Chapter Three

Seeing Things for Themselves: Winch, Ethnography, Ethnomethodology and Social Studies

The Enchantment of Theory

In previous chapters we have frequently referred in passing to points of convergence between aspects of what Winch has to say about social studies and what has been written by ethnomethodologists. In this chapter we seek to conduct a more extended examination of these points of convergence, without underplaying divergences. In pursuing this task we shall also give some attention to other classic and prominent ethnographers, such as Erving Goffman. Our central claim will be that the social studies are, even in the work of the great ethnographers and some ethnomethodologists, too entranced by theory. For sure, ethnomethodology is often closest to being the sort of enquiry into social life which is least exposed to Winch's criticisms. For, while much social studies is resolutely theoretical, operating seemingly according to the principle that every perceived problem of social explanation requires a theory in order to explain away the perceived problem, ethnomethodology proceeds in such a way as to question whether the perceived (perceived by professional sociologists not their fellow members of the society) problems are indeed problems at all.

Resisting Theory's Spell

In the contemporary intellectual culture of the social studies, one of the most difficult challenges is to see things for themselves, to accept the validity and priority that attach to Wittgenstein's injunction 'Don't think – look!' and/or to phenomenology's 'Back to the things themselves!' In a theory-infatuated age that supports many academic factories it is near impossible to have it recognised that this is practically possible, let alone that it *needs to* be done.

The idea that perception is theory laden[1] is now very deeply entrenched and underpins an enormous range of otherwise very diverse points of view—it is thus supposed that it is impossible to even recognise anything save through some theory, therefore even those who unregenerately insist that they have no theory nonetheless *must* have one—they cannot avoid presupposing a theory, regardless of what they say. The idea that there could be resistance to theory thus becomes a nonsense, and if one denies having a

[1] But doesn't Wittgenstein himself believe in the theory-ladenness of perception? Isn't that what his famous writings on aspect-perception, e.g. in section xi of Part II of the *Investigations*, are about? No. For a proper understanding of Wittgenstein on aspect-perception, see Guetti (1993) "Idling Rules", and (especially) Avner Baz's (2000) marvellous work critiquing Stephen Mulhall and others on Wittgenstein on seeing aspects. See also Baker (2004) chs. 1 & 13. Kuhn at his best is compatible with Wittgenstein's non-theoreticistic 'account' of aspect-perception—that is how Sharrock and Read recommend interpreting Kuhn in their (2002).

theory that can only mean that one is deluding oneself, and one's actual theory is implicit or tacit.[2]

Care is required in what one takes 'resistance to theory' to be. It does not arise from any generalised distaste for theory or science either—in natural science theories are both prominent and successful, even invaluable and unavoidable; however, the fact that they are does not validate the idea that they must be so in 'social science' too, for that is to presuppose that the problems in understanding our human neighbours are in akin to those of understanding the remotest parts of the universe, the domain of microparticles or the chemical workings of genetic mechanisms. Winch's (and ours) is not resistance to all attempts to put together theories, even in the social studies. It reflects rather a resistance to a prejudice, a prejudice toward theory or what Wittgenstein called 'a craving for generality'. Whether a theory is needed, whether it can play a useful part, and what it explains are not matters to be decided programmatically, certainly not to be answered in the affirmative as a basis for *setting up* a new discipline or approach on the assumption that the absence of theory is *prima facie* evidence of the need for one. All serious questions meaningfully arise only in specific contexts, and in relation to particular puzzlements; recognising this fact, there is absolutely no

[2] Such alleged implicit theories are often referred to as "tacit". See Nigel Pleasants (1999, chapter 4) for an excellent deflation of the appeal to tacit knowledge.

point in being against theory in a general way, but neither is there any in being 'for' theory in an equally general and *a priori* way.

It is the rather ubiquitous inclination to be 'for' theory in a general way that provokes Winch's 'resistance to theory', where the latter does not seek to exterminate all theorising but, rather, to break the spell that the idea that to have a theory is to have understanding, to mitigate the craving for generality. What is being advocated here is not a scepticism about theory but cautiousness, not merely about claims that theory is needed, but also about accepting that what is on offer as theory does play the role that it is advertised as doing. Breaking the spell of theory requires showing that there are other forms of understanding than the theoretical—unless one begins to redefine 'theory' in a bloated[3] fashion: i.e. so freely as to encompass all forms of understanding, whatever these are (but then there would no longer be disagreement, except about terminology, and thus about what important differences such redefinition might obscure). A cautious resistance to theory bids only to show that there are other forms of understanding than through a theory, and that where these other forms are in operation there is no need for theory, since the kind of understanding they give is different from the kind that theory can provide.

[3] For further explication of this notion of 'bloat', see Read (2000), and also his (2002).

It is not then generality that is the issue, but a certain attitude toward it—such that the understanding's satisfaction comes only from construction of a theory about that which one seeks to understand. One thing about the craving for generality is that in cases where theory is appropriate its returns do not come from merely accumulating thoughts that can be cast in the form of theories, but rather from the resolution of opaque puzzles. The craving for generality as such rarely issues in genuine explanatory theories at all—the main satisfaction seems to come from the righteousness of the conviction that, whatever the matter, there must be a theory that subsumes it—it is enough, that is, to make an *a priori* case that any phenomenon-in-question is amenable to theory. Indeed, Jurgen Habermas (1984 & 1987) for instance often proceeds seemingly on the assumption that any phenomenon-in-question *needs* a theory.[4] This is plain enough in the social studies, and in the debate that Winch initially continued, and which goes on still: is 'general theory' what is needed in the social studies?

