Introduction

The Legendary Peter Winch and the Myth of 'Social Science'

Rejecting the Very Idea of a Social Science

J.L. Austin (1962, 2) once remarked that in philosophy one often observes that there is the bit where it's said and the bit where it's taken back. Well, we have titled this book, somewhat boldly, some might say brashly, but certainly intentionally provocatively, *There is No Such thing as Social Science.* That is where we say it. Will we now begin taking it back? Well, of course the answer to that question will depend on the use to which we are putting the word "science" in our title, in claiming there is no such thing as a social science. Is the use to which we put that term consonant with ordinary English usage or are we, in some sense, leaving ourselves open to a charge of gerrymandering by employing a marginal, specialised or contentious use of "science" so that we might more easily say that there is no such thing as a social science?

To come at this from another vantage point, let us state what we are not against and what we are not saying there is no possibility of. We are not against analytical rigour; such rigour is, of course, crucial to most serious modes of inquiry. Nor are we against a programme of social inquiry which accords a deal of importance to the revision of its claims in light of further (relevant) observations of various kinds; it is, of course, inquiry and not the production and defence of doctrine that practitioners in the social studies are undertaking.

Indeed, we would be willing to go further. We will even allow that the practice of social inquiry can (and does) on occasion learn from the practices undertaken in other modes of inquiry and on occasion these might be those modes of inquiry known as the natural sciences.

One might be inclined to retort that in this case, given what we have just said, we must concede that there *is* such a thing as a social science; that we have just furnished our readers with the 'the bit where we take it back'. Well, our reasons for resisting such a concession are as follows. Rigour, openness to having one's claims revised in light of further study and openness to learning[1] from other modes of inquiry are not enough for those who claim to be defenders of the idea of a social science against those whom they take to be their opponents. Analytic rigour is a *pre-eminent* mark of English-speaking philosophy; but, on Wittgensteinian grounds we would wish to contest strenuously any claim that such philosophy is itself scientific. 'Openness to having one's claims revised in light of further study' is a pre-requisite for being an historian—hardly thereby making history a science. 'Openness to learning from other modes of inquiry' is essential for

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[1] Of course, there are diverse ways in which such learning can take place. One can for instance learn from the (natural) sciences in social inquiry by loose analogy, without in the slightest being committed to regarding one's own inquiry as itself scientific. An intriguing example of roughly this, on our reading, is Thomas Kuhn's borrowings from Darwin and other natural scientists, in the substance of his own philosophising. See Sharrock and Read (2002).

polymaths and spiritual seekers and… Enough said.[2] No; there is always a further claim, of one kind or another, made in order to justify labelling one's inquiries "scientific". It is this further claim in all its variants to which we object. This further claim might be methodological:

- *There is an identifiable scientific method and this ought to be employed if one intends to make a claim to do something scientific.*

Or it might be substantive (or, sometimes, more specifically 'ontological'):

- *Social scientific findings are reducible to the findings of the natural sciences.*

Put another way one might say the following. The term "science" and "scientific" can be employed in one way to do no more than to denote a certain spirit of inquiry. Alternatively those terms might be employed in such a way as to invoke a specific method (or class of methods) of inquiry. And, sometimes the term is employed or invoked such that it denotes a set of specific substantive claims made by practitioners in certain domains of scientific enquiry (e.g. physics, biology, neuroscience, etc.) to which the claims

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[2] Though far more, in fact, could be said here. For instance: The huge irony of the programmatic, science-aping nature of 'social science' is that in this regard it manifestly fails to successfully ape (natural) science – for (real) sciences developed not through aping other sciences, but through actual empirical etc. study that eventually issued, through anomaly, crisis and revolution, in paradigms that gave birth to more unified or at least novel research traditions. *In the very act* of attempting to copy (natural) science, 'social science' invalidates its own scientific pretensions—for science did not and does not proceed by such copying. (For detailed exposition of this argument, see Sharrock and Read (2002), *passim*.)

of one's own inquiry (anthropology, sociology, psychology, etc.) are reducible.[3] The latter

of this trio and the second of the two varieties of reductionism is often referred to as

ontological reductionism. We here call it substantive reductionism; we prefer the term

'substantive reductionism' to 'ontological reductionism' in the present context so as to

allow for a broader casting of our net, as it were. There are some reductionists who, while

being reductionists in the substantive sense would not obviously be ontological

reductionists. Those who seek to reduce sociological claims to evolutionary psychological

claims, for example, are not merely methodological reductionists, simply arguing for what

they take to be legitimate scientific methods, (as does, for example, McIntyre 1996 and

2006), but are substantive reductionists, in that they reduce the sociological to the

evolutionary psychological[4]: the claims of the former, if valid, will always be simply the

claims of the latter. This said, they are not necessarily committed to ontological

reductionism.

We might therefore express the above in the form of questions as to what meaning

one attaches to the term 'social science',

- Is one talking of social science as scientific in terms of it being conducted in

  the scientific spirit: its practitioners acting in accordance with certain

  intellectual virtues?

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[3] There are, of course, many uses of the words 'science' and 'scientific'. We make no attempt here at a

taxonomy.

[4] See, for example, John Alcock (2001).

- Is one talking of social science being scientific in terms of its method being one that is shared with the (or some of the) natural sciences, reducible in terms of methods employed? Or

- Is one talking of social science as scientific in terms of it being reducible to one or other of the natural sciences, reducible in terms of the substance of their claims?

While substantive reductionism is (sadly) both common and prominent in recent philosophical work on consciousness, for example (e.g. Patricia Curchland's (1989) and Paul Churchland's (1988) neuroreductionism), it is, relatively speaking, less-so in the social sciences. However, it is present, even gaining a degree of prominence. Evolutionary psychology would be one example; varieties of materialism would be others.[5] For, evolutionary psychology's misnomer not withstanding, it reduces psychological claims (and sociological ones, where it cares to) to evolutionary biology.[6] However, our targets—

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[5] See for instance Colin Campbell's (1996); compare Sharrock's criticisms thereof, and Read's (both in Coulter and Watson (eds.) 2008).

[6] We are not here denying that 'evolutionary psychology' (sociobiology) has anything going for it: it is obvious and trivial that some social phenomena have roots in biological adaptation from previous epochs. What we have against sociobiology is principally its scientistic and programmatic ambition to reduce most important social phenomena to those roots—it stands in dire risk, among other things, of thereby blighting our ability to understand the phenomena in question. In short: it tends toward crudity, in its portrayal of the practices it would explain and to leave us *less* clear about many social phenomena than we were before we

those we have in mind in titling our book as we do—are also the methodological

reductionists.

One might then paraphrase our title to the somewhat less snappy, though less open

to misunderstanding, "There is no such thing as a social science on the model of either

methodological or substantive reductionism." Why are we so sure there is no such thing?

Well this question is what we seek to answer in the chapters that follow; but, in short,

there is no such thing as a social science on the model of methodological or substantive

reductionism because to be committed to methodological or substantive reductionism is

to be committed to *a priorism*; it is to be committed to something—a method or the

relevant explanatory factors in one's explanation of social action—prior to one's

investigation. The correct method, if one wishes to speak so, is read off the nature of the

phenomena. To embrace a particular methodology from another domain of inquiry owing

to its success in that domain is, one might say, ironically contrary to the scientific spirit: it

is to fail to act in accordance with the intellectual virtues.[7]

It will be retorted that it is we who are being *a prioristic*. …How can we say ahead

of time that there cannot be a science of *x*?——Well, then, must we consider the possibility

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started. In that regard, its failing is indicative and prototypical of the diminishment of understanding that

'social science' can produce.

[7] Thus the first of our bullet-pointed questions above, we are employing to pose a serious problem for those

who would, as most apologists for social science would, be inclined to answer either the second or the third

(or both of those) of the bullet-pointed questions with some kind of 'Yes'.

that there must be a science of morals? A science of abstract objects? A science of things

beginning with the letter 'e'? [8]

The question is, upon whom the onus falls to show that there can be or is a science

of x (where x is society). We submit that the onus is on our opponents to demonstrate an

intelligible sense in which there can be a science of x in this case, just as in the above

cases.[9] It is not us but our opponents, the 'mainstream' who have the orthodoxy of

academic structures on their side (There are far more Faculties of 'Social Science' than of

'Social Studies'), who are being *a prioristic* and dogmatic: whenever they insist, overtly or

covertly, that there is only one legitimate method of human inquiry or knowledge-

acquisition, they are ruling out the possibility that there may be several, or many. They

are already convinced, before any discussion begins, that science is the alpha and omega

of epistemic respectability; whereas we are open-minded. They are convinced that, to be

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[8] For more developed argument and examples, see Read (2008).

[9] The attempt to argue for the possibility of a hard science of sociology is surely inappropriate for someone

who really believes that such a possibility can be realised, since the most convincing proof of their point

would be the delivery of just that science. The appropriate thing to do would be to work out that science.

Those who have taken the obligation to walk the walk, not just talk the programme, have failed and failed

dismally to produce anything but caricatures of understanding or cumbersome machineries for saying not

very much of a genuinely informative kind.

worth anything, social study must be social science; we keep looking[10] for whatever social study actually is and can be.

