Is there any such thing as social science evidence? On a Winchian critique

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> [...] 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. (Hutchinson et al 2008:16)

> '"Social scientists" let us remember are self-appointed to their status as professional explainers, and there is no reason to accept that [they are] entitled to *overrule* the criteria that the indigenous practitioners of child rearing, surgery or politics employ to determine the identity of their actions. These self-appointed explainers are [...] perfectly entitled to set up their own standards of explanation [...] but all-too-often [...] they cannot let the matter rest there. In part that is because [this] would deprive [them] of the idea that the "social scientist" has a *special* role in society, one which makes it responsible for the general run of social affairs.' (p76)

The background theme of my paper is the influential idea that social science evidence can, and should, play a major role in national policymaking and/or in organisational or occupational forms of practice. That it should play this role is the view not just of many researchers but also of funders and some potential users of research. Furthermore, in the past two or three decades, in the UK and elsewhere, there have been growing complaints that social science is not serving this practical function effectively. This has resulted in institutional changes designed to rectify the situation, and increased efforts to demonstrate the value of social science evidence, such as the 'Campaign for Social Science' mounted by the Academy of Social Sciences in the UK. Many argue that future funding of social science depends upon its being able to demonstrate its 'impact' convincingly.[1]

One influential model for how these practical demands should be met is the 'classical' conception of evidence-based practice (Hammersley 2013). This developed in the 1990s in medicine and later spread to other areas, including social work, criminology and education. More recently, it has been extended to government policymaking, in the work of

[1] See https://campaignforsocialscience.org.uk/news/policy-academy-social-sciences-campaign-social-science/. On the notion of impact, see Hammersley 2014a.

the 'nudge unit' in the Cabinet Office.[2] This model treats research, in the form of randomised controlled trials (RCTs), as producing conclusive evidence about 'what works' in policy or practice, and therefore what should and should not be done. RCTs are regarded as the gold standard, on the grounds that they offer *clinching* evidence (but see Cartwright 2007). Of course, this model by no means drives all research designed to have practical 'impact', nor all policies underpinning the funding of social science; and, over time, its interpretation has been subject to some liberalisation. Nevertheless, for many commentators, it provides a benchmark indicating the contribution that social science ought to be making.

Of course, there has been a great deal of criticism within the social science community of this conception of research and of its role in relation to policymaking and practice. However, many social scientists who would reject this model nevertheless believe that their work can and should have a direct and major impact on policymaking and practice. But in my view many of the expectations of, and claims made for, social science are excessive, including those of social scientists. At best what it can provide is descriptive factual information, singular causal explanations (including generalisations to whole populations), and clarification of explanatory ideas. I do not believe it can produce theories specifying conditions under which specific causal processes always operate, or do so with some specifiable probability (Hammersley 2014).

Descriptions, explanations, and explanatory ideas can serve a variety of important, though limited, functions in relation to practical decision-making. However, in order to do this they need to be worked up into what can serve as evidence in the specific context concerned, since this is different from what is required in social science. Moreover, social science is not the only source of factual information about the world, cannot legitimately determine what is to be done in practical terms, and should not be presented as doing so – because other sorts of consideration are necessarily involved about which social science has no authority, including value priorities. In other words, any notion that policymaking or practice could ever be *entirely based on* research evidence is a fantasy (Hammersley 2013).

This is a rather deflationary view of the practical contribution of social science, but there is a line of argument about social science that implies an even more downgraded assessment of what it can contribute. And this is my central focus here. It is exemplified by Hutchinson et al's (2008) book *There is No Such Thing as a Social Science* (henceforth NST). Taking that title at face value, it is not difficult to see that it has dramatic implications for the notion of social science evidence and its role in practical decision-making. Put bluntly, if there is no such thing as social science then there can be no such thing as social science evidence, and it can therefore have no 'impact'. Of course, what

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[2] See http://www.behaviouralinsights.co.uk/. An example of their work is assessing different ways of prompting people to pay fines: 'Some were sent no text message (control group), while others (intervention groups) were sent either a standard reminder text or a more personalised message (including the name of the recipient, the amount owed, or both).' See Haynes et al 2012:10.

these authors are arguing is more subtle than this. They are not denying that there is something that calls itself social science, or even that it may have 'impact'. Rather they are questioning whether it lives up to, and could ever live up to, its name, and whether its impact is desirable. The implications of their arguments, drawing on the work of Peter Winch, undercut much of what passes for social science, and thereby the claims that are made for its contribution to lay practices; even my rather modest ones. They write that: '"Social science" is a quintessential modern myth.' (p27).

In the preface to their book the authors invite dialogue with those not entirely convinced by Winch's arguments. I share with them the view that fundamental questions need to be asked about the character of social science, but I am not convinced by all of their arguments. In particular, I do not accept their radical conclusion about the mythological status of social science, and the implications of this for its role in relation to practical decision-making.

Summarising, NST puts forward the following, interrelated, reasons why conventional social science is unsustainable:

1. It involves substantive and/or methodological reductionism, and this is at odds with the intellectual virtues, and with the nature of human social action;
2. The main issues social science addresses are conceptual rather than empirical in character;
3. Social scientists give false emphasis to the role of methodology in understanding human actions;
4. The method they appeal to is the logic of the experiment, designed to produce causal explanations, when what is required in understanding social action, most of the time, is description of intentions or reasons;
5. The purported aim of social science is general explanations of features of human actions and institutions, yet the need for explanation only arises when there is some specific deficiency in ordinary understanding – there is no possibility of, nor need for, general theories. Another way of putting this is to say that much social theory is philosophical rather than empirical in character, and moreover takes a misconceived philosophical form;
6. In this way social science falsely claims expertise, superior to that of lay people, in understanding human actions.

