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Review essay

THE WITTGENSTEIN CONNECTION

W.W. SHARROCK and R.J. ANDERSON
University of Manchester

David Bloor, *Wittgenstein: A Social Theory of Knowledge* (New York: Macmillan, 1983), xi + 213 pp.

Sociologists often flatter themselves that their discipline has displaced philosophy or is about to do so. Those problems with which philosophers have struggled in a speculative and ignorant way are now to be dealt with on a properly empirical basis. David Bloor is yet another of those sociologists demanding that philosophers recognise that "the competences that are needed to develop Wittgenstein's work do not belong solely, or even primarily, to philosophers" and that they must play a supporting role in helping those (i.e. sociologists) "who must now carry the responsibility for exploiting Wittgenstein's intellectual legacy" (p. 184).

This portentous claim concludes an argument which has been designed as an interdisciplinary polemic between philosophy and sociology. It is Bloor's central argument that Wittgenstein has been misleadingly presented as though he would have been a protagonist of the anti-positivist position in sociology for this is to ignore the fact that there are strong naturalist and sociological elements in Wittgenstein's work. These elements make him much closer to the positivists than is usually thought, something which is demonstrated by some important convergences between Wittgenstein and Durkheim, the arch-positivist. Wittgenstein himself failed to develop the empirical potential of his work, as his followers after him have done, because they have allowed themselves to be inhibited by disciplinary boundaries. They have insisted that their work must be remote from empirical concerns and have failed to see that as it stands Wittgenstein's work is incomplete, lacking the capacity to explain crucial things because this would require scientific, not philosophical work.

Wittgenstein did recognise the continuity of his own efforts with those of science for he was apt to remark that he was talking about the natural history of humanity but he was, Bloor thinks unfortunately, willing to make use of his imagination, to make up examples. Bloor intends to improve on Wittgenstein by replacing

> a fictitious natural history with a real natural history, and an imaginary ethnography with a real ethnography. Only in this way can we make a secure estimate of Wittgenstein's capacity to illuminate life, not as it might be, but as it is; and to describe people, not as they might be, but as we find them. There could be no more disciplined way to see just what his work amounts to [p. 5].

As a matter of fact, there could, and it would be one which would give much more attention to understanding the nature of Wittgenstein's work *as philosophy*, taking much more notice of the reasons Wittgenstein had for warning against the craving for generality and explanation and perhaps testing out the "therapeutic" method that Wittgenstein regarded as the important product of his later work. Were these not unimportant elements of Wittgenstein's thought treated to more than summary dismissal it might have been seen that it is a perverse and irrelevant suggestion to say that Wittgenstein grasped only how life and people might be, and that he had an inferior appreciation of "how we find them" to that possessed by sociologists. It would perhaps also have avoided the erroneous inference that what is wrong with Wittgenstein is due to his lack of empirical sociological evidence.

Bloor maintains that had Wittgenstein or his followers but seen the possibility for the development of an empirical programme they must surely have developed in the direction of something like the sociology of knowledge as that is currently practised by Bloor and his colleagues. He alleges that the main objective of "Wittgensteinian" phulosophers in respect of the social sciences has been to argue against the idea that these could be causal, generalising and explanatory in the way that the natural sciences are but in this connection they are just wrong, as is testified to by successful work in sociology, particularly that on the history of modern science.

There is a fair amount of animus in Bloor's account of philosophy. His argument against them is in large part *ad hominem*, criticising them for being insular, narrow and unimaginative, attributing their refusal to break out of the confines of the disciplinary boundaries to (seemingly) snobbery, timidity, and ignorance. What is wrong with "Wittgensteinian" philosophers is that they are trying to interfere with the progress of science, to prevent the development of causal models and general laws by trying to sanctify "ordinary usage", thus attempting to prevent scientists from making the kind of conceptual innovations that they legitimately must if they are to go about their work. The following is the kind of charge that is levelled at these philosophers, the work of A.I. Melden and R.S. Peters providing the occasion for this:

> It is therefore difficult to allay the suspicion that, unlike sociologists, philosophers don't really want to study practical reasoning at all. They do not seem fascinated by it for its own sake. Could their professed concern with it be just a means to another end? Their goal is, surely, something quite other. They are erecting a barrier to stop, or contain, the advance of causal, deterministic models of mind and action of the kind developed by psychologists and physiologists [p. 74].