Let us put our case still more plainly here, for reductionists will continue to resist and will, no doubt, accuse us of something akin to anti-reductionist 'hand-waving', 'knee-jerk' anti-reductionism, or along with such luminaries as Pinker and Dennett, simply dismiss us as yearning fantasists who either know not what we mean by 'reductionism' or fail to give adequate sense to the term.[11] Our case is that reductionism in terms of

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[10] This expression of ours is intended to echo Sextus Empiricus's brilliant description of the alternative to positive and negative dogmatisms, in Chapter 1 of Book 1 of his Outline of Pyrrhonism (see http://people.uvawise.edu/philosophy/phil205/Sextus.html): "some have claimed to have discovered the truth, others have asserted that it cannot be apprehended, while others again go on inquiring". We "go on inquiring" after or "keep on searching" for—keep an open mind to—methods and truths that are not scientific or not scientifically-arrived at, rather than—absurdly—denying that science has efficaciously inquired into anything or—dogmatically and scientistically—insisting that *only* science has efficaciously inquired into anything.

[11] Dennett remarks, "The term that is most often bandied about in these conflicts, typically as a term of abuse, is 'reductionism.' Those who yearn for skyhooks call those who eagerly settle for cranes 'reductionists,' and they can often make reductionism seem philistine and heartless. But like most terms of abuse, 'reductionism' has no fixed meaning." (Dennett 1995, p. 80). In a similar manner Pinker writes, "Attempts to explain behavior in mechanistic terms are commonly denounced as 'reductionist' or 'determinist.' The denouncers rarely know exactly what they mean by those words, but everyone knows they refer to something bad. (Pinker 2002, p. 10). Dennett's drawing of an analogy between his critics and some mythical yearners for skyhooks is somewhat cheap and maybe even gratuitous. To dismiss (even by inference) those who do not share one's approach as yearning for something nonsensical like 'skyhooks' is

methodologies or substantive claims is counter to the *spirit* of scientific inquiry, and it is the *spirit* of scientific inquiry that is primary. The 'scientific spirit'—what one *wanted* out of attaching the qualities of science to one's endeavour in the first place—militates against both methodological and substantive reductionism in the social studies.

In Defence of Peter Winch?

To our subtitle; why defend Peter Winch? Is he not, as so-often presented, a minor diversion in mid-Twentieth century philosophy of social science? Has intellectual culture not moved on, and overcome Winch, as philosophy in general has moved on from the diversion that was linguistic philosophy? Was Gellner not correct in dismissing Winch's work as a voguish and "profoundly mistaken doctrine"?[12] This seems to be the dominant narrative. Similarly, we often hear the refrain that Winch was railing against positivism[13], but none of us are positivists now. So, no need to read Winch then.

Social Studies as Philosophy

It is now exactly a half-century since Winch wrote his provocatively polemical little book, *The Idea of a Social Science*. When Winch refers to "science" or the "natural sciences" in *ISS* he usually explicitly refers to the "*experimental* sciences". Some contemporary defenders of social studies as science (defenders of the idea of a social science), such as Roy Bhaskar

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nothing more than a cheap and empty rhetorical move, gaining him nothing. We are very far from seeking 'skyhooks'; indeed, by contrast, we remain resolutely on the ground.

[12] See Gellner "Concepts and Society" fn. 1., in Wilson, Bryan R. (ed)(1970).

[13] See, for example, Bhaskar (1998 [1979]) p.2.

and Lee C. McIntyre, believe this leads those who criticise them to begin with flawed premises, for their conceptions of science (they have very different conceptions) do not demand that the conducting of experiments are essential to a science. But this rejection of Winch, were it to be / when it is aimed at him, does not work. Even those natural sciences such as geology and astronomy for which large parts of their respective domains of inquiry are not amenable to subjecting to reproducible experiments are not non-experimental in any logical sense, though they are in practice (largely) non-experimental. Put another way: the unavailability of conditions under which experiments can be reproduced in some of the natural sciences is allegedly similar to a sense in which reproducible experiments are often unavailable to the social scientist. However, this practical unavailability of experimental conditions, though it is well worthy of both ethical and methodological reflection in the *social* case[14] is *not* the locus of the main issue to which Winch draws attention. Winch, as we discuss below, draws attention to the fact that questions in social studies are *logically distinct* from those in the natural sciences.

So, what were Winch's claims? Well, one of his concerns in his *the Idea of a Social Science and its Relation to Philosophy* (hereafter *ISS*) was to disabuse social studies of the obsession with methodology and refocus attention onto the genuine loci of significance in their investigations: meaningful human actions. In short, Winch sought to pull one away from an obsession (which continues unabated) with identifying a methodology so that

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[14] What would a genuine controlled 'economic experiment' look like, for instance? Could it really happen at all, outside a thoroughly authoritarian state in which the answer to the experiment was in effect guaranteed before one started by repression of any human effort to resist that answer? (Think Friedman's Chile).

instead one might spend one's efforts on the identification and understanding of action. Unfortunately Winch's efforts, by and large, went unrewarded. The scientistic obsession with method went deep, very deep. So deep in fact that when someone like Winch came along and tried to treat the obsession he was misunderstood as making a call for a new, distinctive method of social inquiry of his own, replacing the idea of covering laws with the Wittgensteinian notion of rule-following. This was not Winch's intention. Winch was concerned to demonstrate that the social scientist has good reason to look somewhere other than the natural sciences for guidance. In a profound but very predictable irony, Winch was read—(re-)interpreted—through the distorting lens of scientism. Scientism, so powerful a cultural urge that Winch could help relatively few others to overcome it, and so was largely read as simply offering a variant form of it.[15]

Winch's claim in fact was that the central misunderstanding current in the social studies was the desire or tendency to see them as a branch, a new or proto- branch but a branch all the same, of the sciences; when rather one should see the social studies as much more akin to a branch of philosophy. This has been and is resisted by Winch's critics, philosopher and social scientist alike. The resistance is founded in a misunderstanding of the nature of philosophy, which Winch addresses in the opening chapter of *ISS.* The inertia of those who are resistant to social studies being philosophical rather than scientific stems from their being in thrall to a latent but thought-constraining picture of philosophy

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[15] We shall detail in the body of this book some moments where early Winch unfortunately provided hostages to fortune in making this misreading of him too easy; these moments were very largely overcome, in later decades of Winch's life and work.

as either an inferior pretender to science or as a master science: a science of the most general.

Winch writes,

> The argument runs as follows: new discoveries about real matters of fact can only be established by experimental methods; no purely *a priori* process of thinking is sufficient for this. But since it is science which uses experimental methods, while philosophy is purely *a priori*, it follows that the investigation of reality must be left to science. On the other hand, philosophy has traditionally claimed, at least in large part, to consist in the investigation of the nature of reality; either therefore traditional philosophy was attempting to do something which its methods of investigation could never possibly achieve, and must be abandoned; or else it was mistaken about its own nature and the purport of its investigations must be drastically reinterpreted. (*ISS* p. 7-8)

Winch continues to show how this argument fails. It fails because of equivocation on the word "reality".

> The difference between the respective aims of the scientist and the philosopher might be expressed as follows. Whereas the scientist investigates the nature of particular real things and processes, the philosopher is concerned with the nature of reality as such and in general. Burnet puts the point very well … when he points out that the sense in which the philosopher asks 'what's real?' involves the problem of man's relation to reality, which takes us beyond pure science. 'We have to ask whether the mind of man can have any contact with reality at all, and, if it can, what difference this will make to his life.' Now to think that this question of Burnet's could be settled by experimental methods involves just as serious a mistake as to think that philosophy with its *a priori* methods of reasoning could compete with experimental science on its own ground. **For it is not an empirical question at all but a *conceptual* one. It has to do with the *force of the concept* of reality. An appeal to the results of an experiment would necessarily beg the important question, since the philosopher would be bound to ask by what token those results themselves are accepted as 'reality'.** (ibid pp.8-9 emboldened emphasis ours)

So, Winch at once defends a discrete realm of philosophical inquiry; a realm where appeal to experimental results simply begs the (philosophical) question. And he further claims that social studies either belong to this realm or are closer to it than they are to the realm of the experimental natural sciences. The point, to be clear, is that a question is a philosophical question if that which is in question involves, has intrinsic to it, a question as to the subject of the question's criteria for identity. If it does then to try to answer the question through appeal to experimental methods begs the question; to conduct an experiment one has to have already established the identity of the subject of the experiment; it is only then that we can talk meaningfully of conducting the experiment and of the experiment having established anything. Consider: the question as to whether God exists is not the same kind of question as the question as to whether unicorns exist, nor even whether an invisible man exists (as featured in H G Wells's novella). For in asking whether God exists we are asking what would count as Him, and what would count as Him existing: would we have to be able to see Him, must He be tangible, and must He be locatable? Unless the question as to whether unicorns exist is using the name in a radically different way to its usage in English then we have no problems similar to those we encounter with the God question. (Problems indexed preliminarily, indeed, by capitalisation of the 'H' in "Him")

Consider another question. The question as to whether there are any dodos living on the island of Mauritius is a question for which we can easily establish criteria for answering (as we have done, and answered). We can establish criteria for what we are calling a "dodo", what we count as its being "on the island of Mauritius" and so on. Once

we've established the criteria, producing a living dodo on the island would settle the question, just as agreeing that we have conducted an exhaustive study of Mauritius and found no evidence of living dodos will establish the contrary.[16]