While these arguments are closely related, indeed mutually supporting in various respects, they are analytically separable, and I will discuss each in turn. Taken together they seem to challenge virtually any claim that it is possible to produce a distinctive form of social science evidence that could inform practical decision-making in the policy realm, or anywhere else.

Substantive and methodological reductionism

'Reductionism' here seems to mean the adoption of natural science concepts and/or methods on the grounds that natural science is the only provenly successful form of rigorous inquiry. The authors write that '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' (Hutchinson et al 2008:2). *Methodological* reductionism involves the claim that: 'There is an identifiable scientific method and this ought to be employed if one intends to make a claim to do something scientific'. *Substantive* reductionism is an insistence on using concepts from the natural sciences, it is the idea that 'Social scientific findings are reducible to the findings of the natural sciences' (p2).

I believe the authors are correct to challenge both these kinds of reductionism: it is certainly true that any selection of concepts or methods must take account of the nature of the phenomena being investigated; though, of course, we will need some prior means of understanding that nature. Indeed, it seems to me that rather than adopting either reductionism or anti-reductionism, a wise strategy would be to explore the nature of the phenomena while at the same time considering the range of methods that might assist in doing this, including drawing on the experience of other fields – with a view to selecting or developing an approach that will best serve the investigative goals.

This strategy does not seem to be widely adopted by social scientists. Instead, there is a tendency to adhere to relatively fixed models of research strategy, albeit diverse ones. At the same time, however, most social science today shows little evidence of reductionism, in either sense outlined. There are only limited signs of the influence of natural science, and even when there is evidence of this it is not clear that the influence stems from a belief that natural science is the only source of sound knowledge. Indeed, that kind of scientism is vociferously rejected by most social scientists today; and a great deal of effort has been expended by them in denying or downplaying the role of physical and biological processes in shaping human behaviour, to a fault I would say. Similarly, there has been general rejection of any appeal to, or use of, what are seen as natural science methods. Social scientists who in the past held to the idea of a 'natural science of society', like Alfred Radcliffe Brown or George Lundberg, were always few, and are now even fewer. Furthermore, even *their* research bore little resemblance to the practices of natural scientists: they did not adopt natural science concepts and methods in their original forms. Interestingly, the only example coming close to this today would be advocates of randomised controlled trials, mentioned earlier, who have taken this method from medical research. Even so, they appeal to specific arguments about the capacity of RCTs to rule out threats to validity, rather than simply appealing to 'scientific method'.

In short, then, this first argument does not offer much support for NST's challenge to what currently passes for social science. And in my view social scientists could still learn a good deal from natural science, at least in methodological terms, without seeking to ape it.

Conceptual rather than empirical issues

Hutchinson et al (2008:14-15) write that: 'What they are doing, the identity of their action, is simply what the action means for the actors in the social setting' (p26). And they argue that this implies that identifying actions is a conceptual rather than an empirical matter: 'the meaning of social actions is not an empirical matter—it is a conceptual one'. However, there are important questions here about what the terms 'conceptual' and 'empirical' mean.

It is certainly the case that social scientists engage with some issues that are conceptual rather than empirical in character, and that they may confuse the two (Kaufmann 1944; Cohen and Helling 2014). This is perhaps most obviously true in debates about such matters as structure versus agency, a particular target for criticism in NST (pp20-22). But it can also arise when social scientists deal with more concrete issues, such as whether social class divisions have disappeared or declined in 'postmodern' Western societies. Here, often, insufficient attention is given to clarifying the meaning of key terms, not least 'social class' itself. However, I do not believe that most of the conceptual issues that social scientists deal with are philosophical, in the sense that seems to be used by Winch (1958); where 'what is reality?' and 'what is understanding?' are taken as exemplars. Debates about structure versus agency do hinge, at least in part, on the longstanding philosophical topic of free will versus determinism, but they are not typical of most social science concepts in this respect.[3]

However, there is clearly an issue about what the empirical/conceptual contrast is being taken to mean. Besides the notion that it corresponds, more or less, to the domains of the scientific and the philosophical, respectively, there are at least three other interpretations of this contrast, which may be related to one another but need not be. One is the traditional opposition between matters of contingent fact, on the one hand, and matters of convention or procedure, on the other. In these terms the notion of the empirical points to how the world is, or at least how things are in our experience, whereas the term 'conceptual' refers to the meanings that we give to words, and/or to the regulative ideals that we employ (Kaufmann 1944). A closely related distinction is between empirical inquiry as the discovery of new facts, of what was not previously known, and the explication of what we already know, or the provision of reminders about this (Winch 1958). Finally, the authors of NST and Winch use a contrast between concepts that have

3 Of course, in the field of methodology social scientists may engage with issues that are clearly philosophical, such as the nature of science, and indeed the nature of knowledge and of reality; but they tend to do so only insofar as this is relevant for social science purposes. That scientists as well as philosophers are concerned with these matters is not surprising, nor is it surprising that they approach them from different angles. As Kaufmann (Cohen and Helling 2014) points out, methodology exists on the border between social science and philosophy and should be informed from both sides. Later I will raise questions about the smoothness of the relationships involved here.

agreed criteria attached to them, so that the phenomena coming under each concept can be determined with little difficulty, and those concepts where this is not the case, whose reference is uncertain.