The hold of the idea of theory encourages the high level of concern in the social studies (and, no doubt, across the social-studies-infected-humanities also) with *the form* of explanation. If there is only one proper form of understanding, then what form is that? What decides that something is an

[4] Indeed, Habermas is not alone. Contemporary philosophy of social science is dominated by those who take Habermas's attitude to such matters. Anthony Giddens would be one prominent example, Bourdieu another.

explanation is that it *has the form.* Deciding on the form, then, decides how things are *in general* to be explained, and it is this kind of generality that attaches to sociological schemes such as those of Giddens, Habermas, Bourdieu and other luminaries of Grand Social Theory; their sketches of social life are cast in the form that they have elected as *the* form of explanation. Naturally, in direct consequence, the disputes amongst them are over whether their own scheme gives the best form in which any phenomenon is to be explained. What, therefore, look like disputes over how *this* phenomenon is best explained will be revealed, on inspection, to be spats over how *any* phenomenon is rightly to be explained.

Despite the seeming self-evidence of these academic imperatives, Wittgensteinians and ethnomethodologists make the effort to break their hold over the life of thought, and, indeed, aim even to put them into full scale retreat, so pervasive and influential have they become—even though they seem so widely, deeply and unquestionably entrenched. The difficulty of the task cannot be under-estimated, since it involves breaking the spell that a self-reinforcing circle of reasoning can powerfully hold—it is *obvious* that theory is the thing, that thought and theorising are everywhere synonymous, and that there *just can't* be a plausible alternative to this (this is what we mean by 'prejudice'). Claims to be 'without theory' then can just be discounted in advance, without need to explore claims to an alternative, certainly without

need to examine them with care and in depth, for, as is plain, they cannot turn out to be true.

Scaling these battlements is perhaps near impossible—though one may hope that the modern academy may before too long be recognised as by now having become in this respect the equivalent of the Catholic Church in the 16th century—but there is nonetheless a need to put the alternative point of view on the record and keep it there.[5] It *is* possible to think differently than the orthodoxy imagines, but to do this calls for changes that, like turtles, go all the way down.

Wittgensteinians and ethnomethodologists have taken on this seemingly-futile task, in parallel much more than in common cause, though there have been attempts to bring them into closer alignment. This chapter will look at some of the features that make them seem rewardingly similar, but will also highlight the ways in which they are significantly—and at moments perhaps irreconcilably—different. The understanding of ethnomethodology that brings it closest to Winch's sociology is not one that would necessarily recommend itself to many ethnomethodologists, but that is precisely because of the above mentioned difference in understanding that keep them at arm's length.

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[5] As C. Wright Mills held, in the darkness of certain times the best one can realistically hope to do is to keep hope alive for the future.

If perception is theory laden, then so too must be description, which defuses the possibility of any alternative to the all consuming pre-eminence of theory being persuasively established by our advising of a relocation of the issues involved by placing description at the centre. If description is of necessity theory derived, then it cannot be meaningfully proposed that a concern *for* description could possibly displace the obsession with and obsessive production of theory. This, though, only reflects the circular character of the reasoning here—if we have *decided* that all description is of necessity theory-laden, then there is no further argument. It makes no sense to suggest that the issue of describing is a separate one from that of theorising, that discussing description could differ from, let alone replace, considerations of theory (save to the extent to which the salience of theory would be explicitly recognised). Such a disadvantage is ours. It has also always been such for ethnomethodologists and those who follow Winch.

Ethnomethodology's Program

Developed principally by Harold Garfinkel[6], ethnomethodology was much influenced by the phenomenological tradition, an influence exercised on Garfinkel especially through the work of Aaron Gurwitsch, Alfred Schutz and

[6] Garfinkel's *Studies in Ethnomethodology* is rightly, in our view, considered a classic; though it is still too little-read, suffering a similar fate to that we are essaying herein to have been suffered by Winch's writings.

Maurice Merleau-Ponty. Following Schutz's reconstruction of the philosophical premises of Max Weber's conception of social action, ethnomethodology continues in the 'social action' tradition. A revaluation of the idea of sociology as the study of social action, when applied in the theoretical and methodological doctrines of modern sociology, leads to the conclusion that the character of actual, real world, practical action will invariably escape the theoretical construals and methodological applications of those doctrines, that the organisation of everyday social life is *presupposed* in the practice of professional sociological inquiry, rather than portrayed by it. Such presupposition results in a systematic underrating of the extent to which the supposed problems of professional sociology are already solved in society and are resolved in and through social practice[7]. For example, because of its generalising ambitions, professional sociology aspires toward the production of a systematic vocabulary for the description of social action, failing to incorporate the fact that standards of 'adequate description' for practical affairs are provided and deployed in the everyday practices of society. Ethnomethodology therefore emphasises the extent to which social action—*competent* practical action—involves 'mastery of natural language'. Professional sociology too relies on such mastery, which is why social order is

[7] See Linblom and Cohen (1979) for an account of the way such problems feature in the policy sciences.