In contrast, if someone, a sceptic, were to ask whether anything exists outside our thoughts, producing for them a living dodo as evidence of something that exists outside their thoughts will simply beg the question. Indeed, consider that their asking the question in the first place indicated that asking questions of an interlocutor had not satisfied them of the reality of the external world—the externality of the interlocutor. All these things, everything, is what they question the reality of, for they are questioning reality *per se*: reality as a realm constitutively independent of their own thoughts. Pointing at something, even taking the questioner by the hand and leading them to touch the thing, having it peck their fingers, leaves their question untouched if not, presumably, their fingers. For the sceptic can simply respond that the thing pointed to, just as with the

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[16] And pretty much the latter is what has actually happened: there *are* no dodos on Mauritius. Of course, as with those who hold out for the existence of the Loch Ness monster, there might well always be those who insist that we might still find a dodo on Mauritius, just as we might one day find Nessy in Loch Ness and little green humanoids on the planet Mars, all previous attempts to do so not withstanding. But such resistance is psychological not logical, as it were; for such resistance is based in a *desire* for the dodo to exist or for Nessy to exist, or for (human-like) life on Mars. It is not based on lack of criteria for what would count as a dodo or for what would count as Nessy and so on. (Some might object to the example of the Loch Ness monster in that it is not established what sort of creature it is we are looking for. However, we have a clear idea what it isn't (dolphin, seal, shoal of fish, beaver, submarine made to look like a large sea creature, and so on)).

person pointing, does not *really* exist, for you (who?...) have yet to demonstrate its reality, its existence outside the mind (the only mind that really exists). The question posed was a question about the nature of reality in general. What was/is then in question is what *counts* as real. Providing the sceptic with something that one would ordinarily count as evidence of the existence of that particular thing is to offer 'something' to the sceptic as evidence, the status of which their question had already cast in doubt, not just as evidence but as *anything* outside of their own thoughts. We are not implying here that such sceptical questions are unanswerable, that one must concede the sceptic's point.[17] We are saying, with Winch who follows Wittgenstein,[18] *that the idea that one can refute such sceptical questions by recourse to experimental methods is deeply confused.*[19] The sceptic's question is a philosophical question about the nature of reality. It is thus a question about the place the concept of reality has in the lives of those who have grasped it, those who have a life with that concept.

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[17] Far from it—we would suggest indeed that they can ultimately be shown to the sceptic to be nonsensical. (See for explication many of the essays in *Wittgenstein and Scepticism*, edited by Denis McManus (2004))

[18] See especially Wittgenstein's *On Certainty*.

[19] It is well worth comparing here section xiv of Part II of Wittgenstein's *PI* (the very close of the book, as Anscombe and Rhees arranged it): "The confusion and barrenness of psychology is not to be explained by calling it a "young science"; its state is not comparable with that of physics, for instance, in its beginnings. … For in psychology there are experimental methods and *conceptual confusion*. …// The existence of the experimental method makes us think we have the means of solving the problems which trouble us; though problem and method pass one another by."

How does this relate to social studies? Well consider a further, third, question: are there any altruistic actions in the UK in 2008? You might answer no to this question, having read a book by the populist evolutionary theorist (and former CEO of Northern Rock bank) Matt Ridley; a book bought for you (as a 'gift') by your—as you now realise—self-serving mother. However, we suggest to you that you might want to consider the case of our friend Reuben, his dog, Spot and his Aunt, Bola. Reuben's dog Spot required a rather expensive operation on one of his claws. Aunty Bola sold her house and her belongings and moved into a hostel so as to provide funds to pay for Spot's claw operation. You 'point out' that Reuben's Aunty Bola was merely acting from self-interest.[20] The reasons you might provide for such a response to us are many and easily imagined, and there is little need to rehearse them here. The point we wish to make is simply that what is in question, what is intrinsic to the question as to whether there exist altruistic actions, is the question as to what one would count as such an action. The criteria for altruistic actions, taken in abstraction from the meanings those (altruistic) actions have for those undertaking them, is up for grabs, as it were; whether an action is altruistic or not is

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[20] The explanation from self-interest that you insist upon might be grounded in some theory to which you subscribe regarding the evolved behaviour of the human species, the place of the keeping of pets in that evolution and possibly also of the evolutionary-function of the neatness of one's pet's claws: Spot having neat claws means he is likely to be a more attractive mate to other dogs in the park of a morning, which means Reuben, too, has more chance of striking up a relationship with possible mates who are also dog-owners or dog-walkers. It is in the species' interest that Aunts have nephews in relationships that are likely to bear fruit, and so on. Spot's claw has more significance put like that…

not something that can be settled by experimental methods. It is rather a purpose-relative and occasion-sensitive matter. To abstract from the purpose of the action and its occasion is to abstract from that which conveys upon the action its identity. So, imagine the most generous act possible and then imagine a non-altruistic motive for that action. One can always be suggested, with a little thought, and rejecting the validity of such a suggestion is not achieved by simply pointing to the original action as originally imagined.

So we have our three questions,

1. Are there any dodos alive on Mauritius?

2. Does 'the world beyond my thoughts' exist?

3. Are there any 'truly altruistic' acts?

So question one is an empirical question; questions two and three are conceptual questions. They are in one important sense questions about the meaning of the words 'world' and 'altruism', respectively. The answer to question one can be settled empirically, by producing a dodo (or, equally, by thoroughly checking Mauritius for dodos and finding none, which is in effect what hunters *et al* did to this island and to this unfortunate species in remarkably short order, some time ago now). The answers to questions two and three cannot be so settled. We might put the difference as being one of scope. Empirical questions rest upon agreement in criteria; it is *only* because we agree that we can even talk of something being evidence without begging the question of those to whom we are providing said evidence. Conceptual questions are questions wherein and whereby such

agreement cannot be assumed in abstraction from specific occasions and purposes.[21] What this means is that the craving for generality which is central to the idea of a social science is undercut by the nature of that which the social scientist seeks to explain: its occasion sensitivity and it purpose relativity.[22]

Why do we go to such lengths to make our point here? Well, as we noted above, Winch is often misunderstood. So, once again, it is not the unavailability of the conditions for reproducible experiments in the social studies which drives Winch's claims in *ISS*. It is rather the nature of questions in the social studies that, Winch claims, have more in common with philosophical questions than with scientific questions.

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[21] No absolute line or gulf is being suggested here between empirical and conceptual (for explication, see for instance Kuhn's work and Wittgenstein's *On Certainty*). That a complicated grey area certainly exists is irrelevant to the distinction between empirical and conceptual remaining a sound and useful one, Quinian qualms notwithstanding (Those qualms, we submit, are no more pressing than the Sorites paradox.)

[22] 'Occasion-sensitivity' is Charles Travis's term (see his (2008) *Occasion-Sensitivity: Selcted Essays.* Oxford: Oxford University Press.) We borrow it because we find it apposite to our purpose here; we use it in a way which does not necessarily draw upon Travis's arguments, though we do by-and-large agree with those arguments, and (highly) recommend them to our readers who are interested in some of the central questions in philosophy. 'Purpose-relativity' of the meaning of actions does not entail Relativism—about culture or truth, and so on—but means only to draw attention to how the identity of an action is intimately related— we might say internally related—to the purpose of the action. More on this below and in subsequent chapters.

Resisting Winch: Reaffirming Social Science

Now, there has been considerable resistance to the implications that Winch took to follow

from this (social studies as philosophy, rather than science). One prominent and trenchant

recent advocate of Hempelian deductive-nomological methods in the social studies, Lee C.

McIntyre, argues for what we termed above 'methodological reductionism'. While he

claims to not reject what he terms 'interpretivism' (we challenge this as being an apposite

term for denoting Winch's discussions, below) McIntyre argues that

> [I]n order to understand the meaning of human action, we must attempt to put it in context, and at
> least part of the ideal context would include an account of the causes that led up to the event itself. The …
> example of interpreting the meaning of a film is instructive. Could one really be a film critic if one knew
> nothing about how films were made? That is, even if one were concerned with only understanding the
> "meaning" of the film as a "text" and only sought to give an interpretation of it in order to evaluate its quality,
> wouldn't one also have to know something about the causal genesis of film? Wouldn't one need to know about
> editing, producing, and sound direction? Are these really alien to the interpretation of a film's meaning? Surely
> we must care to know whether some of the subtleties in the final product were intentional—or whether they
> were artefacts of the medium itself.
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> Perhaps the same is true for the interpretation of human action. Even if one professes to be concerned
> solely with interpreting human action, there may still be a role for causal inquiry. One must know something
> about prior events that led up to the action as well as how the decision to act was formulated. What were the
> factors that influenced the thoughts the agent had? Could he or she have acted differently? To what extent was
> the action a function of intentions versus constraints imposed by other "human" factors? Was the actor tired,
> desperate or hungry—had he or she just had a fight with someone? How do people normally act under these
> circumstances?