None of these distinctions is entirely straightforward. This is perhaps most obvious in the case of the first. The term 'science' is far from certain in meaning, not only because of its honorific, or occasionally stigmatising, connotations, but also because of disagreement about both its intension and extension. 'Philosophy' is even worse: it is one of those concepts about which there appears to be no agreed sense, even among self-identified or officially designated philosophers. Indeed, there seems to me to be an important ambiguity even within the broad tradition of philosophy in which Winch and Hutchinson et al locate themselves: between an exclusive focus on conceptual analysis, on the one hand, and what is often referred to as metaphysics, on the other, where the concern is with general truths about the nature of the world or of our experience. On some readings of Wittgenstein, his focus is entirely on how the use of language can lead to confusions and the dispelling of these, or in the case of Ryle or Austin the task is taken to be to map the 'logical geography' of particular concepts or the subtle implications of different words. By contrast, Winch's discussions, not just in his first book but also in his later work, for example that on Simone Weil, seem to involve seeking to understand, to 'get right', the nature of such general matters as reality, understanding, thought, etc.[4]

The distinction between the contingently empirical and matters of convention has quite a long history in philosophy, but it has been interpreted in different ways, as well as sometimes being rendered more complex, for example through distinguishing between a priori/a posteriori and analytic/synthetic, rendering a fourfold set of categories rather than a dichotomy. Some philosophers, for instance Kaufmann (1944), have offered useful clarifications of this distinction, often stressing the role of procedural rules in science. Others have of course sought to erode or erase the distinction, notably Quine.

A further complexity might be noted. There is a sense in which Wittgenstein's approach to meaning is empirical, in broad terms, in that he calls on us to 'look and see' how language works. Here he is contrasting his approach with that characteristic of Russell, say, where an abstract or idealised model – that of logic – is imposed on ordinary language use. While Wittgenstein does not engage in explicit sociolinguistic study, relying instead on his own experience and knowledge of the German and English languages, he *does* treat language use as rooted in basic human practices. And much the

4 It seems to me that Hutchinson et al's attempt to recruit Winch to the camp of those who interpret Wittgenstein as entirely concerned with dispelling confusions, thereby offering no positive doctrines, is at odds with his discussion of his philosophical bearings in his first book (Winch 1958:ch1) and with some of his other work.

same is true of Winch. Furthermore, it is these practices that are to be examined if philosophical problems are to be resolved or dissolved.[5]

The idea that empirical sciences are concerned with discovering new facts whereas conceptual inquiry is a matter of explicating or reminding us of what we already know is troublesome in slightly different ways. It is true that a common complaint about sociology, and perhaps about social science more generally, is that it does not discover new facts (see Baldamus 1976), that it simply recycles common-sense knowledge, or collects knowledge from one group of people and communicates it to others. It is also the case that some economists treat basic economic laws as *a priori* in nature, and therefore as the product of intuition or introspection. Yet, while it is certainly questionable whether the social sciences, including psychology, have produced any scientific laws through empirical inquiry, it seems clear that they *have* generated much information of a more modest kind that was not already widely known: whether about processes of perception and illusion; about changes over time in familial relationships or patterns of economic organisation; or about the existence and operation of positional goods; to take just three examples.

Of course, the question of what is already known is by no means straightforward, and not just because it can be a matter of degree across a population, or because there can be variation in depth of understanding and level of detailed knowledge. Take the case of linguistics in the style of Chomsky: one formulation of what this has produced is 'explication of the knowledge that speakers must have if they are to be able to speak a particular language'. Yet, even if this is accepted, and analogies with the mathematical knowledge allegedly required to balance on a bicycle suggest that it may not be, it is unclear whether it amounts to a case of reminding speakers about what they already know. The starting point is certainly their recognition of what is or is not grammatical, or indeed linguistically meaningful, but the rules that the linguist identifies as underlying the combination of words in syntactically acceptable ways within a particular language are matters about which speakers can be claimed to be only partially aware at best. If this is true, it would suggest that linguistics is an empirical rather than a conceptual form of inquiry, in the sense that it discovers new facts rather than providing reminders. And we might reasonably ask whether the same might not be said about much of psychology and social psychology, economics, and other fields of social science.

Of course, Hutchinson et al may deny that Chomsky's account of transformational grammar is necessary to account for speakers' differentiations between grammatical and ungrammatical phrases; and/or they may dismiss it on the grounds that what is being accounted for – the judgments of an ideal speaker-hearer – is a false abstraction from

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5 Hutchinson et al note that the distinction between conceptual and empirical matters is not hard and fast. They write: '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 [...]' (p10).

actual language use. In effect, what is involved here is a denial of the possibility of propositional knowledge about 'knowledge how'. But the implications of taking this line are very substantial, probably ruling out not just most of linguistics and a great deal of social science but also substantial portions of the humanities, and here not just much of philosophy but also historical work that goes beyond relatively straightforward explication and narrative, for example in seeking to produce definitive explanations for particular events, for the consequences of events or ideas, etc. It also renders speculative, or simply instrumental, much of what lay people claim to know about social life. All this is not, of course, in itself an argument that this position is false, but these implications should give pause to anyone planning to adopt it.

As regards the final contrast, between concepts governed by agreed criteria and those whose meaning is uncertain, my main point would be that what is involved is a dimension rather than a dichotomy. Actually, what is involved is even more complex. What needs to be said, I suggest, is that the meaning of particular words not only varies somewhat across contexts, but the degree to which there is agreement in meaning among users of them also varies in this way. In part this reflects the fact that some practices involve sustained attempts to fix the meaning of key terms. Examples here would not only be particular sciences but also law and medicine and other professions. It may be true that there are some essentially contested concepts, in the sense of concepts whose meaning is rarely if ever a matter of agreement in any context, but I think the term 'essentially' here is misleading here, in that it fails to take account of the context-sensitivity of utterance and word meaning. However, none of this fully engages with the point that Hutchinson et al are making.

This final contrast is, I think, the main one for them, and indeed for Winch, and it relates to what is taken to be a distinctive feature of social as against natural science: that in describing and explaining people's actions we cannot avoid referring to their intentions, motives, reasons, perceptions, etc. It is through these that actions are categorised as being of one kind rather than another. And their key point is that these categorisations are context-sensitive, in a way that resists identification in the manner that is required by science. This is, of course, a problem with which social scientists of a positivist persuasion have long been concerned, but their attempted solutions, whether treating participant meanings as mere epiphenomena or seeking to translate them into behavioural indicators, have not been successful. It is also a problem that is at the core of ethnomethodology, where it is treated as irremediable in any general fashion.