With the upshot that, if these arguments were right, "in psychology, the role of Galileo must be forever unoccupied."

It is useful to Bloor to cast the argument in terms of science versus philosophy since there will be many who will immediately concur that there is no basis on which a bunch of scientific amateurs (i.e. philosophers) should be allowed to claim authority over the work of the professionals and hence the question of whether psychologists and physiologists need to build causal models of mind and action is to be decided by the exigencies of their work, not on some *apriori* basis. Furthermore, one can easily discount the arguments of these philosophers which seek to say that our contemporary concepts rule out certain possibilities by pointing to obvious cases in which scientific discoveries have, in the past, changed our concepts, despite the protestations of the historical equivalents of our contemporary Wittgensteinians. In addition, there is the general force which, in our culture, the appeal to science has: the development of empirical knowledge is progressive, and attempts to prevent this are retrograde and will, in any case, be overwhelmed by the inexorable growth of scientific knowledge.

However, whilst it is useful to him to cast the arguments in those terms, it is quite seriously misleading to do so. The argument is not between philosophy and science at all, but is an argument within philosophy. Let it not be overlooked that the aim of Bloor is to make out the lineaments of a philosophical position – naturalism – in Wittgenstein's writings and to make a case recommending much more widespread acceptance of that outlook. It might be that some Wittgensteinian philosophers are motivated by an anti-scientific spirit and that some of them do overstep the bounds of their competence in their comments on science but it is far from being convincingly shown that there is something intrinsic to the Wittgensteinian position which is either anti-scientific or mistaken in the skepticism it shows about the importance of causal models and general laws in the understanding of "the mind" and human action.

Wittgenstein emphasised that his own work did not interfere with science and mathematics, it left verything as it was. It was important to him to distinguish philosophy from science and mathematics not because of some prejudice against empirical inquiries but because his own investigations did not result in either scientific findings or mathematical results. It was important to be clear about that. Hence, Wittgenstein certainly did not regard his philosophical work as giving anyone a license to intervene in or to restrict the work of science.

Peter Winch is regarded as one of the principal representatives of Wittgensteinian thinking about the social sciences, and he is prominently criticised by Bloor, both for distorting Wittgenstein's teaching and for putting forward mistaken arguments about sociology. However, if Winch is one who argues against the development of sociology in the direction which Bloor regards as suitably scientific he does not do so because of some anti-scientific spirit. At the beginning of *The Idea of a Social Science* Winch (1958, p. 2) makes it quite plain that philosophy has no business criticising science, and that it is no part of his intention to do so.

What troubles Wittgenstein and his followers is not science, but the metaphysical claims which are made about or on the basis of it. If we consider (say) Melden's *Free Action* (1961) which is a target of Bloor's criticism then we will find that the problem is not primarily with the possibility of a causal psychology but with the fact that "what any man may do even in moments of the most sober and careful reflection can be understood and explained, has seemed to many a philosopher to cast doubt upon our common view that any human action can ever be said truly to be free" (p. x). The claim that science will show that all human actions are caused and that they cannot, therefore, be free is not, of itself, a scientific hypothesis and one who contests such a proposal is no more hindering the progress of science than the one who proposes it is favouring it. What does need to be argued about is not what science might hope to do, about what science can practically achieve, but about the sense of what is now being claimed for it.

If someone says that the process of causal explanation in science is remorseless and that, eventually, science must show that everything is causally determined with the consequence that we will have to recognise that no action was ever really a free one then it seems they are saying something about what science can do. It appears, then, that to resist their arguments we must argue that science cannot do those things, that they are not practically possible. And how can that be an *apriori* matter? Who can have the temerity to say in advance that science cannot fulfill the prediction that the "determinist" has made for it? The properly applied Wittgensteinian strategy does not, however, make this kind of objection for to do so is to concede far too much to the determinist. It is to respond as if the determinist has made an intelligible prediction but this is precisely what must be called into question. As it is by Melden who says:

> what has recently become evident is that a veritable maze of muddles pervade and surround the familiar accounts offered by classical determinists of just such central and crucial notions as those of action, consequence, motives, circumstances and conditions, intentions, reason and the like [p. x].