presupposed in its professional practices, rather than reflectively considered by them. Competent participation in social life irreducibly requires the capacity to express, to describe and report, what others are actually doing as well as what oneself is up to. Such competence must not be thought of as a generalised one, for what are the appropriate, recognisable things to be doing on any occasion depends upon the social setting of one's activities, of the practices that one is involved in, meaning that the requirements for the correct description of actions are connected to the socially organised occasions on which those doings take place. Ethnomethodology's studies are very prominently exercises in giving perspicuous display to the way in which 'language use' and 'social organisation' are interwoven on occasions of 'social action', thus demonstrating that attempts to develop generalised portrayals of social organisation and practices will unavoidably run up against the fact that, in order to say, appositely, what people are doing, a professional sociologist will require not only a knowledge of the official vocabulary and procedures constitutive of sociological theories and methods, but, invariably, a 'vernacular' familiarity with the practice and occasions about which their official sociology purportedly speaks.

As can be seen from this brief sketch, there are strong parallels with Winch's insistence that professional sociological researchers do not stand in the relation of external scientific witness to those that they purportedly 'observe', but are, rather more like apprentices and collaborators,

appropriating their understandings from the members of the society at least as much, if not much more so, than from their formal sociological doctrines and procedures. The unreflective treatment of language-in-context, of language in its practical domains, is diagnosed, by both Winch and ethnomethodology, as causing considerable obscurity and confusion about the relationship between professional sociological discourse and 'vernacular' ways of speaking. In both cases, some of the cure is to be sought through perspicuous examples of the way in which the actual practices of language use in context are incongruous with the preconceptions that theorists would impose upon them.

Identity of Action

Colin Campbell (1996) is probably right to argue that identifying the subject matter of sociology as 'social action' is the recent and current orthodoxy, though in doing so he risks overstating the unity and coherence amongst 'social action' approaches. For many of those who adopt 'this' approach, the central issue remains that of explanation: either (a) how are we to explain social action (for example, is it determined by structures or by subjectivity?) and/or (b) how are we to explain the part social action plays in generating social structures (e.g. do actions generate structures or do they stand in a relation of duality with structures)? So while the disciplinary orthodoxy is, at least nominally, as Campbell describes it, among those that claim to work

within this orthodoxy there are still many who have yet to learn the true significance of the shift to social action. For it is not enough to merely talk of *social action*, as Giddens and Habermas do in their social theories. One has to have genuinely learned what the shift amounts to. Winch and Garfinkel's attempt to take the lesson that talk about 'social action' puts the issue of *the identification of social actions* (description) in prime position remains very much a minority and marginalised view.[8]

The simple point about 'social actions' is that the relevant criteria of identity belong to the social settings in which those actions occur, and are not contrived by or taken from the theories of social science (except in a secondary and derivative case). An action is such, as we have already stated

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[8] Authors such as Habermas and Bhaskar domesticate and thus marginalise Winch by depicting him as a hermeneutic stage—a mid twentieth century knee-jerk response to positivism—in the philosophy of social science, to be transcended by their own unifying critical social theories (again see Pleasants *op cit*). Similarly, text books on sociology might have a chapter devoted to ethnomethodology (sometimes lumped together with the Chicago school, Symbolic Interactionism, Goffman *et al*) which depicts it as just one of the many methodologies on offer to the student in the social studies: "try this one" might be the implicit message. Here 'methodologies' are lined-up like different brands of shampoo on the supermarket shelves. While this one is good for combating alopecia it doesn't give your hair the all-day shine and body-lift of the other brand. Ethnomethodology then is, if given credit at all, seen as responding to one or two concerns while being weak its ability to acknowledge and respond to others.

in this book, *only* under a description. And this should not be confused with the thought that there are two things, action on the one hand and description on the other. Clearly, an action and a description of an action are two analytically distinct things such that the action identified is the action described, but this is, in the present context, an irrelevance—the point is that a description of an action identifies that action as what it is. It only does so—only successfully, correctly, identifies that action—if the action is as described. The action *is* the action described, otherwise it is misdescribed, and it is in *this* sense that action and description are internally related: in this sense to see the action, to identify it, is to see it under a description: action and description are as one (though this is of course not to hold that the "*act of* describing the action" and the "action described" are the same). The correct ways of speaking of action derive from the practices in which the actor is engaged, the criteria for correct description being those that are applied by *competent* participants in the practice—which is one reason why ethnomethodology regards membership as a matter of competences, and its own exercise as a depiction of competences. If one is blind to the description of the action as would be understood by the competent actor—what the action is, given the social setting, given the actor's purpose—then one has simply failed to establish what they are doing. And unless one has done that, established what they are doing, then one is in no position to explain why they are doing what they are doing—where the other horn of the dilemma for social theory is that,

understanding *what* people are doing obviates the need for a *why* question, or put another way, means that the description of their action answers any *bona fide* why question; those *bona fide* 'why' questions are not the theorist's questions but those of e.g. beginners, learners, and strangers.

People—actors/members—don't merely make bodily movements in some extensionally-described type way which it is then down to sociologists (or psychologists) to render evaluative or intensional through some theoretical representation. People do things; very specific, variegated things. To ignore what it is they are doing, to simply set that aside in the name of one's theory, is to refrain from observing what they are doing. It is, in Harold Garfinkel's illustrative phrase, to tear down the walls to gain a better view of what is keeping the roof up.