It is important to note, however, that this conclusion relies on a particular conception of what is required for scientific inquiry. While not wishing to deny that there are problems about this, there is a danger of adopting excessive requirements of fixity and specificity of meaning. All that is necessary is that concepts can be applied with a level of agreement, in the Wittgensteinian sense, that is adequate for the purposes of social inquiry (see Kaplan 1964). It is also important to take account of the different sorts of concept that social scientists use, and as part of this to examine various conceptions of the nature of concept formation in the social sciences. These include the notion of ideal types, and in particular perhaps Schutz's attempt to show how these can be grounded in the typifications that we all use in everyday life. The critical point here is to see social science as no more than a refinement and development of practices in which everyone engages, analogous to other specialist activities such as the various professions, some of which themselves involve distinct forms of inquiry. They too must face the problem of vague and fluid meanings, and resolve it more or less satisfactorily for their practical purposes. Whether or not the social sciences are able to do this seems to be a matter that must be judged in terms of their requirements, rather than by the sort of abstract philosophical argument Hutchinson et al employ.

There are three issues here: what the aim of social science is; what requirements are laid down by this goal; and whether they can be satisfied, at least approximately. Within contemporary social science, all three are, of course, matters of disagreement and dispute. However, we should not move immediately from this to the conclusion that there is no viable enterprise here. Much depends on what one believes that social science has managed to achieve, and how this is to be judged. But this is not a philosophical matter, in the sense of that term employed by Hutchinson et al.

The other element of the argument these authors put forward to support the claim that much social inquiry is conceptual rather than empirical, in the third sense I have identified, is ontological in character. They use the fact that much social behaviour amounts to rule following as a basis for this. They write:

> With respect to something like a rule […] the rule and the action which follows the rule cannot be identified independently of each other, since the rule is a rule prescribing the action and the action is properly identified as an action according to the rule—it is not an *empirical* question as to which kind of action follows from obedience to a rule since the identity of the action type is given in the sense of the rule. I.e. understanding a rule is in many cases knowing what to do—'driving on the left' is the action that corresponds to the rule 'drive on the left'. It may be an empirical question as to whether one can find an instance of someone obeying that rule and performing the action, but the connection between the rule and the action comes from *understanding the rule* not relating empirical occurrences. (p54)

This is a point that would have been accepted by Weber, not just Winch, I suspect. But he emphasised that whether or not the action implied by espousal of some norm or value actually occurs is an empirical matter. While Hutchinson et al accept this, they might not accept the methodological conclusion that Weber draws from it: the need to produce explanations that are causally adequate for both compliance and deviance. For him, the relations among the components of an ideal type are conventional or 'logical', and in that sense internal, but he insisted that in employing ideal types we must document the degree to which action corresponds to the patterns they identify; and, where there is deviation, investigate why. So, an ideal type simply tells us one pattern that could reasonably be expected to occur. Hutchinson et al seem to suggest that only one description of an action will apply in any particular case, and that this will correspond to the rule being followed, or perhaps to the intentions and reasons of the actors involved. However, there is a question about the status of rules, intentions, and reasons: whether these are simply what the actors say they are, or whether they are what an analyst can infer from the activities in which the actors are engaged. I do not believe that NST is proposing that we must simply accept what particular people say they are doing; rather, they see the meanings involved as publicly available in some sense. In line with ethnomethodology they would presumably reject the 'glosses' that people provide as descriptions, in favour of the researcher observing their activities and producing her or his own (Psathas 1999).

In the quote above, the authors concede that whether or not people obey a rule is an empirical matter, and this allows that there is territory for an empirical science to investigate, even if the particular example of a rule they use – 'keep on the left of the road' – does not promise conclusions that are likely to seem very interesting from the point of view of policymaking or most kinds of practice.[6] But, of course, obeying a rule like 'drive on the left' is by no means typical of the variety of forms that human social action can take. That rule involves a relatively simple and concrete specification of what obeying the rule would mean, even though it is not *fully* specified (nothing can be). Other rules are much less closely specified. For instance, the performative utterance of promising is interesting because there are both institutionalised forms, as in the wedding ceremony, as well as more informal versions; and in the case of the latter there is more scope for disagreement about whether or not a promise was made, and about what would count as having honoured it.

Hutchinson et al also use another line of argument to make their point about the conceptual rather than empirical character of any identification of action meanings. They draw a contrast between human actions and physical phenomena as follows:

> 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. [Hence these are empirical matters.] 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. (p15)

There are a couple of questions I want to raise about this. First, as regards Austin's (1962: 114–115) pig, it is worth noting that there are many varieties of pig, not all of which may be immediately recognisable to us. His point, I think, was that faced with the

[6] Some closely related issues, such as 'why do people sometimes jump red traffic lights', are likely to be more appealing.

type of pig with which we are most familiar we do not need evidence that it is a pig, we simply see it is. But there is a gradient away from this, in several dimensions, corresponding to types of feature (coat, shape of snout, etc), along each of which we will eventually find more problematic cases. Moreover, there are issues about the boundaries around what is included in the category: a Guinea Pig is neither a pig, biologically speaking, nor from Guinea! In the discipline of biology, 'pig' is a species category whose defining features are probabilistically associated rather than categorical. Appealing to another favourite philosophical creature, we can ask: Is it a swan even though it is black? Or, to dredge up a memory from childhood, is an ugly duckling a swan? What these questions point to, in part, are disparities between lay people's application of species terms and those of biologists. For these reasons, I do not believe that the distinction that Hutchinson et al are making between judging and recognising is as sharp as claimed. Indeed, Austin recognised the fallibility of our perceptions, just as much as he insisted on the fact that in many circumstances there are no reasonable grounds for doubting their soundness.