If some Wittgensteinian philosophers do suppose that the reason why we need to inspect such terms as "motive", "action" and so forth, because these have special authority and are, in consequence, impervious to modification in the light of scientific discoveries then, we think, they are mistaken. But to say this is not to take the usual alternative view which is that scientific discoveries will show that we were mistaken to use the words "motive", "action" and the rest as we do. The relation of scientific discovery to our ordinary language is more complex than these unduly simple options suggest.

The reason why it is important to examine ordinary usage in these contexts is not because it embodies truths that the determinist (or some other metaphysician) is denying but because (if Wittgenstein is indeed correct in the broad outlines of what he is saying) the expressions of our ordinary language will be being misused in formulating the metaphysical thesis and hence that thesis will only seem to be saying something significant.

The *prospects* of the social sciences are a frequent source of metaphysical suppositions. It is often suggested that we had better detach ourselves from many of our contemporary ways of speaking and thinking because the appearance of a social science Galileo will shake the certainties of our life as devastatingly as the original Galileo shattered those of his time.

If someone were to express the pious hope that sociology might have its Newton, that someone would come along and reshape the discipline as effectively and thoroughly as Newton did the physical sciences then we should not want to make an argument ruling out the possibility of such a development. If they were to express speculative wonder about whether and how such Newtonian ahievements might affect us, whether they would have consequences at all comparable with those of the original then, again, we would be willing to try to imagine how they might be. If, however, someone wants to tell us that the social sciences *will* have its equivalent of a Newton, and that we must therefore realise that our current ways of thinking and talking are primitive and pre-scientific ones then we shall want to argue that such a person does not properly understand the nature of many of our extant practices and concepts. These are neither crude hypotheses nor proto-laws and hence if the progress of science does lead to the displacement of them it will not be because poor hypotheses have given way to better ones. Further, there are good reasons for asking whether the kind of thing that science does *could* replace the kind of thing we now do – not because they embody some correct theories but because they play a part in our lives which would not be a legitimate role for science to take over. Questions of this sort often get answered by the charge that they are doubting the established facts. Science is already doing these things, is already well on the way to creating the kind of causal explanations that will obviate our current "folk psychology".

Such arguments call for an assessment of the actual character of some of the contemporary achievements of science – are they really what they are claimed to be, do psychologists really have causal models that explain the mind or human action?

The fact that somebody says they have a causal model which explains human actions does not mean that they do have such a model. That they can present us with something that might look like a causal model of human action does not have to convince us that they are right. One can ask (for example) whether the model really does work *as a causal model*, whether it does (or could) explain what it claims to explain, and whether such things as it does explain are human actions? What is often at issue in such cases is not essentially about whether the scientist has the facts right or has understood important things about the nature of the phenomenon described but about whether the facts and findings are appropriately characterised as identifying causes and explaining actions. The questions are, in short, about how we use such expressions as "cause", "explain" and "action".

What Wittgensteinian philosophers are in fact trying to do is not to prevent the development of science but to get the enthusiasts for it to be much more specific about the nature of science's actual achievements and about the precise force of such implications as scientific work may have for our workaday ways of thinking and talking. What makes this kind of response less than attractive to the partisans of naturalism and similar doctrines is not that it rules out the possibility of a social science but that it considerably deflates its importance. The development of a causal, generalising sociology *may* not have the cataclysmically transforming importance that many seem to expect of it. Whilst one cannot conclusively rule out the possibility of the appearance of a sociological Galileo one can argue that there are just as good reasons for thinking one may not materialise as there are for thinking the opposite and one cannot, therefore, suppose that sociology is now to be justified or excused on the grounds that it is awaiting its Galileo.

Peter Winch is conventionally taken as someone who argues against the possibility of the kind of sociology that Bloor likes to envisage but his argument is less about what sociology might become than what it is like now. Winch does not (we think) argue that sociology might not make factual inquiries, does not prohibit the possibility of studying, in a systematic way, the lives, practices and institutions of people. He implies, however, that this is not what sociology *as it exists* is really interested in doing. The central problems of the discipline are not factual, but philosophical in character, being – specifically – those of an epistemological kind. Though there is sociological work which does seek to accumulate the facts about society, work of that kind does not comprise the central concern of the discipline. That is far more concerned with questions about what it takes for something to qualify as a fact, about the proper form that explanations can take, about the conditions for objective knowledge, etc. These are the kinds of questions which have made up the business of epistemology and they are not ones which arise from or will be answered by sociological researches (not least because they are concerned to lay down the preconditions of those). It is this that leads Winch to identify sociology as, essentially, misbegotten epistemology. It is asking epistemological questions, but is doing so under the impression that it is tackling scientific problems. What gives a sense of potential significance to such factual inquiries as sociology makes is, often, the idea that they will eventually contribute to the solution of the key problems of the discipline. It is the thought that by applying themselves to scientific research that they will conquer problems that have defied the philosophers that seems to motivate much sociological research. If Winch is right, doing research is not the way to deal with those problems which would mean that, from the point of those doing studies of that kind they would become pointless.