Now, it is true that an action gives rise to a number of possible descriptive renderings and it is frustration at this point which sometimes leads to confusion. For example, compare Hillel Steiner's (1994) discussion of his going to see the auditorium performance of *Richard III*, which can be given various renderings. Steiner claims that there are numerous competing intensional descriptions of an act-token; he writes:

> An act-token is fully identified, then, by an *extensional* description of the action in question: a description indicating the physical components of that action. There cannot be more than one act-token (of a particular act-type) answering to the same extensional description, i.e. having the same set of physical components. Purely *intensional* descriptions of actions, by contrast, do cover more than one act-token. Such descriptions are couched in terms

> of the purpose or meaning attached by the actor (or others) to what he does: my attending *Richard III*, my running for a bus, my throwing a ball and so on. It's true of each of these descriptions that there are many events that would answer to it (Steiner 1994, 36).

This passage is an exemplar of a particular confusion, rather common confusion; it is a confusion that we dubbed, in our *Introduction*, the fallacy of extensional primacy. For Steiner, the only description which correctly picks-out the act-token in question—picks out the event—is the extensional description: a description which brackets-out—sets aside—(on Steiner's own admission) the purpose and meaning of the action, one that merely describes the actor's behaviour in terms of the physical components of the action. All other descriptions are intensional renderings and thus glosses on the action. But think about the opening sentence: "An act-token is fully identified, then, by an *extensional* description of the action in question: a description indicating the physical components of that action." First, how is an action "fully identified" if that 'identification' involves leaving out that which makes it an *action* as opposed to mere movement or behaviour? Extensional renderings of actions are not, we submit, descriptions of *actions*. Or, put another way, so we are not taken to be simply policing the meaning of the word "action", if we accede to Steiner's claim that an "act-token is fully identified, then, by an *extensional* description of the action in question" the words "action" and "movement" become synonymous, thus leaving us with a diminution in the

resources our language at present affords us, so that we can distinguish between actions people undertake and the movement of bodies. An act-token (to use Steiner's language) extensionally described does not actually identify the action that is supposedly being described but merely reports a physical state of affairs or process (we consider it misleading to even call it an 'event', as does Steiner).

The motivation for Steiner's position seems to be that a plurality of *possible* descriptions leads to each description being imprecise and thus open to contestation; that is, he sees 'numerous possible intensional descriptions' as equal to 'numerous competing candidate descriptions'. But it does not follow that different descriptions of an action are of necessity *competing* descriptions, as the whole idea of 'action under a description' that we have appealed to before is intended to explain. Equally, Steiner's account infringes the distinction between grasping a rule/seeing the action on the one hand and interpreting a rule/interpreting the action on the other that we discussed in chapter one. We suggest that Steiner is led to his position by assuming that all (intensional) descriptions are *interpretations* of pre-interpreted (extensionally-characterised) behaviour, as if the meaning of a piece of behaviour is projected onto that behaviour by observers: social scientists, and psychologists, (etc.) but such an understanding must presumably relegate the actors themselves to the status of observers of their own behaviour! What we have therefore, is the manifestation of a latent dualism in Steiner's thinking.

So, we wrote above, "Now, it is true that an action gives rise to a number of possible descriptive renderings and it is frustration at this point which sometimes leads to confusion." What we take the word "action" (and thus why some descriptions would be *renderings*) to denote in that sentence is evidently different to what philosophers (and social scientists) such as Steiner take it to denote. Steiner takes "action" at bedrock to be extensionally-described movement: described in terms of physical components, the actor and spatial and temporal location, only. While we, following Winch (and Frank Ebersole,[9] too), take action to be bedrock, and thus, at bedrock to be meaningful action: on that view, moving ones arm is an action, one's arm moving is a piece of behaviour. On Steiner's account then, any non-extensional description is a rendering. On our account only theoretical or interpretivist descriptions are best-termed as "renderings", and we include Steiner's extensional descriptions in this category of rendering-descriptions. On our argument, there is a description which *identifies* the act, which is not a rendering, which is not an interpretation, and this is an intensional description. Steiner makes an oft-made mistake; it is a mistake which has its roots in scientism. The mistake is to assume the priority of extensional description, to assume that only this form of description is not an

[9] See Ebersole's (2001) (excellent) "Where the Action is", Chapter 15 in his *Things We Know: Fifteen Essays in the Problem of Knowledg*e; also Chapter 6, "the Analysis of Human Actions", in his (2002) *Language and Perception: Essays in the Philosophy of Language.*

interpretation, to assume this is what actions are at bedrock. This is prejudice. This is to commit the fallacy of extensional primacy. Extensional descriptions are interpretations (just as much as Freudian explanation of an action in terms of the unconscious desires of the actor is an interpretation), to the extent that they are not identifications of the action but renderings of the action; a rendering of it exclusively into physical movement extended in space and time—and, of course, the prioritising of the extensional in this way is entirely notional, and no 'social scientist' is in any position to provide rigorously extensional descriptions of actions. To repeat, 'at bedrock' the action is a meaningful action and its description-which-is-not-a-rendering is the description which correctly identifies the action in terms of the social situation and the purposes of the actor.