Secondly, the authors of NST are at pains to insist that most of the time as members we recognise other people's actions without engaging in interpretation; in other words, we recognise rather than judge or interpret. It might be said that we recognise (rather than judge) them to much the same extent as we generally recognise pigs and swans at sight; despite the fact that there can be problematic cases in both realms. So, for example, much of the time we can recognise whether or not people are following particular rules. If so, then, any contrast between physical and social phenomena is weakened from the other side as well.

Presumably, the crux of the authors' argument here is that accounts of intentions and reasons, unlike pigs and swans, are occasion-sensitive; or, switching to the language of ethnomethodology, they are indexical. But, even if this is conceded – and I am not entirely sure about this – my response would be that social scientists necessarily rely on their ordinary capacity to make sense of people's actions (and many sociologists acknowledge this), and that there is no more of a general requirement for this capacity to be explicated if they are to do their work well than there is in the case of lay people's making sense of one another for practical purposes. A need to check that the meanings concerned have been grasped correctly only arises when there is some problem, or where there is reasonable suspicion of error.

However, perhaps what is at issue for Hutchinson et al is the relationship between the everyday concepts we use to make sense of people's behaviour and social scientific categories? But, as noted earlier, I think there are great dangers here in adopting the dichotomy between professional and lay reasoning, as if what is involved on the two sides were completely different. Indeed it is important to note that there is much heterogeneity on both sides of this line, but especially under the heading of 'lay'. The meaning of the term 'lay' is, of course, always relative to some particular profession. Under the heading of 'ordinary or lay activity' as contrasted with 'social researcher' is included, for example, legal investigations and processes, social work inquiries and decisions, the reportage of journalists, as well as many other specialised activities. And it is quite unclear (to me) why these should be seen as ordinary, while social science is treated as extraordinary. The fact that some scientistic social scientists overinterpret such a distinction is no grounds for adopting their exaggerated version of it. Moreover, any issue of the relationship between 'everyday' and specialised concepts arises in some of these other fields as well, not just in social science. And, once again, how satisfactorily any problems with this relationship can be resolved is a matter for judgment *within the specialised practice*, not a matter on which philosophers can legitimately legislate.[7]

Of course, one of the targets that Hutchinson et al have in their sights are those social scientific approaches to explaining human actions which specifically reject everyday meanings, for example dismissing them as ideological, or treating them as epiphenomenal products of real underlying causes. But their objection here seems to be to such explanations being applied in a general way *to all actions*, not to such explanations in themselves. Indeed, I think they acknowledge that this type of explanation may be relevant on specific occasions. And many social scientists take the same view, on the grounds that there is a primary requirement that the intentions and understandings of actors be understood before the development of any social scientific explanation for their actions is developed. It is only if these do not seem to account for what was done that one must turn to a different explanation.

In this respect, and others, there is certainly a need for some social scientists' bloated claims to expertise to be challenged, but I do not believe that this requires declaring their field of investigation to be largely non-empirical and denying that they have *any* expertise. Lawyers, police officers, journalists, and others are all engaged in empirical inquiries into social phenomena, of one sort or another, and they can all legitimately claim forms of expertise in relation to these. Why is the same not true of social science?

Emphasis on methodology

Current public rationales for the value of social science usually place emphasis on its possession of *methods* that, so the claim goes, can produce more reliable evidence than that available from other sources.[8] This emphasis on methodology is also reflected in campaigns for improved training of social scientists; especially in quantitative methods, though also in qualitative methods – indeed, even in conversation analysis. This emphasis on methodology has produced a boom in 'research training' courses at postgraduate level, and there has also been an exponential increase in social research methods texts over

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[7] Which is not to say that they cannot make a useful contribution. Indeed, this was a topic that Schutz (1962, 1967) devoted some attention to, very helpfully in my view.

[8] Another common rationale for the superiority of social science evidence is an appeal to social theory as a source of systematic insight. However, this has come under sustained criticism, and it seems to have little rhetorical value in the public sphere today, probably because 'theory' is seen as very close to 'ideology', when it is not dismissed as useless.

the past several decades (see Hammersley 2015). The desirability of this is certainly open to question.

Courses and books on research methodology contain various sorts of content, including what might be called methodology-as-technique and methodology-as-philosophy. And some of what falls under each of these headings is open to criticism. The first frequently seems intended to proceduralise social science practice, whereas much of the second consists of misguided and misleading philosophising. At the same time, I am not convinced that either the appeal to social science methods, or the claim that these are more likely to produce sound knowledge than alternatives, is entirely spurious. Indeed, I believe that a good case can be made that many social scientists give *too little* attention to methodological issues. The predominant view seems to be that once one has learned 'the methods' one does not need to give much attention to methodology thereafter (see Hammersley 2011:ch1). In my view, the result, very often, is shoddy work.

NST challenges the very idea that methods are required to understand social actions, though it is noticeable that the term 'method' is sometimes used to refer to Wittgenstein's approach to doing philosophy (Hutchinson et al 2008:71-3; see also Baker 2004). This points to the need to clarify what the word 'method' is intended to mean. Part of NST's argument relates to the normally unproblematic character of our understanding of other people, and I will leave discussion of that to a later section. But it also seems to be implied that inquiry, when this is genuinely required, cannot be pursued effectively by following some single, standard procedure, that what is required is a more attentive and thoughtful approach, one that takes account of the distinctive features of the problem being addressed. I think this is correct, but it clearly does not rule out all talk of methods, or their value.

It is also worth noting that it is not usually claimed by social scientists that the methods they employ are completely different from those used by lay people. As with other specialist activities, the techniques involved are refinements and developments of practices that are more widely used in society. But this does not count against the idea that there is a need for social scientists to develop their skills in these techniques in order to do their work well.