Thus, one may conclude that interpretive and nomological modes of explanation need not exclude one another; although given their differing commitments to what it is most important to have explained about human behaviour, they will inevitably be at odds with one another about the proper focus of our inquiry.[23]

There is much here that could with reasonable charity be heard as unobjectionable. But, insofar as there is a case here for Hempel and against Winch, what does that case really amount to? The implied distinction between levels of description is misguided; as if there is an interpretive and a causal 'level', both being ways of describing 'the same thing' only from different 'perspectives'. Describing a punch in a way which does not involve the intentions of the person throwing the punch *is no longer a description of a **punch*** but a description of a type of movement. This aside, what does McIntyre provide us with by way of examples of putatively causal influences on a meaningful action? He writes: "What were the factors that influenced the thoughts the agent had? Could he or she have acted differently? To what extent was the action a function of intentions versus constraints imposed by other "human" factors? Was the actor tired, desperate or hungry—had he or she just had a fight with someone? How do people normally act under these circumstances?" (ibid). These are all things which can and would, where relevant, be easily included in any account of the meaning of an action and none of these examples are obviously examples of factors which must be, are best, or even fruitfully explained by subsumption under covering laws. (Though, needless to say, various 'natural laws' will be

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[23] Lee C McIntyre *Laws and Explanation in the Social Sciences: Defending a Science of Human Behaviour* Oxford, Westview Press 1996 pp. 129-130

potentially 'relevant to' / in play in the situation under description, even if rather

remotely—e.g. perhaps biological 'laws' covering the function of water in the body.[24])

Whether someone could have acted otherwise, in acting as they did, is not obviously best

captured by the deductive-nomological method, as McIntyre seeks to establish. It is best

captured by knowing something about the actor as a person and their relation to the

context, the social situation. Of course if the only 'alternative' to acting as they did would

imply acting in violation of the laws of physics then that does not so much serve as a

reason for acting as the person did, but makes manifest a constraint upon their actions.

But saying this does not mean that we see such constraints as *beyond* or *outside* the

meaning of the action. Such things are the *background* against which the action *has sense*.

One's description of the meaning of a leap of joy has no more sense (cannot be said to

have that meaning) devoid of its background context than a smile devoid of its face.

Similarly, questions as to whether a film is shot on film, or on video, or HD digital

are the background against which a critic makes his appraisal and pens his judgement of

the film. The critic is not oblivious to the practice, logistics and material constraints of

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[24] One reason why it is worth pointing this triviality out is the danger—a danger that Winch sometimes lets

his formulations foment—that Winch (likewise, Wittgenstein, or Kuhn) will be (mis-)interpreted as an

'Idealist' or Anti-Realist of some kind. The *whole person*, the whole 'rational animal', and not just their

'mental life', is what interests Winch/Wittgenstein; and indeed the whole person in their *worldly situation*.

Not just biological but also ecological (as well as physical) 'laws' are ultimately 'in play' in the – in any –

situation. But this is, as we note, trivial and obvious, not a cause either for scientistic ecstasy or

schadenfreude.

filmmaking. When it is pertinent to what he wants to convey about the film he will refer to elements of the practice explicitly; when it is not judged pertinent to what he is claiming regarding the film's meaning he will leave such things in the background, as the unsaid non-manifest context, so to speak. Some critics might think that the use of extreme close-ups shot from an extreme distance through a telephoto lens (when the decision to do so is purely aesthetic), rather than, as is standard, shot close-up to the subject with a standard lens, is relevant to the point they wish to make in their appraisal and interpretation of the film. Others will think it merely an inconsequential stylistic tic of the director. Either way, such things are not thought of as any more beyond the limits of interpretation of a film than the constraints of gravity are thought beyond the limits of interpretation of a person's Earthly actions, it is merely that in both cases sometimes they are relevant to the point being made and thus should be a factor in one's description and sometimes they are not so relevant and can be left *entirely* in the background, *simply* assumed.

However, we would not want to stop here; it is instructive to really try to imagine what it would be for a film critic to be oblivious to the basic practice, logistics and material constraints of film making, or a sociologist or anthropologist to be oblivious to basic physical and biological realities. Would such a person—let's call them 'McIntyre's film critic' and 'McIntyre's interpretivist sociologist'—be recognisably a critic or a sociologist; would they even be recognisably *members* of our culture? They would rather resemble members of a (rather extreme and exotic) cargo cult. In the case of 'McIntyre's film critic' would he not be as (or, even more) likely to believe the actors on screen to be Gods called

into existence by the folding down of cinema seats and the drawing back of velvet curtains? For if the film critic really knows nothing of the basic practice, logistics and material constraints of film making he knows nothing of what is and is not a film. Similarly, 'McIntyre's interpretivist sociologist' who knows simply nothing of physical and biological reality; what could he meaningfully say that we would understand? How could *he* live? We say this not in an attempt to refute anything McIntyre writes but merely to show that the dichotomy he sets up makes little sense. 'Interpretivists' (as McIntyre identifies non-Hempelians) are neither oblivious to physical reality nor do they think it always insignificant to their inquiries (though of course the degree of significance regarding a particular account of a particular action might well be what gives rise to many debates between sociologists of all kinds.).

One thing that is raised by a discussion such as that initiated by what McIntyre writes (above) is how Winch is usually discussed, by would-be friend and would-be foe alike. That is, as one of the "interpretivist" or "*Verstehen*" school in the philosophy of social science. Now, in one sense there is no problem with Winch being claimed or identified as a member of this school, if membership is open simply to those who oppose positivism and materialism in the philosophy of social sciences and favour some form of analysis which accounts for the meaning of social action. However, there are crucial differences between Winch and other prominent interpretivists, and the difference raises questions as to whether calling Winch an interpretivist is perspicuous. In short (for we discuss this in subsequent chapters) Winch follows Wittgenstein in drawing a vital

distinction between interpreting a rule and grasping/following/obeying it. What it is to understand a social action is to grasp the meaning of the action as do the participants. One does not generally go through a process of interpreting another's words as they engage in conversation—one *hears* the words. Similarly, one does not interpret the punch; one sees it (one would hope). The thought that interpreting must be taking place in both cases is born of prejudice and results in fallacy. This fallacy we might dub the fallacy of extensional primacy. This fallacy is born of the tendency to think that what is real or what has ultimate significance, what is primary, is that which is extensionally described, with all other intensional descriptions being merely ways of 'tarting-up' this perhaps more authentic but certainly more primary extensional world (The gendered descriptor here is salient: 'extensionalism', a close cousin to physicalism, is a fantasy of 'hard' science, psychologically-attractive to a gendered wish to avoid 'softness'). The fallacy has its roots in empiricism (and flowers in some forms of reductionism) and the idea that it is we, our minds, that add the meaning onto the world or the sense impressions caused by the (bare, unclothed or mechanical) world.

The German *Verstehen* can be translated as either 'understanding' or 'interpretation'. If Winch is to be seen as close to the *Verstehen* tradition, then we would favour the former translation, with the rider that when we say that what we seek is 'understanding' we are saying no more than that we seek to understand the action, in a perfectly everyday, sense. We do not seek understanding in some abstract sense, only available through application of one or another 'methodology'. One seeks understanding in that one seeks to grasp the meaning (in the same way) as ordinary members of the

culture do. Expertise in social understanding is a maturational art, not a science open to

expertise in any 'academic' sense of *that* word. There is and can be no *elite* of independent

experts in (the genuine content of) social science. In an important sense, we are *all*

practical experts—as, very roughly, we are all (all writers or readers of this book, in an

important sense) experts in practical use of the English language.

Participants in a conversation do not need to constantly be interpreting each others'

words. Social actors interacting within society need not constantly be interpreting the

meanings of each others' actions. There is no *process* of understanding running along

which enables them to see the meaning in each other's actions and words. This is where

Winch differs from *Verstehen* theorists such as Collingwood and Charles Taylor. So; one

should *not* fall into the trap of seeing understanding as some sort of mysterious property,

like some non-rational emotive capacity. This seems to be Manicas's concern,

> We must not think of *verstehen* as some sort of special, intuitive, sympathetic understanding, **a reliving of the experience of others**. *Verstehen* is something we do all the time. We are engaged in *verstehen* in **judging that** a person on a ladder is painting the house, in **judging that** the expression on another's face is distress produced by our careless remark, and so forth. We learned to do this, indeed, when we learned to use language. There is nothing dubious about such **judgements** since, as with **any judgement,** they **require evidence** and may, subsequently, be rejected. (Manicas, P. T. *Realist Philosophy of Social Science* p. 64, emboldened emphasis ours)

Now, we can very much take Manicas's point about *Verstehen* not being "a sort of

special, intuitive, sympathetic understanding, a reliving of the experience of others."