Bloor in fact concedes much of Winch's point, for it is his own objective to persuade sociologists away from their metaphysical preoccupations. He wants precisely to argue that his sociological colleagues should see that they can retain the distinctive styles and preferred methods that they favour but that they should recognise that "these differences do not have to be seen as deeply opposed metaphysical commitments...In principle these methodological divergences could assume the status of procedural and technical preferences. They could cease to be the centre of attention and become nothing but tactics for achieving the same strategic goals" (p. 180). This grants that it *is* the metaphysical aspects of sociology which are at the centre of attention and, so far as we can see, to recommend that these be set aside is to advise nothing less than much the same drastic re-appraisal of sociology that Winch attempted to make.

The difference between Bloor and Winch is this. Winch thinks that if you give the epistemological problems of sociology back to philosophy (where they belong) then you take away the really interesting questions. Bloor thinks that if you take away the metaphysical problems from sociology then the really interesting problems start to appear. It is at this point that Bloor's case about the explanatory inadequacy of Wittgenstein's philosophy (and of its subsequent interpretation) becomes relevant.

Bloor thinks that Wittgenstein has given a partial interpretation of knowledge, has shown how science and mathematics can become the topics of sociological and anthropological inquiry. Wittgenstein has shown us that knowledge is conventional and social, that the kind of knowledge that we have is created in and through our collective lives. He has not, however, explained why we have the knowledge that we have, what it is about our collective lives that has given us the science and maths that we have got. Here is the gap into which sociology can enter, and Bloor puts forward a case for relating knowledge to sociological variables and, particularly, to group interests.

Wittgenstein's disclaimers that he is not doing science, that generalisations are no use to him, that explanations are not needed, are simply disregarded by Bloor. He is apt to characterise these as Wittgenstein's prejudices and to claim the right to have different prejudices. However, these are surely rather more than prejudices of Wittgenstein's and they are rather too central to his work to be detached from the rest of it in this way. Wittgenstein is not, we would suggest, trying to explain how we acquire knowledge at all. He is not trying to explain how we have come by the mathematics and science that we have but is, rather, trying to dispel some misconceptions about the nature of the science and the maths that we have and to clarify how it is that this maths and science work.

There is a strong tendency in human thinking to sublime our practices, to conceive them in such an aesthetically purified and idealised a fashion that they begin to seem almost supernatural. We make them mysterious to ourselves: how could such wonderful things come into being? One answer is, we have made them ourselves but this answer is hard for those who have sublimed a practice to accept. How could such fallible, profane creatures as ourselves produce such unfailing perfection. If maths and science were just the product of our practical collective lives how could they work, how could they apply in such a penetrating and general way to nature if they were only contingent creations of particular civilisations?

Wittgenstein's method is designed to correct the tendency to sublime, to draw people's attention to the fact that this is what they are doing and, thereby, to discourage them from continuing in that way. In his later philosophy, and particularly in his work on mathematics, he is trying to make the suggestion that we have created mathematics, that it is a contingent phenomenon, persuasive to those who want to sublime it, to show that (i) maths is not sublime in the way they think it is and (ii) that recognising that maths is our creation does not mean that it does not work. Mathematics is not affected by our decision in this respect, it loses none of its use or practical power if people stop thinking of it in a rather superstitious way.