Reliance on the logic of the experiment

Besides displaying reservations about the notion of method and methodology in general terms, NST also seems to challenge a specific mode of inquiry that is taken to be central to social science: the logic of the experiment (Hutchinson et al 2008:Intro and 120).[9] I noted earlier the recent influence on social science of the evidence-based practice movement, which treats RCTs as superior to non-experimental methods, and as more or less guaranteeing sound conclusions about 'what works' in terms of policy and practice. This is a kind of social science that, I take it, NST would reject on the grounds that it involves methodological reductionism as well as misconceptions about the nature of human actions: treating them as akin to physical processes.[10] Hutchinson et al recognise that only a relatively small proportion of social science uses experimental method, but they claim that a great deal of it relies on more or less the same 'logic'.

It is certainly true that, though non-experimental, much survey research depends on the idea that if cases can be compared that involve different values on the variable(s) comprising the hypothesis being tested, and if this can be done in samples of cases where potentially confounding variables are more or less constant, then sound conclusions can be reached about whether or not the causal hypothesis is true. There is a further parallel with experimental method in that survey analysis generally involves measurement of variables, and statistical theory is used to allow for random error.

The parallel with the logic of the experiment is, of course, more uncertain in the case of qualitative research. Some of this research involves the comparative method, which can be seen as another name for the logic of the experiment, but much of it does not. However, it is arguable that even this is still concerned with reaching causal conclusions, despite denials: it searches for patterns (in effect, correlations) and sequences (in the manner of what is sometimes referred to as process tracing), and also relies on participants' own accounts of what caused, or causes, what. In many respects there is a parallel here with much historical work, in which there is an attempt to identify causal patterns by considering counterfactual conditionals, but in social science there is more scope for the comparative method.[11]

So, there are grounds for arguing that 'the logic of the experiment', in the form of comparative analysis, does inform much social science. Hutchinson et al's challenge to the appropriateness of this approach is based on the argument that, generally speaking, causal analysis is not required in understanding people's behaviour: that this generally occurs instead by grasping their reasons. I will examine this argument in the next section. However, it is worth pointing out here that the comparative method can be applied even

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[9] There is a complication here that I do not believe is significant for the argument but which ought to be mentioned. Winch (1958) takes Mill's (1843/1974) discussion as exemplifying this conception of what social science involves. At the same time, quite rightly, he recognises that Mill did not believe that his canons of causal analysis, designed to discover empirical laws, could be applied in social science, on the grounds that there are too many variables. Instead he recommended a deductive method. However, few social scientists have followed this recommendation.

[10] However, we should note that advocates of RCTs often play down the role of theory, which is a major target of criticism for NST (see Chalmers 2003; and Hammersley 2005).

[11] On the use of the comparative method in history, see for example Sewell 1967.

when the focus of inquiry is on determining what is a person's intention (especially, but not only, where this is disguised), or what the function of an utterance is or is taken to be. An interesting example is the 'next turn proof' and the study of 'deviant cases' in Conversation Analysis (Schegloff 1968; Sacks et al 1974:729; Hutchby and Wooffitt 2008:14-15 and 90-1). Indeed, it seems to me that most people use the comparative method informally in everyday life, when faced with puzzling phenomena.

So comparative method is not restricted to causal analysis, in the narrow sense of that phrase, to natural science, or even to social science. Furthermore, it is not tied to the goal of discovering scientific laws, in the way that Hutchinson et al seem to assume in their references to the covering law model (see, for example, Hutchinson et al 2008:7). It can be focused instead on detecting singular causal relationships occurring in particular contexts. On my reading, this was Weber's view of the task of social science, contrary to Winch's claim that he was committed to the discovery of statistical laws (Winch 1958:113; Heidelberger 2010; Hammersley 2014c).[12]

Reasons and context-sensitive understanding of human actions

Perhaps the core argument in NST is that we all engage routinely in understanding human actions in everyday life, for the most part without experiencing any difficulties, or finding any need to engage in 'interpretation', to draw on sociological theory, or to deploy 'the logic of the experiment'. In other words, most of the time we see, or know immediately, what people are doing and why. Moreover, in explaining what they are doing we appeal to the reasons they have for doing it, reasons that are intrinsic to the activities in which they engage. Hutchinson et al write that Winch had:

> no theory of how society is, of how humans are, nor even of how sociologists ought to conduct themselves. He investigated the concept of 'society', the concept of 'social science', and the concept of 'philosophy', and found that certain oft-made methodological and philosophical 'mistakes' were less likely if one attended to the results of such investigations, which were, after all, intended to put us back in touch with things which in our everyday practice we all of us already understand perfectly well. (p57)

I take it that what is involved here is not a denial of the relevance of causal explanations, but rather an insistence that explanation of any kind is only called for when there is a recognised deficiency in our immediate understanding of particular actions on particular occasions, for example when someone does something we did not expect, or when we can see no reason for what they have done. What is opposed is the idea that there is any room for general explanations of human behaviour, precisely because there

[12] My view is that more creative and sustained use of the comparative method could improve both quantitative and qualitative analysis (see Cooper et al 2012).

is no general problem in understanding what other people do. So, much of the time there is no need for explanation; and, where there *is* uncertainty or puzzlement, the first task is to look for reasons. There is only a need for *causal* explanations for human actions when explanations in terms of reasons have failed. Even then, those explanations are specific to the circumstances, they are not of general application.