Indeed, it is rather (as ordinary understanding, rather than as extraordinary

interpretation) what "we do all the time"; there is nothing magical or queer about it. However, it is also, as we noted above, not something that draws decisively or (usually) *at all* on evidence (cf. our discussion of the three questions: of the dodo, of reality, and of altruism). Manicas is guilty of not paying close enough attention to the different uses to which we put "to judge" / "judgement". One's judgement as regards a *fact* or a material state of affairs is made when the resources for *knowing* and thus for stating the fact are not fully available to one. It falls to one then to judge rather than apprehend and state the fact, *x*, in the form of a true proposition: *x* is *y*; when the fact, *x*, is fully disclosed then the question of evidence becomes redundant and there is no need to talk of judging that *x* is thus and so. To invoke Austin's discussion in *Sense and Sensibilia*, we judge there to be a pig in the vicinity only when the pig is not before us: we judge a pig to be in the vicinity on the evidence provided by the trough of partially eaten turnips, the fresh trotter marks in the mud of the fenced-off area, and so on. If and when the pig emerges from the sty and stands grunting and snuffling before us this is not further evidence of a pig being in the vicinity, making our judgement stronger, but it is rather the moment at which judgement is made redundant as we have apprehended the fact: here is a pig.[25]

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[25] As Austin puts the scenario (in his unmistakable style): "The situation in which I would properly be said to have *evidence* for the statement that some animal is a pig is that, for example, in which the beast itself is not actually on view, but I can see plenty of pig-like marks on the ground outside its retreat. If I find a few buckets of pig food, that's a bit more evidence, and the noises and the smell may provide better evidence still. But if the animal then emerges and stands there plainly in view, there is no longer any question of

Judgement regarding the meaning of a social action is not of this variety of judgement. For the use of 'judgement' in the case of empirical matters, the only use Manicas sees, is, we might say, internally related to the concept of evidence. In contrast, judgements regarding the meaning of social action are not so related to the concept of evidence. For, as we explained above, the meaning of social actions is not an empirical matter—it is a conceptual one. A judgement as to the meaning of a social action comes into play (as opposed to our merely/simply *grasping* the meaning) when the meaning is not fully to hand, when it is beyond our grasp. This might happen in situations where the context is unclear, where we are observing another culture (people with seemingly very different ways of doing things to us) and so on. There are situations where while we cannot see the pig we can judge there to be a pig (or not), just as there are situations where while we haven't grasped the meaning of an action we judge it to mean *x* or *y* or *z*. This similarity is what leads Manicas, and others, astray; it leads them astray for the similarity ends here. Judging there to be a pig in the vicinity, like judging there to be dodos on Mauritius, is rendered redundant by production of a pig or (hypothetically, as once upon a time one could do) of a dodo on Mauritius. Judging whether a dance is a war dance or simply an entertaining way of passing the evening, is not settled by pointing at the dance in question, no more than judging an act to be altruistic is settled by pointing at the action. The action's identity is a conceptual not an empirical matter.

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collecting evidence; its coming into view doesn't provide me with more *evidence* that it's a pig, I can now just *see* that it is, the question is settled (Austin 1970, 115).

Beyond science: Winch's continued relevance

McIntyre and Manicas both want to defend some sort of naturalism in the philosophy of social science. McIntyre is defending Hempelian deductive-nomological methods and Manicas a version of 'realist' philosophy of social science. We would not wish to leave our readers with the impression that it is only naturalists such as McIntyre and Manicas that need take note of Winch's writings. It was in the 1940's that Robert Merton tried to popularise the earlier pronouncement of that 'Dean of social science' W. I. Thomas that 'a science which hesitates to forget its founders' is lost, challenging sociologists and other social theorists to give up pondering over the work of their predecessors and to get on with some empirical research and theory-building that would be sufficiently focussed to support knowledge accumulation. This campaign did not succeed. Stephen Cole's (2001) edited collection *What's Wrong With Sociology?*, and Lee C. McIntyre's (2006) *The Dark Ages* testify that there are not many who are willing to describe what has gone on in 'empirical social science' since Merton's day as involving anything much worth calling progress. McIntyre's book is a full-scale recognition of how little progress sociology has made in Merton's terms, for it is a reiteration of Merton's complaint that we are largely bereft of *scientific* understanding of how society works and how its problems might be solved.

Contemporary sociology in Britain and Europe, in contrast, is often *very much* a function of the way in which its history is understood, the main most recent contributions to 'Sociological Theory' proper offering few fundamentally novel ideas, attempting

instead the combination of diverse, often supposedly conflicting, conceptions from sociology's stock of long standing doctrines: consider the Grand social theories of Habermas, Giddens and Bourdieu. Whilst these 'theoretical' schemes are almost invariably offered with an avowedly ecumenical intention (their *spirit* is perhaps less ecumenical than is advertised) they do not really achieve much in the way of the sought-for integration across social science, but only add to the Babel-like situation, each acquiring their own enthusiasts, but failing to attract more than a few of those dissidents who, purportedly, should be ready and grateful to be drawn into their big tent. The very ambition for 'Sociological theory proper' is, however, one which has now been demoted, often out of disillusionment with and consequent opposition to the very idea of science generally and, thus, with the 'scientific sociology'.

However, it is not our view that the move away from 'scientific sociology' is to be welcomed as a move away from 'science', for, as we discussed above, the thought which forms Peter Winch's doubts about 'the idea of a social science' is that 'scientific sociology' had much more to do with philosophy (both metaphysics and epistemology) than it had to do with anything genuinely scientific at all. The idea of *opposition* to science seems supernumerary. The ostensible 'move away from science' is not actually that at all, it is much more a repositioning *within philosophy*. Winch has not, in our view, lost much relevance because things have changed a great deal within the 'social and human sciences' since 1958—*plus ca change,* after all.

As we noted above, Winch's main argument, in short, was that 'sociology' (and similarly much of anthropology, psychology, economics, linguistics…) in its then main

tendencies, was really philosophy presented in a form which could only mislead both those who might be considered customers for its promised deliverances, but also those who practiced its arcane and often shambolic arts. Not only did Winch see 'sociology' as a species of philosophy, he saw it as the wrong kind of philosophy, one which attempted to be, can we call it constructive, in ways that are not compatible with its character as philosophy. Winch's call to 'social scientists' was not then to give up science, but to give up (the wrong kind of) philosophy, and rather to philosophise in a spirit that they would actually find satisfying, instead. To philosophise—that is, to reflect, to think, and to look— in such a way that would actually deal with their intellectual needs, rather than always leaving them with the sense that 'more research is needed', because no real progress had been made with resolving or dissolving the philosophical needs that underlay their inchoate effort to empirically-or-theoretically-research their way out of them.

In some ways, the passage of time has done much to vindicate Winch's point about the quintessentially philosophical concerns of social sciences. Since Winch wrote there has been, across a whole range of disciplines, a very noticeable 'turn to the social' which, to some degree Winch himself inadvertently inspired, a change which has been so extensive and influential that it is often necessary to talk about 'social thought' rather than about 'sociological theory' to avoid misrepresenting the situation as the sole vehicle of theorising about the nature of social reality. A massive change during this period has been in the vastly increased receptiveness of Anglo-Saxon thought to 'Continental' and, especially, French, thought. The simple fact is that the most prominent figures in such thought—Althusser, Levi-Strauss, Foucault, Derrida, Deleuze *et al* have only rarely been

sociologists by profession, and have much more commonly been philosophers, openly

engaging with philosophical problems and without much of the concern for investigative

empirical scruple present in Anglo-American sociology. The explicit recognition of the

philosophical nature of the problems at issue is not, from Winch's point of view, a

satisfactory one, for, from his point of view, the wrong kind of philosophy is still

involved.

  Insofar as Winch was paid much attention, it was usually on the assumption that if

science was surrendered, then it was alright to continue with the *philosophy*—with,

roughly, sociological theory, with philosophical theories of the phenomena in question

(e.g. 'rule-following')—on the assumption that the possibility of a new basis for the 'social

sciences' should be considered: did Winch point to the right way forward for what he

would call 'social studies' or did his own views about what ought to be done fail the test?

But the idea of 'a right way forward' is one which is meaningless to Winch's thought,

since the notion of philosophy *proper* which he inherits from Wittgenstein is of a

philosophy that, in important ways, makes no attempt to go anywhere, for philosophy is

not in such business. Hence, there is no right way of going there, though there is the

possibility of an illusion that there *is* somewhere to go ((see Kevin Moore's (2000) attempt

to communicate this lesson about Wittgenstein's thought to psychology)). Thus *insofar* as

Winch was given attention, that attention was overwhelmingly negative, often completely

dismissive, and in those comparatively rare cases in which he was given favourable

assessment, this was often based on seeing him as a representative of an alternative

approach within sociology, a hermeneutic kind. Negatively, Winch was/is set aside as

incipiently idealist, and thus profoundly out of tune with a sociology that insists on becoming ever more relentlessly materialist, one where, having seen through Cartesian mentalism, the need is to focus on *the body* instead of *the mind.* Positively, Winch was/is considered as offering a new methodology for sociology.

Both these, the negative and the positive responses remained entangled with issues that Winch thought were philosophical in nature, and thereby ultimately pseudo-problems, problems of our own making, borne of our own confusions. Mostly left out of play in sociology itself, Winch has received considerable and continuing attention in the 'philosophy of social science', though for Winch himself the idea of a distinct branch of sociology that specialised in its philosophical problems, acting as an auxiliary to sociology itself would itself be a nonsense, a manifestation of the view of philosophy as an underlabourer to empirical/scientific inquiries that Winch repudiates at the very beginning of *ISS.*

In 'the philosophy of social science', Winch has been the focus of the long-running, and still continuing, debates about rationality, whether 'rationality' is a general idea which might be used to assess different societies and their practices comparatively, or whether each culture or practice must have its own inherent and potentially distinctive rationality. In this context, Winch is seen as advocating that reality is grasped through concepts, that different communities have to be understood in terms of their own concepts, and that, therefore, each community must confront its own distinctive reality—this makes him out as a 'relativist'. This line of thought is often identical to or runs parallel with the ascription to Winch of a doctrine about the inherent limits of cross-

cultural understanding, implying, if not explicitly asserting, that only those who belong to a culture can really understand it, and that it is impossible *really* to understand another culture except from 'inside it'.

