; when that happens it is treated as an unintended consequence; it is rather seen as a piece of rational ordering in an otherwise haphazard process. The bosses in the client system often do their utmost to internalize very high scientific standards in their students, even if their methods, such as ruthless bullying, might seem destructive.

If patronage, anti-democratic tendencies, power, and politics are problems for scientific institutions, then Popper's own ideas suggest some thought needs to be given to their design and manning<sup>18</sup> and in particular to their mechanisms for self-reform. Otherwise it is unclear where the methodological rules of the kind he puts forward find their home, in what forum and under what rules they can be debated, and how if at all the amended or supplemented rules that emerge from the debate can be promulgated.

To this it might be objected that Popper thought that science aims at ever more falsifiable hypotheses, ever-deeper problems, which in his later philosophy he was content

to call the truth. Ought not scientific institutions be designed and judged solely against their fostering of that aim? My answer is that there is perhaps a problem with the pursuit of truth as an organizing principle for societies or social institutions: truth can undermine organization, create problems for community building, for institutional cohesion.<sup>19</sup> One reason is that truth is indifferent to all other authorities, such as tradition, the law, custom, seniority, training, or hierarchy. Institutions and social relations dependent on any of these are vulnerable to truth. Furthermore, what the truth is on any matter, on Popper's view of science, is open to constant dispute. Thus there is at best the possibility of limited and temporary consensus developing around truth, not to mention the fact that truths are subject to interpretation, especially in their application. For these reasons, the scientific pursuit of the truth is a most unpromising prospect around which to organize society and its institutions. Truth and social institutions seem to be inimical to one another; either truth undercuts the very structure of the institution itself; or the institution develops structure and practices that inhibit the pursuit of truth. The sociology of how these tendencies are struggled against and partially overcome is a problem Popper's methodological revolution directs us to explore. Popper, as I have tried to show, was always a deep and penetrating thinker about society and social institutions; thus is it a matter of bitter regret that the opportunity of discussing these difficulties with him is now lost.

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<sup>1</sup>. Early versions of this paper were read to audiences at the Central European University, Prague and the University of Warwick on the 1st and 9th of June, 1994, respectively. I am grateful for the critical discussion at both those occasions, and especially to Ernest Gellner, whose thoughtful questions and trenchant writings have affected its final form. The argument sketched here was set out at greater length in a monograph entitled The Republic of Science. The Emergence of Popper's Social View of Science, 1935-1945, Amsterdam and Atlanta: Rodopi 2001..

<sup>2</sup>. The original text of Logik der Forschung is preserved, and translated for English-speakers, within The Logic of Scientific Discovery of 1959. In working with it one needs always to bear in mind Popper's own warning that every translation is an interpretation (The Open Society and Its Enemies, 4th edition, vol. I, Addendum III, 'Reply to a Critic', p. 326), and hence that in any translation things are always added/lost.

<sup>3</sup>. Other "social" interpretations have been put forward by Jeremy Shearmur, "Epistemology Socialised?", Etc, vol. 42, 1985, pp. 272-82; W. W. Bartley, III, Unfathomed Knowledge, Unmeasured Wealth: On Universities and the Wealth of Nations, La Salle, Ill.: Open Court 1990; Ronald Curtis, "Institutional Individualism and the Emergence of Scientific Rationality", Studies in the History and Philosophy of Science, vol. 20, 1989, pp. 77-113; and John R. Wettersten, The Roots of Critical Rationalism, Amsterdam: Rodopi 1992. None of these, as far as I can discern, anticipates the present approach.

<sup>4</sup>. Robinson Crusoe, alone on his island, cannot create science. Science requires social institutions.

<sup>5</sup>. The anachronistic language is an allusion to Michael Polanyi.

<sup>6</sup>. The passage ends with the phrase, “going beyond rational argument”. This parting sally of Popper’s suggesting that decision goes beyond rational argument is consistent with the view he articulated in The Open Society. It has been the center of some controversy, with his disciple and critic Bartley ending up labeling Popper “fideistic”, in the second edition of his The Retreat to Commitment, La Salle, Ill: Open Court 1984, p. 104 and p. 215n. This seems to me a fundamental misunderstanding of a simple point.

<sup>7</sup>. Admittedly this account is an over-simplification. Social rule following is not like a meeting agreeing to follow Roberts’ Rules of Order. Institutions and the apparent rule-following behavior that constitutes them are mostly forms of spontaneous social order (‘just grown’, see below), for which the classical example is the market. On this see Shearmur, “Epistemology Socialised?”, op. cit., and especially Curtis, “Institutional Individualism”, op. cit.

<sup>8</sup>. Two notable exceptions are Ingvar Johansson, A Critique of Karl Popper’s Methodology (Stockholm: Academiforlaget 1975) and Mark Blaug’s The Methodology of Economics (Cambridge 198).

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<sup>9</sup>. In the book version of “The Poverty” of 1957 this is at p. 136. In the original articles (Economica, vol. 12, 1945) it is at p. 80, with only minor syntactic changes.

<sup>10</sup>. The argument that Popper’s methodology is redundant and this his other idea of the logic of the situation is sufficient to make sense of the progress and rationality of science was made by J. N. Hattiangadi in “The Structure of Problems, I and II”, Philosophy of the Social Sciences, vol. 8, 1978, pp. 345-65 and vol. 9, 1979, pp. 49-76, and “A Methodology Without Methodological Rules”, in R. S. Cohen and M. W. Wartofsky, eds., Language, Logic, and Method, Dordrecht: Reidel 1983, pp. 103-151. This seems also to have influenced Curtis, “Institutional Individualism”, op. cit.

<sup>11</sup>. The Open Society and its Enemies, Chapter 23, pp. 218-220. Objectivity is shown to depend on cooperation; the empirical method itself is always piecemeal and can work only if we are each checked by others; individual bias and prejudice can not be eliminated and the institutions are there to compensate for it. Using other words, virtually the same argument opens Michael Polanyi’s “The Republic of Science”, Minerva, vol. 1, 1962, pp. 54-73.

<sup>12</sup>. Malachi Hacohen, “The Making of the Open Society: Karl Popper, Philosophy and Politics in Interwar Vienna”, Ph. D. dissertation, Columbia University 1993, chapter 8, makes a convincing case that this is not unconnected with the Methodenstreit between German and Austrian economics. See also his book version, Karl Popper: The Formative Years, 1902-1945, Cambridge: Cambridge University Press 2000.

<sup>13</sup>. Incidentally, but relevantly, the aim of the group was to care for one's soul, not to gain knowledge of the world.

<sup>14</sup>. Nor, I think, is it quite as republican as it is depicted in Michael Polanyi, "The Republic of Science", op. cit.

<sup>15</sup> Following on the interesting work of Fuller, I have discussed these matters further in "Fuller on Science", Philosophy of the Social Sciences, forthcoming.

<sup>16</sup>. K. R. Popper, "Normal Science and Its Dangers", in I. Lakatos and A. Musgrave, eds., Criticism and the Growth of Knowledge, Cambridge: Cambridge University Press, 1970, pp. 51-58.

<sup>17</sup>. I am thinking of Kuhn's talk of Gestalt switches, the weight of opinion, and the like.

<sup>18</sup>. "Institutions are like fortresses. They must be well designed and manned", The Open Society and Its Enemies, chapter 7 (III), p. 126.

<sup>19</sup>. Ernest Gellner has made this argument in several places, most succinctly in Conditions of Liberty, London: Hamish Hamilton 1994, pp. 31-32.