Steiner might object that we are being unfair to him; he is not in the business of social explanation; he is not a sociologist but a political philosopher. However, the mythological mistake he has made is crucial. Steiner's reason for translating actions into extensional language is to resolve normative disputes over matters of distributive justice. In rendering the action as he does through describing nothing but the movement of the physical components he *fails* to describe *the action*. Thus, in employing such a tactic in the attempt to arrive at conclusions as to whether a given actor is free or unfree to undertake a particular action Steiner unwittingly cuts himself adrift from the very thing he is seeking to adjudicate on: the freedom or unfreedom

of an actor to carry out a particular action; for he has not uniquely identified the action but only rendered it.[10]

Winch and Garfinkel: Seeing an Action

The sociologist, qua sociologist of 'social action' is in no position to make an identification of any putative *explanandum* since the proper criteria for identifying the 'doing to be explained' do not belong to nor derive from theoretical schemes, sociological or other, but from the social settings within which the activities occur. This is not to say that sociologists *can't* make correct identifications of (some) social actions, but that their capacity to do this does not originate in their sociological training or distinctively professional expertise, but in their mastery of one or more of society's practices. It may come either from the diffuse and general familiarity that sociologists (as themselves members in the society they typically talk about) have with a range of everyday practices or it may derive from the 'anthropological' opportunities their professional role has provided to familiarise themselves with domains of expert technical practice they would not otherwise have come across in their own everyday experience (though these domains are those of *someone else's* everyday practice, and the sociologist

[10] How this plays out in Steiner's theory is that he is led to deny that threats are (in any way) restrictions on freedom. This will be seen to be pertinent to the arguments of the following chapter on conservatism.

researching them proceeds rather more like tourists than like any kind of natural science investigator). The identification of social actions is not an operation conducted by an observer using criteria independent of the occurrences being identified, but is a form of participation in the social setting to which the activity-in-question and the identification of it belong—at the very least, the sociologist is borrowing the criteria of the setting *if the identification of the action is indeed to successfully identify the occurrence-in-question as the action-it-performs.*

Again, there is no duality of action and description, for an action is the action it is correctly identified as being. To understand what an individual is doing is, by the same token, to understand a great deal about how the social setting featuring the doing actually works. It is to understand what is going on in and through the application of mastery, of being personally competent to apply what we are calling, for convenience, 'the criteria of identity'. One can say on one's own behalf what someone is doing because one can satisfy certain socially required conditions for making such decisions. These may include occupying certain entitling social positions, which explains why many of professional sociology's identifications are, of necessity, second-hand. For a simple, but resonant, example, the determination of cause of death as officially suicidal is within the power of an appointed officer, and sociologists, not being coroners, cannot themselves make competent counts of suicide rates but must, rather, *if they are to be able to talk about suicides at all*, be dependent

upon the determinations that coroners make for the location of instances of this activity. Further, as Harvey Sacks pointed out, the 'correctness' of an identification here is not a matter of evidentially satisfied criteria alone—perhaps even at all—…identification is a normative matter, and 'correct' identifications are ones which are 'proper' or 'appropriate'. Bare correctness of an identification does not ensure its relevance: any person can be correctly identified in an open ended variety of ways in this 'bare' sense, but whether they are *correctly* (in a real world, practice sense) identified is sensitive to many features of the practice within which the activity is situated and the role that the identification plays there.

The criteria of adequate identification are not those of sufficiency to get a sociological discussion going, but those which apply within a social setting, and, as the preceding brief deliberation has indicated, if one gets to the point of being able to make genuinely adequate designations of actions then one already knows a great deal about how the social setting, within which the action is a point of reference, works.[11] Such knowledge is practical, built into

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[11] There is an analogy here with the claim that Sharrock and Read (2002) made regarding the role of the philosopher or historian of science, in their *Kuhn*: throughout that book, and especially at its close, they argued that science is *difficult*, and that scientists' work cannot be gainsaid by the would-be normative intervention of philosophers etc. . In order to actually make a difference to the science, *one has to be in-principle-competent in the given scientific speciality in question*. Likewise: in order to actually make a difference to the area of society (e.g.

the mastery of whatever range of practices is required to make competent designations, and it is, as such, amenable to being taken for granted to the extent that it disappears from view.

This is why Winch and Garfinkel direct our attention to, respectively, reflection and explication rather than to 'empirical research' in the canonical forms of sociology. Garfinkel, it is true, is intensely interested in studies in a way that Winch was not, but it should be noted that Garfinkel does not attempt to (artificially) dignify these efforts by making them out as methodology, but emphasises their commonplace status as matters of taking a look at activities, hanging around with practitioners, training up in the activity, and treats them as a means of accessing what, for any experienced participant, will be apparent and transparent matters.