Winch's work remains a focus of live discussion even into the present, though interest in it is mainly confined to the ghetto of 'philosophy of social science', and is much more often the target of critical devaluation than of approving support—and still less often is such approving support, support of the true Winch, for it is more often approval of an alleged 'Winchian' methodology for social science, or of a 'Winchian' relativism, *both* of which are dire contradictions in terms, unhappy sides of the same unhappy coin. Unfortunately, the Winch that comes in for criticism, like the Winch(s) that come(s) in for praise, never existed. As we will try to explain, in different ways, and at some length, Winch never supposed that there was a 'social science' problem about the possibility of 'understanding another culture', that there were any *inherent* difficulties—bordering on impossibilities—in understanding another culture (after all, it is only 'another culture' by virtue of exigencies of birth and biography), only reminding us, instead, that there are familiar and practical difficulties that we all encounter at points in our lives in understanding other people and their ways—e.g. 'I'm completely out of sympathy with them', 'I can't get the knack', 'I don't see the point of it', 'I haven't got the time to spend finding out' and the like. *Insofar* (not far!) as Winch is 'to blame' for reigniting the controversies over rationality and alien cultures after the 1960's, then much of the further discussion has been a waste of time, faulting Winch's supposed solutions to problems that he did not accept *were* problems.

If Winch's arguments are rescued from their ghetto confinement, then they are seen to be arguments about central features of the *whole* discipline—features of the very idea of 'social science'—ones inviting scepticism about *how far* the innumerable ventures being pursued in the name of 'sociology' (or its spin offs such as 'cultural studies' and 'media studies') are engaged in truly empirical inquiries, and *how far* they remain motivated and bemused by philosophical—or, as Winch would sometimes call them, 'conceptual'—problems. In these connections Winch's views are deeply dissident for they imply that

a) the difference between 'conceptual' and 'empirical' problems is not well understood in 'social science';

b) that the division between the 'conceptual' problems and the empirical inquiries reaches much further into the supposedly 'empirical' parts than most sociologists imagine, and is certainly not remotely captured by the difference between 'sociology' on the one hand and 'philosophy of social science' on the other; and

c) that very often the appearance of being an effort to solve an empirical problem, one with genuine factual content, is only superficial and is seriously misleading, to those engaged in the problem-solving, fact-finding effort as well as to onlookers.

Winch did not – and did not need to – hold that 'social science' has *no* empirical content, only that many of its significant and central concerns do not hinge upon, and will not be resolved by, factual investigations. That it has some empirical content does not bring it significantly close to the natural sciences, nor even mean that its main business is

finding out hitherto unknown facts. Winch was insisting that the problems he was talking

about were problems *in sociology* (anthropology, politics, etc), in the sense of being

problems that are present in sociology's main efforts at theorising and explaining, and *not*

just problems to be debated in the marginalised literature of 'philosophy of social science':

the very fact that it could seem otherwise is surely symptomatic of the problem his

diagnosis identifies.

Winch may have been sidelined in sociology subsequent to his key contributions.

This does not, however, mean that his arguments are irrelevant, even less that they have

been proven wrong by subsequent developments; only that their character and

implications *still* await proper recognition. *If* we are right in our presentation of Winch,

and *if* Winch is right in what he argues, *then* what he says is not only continuingly

relevant to 'social science' but it invites a fundamental rethink (*in* would-be social science

disciplines). Rather than just assert that this is so, let us—necessarily briefly—point to two

very prominent areas of recent social thought to which illustrate the case that Winch

developed.

Recent sociological thought has involved a largely unresolved struggle over the

necessity for sociological theory, and it is the two 'sides' to that struggle that we treat as

the material of our final introductory illustration. 'Post-modern' views subsume the fate

of 'sociological theory' under that of 'science', holding that generalised doctrines about

the totality have lost credibility in the modern world, not least as a result of the reflexive

application of favoured social science ideas to social science's own doctrines. Sociology

has often presupposed a difference between itself—scientific or at least empirical—and

ideology, and the stock form of a huge proportion of its work has become: people *think*

they know what they are doing, but we sociologists (psychologists, anthropologists,

'cognitive scientists', etc.) will show (through employment of our methodology or through

theoretical reconstruction) that they do not. The pre-eminence of this form of procedure

owes much to general scientistic prejudice in the academic etc. world, but also owes

much, slightly more specifically, to the concepts of 'ideology' (which can be traced back to

Marx) and 'the unconscious' (derived from Freud, latterly via Lacan). But if the Marxist

and Freudian lineages are credited with showing that the language is irreducibly

saturated by ideology and unconscious determinations, then what about 'social science'

discourse itself? Such a simple turn can yield a strongly negative assessment—such

discourse *can only be* itself an expression of ideology and the unconscious. It is not that

everyday discourse does not—because it cannot—represent reality, whilst 'social science'

discourse can, but that the very possibility of representation itself goes into crisis. The

impulse toward general theory becomes a discreditable form of the will to power, and the

aspiration for positive representation gives way to a drive at the perpetual destabilisation

of all purported representations (with the notion of 'theory' being redefined into a very

different form than structures of logically arrayed general structures, not least because the

idea of 'logic' has itself become suspect).

Rather than concede to such sceptics, sociological theorists have sought to reassert

the need for old-style theorising, a scheme of comprehensive generalities that can

encompass the order of the social totality (on behalf of, at least in some cases, rounding off

the 'unfinished project of modernity'). The principle form which the 'return to Grand theory'[26] or, alternatively, the move 'back to sociological theory'[27] have taken is that of synthesising pre-existing sociological doctrines. This involves identifying the central problem of sociology as a false polarising of doctrines, ones which set up a dichotomy between 'structure' and 'agency', 'objectivism' and 'subjectivism', 'idealism' and 'materialism' among other dualisms. In other words, the project of sociological theory as such has become rather generically known as the agency-structure problem, one of treating the supposed polarities as two sides of the same coin, and incorporating them within a single over-arching scheme which will 'explain' how societies maintain their unity ('soft' rather than overtly coercive power is the simple answer often given).

The need for these unificatory schemes results from disputes between rival schools of sociologists who—at least allegedly—affiliated themselves with one or other poles of the polarity that the above—purported—dichotomies constitute, where an assault from those positioning themselves on the 'agency' or 'subjective' extremes on the legacy of classical sociological theory seemed to threaten the whole tradition spawned by, especially, Marx. Sociology's founders, such as Marx and Durkheim, had been adamant that society is more than just an ensemble of individuals, and that complex forms of social organisation which *take precedence* over individuals arise, 'structures', that are the proper subject matter of sociology. If society—in some sense—consists only of individuals, then

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[26] Cf. Skinner (1985).

[27] Cf. Mouzelis (1995)

there are no 'structures' to be studied, let alone that can be appealed to in explanation of

the actions of individuals, since it is the fact that structures 'precede' individuals that

means that they are explanatory of what individuals do. The desire for unification arose,

then, from the need to defend the need for 'structure' in sociological theory, whilst

making concession to those who advocated the indispensability of agency. Up until the

attempts at structure-and-agency synthesis, theorists of 'structure' were as misguidedly

one-sided as those who commend agency, for they had excluded agency from the account.

What kind of problem does the contrast of 'structure' and 'agency involve? In

reality, it is nothing but a continuation of the metaphysical debate over 'determinism' and

'autonomy'. How far are individuals *made* to do what they do, and how far are they

exempt from any kind of compulsion?

The exercise is conducted as if it were an issue in ontology. The argument is

whether 'social structures' really exist as well as 'individuals' (to which structuralists had

traditionally responded with the views that 'individuals' have no real existence but are

merely products of social structures). Thus, the need for a concept of 'structure' calls for a

demonstration that 'social' and not just 'individual' facts are real. However, the argument

is not just about whether it can be established that structures *are* real, but involves the ploy

of arguing that the idea that they are not would put sociology in a completely implausible

position. The idea of 'agency' threatens the prospect of extreme—incredible—

voluntarism. Individuals would be completely free to do anything that they wanted.

There would be nothing, save the laws of physics, to limit their actions. That people

should be free in such a way is just not a conceivable state of affairs, which, as the

'structure' and 'agency' confrontation segues into a combination, shows the need for something to set a limit to such extreme voluntarism—and what would or could be more suitable than a concept of 'structure'.

But surely there is no need for the doctrines of a sociological theory to establish that we are not free in this—somewhat ridiculous—sense. If social scientists think that we, as ordinary persons, imagine ourselves as either omniscient or omnipotent then they are surely the ones who have the utterly implausible conception—if there *are* sociological theorists who envisage individual action as free in this extraordinary fashion, they surely need correcting, but this does not prove they need a notion of 'social structure' to save them from folly, but, and only, a healthy reminder that they do not, in their own real lives, imagine that actions are like this at all.

The *problem* is really a result of thinking that our participation as individuals *in society* need be thought of as a relation of causal determination. The idea that we, as members of the society, are not *really* doing what we think (i.e. imagine) that we are doing feeds on the idea that our intentions and purposes are not what make us do what we— think that we—want to do. Some think this must be so because intentions and purposes are non-material, mental phenomena i.e. epiphenomena, and therefore cannot qualify as real, material, causes. The *real* causes of our behaviour are essentially unknown to us, and so it is society, not ourselves, that makes us do what we do. Sometimes, the argument is phrased in terms of 'constraint' rather than causation, where social structures are seen as restricting what individuals can do, preventing them from acting in ways which they might personally prefer in favour of ways that the social order requires of them.