What Wittgenstein does need to impress on those he is arguing with is the fact that mathematics are a product of the exigencies of our life, we have the mathematics that we do because of the history and way of life of our society. Mathematics is contingent. If this is grasped, then one can begin to see much better how mathematics works, how its collectively created character shows itself in the practice if mathematics, calculating etc. It is quite unimportant to this that we should know what the specific exigencies which have given rise to our mathematics are. Hence it is anything but a deficiency of Wittgenstein's work that we lack an explanation of the actual course of history which has given us that mathematical legacy. This is not to deny of course that one might undertake an examination of the course of development of maths and science nor of the interest that might be found in that, but doing this does not amount to the completion of Wittgenstein's programme nor the answering of a question that he has left unanswered.

The sociology of knowledge is put forward as a demonstration that sociology can progress toward the causal explanations and general laws that Wittgensteinians regard as impossible. It might be more accurate to say that many of them regard them as redundant, rather than impossible. How well do Bloor's examples actually answer such critics? Not all that effectively in our judgement. Let us take one or two of the cases he presents.

Sociology can be like the natural sciences, so much so that it can have principles which are analogous to those of physics. Wittgenstein's own account of language-games can be the basis of a theory which can be formulated in a general way. There is a principle of the superposition of language-games which is "at once simple and powerful" and is identified as such by analogy with the superposition of waves in physics.

> The principle is this: a language game may serve more than one purpose at once. Two, or perhaps more, needs may be satisfied by a single move. Two, or more, purposes may be furthered simultaneously [p. 110].

A textbook on physics (Sears et al., 1982, p. 422) states the principle of superposition of waves in this way:

> The resulting motion of the string is determined by an extremely important principle known as the *principle of superposition*, which states that the actual displacement of any point on the string, at any time, is obtained by adding the displacement the point would have if only the first wave were present, and the displacement it would have with only the second wave.
>
> Mathematically speaking, the principle of superposition states that the wave function describing the resulting motion of the above situation is obtained by adding the two wave functions for the two separate waves...

It is because he attempts, by drawing his analogy, to make his work seem closer to that of natural scientists that Bloor shows us how great is the distance of his work from theirs. The physics textbook says something that is general but definite. It tells us that we can – for *any* string at *any* time – make certain calculations but the principle of superposition of language games says nothing comparable. It tells us that a language game may serve more than one purpose at once, but it does not tell us which language games will, nor what kind of relationship there might be between language games and purposes, whether there is a definite number of purposes that any particular kind of language game can serve, what properties of languages games fix the number of purposes they serve and so forth. It does not even tell us if there is to be some definite sort of relationship between a language-game and the variety of purposes it will serve. It has only the appearance of generality, and it gets that because of its vagueness. (Let it be noted that we are not criticising Bloor because his general principle is no firmer or substantial than most of those created by sociologists nor are we suggesting that he might easily have done better for we know that neither he nor anyone else can readily improve on his principle. We want only to point out that the achievements of sociology to date do not give good grounds for anyone skeptical that sociology is moving toward a powerful explanatory scheme reason to retract their view.)

But if the principle of superposition of language games does not really have an analogue in physics, does this not mean it has nothing of interest to say? Even if it does it will hardly eliminate the suspicion that if sociology was able to produce some general laws these might well prove redundant. If it had been named by analogy with the colloquialism as the "two birds with one stone principle" then the commonplace character of the claim may be much more apparent.

When Wittgenstein feels called upon to point out that human activities can satisfy a *multiplicity* of needs, it is not because he thinks he has found something informative but because it is something well known but often and dangerously overlooked.

The test, surely, must be in the application. The principle of superposition is really a methodological rule, enabling us to make inquiries into science, to determine the social structural conditions of its production? What is the payoff?

The following sorts of things are related:

> In the reception of non-Euclidean geometry both its supporters and critics were simultaneously participating in the enterprise of mathematics *and* exchanging important messages about professional autonomy,

leading to the conclusion that

> the esoteric debates over non-Euclidean geometry and its relation to projective geometry constituted an oblique commentary on matters of social import. The same holds for Galton's technical innovation: the concept of correlation. This was at once an innovation in mathematics *and* in eugenics *and* in the social position of scientific experts.

Information *is* being conveyed here, but what is it about? Is it something of a general kind or is it about the specific cases? Is it a *discovery* that scientists are human beings, that they have mixed motives and can promote their personal, ideological, professional, class and other interests through their work? We think not. Does this not, however, represent some steps toward a general law? We cannot see how it does this, for it is utterly vague as to how the case studies of particular scientific controversies are to be generalised except, again, into a methodological rule, look carefully for social interests and except to find these often very indirectly involved. What is being discovered (at least as far as we are concerned) is that in *this* case (i.e. the reception of non-Euclidean geometry) *these* were the issues involved, *these* were the non-technical interests that were affected, *these* the ideological allegiances which motivated the work and shaped preferences and decisions etc.