Both are concerned with what is 'built into' the capacity to make identifications ('identifications' *means* 'successful identifications') of activities

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the level of suicide therein) that one is investigating, *one has to be competent in the art of (e.g.) being a coroner*. It is not enough to be a good philosopher, sociologist or historian. One cannot intervene in the object of one's study (be that, the work of some community of scientists, or the work of some community of social practitioners, and *ethnomethodologists*), except by virtue of one's mastery of *their* practice. A given scientific discipline or speciality is advanced by competent and intelligently innovative science; likewise, knowledge of the level of suicide in one's society is advanced by competent and intelligently innovative work by coroners etc. . Not by sociological study that fails seriously to involve and refer to that work.

in which people are engaged, pointing towards what is involved in operating as a full-fledged participant in a practice. The concern leads in different directions—Winch's reflections focus upon what is 'built into' the diverse forms of language that we use in our affairs, the need carefully to differentiate one from the other, and to avoid being led into philosophical confusions by mistaking one form for another on the basis of superficial similarities—taking a linguistic stipulation for an empirical proposition, for example. For Garfinkel 'explication' is more a matter of giving attention to the extensive array of organisational and situational considerations that enable participants competently to 'speak definitely' of the passing scene of social affairs to which they are witness, to observe and report 'what is going on' before their eyes, to identify actions in the ways that they do. The repertoire of practical understandings (often, but sometimes confusingly, called 'common sense understandings') that is ubiquitously relied upon is illustrated by the activities that are reviewed, a tangible reminder that for *both* those being studied and those conducting the study, what Winch calls 'the intelligibility' of social life derives from the understandings indigenous to the social setting in question.

In other words, neither Winch's nor Garfinkel's investigations face the question that troubles so much of sociology: what empirical measures are adequate to establish the generality of connections between one aspect of social life and another? The connections that they are concerned with are

already present in the very materials themselves, they are ones that are formed in and through social activities, and the effort is in tracing how intelligible connections between one activity and another are mediated by the ways of—what are, for those engaged in them—familiar practices. In many respects, the generality of practice is antecedent to the identification of occurrent instances, for the performance of actions is as an application of a social form (what is said is said-with-words-from-the-language, an advancing of a pawn one space is a-move-in-the-game-of-chess not barely 'a game of chess'), and its intelligibility involves seeing that the performer is e.g. acting as anyone might in this context, acting-according-to-a-rule, is applying what is for all players a mandatory policy, is performing the usual courtesies etc. etc. *ad. inf.*

There is a comparable understanding between both Winch and Garfinkel that sociological thought is very much a second stor(e)y job, and that the exercise in which it is engaged—*as professional sociology*—is not description, but *redescription* (or representation rather than presentation).[12] Winch and Garfinkel concur that competence in social affairs involves the capacity to describe them, to say in ways intelligible/acceptable to fellow

[12] There is a sense in which this is overt and explicit in authors such as Habermas. Sociology's job is to *re*-present in such a way as to be liberationist or critical of current social practices, which prevent the realisation of the Enlightenment project. But as we've noted, unless one identifies the action (in its own terms) first one fails in any attempt at criticism.

competents, what is being done and is going on as an *otherwise* unremarkable part of participation in those affairs. Professional sociologists are utterly dependent upon the availability of those forms of descriptions if they are to have anything whatsoever to talk about. Garfinkel is at pains to highlight the numerous practical circumstances which may need to be attended to on any occasion in which someone is to say definitely what they or someone else is doing, and the whole of conversation analysis may equally be seen to detailing the circumstances which require a very specific form of words as a proper contribution to the use of language in ordinary conversation. What parties in a practice are doing is intimately, intricately and inextricably interwoven with what they can be said to be doing, can be described, formulated or reported, as doing amongst themselves (account-able in Garfinkel's terms). Thus, what participants in a practice according to the ways of the practice can be correctly described as doing is the (intended) object of the professional sociologist's empirical reference (though due to inattention to the nature of identifying criteria they may, in actual cases, miss those references, but this is perhaps due to the fact that sociologists are very rarely interested in more than—perhaps even as much as—the roughest and readiest identification of anyone's actual actions).

On Sociological Redescription…

Despite doctrines premised upon the assumption that the professional sociologist's understandings are at odds with those of the 'ordinary members of the society' there is not really much disagreement *in sociology's actual practice* with the ways in which activities are indigenously identified. Professional sociology does not provide an extensive re-classification of things that people are doing. That is, they have no substitutes for commonplace descriptions such as 'standing six places from the front of the bus queue' or 'scoring an equaliser in injury time'. It is really the theme of the foregoing remarks that the availability of such actions, so described, is the taken-for-granted starting point for the professional sociologist's redescriptions, and those taken-for-granted identifications are *absorbed* into the redescriptions that are given as (professional) sociological descriptions. Professional sociologists do not *in actual practice* want to change or contest these everyday descriptions, but want to argue, instead, about the understandings that attach to these actions when they are considered from the point of view of their illocutionary or perlocutionary effects, or from the point of view of their placement in some postulated social system or some protracted span of historical time, or, again, from the point of view of an analogy with some other activity. Space allows little elaboration or illustration, so brief mention of that one man industry of redescription, Erving Goffman, will serve.

… the case of Erving Goffman

Goffman's work was exceptional in the *number* of schemes for redescription that he sought to create, and may thus remind us that much of the sociology profession's motivation is to provide a general framework for redescription—many important sociologists labour lifelong on a single such scheme, but not Goffman. Goffman's work often begins with the most ordinary of occurrences—his best known work (*Presentation of Self in Everyday Life)* literally begins with someone showing off on a beach (an illustration taken from a novel). Goffman does not wish to question that the person is indeed playing the show-off but wants to develop a general scheme based on describing this activity in terms of one of its effects (that of 'projecting a self') i.e. showing off on a beach might impress some people that you are a pretty striking individual or others that you are an exhibitionist pratt. All kinds of things, wearing white coats and stethoscopes in hospitals, sealing the restaurant kitchen off from customer view etc will subsequently be redescribed in terms of this *scheme,* which is itself built on an analogy with the theatre, the idea of the stage and backstage.