Since what people have in mind is a general theory it is all too easy to suppose that the idea that *something* makes us do the things we do identifies the explanatory form for actions—actions result from causes. From that springs the idea that *everything* we do we are made to do. For some people, this thought brings a *frisson,* or is somewhat uncanny—we never really know what we are doing, our actions are the mere effects of unknown causes. Such ideas are not ones that come after empirical inquires, but ones that go before them, and do not themselves constitute hypotheses that will be tested but provide a basis on which hypotheses might be constructed and thus determine how the results of any inquiry will be permissibly understood.

The idea that we are *always made to act as we do* would be in conflict with the idea—presumably that of extreme voluntarism—that we are *never made to act,* being always free to do absolutely whatever we want. We opened this *Introduction* with J. L. Austin's maxim about philosophers, that there is always the bit where they say it, and then the bit where they take it back; well, it is as well to bear this in mind here, for we are not saying that it is easy to convict *any* real sociologist of holding such unrelentingly determinist conceptions, at least with any consistency, but there are, for example, 'anti-humanists' who want to argue that individual human beings are, *as individuals,* virtually insignificant for social science, which is essentially about 'structures' and not about 'individuals' at all. (Such 'anti-humanism' is present for instance at key moments in the thought of Lacan and some of his followers, of Althusser, of Foucault in his 'archaeological' phase, and perhaps also of Levi-Strauss.)

The idea of individuals as mere puppets of social structures (if that is what determinism in the sociological context means) may draw some but it repels others—it is too much to believe that *everything* that we do is something that we are *made* to do, and it is perhaps this that gives a toe-hold for the idea of a synthesis. If we cannot deny that some things we do are things we *are* made to do, and others are cases in which we *are* indeed quite free to do just what we want, then the two seemingly polar extremes can be brought together.

A synthesis premised in such preconceptions must surely lead to the idea that people engage in two kinds of behaviour, that which is determined by the social structure and that which is autonomous or free. There are 'margins of freedom' within the limits of determination, it is said, sometimes allowing people to 'break out' of the control that the structure imposes on them.

We should now note that the notion of structural determination is as often and as much wound up with the idea of political regulation as it is of causal determination, though these are rather different issues in truth. Thus, social structures are thought of not as simply causes which produce whatever effects they produce … they are thought of as machineries of control which seek (in a metaphoric sense) to control *all* our behaviour, in which objective they are seen as being very largely successful.[28] Thus, individual

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[28] John McGowan (1991) writes, "postmodern theory, I argue, is driven by the simultaneous fear that a monolithic social order shapes contemporary life and hope that a strategy of preserving pluralism ((difference)) can be found."

autonomy, agency, is that behaviour which escapes the control of the social structure, and which can only be behaviour which defies or escapes the dictates of the structure.

We have been treating the attempt to reassert sociological theory by way of syntheses, as mainly defensively motivated, responding to one line of threat, which was that of eliminating the notion of 'social structure' from sociological discourse. Another line of threat, as already-intimated, is that arising from what we have called 'postmodern' conceptions, ones that have become critical of the idea that an all embracing theory of society is possible, ones which have, in important ways, attacked the possibility of determinate meaning in the context of theoretical reason.[29] One basis given for thinking this is that society is too diversified to be brought under any *single* theoretical scheme; another is, as mentioned, that in any case such a scheme will not be an objective representation of social reality, but only a disguised means by which one part of society seeks to impose its conceptions and needs on all the rest. A general theoretical scheme simply cannot be an objective portrayal of society because it is an attempt at representations, and all representations, as mentioned above, are pervaded by unconscious psychological and ideological impulses and do not really capture anything beyond themselves, but provide—for those of us who make naïve use of them in our daily

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[29] We do not make any attempt at a thorough treatment of post-modernism or post-structuralism, here. For a reasonably detailed critical engagement with Derrida on deconstruction see Hutchinson *Shame and Philosophy* chapter 2, section 5.

affairs—only an 'effect' of representation, but not the real thing [30] itself. In other words,

language possesses only partial meaning, in the sense both that it only captures part of the

picture, and in the sense that the picture it paints is on somebody's side.

The attempted subversion of the idea of general theory is, however, itself a product

of *theoretical* deduction, one which initially accepts the idea of a language as a self-

contained system, whereby the meaning of expressions is *entirely* determined by the

internal relations between the elements—the signs—of the language. (One can blame

Saussure for kicking this off.) From this, it follows that the relationship of language to

anything outside it must be arbitrary, and also entirely so, for any part of the language is

shaped by its place in the language system as a whole. But if the aim of a general theory—

amongst other representations—is to (re-)present things as they are in themselves, then

this idea of language erodes that possibility, for the way things are in themselves plays—

by definition—no part in fixing the forms of language, leaving the language without any

intrinsic connection to that which it purportedly represents. Take this line of thought—

often dubbed a 'structuralist' one, owing to its affiliation with 'structuralist linguistics'—a

step further, and suppose that language is not so tightly closed a system as had been

presumed. Without withdrawing from the idea that language is a system of arbitrary

signs, allow that the language is not quite so systematic as the original theory held.

Admit, that is, that there is some 'play' in the relations between the signs in the system

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[30] The preservation of the very idea of the real thing itself here makes clear already that post-modernists are

merely sceptics in a new guise. They have not fundamentally shifted the debate, as Wittgenstein does; they

are merely disappointed Realists.

and the idea that the definite meaning of expressions is fixed entirely by the internal relations of the system—the signs are defined in relation to each other, but now only comparatively loosely, meaning that if there is to be any fixity of meaning it must be supplied from some source other than the relations of the language itself (this is a move from 'structuralism' to 'post-structuralism'). The necessity to fix meanings in actual instances calls for the intervention of power, the imposition of a definiteness on relations that are not intrinsically definite, and such power will be driven by the unconscious psychological and ideological impulses already mentioned. In simple terms, language cannot say anything definite about the nature of things because language itself is not itself definite. Hence, language cannot *really definitively* be about anything other than itself, since what can be said in the language is a product of the language structure itself plus the unconsciously operating needs to portray things in one way rather than another. 'What there really is' (such that such talk might be allowed at all) is not only outside language but necessarily beyond any possibility of—cognitive—contact.

Arguments like these can again seem either quite thrilling or deeply perturbing or, just plainly and obviously wrong. Whichever of these reactions they elicit in their readers they seem to leave no one feeling indifferent! Should one embrace such arguments, one can have the sense that one has (finally!) seen through all the delusions that human beings have lived by for millennia, that *one* has understood that nothing is what it seems. It is in this sense that one can see the strong affinity with scientism and grand social theory; all of them—be it McIntyre's attempted rehabilitation of Hempelian deductive-nomological scientism, Habermasian grand social theory, or Derridean deconstruction—claim to

provide the methodological lens which will enable us to identify and thus break free of prejudice, whether that be conferred by ideology or the unconscious.

In the case of post-modernism however, there is, as already implied, a further implication... At the same time as we unmask unconscious prejudice, we have also grasped that there is no point in trying to say what things really are in contrast to what they appear to be. The exercise can only be one of exposing illusions without attempting to set up new ones in their place (and this makes the status of 'postmodern theory' itself a kind of imponderable). 'Reality' is now merely a delusional outward projection created by the workings of the language and of power relations that jointly hold us in their unwitting thrall, leaving us all in the same boat, and unable to say, with any validity, that one thing is better than the other, simply reducing all disputes to disputes of taste. Within the last three decades there has been an intense, ferocious and bitter struggle—called, in some contexts, 'the Culture Wars', and in others 'the Science Wars' and ranging across many different disciplines, including historical and literary studies—between those who embraced some version of these ideas and those who vigorously resisted them (the conflict peaked and has died down, but this does not mean that the divisions that produced it have ceased to exist).

The first point to make is that the two sides of the Culture and Science Wars is that many of their disagreements can be crystallised as being over whether language has a necessary or an arbitrary connection with reality, and in that respect they, so to speak, take in each others washing. Those on the one side, often calling themselves Realists, insist that language does *and must* have a necessary relationship with a reality external to and

constitutively independent of it if is to count as providing 'successful' representations. Our language is, in important respects, the way that it is because of the conditions that things in themselves, external reality, create for representation—if we're not going to be making mistakes all the time (and if we did we would soon be extinct) then the way we represent the world to ourselves and each other *must* at crucial points fit with the way the world is, must correspond to the intrinsic properties of whatever it is that it *does* represent. For those, the persistence and success of our way of life testifies to the truth of the arguments. Their opponents, though, show that if one accepts their—the opponents'— picture of what makes language meaningful, then there *just can't* be anything necessary about the relationship between language and anything external to it. The organisation of language is arbitrary which means, in the end, that language only 'represents', it never really represents at all.