I have sympathy with much of this, but there are a few points I would want to make. First, much social science is, in fact, concerned with explaining what is, in one way or another, untoward. NST denies this:

> Sociologists just don't have genuine empirical problems—in the way that Keynes at least had the occurrence of the business cycle to explain—of the sort that would motivate a genuinely explanatory venture on their part. Their 'problems' are mostly artefacts of the prior possession of their theories (or as much 'sociological' research is, are addressed to administrative, quasi-administrative or frankly political problems—is there an 'underclass', what stops people rising up in revolt, etc.). Their theories do not originate as genuine responses to things that puzzle us. (p37)

The authors refer to only a few kinds of social science explicitly: social theory, as in the work of Giddens, Habermas, and Bourdieu, along with discussions of the structure-agency issue; Goffman's metaphors and elaborations; and anthropological studies of communities, notably Evans-Pritchard's *Witchcraft Among the Azande*. But a great deal of social science research is concerned with describing and explaining how particular social problems arise, why they persist, how they have changed, etc, as well as with investigating policies and practices associated with them.[13] In this work, most of the time, social scientists are not inventing problems: the problems they investigate are ones that ordinary people are only too well aware of, and often have their own theories or explanations for, but disagree about these.[14] Indeed, it is lay disagreement that prompts much social research. Furthermore, any explanation produced will be developed within a value-relevance framework, whose constituent value judgments may not be accepted by everyone.[15] However, the disagreements are often generated not just by different value priorities but also by conflicting factual assumptions. And the task of social science is to resolve such conflicts.

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[13] While such work often appeals to the work of social theorists, this is done largely for purposes of what we might call theoretical orientation.

[14] Hutchinson et al seem to recognise this, referring to 'administrative, quasi-administrative or frankly political problems—is there an 'underclass', what stops people rising up in revolt, etc.' (p37), but seem to reject these problems as not genuine. Of course, they could argue that lay concern with such issues indicates the infection of commonsense by social science, but evidence would be required for this claim, and we should note that it takes the form of a general explanation.

[15] This is true, for example, of studies of social mobility: see Hammersley 2014c:ch5.

The second point is that while social scientists sometimes look at these problems, institutions, or processes from an angle that is not commonplace, there can be justification for this. Such different perspectives may provide fresh understanding of what is involved whose value is recognised by at least some of the people who are practically implicated in it. This relates to a broader point: that, in the language of phenomenology, any understanding of a person or situation has an inner and outer horizon, and under the operation of various sorts of relevance system these may be explored (Schutz 1970). Changes in what is treated as relevant may be 'imposed' by circumstances or may arise from motivational shifts. We can also note that distinctive relevance structures are built into particular activities. In these terms, it could be argued that it is not surprising that the perspectives adopted by social scientists are different from those of the people they study, in some respects.

That said, I do think that there are often grounds for questioning the ways in which social science perspectives, or 'theories', are put forward: in that they are often advocated as if they were the only correct view, all others, especially more commonplace ones, being ideological (see, for examples, Hammersley 2014:ch6). In my view this is unjustifiable. It could also be argued that social science perspectives are often presented as, or serve as grounds for, the kind of policies and practices that would come under Oakeshott's (1962) label of 'rationalism', according to which established practices and institutions must be completely abandoned and replaced with some new alternative. In my view, there are good practical reasons to reject this too. If rejection of this kind of rationalism is implied by Winch's position then it does amount to conservatism (with a small 'c'), but there is nothing wrong with this.[16]

A third point, closely related, is that it seems to me that ethnomethodology, which NST presents as largely compatible with Winch's orientation, requires the analyst to adopt a very different perspective on mundane social processes from that of people participating in those processes, namely one concerned with how intelligible order is produced. In our everyday life, most of the time, we do not pay direct attention to how we are doing what we are doing, in the manner required by ethnomethodological investigation, even less do we write detailed accounts of the practices and competences involved. This is a pertinent illustration of the fact that non-standard perspectives can be fruitful.

The final point is that if we apply this argument about the purpose- and context-sensitive nature of all accounts to Winch's critique of Evans-Pritchard's analysis it generates some questions. Surely one of the implications of the local, situated character of accounts is that what is seen as needing to be explained will partly rely on what is taken by the

[16] Almost all discussion of the relationship between research and policymaking assumes that it is always a good thing for the former to contribute to the latter. Long forgotten here, it seems, is the view that was predominant among my generation in the late 1960s and early 70s, according to which the modern state is, to put it crudely, part of the problem rather than part of the solution. Interestingly, that view can be supported not just by Marxism and Leftist anarchism, but also by neoliberalism.

analyst to be existing knowledge. Thus, the reason why Evans-Pritchard thought it necessary to explain the witchcraft beliefs of the Azande was precisely because he held them to be, and knew most of his readers would believe them to be, false. If they had been unproblematic from this point of view he may have felt no need to try to explain them, and in these terms perhaps no need even to describe them. Winch is suggesting that Evans-Pritchard should have suspended his judgment about the validity of Zande beliefs, on the grounds that this could facilitate deeper understanding of those beliefs and of Zande society. Here, once again, the new approach being suggested seems to be fruitful.

What Hutchinson et al oppose is the idea that social science can provide general causal explanations that should replace, because truer, those of participants; the latter being viewed as mere rationalisations. That it is a common assumption on the part of social scientists that their work can do this is true, even if it is usually selectively applied: only *some* people's accounts are challenged or dismissed. The authors' rejection of this approach is surely correct; though it is at least partially in line with those versions of social science that emphasise the need to grasp 'the native point of view' (Malinowski 1922), the importance of starting from 'subjective meanings' (for example Austrian economics), or the requirement to adopt an 'appreciative' rather than a 'correctionalist' stance (Matza 1969). As this suggests, there is a considerable body of social scientific work that is not open to this criticism.

Aside from this, some clarification may be required regarding what terms like 'general' and 'theory' mean in this context. While there may be social scientists who aim to discover universal laws, and there are certainly quite a few concerned with resolving the structure-agency problem, most social scientific work has much more specific foci: on the nature of currently occurring patterns of behaviour, or even behaviour on particular occasions, and its explanation. What is being studied may be general in the sense that it relates to patterns that occur across a particular population or category of people, but it usually involves some time-place restriction, implicit or explicit.[17] Once again, what is practised here parallels what historians do. Furthermore, we should note that by their very nature policies are general in this sense (they consist of instructions to treat particular types of case in particular ways, until further notice), so that if social science is to provide evidence that will be useful in evaluating policies, this focus on aggregates is probably unavoidable. But, in any case, what are produced in the way of conclusions are very different from theories in the sense of claims that, given certain conditions, some type of outcome will always occur. While many social scientists have claimed to produce theories of this kind, in my view they have failed to establish their validity, and this signals something important about the nature of the social world.[18]

[17] An example would be sociological research on social mobility (see Hammersley 2014c:ch5).