The status and character of these things as occurrences on a beach or in the preparation and service of food is taken for granted and remains intact throughout as Goffman highlights formal similarities between *aspects* of activities with very different purposes, constituent concerns and participants. In another book, Goffman (1963) contrives a scheme to redescribe social relations in terms of the conventions found in etiquette books. These

conventions are to be used to illustrate how the observance ensures the integration of the face-to-face encounter, the effects they have in allowing people to share a common focus of attention, to contain their embarrassment or inclination to embarrass others by laughing inappropriately. Again, and finally, though not exhausting Goffman's variety and invention, in yet another study (Goffman 1969) he adopted espionage as an analogy for social relations, treating these as a matter of 'the control of information', the concealment and discernment of which is—supposedly at least—a speciality of espionage agencies. In each case, the commonplace identity of all sorts of actions identified in quite ordinary terms provide Goffman's illustrative materials, though that level of identity is only superficially considered, and features noted only relative to their match with Goffman's reclassification of them in one or other of his schemes (which are, in fact as much dictionaries as anything else, with a very high definitional content). The things that, according to Goffman, people are doing are not rivals to the things they think they are doing. That is, it is not as though, when they think they are sharing a tasteful joke, they are, instead, achieving the integration of the face-to-face encounter. It is, rather, that *when* they are sharing a tasteful joke, they are, *by doing this* (unwittingly) effecting the integration of the face-to-face encounter.

Goffman's proprietory vocabulary does not describe anyone's actions *as such*—indeed, one cannot 'integrate the face-to-face encounter' as a direct action of one's own, but can only do such a thing by doing something else

(telling a joke, concealing amusement, holding back embarrassing information etc.) The often considerable effect that Goffman achieves is not produced by his finding any new facts on his own behalf, for that whole effect depends upon making his readers feel that they are seeing things that they are entirely familiar with in a fresh light, inspecting them from an unusual angle, though people who feel that way often suppose that what Goffman has shown is that we are all very manipulative in all our dealings with each other, perhaps literalising Goffman's analogical use of confidence tricks as a way of generating formal resemblances.

Despite his peculiar and highly distinctive status, Goffman is not being singled out as singular, nor is his work being simply dismissed. It is not by producing schemes for the redescription of action that he achieves distinctiveness, but in the ingenuity of his schemes and his fertility with them, and all this is invoked to make the point that Goffman's schemes, like theirs, are not rival to commonplace descriptions, but rework them in the context of a purportedly uniform scheme which pick out things *about* those actions, where there identity as the (everyday practical) actions that they are can be taken for granted, and to allow passing suggestion of the theme that there are always opportunities for considering the relation, and usually the difference, between what professional sociologists seem to be doing and what, in practice, their achievements amount to.

An exercise such as Goffman's does not enable us to understand an activity which really puzzles us, but seeks to relate those actions which do not (from the point of view of their intelligibility) puzzle us to the themes and preoccupations of his professional colleagues. Goffman is often associated with those who are suspicious of positivism, such as many symbolic interactionists, but Goffman's own broader views are often in deep sympathy with atomistic and mechanistic conceptions of understanding (hence the appeal of game theory and ecological biology to him.) Goffman saw himself as predominantly engaged in setting out a specialised area of analysis—the analysis of the face-to-face situation—which could occupy a place within a broader scientifically explanatory sociology.

Winch and Ethnomethodology: Some Differences

Whilst there might be broad agreement of the sort outlined between Winch and ethnomethodology, there are reasons for wondering whether the agreement can go much further, and whether, even, the extent of it can be fully recognised on either side. Ethnomethodologists are apt, like sociologists more generally, to think of philosophy as a non-empirical pursuit, and one which is to be disparaged as such (see Melinda Baccus's (1986) discussion of Winch; here it is clearly recognised that Winch has some relevance to ethnomethodology's concerns but where this is minimised as essentially programmatic and non-empirical). Clearly, Winch's whole campaign is to

liquidate the idea that philosophy is an attempt at *a priori* knowledge of the world: that philosophy's problems are 'conceptual' in nature, and that they have no empirical content. Thus, it would be profoundly misguided to suppose that philosophy's problems can be taken over and answered by empirical investigations.

*On What Ethnomethodology Should **Not** Be: Pollner's Scepticism*

Melvin Pollner's (1987) *Mundane Reason* is often seen as affiliated with ethnomethodology, in a way which makes the latter seem able to issue a frontal challenge to our most fundamental assumptions, such as, for example, that we live in one and the same world. To summarise Pollner's argument rather baldly, it is addressed to the idea of the 'natural attitude', taken from the phenomenological tradition, that provides us with basic expectations such as that, for example, other people—from a different physical and temporal history, a different social background experience, perhaps—experience phenomena that basically correspond to the ones that one experiences. Pollner takes the idea that the natural attitude features certain 'theses', such as the 'thesis' that we inhabit a world known in common rather literally, i.e. as a proposition expressing an hypothesis (that the world is the same for you as it is for me).