This disagreement is not an *empirical*, but very much a philosophical, dispute, one which proceeds as many philosophical disputes do. That is, both sides can be seen to share certain fundamental premises. The (post-modern, etc.) critics do not dispute the initial premise that the idea of a representation is of something that represents the intrinsic nature of reality as it independently is. Rather, they leave that idea in place and then ask whether anything can possibly satisfy this requirement, going on to prove—to themselves at least—that *nothing can* meet this requirement because, (as we have put it in this condensation and simplification of very complex controversy) a true representation would be entirely culture-free, unaffected by all ideological distortion, but all signs are contingent, and can therefore stand in only an arbitrary relationship to anything outside

the language, meaning that *no* signs are culture free, and consequently that there are no *true* representations, only things that have the false, deceiving appearance of representing things. Rather than putting the initial premise in question, that 'representation of reality' requires a necessary correspondence, the critics accept this, and thus 'find' that the very idea of representation is thrown into doubt. One can, however, equally well put that premise into question, wonder whether this is a good characterisation of things that we would ordinarily call representations, and thus find out that one could consider that 'being arbitrary' and 'being a representation' are not antithetical notions—as though we were to take the fact that some measures operate in feet and inches and others in metres and centimetres as proving that there is no such thing as measuring the length of anything. Neither 'feet' nor 'metres' offer themselves as intrinsic properties of objects, but only as units of measurement which can—and manifestly are—used to determine the degree to which different objects might be the same as or some different length from each other…

The Continued Need to Read Peter Winch

To return then to our title; one would be mistaken if one thought that the claim that there is "no such thing as a social science" implied that we are in some—inchoate and incoherent—sense anti-science. We are anti-scientism, yes, but even that doesn't capture what we really mean to get at in what follows. We are anti the driving-thought of much of what passes as social science and philosophy of social science today (and dating back to when Winch authored *ISS*). For Grand Social Theory—as propounded in various guises

by Bhaskar, Bourdieu, Giddens, Habermas, and so on—deconstruction as a methodology in the social studies—as exemplified in exercises in the deconstruction of textual authority pioneered by Derrida (e.g. Clifford 1986, Ashmore, 1989)—and scientism—as propounded in positivist guise by authors such as McIntyre and in realist guise by authors such as Manicas—all claim to provide, by way of furnishing us with a methodology, a lens through we will finally be able to see the ideologically-driven or unconsciously-driven prejudice about our status as social actors, the way we relate to our social institutions and norms, and the identity of our actions: what we are really doing. Ours is not an attempt to say that no-one (social actors, members of a society: *people*) can be mistaken about these things, but that people being mistaken about what they are doing and how social institutions impact upon them does not imply that what is required is a methodology of social science, or a theoretical framework, so that we might apprehend what it is they are really doing or see how a person's relation to a social institution must be.

The analogy between a methodology (or theory) and a lens, which we invoked above, can be briefly explored in a little more depth, here. The fact that you might mistake a coiled rope in the corner of your garden for a snake on a dark night, or that you just missed the typos in the paper you wrote despite three read-throughs does not mean that you need spectacles. Similarly, a person might act in a manner that they only later, following a discussion with a colleague, identify as motivated by envy, or, a person only late in life might come to see that many of their moral beliefs, that they now consider to have negatively constrained the choices they've made, stemmed from their relationship to the institutions of the Church and the way this unconsciously structured their beliefs.

It does not *follow*, in the former cases, that we need a pair of spectacles: that is settled by a visit to the optician. The mistakes in question are due perhaps to similarities and poor conditions in the case of the snake/rope and to carelessness (not being attentive enough) in the case of the missed typos. Spectacles will not make a room lighter and will not make one more attentive. Similarly, coming to see that my action was motivated by envy is a matter of honesty with oneself (sometimes, a colleague's honesty forces one to acknowledge such things and be honest with oneself), and coming to recognise the influence the church has exercised over the choices one has made is a matter of seeing the contingent nature of those choices (that there were other possibilities) and that the Church's teaching does not allow (denies) that the choices it recommends are contingent (denies other possible choices if one is to be a good person[31]). But even this analogy doesn't quite get to the nub of the issue we want to draw out here. For methodologies do not work like spectacles, correcting a deficiency in vision, bringing our vision back to 20-20 vision. No the analogy would be better between the role claimed for methodology in the social sciences and the X-ray spectacles of science fiction (with the rider that what they enable one to see—below the surface—is what is most significant; is what is real).

In short, there are a couple of themes that recur again and again and again, throughout this book, and we make no apology for this repetition. These are the themes of the identity of an action and of the everydayness of understanding. They recur because so much of the misunderstandings and criticisms of Winch, and therefore also of those of us

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[31] What one, therefore, comes to see in this case is the contingency of is the Church's conception of the good.

who believe his teachings to be as, if not more, relevant now as they were fifty years ago, are based on a failure to grasp these points in full. So, whether we be explicating Winch (chapter one) explaining why he is not an Idealist (chapter two), demonstrating the affinities and differences with Ethnomethodology and other 'qualitative' sociologists, such as Erving Goffman (chapter three), or defending Winch against the charge of conservatism (chapter four) we find ourselves returned to the same issues: understanding what a person is doing is an perfectly mundane and everyday affair; where it is difficult—as when we—as *occasionally* happens—come upon a people who seemingly do things in a very different way to us—then we need to make more of an effort, just as we do when we are reading a book we find hard going, *not* leap to supposing that a pair of spectacles (i.e. a sociological method/theory) is what would help us. Our difficulties in reading are with what's in the book, not with the eyes we use to do the reading.

What they are doing, the identity of their action, is simply what the action means for the actors in the social setting: that identifying this is sometimes hard, and involves sometimes (e.g.) lateral thinking, does not equate to there being any need whatsoever for a social theory / social science.

In subsequent chapters we go into many of the issues we have introduced here in more depth. We have sought—in outline here, and in depth in the body of our book—to defend Winch from what we see as almost ritualistic misunderstanding. 'Ritualistic', in something akin to the pejorative sense used of that word in the likes of Frazer and Evans-Pritchard, rather than in the more open-minded sense present in the work of Wittgenstein and Winch… Unthinking, *functionalistically* beneficial to group-solidarity, and positively

superstitious… The superstition being that of scientism (and, by extension, grand social theory and post modernism) making it impossible to see room for an alternative way of thinking that could threaten the imperium of the only Method permitted any viability and importance...

The social studies, unlike the 'social sciences', not only begin but also end with non-academics, with (competent) members of a community. Social study is above all something that we do most of the time, we humans. It will be evident throughout that this is a thoroughly *Wittgensteinian* interpretation of Winch. We believe that much of the (rampant) misunderstanding of Winch's project is a result of a failure to understand the centrality for Winch's 'philosophy of social science' of Wittgenstein('s)—or at best a failure to understand Wittgenstein himself. We take seriously Winch's Wittgensteinian (therapeutic) heritage, and suggest that a Winch read after the fashion of Wittgenstein (read aright[32]) is immune to the main charges against him. Wittgenstein is no Idealist or Relativist, and just so, Winch is not; etc.

There are not that many books that become legends in their own authors' lifetimes. *The idea of a social science and its relation to philosophy* is such a book; we mean, in *this* book, to dispel the legend. Peter Winch was a very fine philosopher indeed, but 'the legendary'

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[32] See Hutchinson (2007) and Hutchinson and Read (2008).

Peter Winch, the Winch hated or despised[33] by his many 'foes' and loved by his few 'fans'

is a lesser, fictional, character. The true Winch was more radical than foes or fans allowed.

He was a (Wittgensteinian) philosopher who for the first time made it fully possible to see

that 'social science' not only had no clothes, but that there isn't an emperor at all, either,

and no need for one. There is no 'there' there. There is and can be no such thing as social

science, in the sense in which advocates of a science (or even grand theory) of society have

wanted there to be. Winch did not give us a new way of doing social science, a

'Wittgensteinian, rule-governed' way. To echo his supposed nemesis, Donald Davidson

(2001 [1974]), and the only partially sympathetic A. R. Louch (1963: 273): he intimated the

absurdity of *the very idea of* social science. . . The legendary Peter Winch is a fictional

character[34]—the true Winch was far more important and far more radical in teaching that

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[33] This might strike our readers as a little hyperbolic. See Gellner *op cit* if one wishes to confirm that it is not

hyperbolic.

[34] For an example in this regard see Winch's review of James Bohman's (1992) *New Philosophy of Social*

Science; Winch writes of Bohman's book: "The second of the introductory chapters is particularly difficult for

me to discuss, since a great deal of the argument hinges on criticism and rejection of views I am alleged to

have held in my 1958 book *The Idea of a Social Science.* My difficulty is that I recognize as mine hardly any of

the views discussed; indeed, most of them are views which I *criticized.* To substantiate this in detail would

quite inappropriately take up the remainder of the space I am allowed for this review, but there is one, as it

were scholarly, point that I fear I must make. Not merely does Bohman nowhere take account of, or even

mention the existence of, a number of other papers by me which are certainly relevant to the interpretation

of my position; he even describes approvingly some criticisms made by Alasdair MacIntyre of my paper

"Understanding a Primitive Society" without finding it necessary actually to refer to that paper, even to the

'social science' is a quintessential modern myth. But myths in the would-be form of science are the worst kind of myths—for, far more extremely than magic or religion, they cannot *bear* to admit their own nature, not even through a glass darkly…

It falls to us at this point to add only that much of what we have said already and much of what we say in what follows *should already be clear* if one reads or has read carefully Winch's *ISS* and a few subsequent papers: i.e. "Understanding a Primitive Society", "Trying to Make Sense", "Can We Understand Ourselves", and the *Preface* to the Second edition of *ISS*, and maybe add Rai Gaita's short but very insightful introduction to the 50th anniversary edition. However, the last 50 years since the publication of Winch's book demonstrate that careful reading has not been in abundance when it comes to Winch's work. We dare to hope that the present text might go someway to reversing this trend.

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extent of including it in his bibliography. So much for the "virtual dialogue" between interpreter and interpretee on which Bohman later rests so much theoretical weight" (Winch 1995: 473).