[18] In the past, I believed that there were some examples of theories of this kind (Hammersley 1985), but I have subsequently come to the conclusion that they constitute rational or ideal-typical models that have interpretive adequacy, in Weber's terms, but whose causal adequacy in explaining what is going on in particular situations always requires investigation (Hammersley 2014c).

On the nature of the critique

In moving towards a conclusion, I want to make a few observations about the nature of NST as a critique of social science. A negative way of characterising it would be to say that it amounts to philosophical imperialism. For example, at one point the authors declare that:

> […] Winch's title is best unpacked as "On the Very Idea of 'Social Science', on how philosophy can dissolve it, and on how philosophy can do this in part by taking back to itself what was stolen by 'social scientists'". Winch is not trying to put 'social science' right, but to say that the *whole idea* is wrong-headed. Hence, no "Here's how to do (and not to do) social science aright", nor "here's a better method for social scientists". Winch is pressing questions on would-be social scientists as much as making proposals to them: "*What* are you trying to do? What genuine *empirical* problems are you trying to solve? Is there any clear idea of this? How does the idea that a 'social *science*' is needed get a hold in the first place?"(p31)

Of course, what is involved here is not as simple as one discipline claiming territory from another: Hutchinson et al present themselves as attacking bad philosophy that has infected social science, and they do allow (rather grudgingly) that there may be some scope for empirical social inquiry. They comment that there are:

> genuinely empirical and or fully political 'policy studies'. […] In brief: Some of social science is harmless quasi-bureaucratic local 'policy studies'- type work. e.g. What proportion of the population have home access to inside toilets?[19]

[19] They write that: 'the vast majority of "human science"' is 'misbegotten epistemology and metaphysics' (p37). And they illustrate this with the following examples of the questions it addresses: '"What is the structure of Modern society?", or "Does 'society' really exist?", or "Who *really* holds power?", "How obedient are human beings?", even "What is human nature?" These questions are —where they are not just matters of common sense—philosophical questions, at best, Winch suggests. Social theorists want to choose how to live, and to understand what it makes sense to say … in short, to do philosophy (including here ethics and political philosophy), *by other means*—but the means are singularly ill-chosen' (p37). I do not believe that this is an accurate depiction of the issues that most social research addresses. These formulations of questions are much more abstract than the investigations normally carried out: the latter are more likely to be concerned with, say, the social class structure of modern Western societies, or who holds what sorts of power in a particular society at a particular time. What is true is that some social scientists have a tendency to dress up their studies in terms of a more abstract and general focus than these warrant, as regards what is actually investigated. But we should take them at their deed, not their word.

However, we can draw an interesting contrast between the attitude of NST towards ordinary practices, even the strange practices of other cultures, which is to treat them respectfully, and their attitude towards social science practice, which is dismissed as 'wrong-headed'. In these terms, what is offered is not an internal critique but an external one: after all, most social scientists do not see themselves as practising philosophy, and certainly not of the kind to which Hutchinson et al are committed. And, as such, it seems to be at odds with Winch's (1989:chs 2 and 3) rejection of the very possibility of external critique, and with his treatment of Azande magic (Winch 1972:ch2); though he too may be open to charges of 'philosophical imperialism' in some parts of *The Idea of a Social Science*. An interesting contrast here is the work of Greiffenhagen et al (2011 and 2015), despite the overlap in authorship with NST. Here the approach is to describe what social scientists actually do, the problems they encounter, and how they resolve them. Frustratingly (for me), while there is a hint in these two articles of internal criticism of the practices described, the authors do not develop this explicitly into a methodological assessment. In my view, the appropriate form of critique of social science lies somewhere between the approaches of NST and that of Greiffenhagen et al.

My point here comes down to this. It seems to me that in order to try to find out what is wrong with social science it is necessary to start from what social scientists do and why they do it, including what problems they face. If therapy is being offered, those offered it must surely be brought to recognise that they are in need of it, and this requires starting from how they understand themselves. This does not require that their self-understandings be simply accepted, only that grasping these accurately is an essential starting point. While I believe that this recommendation is very much in line with the position of Winch and Hutchinson et al, it is not for the most part what they do in approaching social science. Of course, what prompts their critique are the imperialistic claims of much social science in relation to common-sense knowledge and understandings, and perhaps also towards philosophy. But this does not justify their approach: 'two wrongs make a right' is hardly in the spirit of Winch's ethics. Furthermore, one result is that the critique is simply ignored or rejected by most social scientists. This is unfortunate because they could have much to learn from the work of Winch and from NST.

Conclusion

My interest in this paper has been to consider *There is No Such Thing as a Social Science* (Hutchinson et al 2008) against the background of current debates about the role of social science evidence in relation to public policymaking, and practical decision-making more generally. As should be clear, I agree with several of the arguments put forward in this book, but I regard the radical conclusion reached by the authors as false. If what is meant by the word 'science' in the label 'social science' is 'inquiry modelled on physics', then the proposition that the title of their book puts forward is true enough. But I can see no good reason for adopting such a narrow definition of 'science', and it is not one that guides most social science today.

That there is much wrong with social science is certainly true, but in my view the problems are less fundamental, if no less serious, than is suggested by NST. While social science has limits that need to be recognised, ones that currently go unobserved much of the time, there *is* such a thing as social science (on a more reasonable interpretation of what that term means), and it can produce evidence that may be of use in practical decision-making, even if it cannot do this in the dramatic, direct, and determining way that is widely assumed.

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