That we don't find these studies surprising or particularly revealing may perhaps be discounted against the fact that others find what they report not only surprising but unacceptable. This kind of sociology of knowledge is not as bland as we are trying to make it out. No doubt those who are disposed to sublime the sciences and mathematics *will* find it hard to swallow and will perhaps have the suspicion that such people have traditionally had toward the sociology of knowledge, namely that it is trying to demean scientific work and to diminish its importance. The capacity of the sociology of knowledge to convey that impression has been reinforced by its inclination to treat the idea of a sociological explanation as being paradigmatically identified with, if not exhausted by, interest explanations. It is hard enough for some to grasp that science and maths are human creations, and to ask them to accept that they are created out of the basest power struggles over material interest is just too much. If we are right, then the problem which actually shapes the new conception of the sociology of knowledge is not that of formulating an explanation of our knowledge but, rather, that of countering subliming conceptions of it (and Bloor is overtly concerned to undermine the idea of the purity of the quest for knowledge).

Bloor may say that it is no part of his programme to demean things but his practice belies this. One of the things Wittgensteinians are apt to emphasise is the relation, in the sphere of human action, between explanation and justification and it is this which makes them doubtful that causal explanations can work in the same way that they do with natural events. The notion of boundaries is one which is important in Bloor's conceptual scheme and it is used as part of his argument against philosophers, though its function seems to be less that of explaining than of "explaining away". It "explains" the philosopher's refusal to grant the conclusions which Bloor finds compelling by attributing "low" motives to them – they are concerned only for their discipline's status and purity. The use of this tactic reaches its lowest point in "explanation" of why Norman Malcolm's arguments in his *Dreaming* (1959, p. 146) have failed to win acceptance:

> Does this mean that the author of *Dreaming* must be a member of an isolated and quarrelsome little group huddled at the bottom of the grid-group diagram? No; but what his rhetorical methods show is that, after a fashion, he is trying to get to such a location ...he is inviting us to draw a boundary round our everyday life and to cut ourself off from science, keeping our practices pure, simple and stable. What the theory predicts is that it is only under the conditions defined by high group and low grid that Malcolm's strategies could ever win acceptance. These are the only circumstances in which his claims could have credibility... Properly understood, a glance at the grid-group diagram shows why so few people will believe Malcolm's claims: his language game does not fit our form of life.

Even if we were to accept that Wittgenstein's notions of language game and form of life were part of an explanatory scheme we could not agree that they could be used in such a crude way. Wittgenstein pointed out that people who have devised a new notation can make the mistake of thinking they have discovered something and the use of Mary Douglas "grid/group" scheme is presented as though it were a theory but is actually used simply to give a complex and cumbersome rewriting of the fact that Malcolm's arguments have not won very widespread acceptance. It explains nothing at all, but it does make it sound as if Malcolm's mistake was to engage in a pathetic reactionary struggle against inexorable forces. Bloor thinks that his way takes us away from the pseudo-explanations other sociologists have given but it looks to us as though he just adds to the stock of them.

Taking Wittgenstein seriously does not involve insisting that his injunctions to describe, not explain, and to look, not think mean that "If we are going to describe, then let us really describe, if we are going to look and see, then let us really look and see" (p. 183). Wittgenstein's words were not a call to create a programme of empirical research but for us to take notice of things that are staring us in the face. His objective, as he made quite plain, was not explanation, but clarification.

Whether or not the sociology of knowledge is a way forward for sociology is one thing, but if David Bloor wants to claim that it is the rightful claimant to Wittgenstein's inheritance then he will surely have to prove a more legitimate connection than this.

REFERENCES

Malcolm, N., *Dreaming*. London: Routledge, Kegan Paul, 1959.

Melden, A.I., *Free Action*. London: Routledge, Kegan Paul, 1961.

Sears, F.W., Zemansky, M.W., Young, H.D., *University Physics*, Sixth Edition. Reading: Addison-Wesley, 1982.

Winch, P., *The Idea of a Social Science*. London: Routledge, Kegan Paul, 1958.