As Pollner understands 'the natural attitude' it is the conviction that this hypothesis is true, that the *world* is the same for you as it is for me. An

hypothesis is bivalent, capable of being both true and false, and empirical in nature, meaning that its truth is a matter of evidence. The natural attitude conviction that the world is the same for you as it is for me seems to Pollner to be one that is not empirically justified, for it is held in face of the fact that there is counter-evidence. Evidence, that is, that not everyone's experience is congruent with everyone else's, as is the case with mentally ill people who seem certain of things that the rest of us may suppose 'defy common sense'. In more mundane cases, there are discrepancies between the experience of individuals in their everyday affairs, as traffic court hearings reveal divergences between the testimony of witnesses as to the speed at which a motor cycle was travelling. Pollner sees these cases as potential counter-evidence to the natural attitude, its failure to refute the natural attitude despite the manifest nature of such purportedly 'perceptual' disjunctions being due to the way in which 'the natural attitude' *explains away* such counter evidence (bringing us very much into 'Understanding a primitive society' territory here). Rather than accepting that the experience of the insane, or the respective experiences of disputing witnesses, show that there is no 'world known in common', the natural attitude explains this 'counter-evidence' in its own terms i.e. it keeps the assumption of a 'world known in common' intact, and decides that the experience of the 'dissidents' is invalid, that there is something wrong with their perceptual capacities. These dissident experiences are not treated as standing on an equal footing with

those experiences which conform with the world (conventionally) known in common.

Thus, the courtroom disagreement between a police officer and a motorcyclist over the speed at which the latter's motorcycle was travelling *could* be treated as a product of equally *bona fide* experience, causing us to review our supposition that a motorcycle can only travel at one speed at any given moment in favour of the alternative, that it might be capable to two different experiences. Thus, for Pollner, the hypothesis of the natural attitude is justified in only a circular fashion, for it uses itself to deny potential counter-evidence any admission. Pollner's 'radical' proposal is to put everyone's experience on the same footing, to accept that both our own and 'the deluded' persons perceptions are genuine experiences. In other words, in *our* reality there may not be any superhuman powers, but in *someone else's* reality there are. One is on the way to 'multiple realities' and a good deal of *metaphysical*—not sociological—confusion, indiscriminately mixing together empirical materials and conceptual confusions.[13]

Pollner's difficulties reflect the fact that it is a mistake to treat the 'thesis' of a world known in common as an empirical hypothesis (as though our expectation that our fellow pedestrians will not walk directly into us were some sort of theoretical desiderate), when the 'assumption of a world known

[13] Compare here, once again, the wording of the last paragraph of the *Investigations*, Part II.

in common' does not function in anyone's life in such a way. Pollner's evidential demonstrations do not show that the 'thesis' is an empirically unjustified and evidentially asymmetric proposition, but only that it is not an empirical proposition, supported or undermined by evidence—in reality Pollner unwittingly highlights its *normative* status, as a standard of correctness, not an empirical generality. Pollner's confusions perhaps originate in a mis-taking of the phenomenological idea of a 'description of my experience' which slips from the idea of this as a 'bracketed' exercise, one abstaining from judgements of veracity, to the idea of it as a description of my experience *tout court*, exempt from the qualifying restraints of **phenomenology's specialist purposes.** The description ('reporting' would probably be a much better word) of my experience does not, as we normally understand it, outside the province of phenomenology's restricted exercise, depend for its correctness upon the sincerity or veracity with which I report how it was with me, but upon the states of affairs one claims to have experienced. 'Reporting my experience' is not a matter of engaging in a Cartesian style scrutiny of my personal 'subjectivity', as opposed to reporting on 'objective' states of affairs, but is a matter of reporting on those states of affairs that I have encountered, undergone etc., and the effects that they have had on me or the significance that they have for me. The claim to have seen a motorbike going forty miles an hour in a thirty mile an hour zone is not a cautiously hedged claim about my 'subjective experience', it is a claim about

what *the bike* was doing. This latter claim is not to be established by my testimony alone, for it is part of the work of the court hearing to establish whether my testimony is to be accepted as a report of what happened, rather than a report of how things seemed to me, what I thought I saw and so on. That experiences and perceptions must satisfy certain conditions to count as authentic is not a supernumerary addition but an integral part of our practical understanding of what an experience or perception is (e.g. we look for a more appropriate light under which to view a fabric so as to decide whether its colour is the one we want).

Pollner's arguments seem to entail a radical ontological claim, abandoning the 'natural attitude' assumption of a world known in common, in favour of multiple realities, but this does not really invite us to now accept something we might have thought physically impossible—that a bike should go at two speeds simultaneously. Rather, Pollner's proposals are unintelligible. The courtroom claims—the bike was doing 30mph, the bike was doing 60mph—are rival claims not because of the assumption of 'a world known in common' but because of the nature of the numbering and measurement systems on which he and the parties to the courtroom are all relying. The number system and our conventional methods of speed assign unique numerical values. That is how they work, their logic. In effect, then, the number system and the speed values it is used to compute operate contrastively—to say that a bike is doing 40 mph is to exclude or deny that it

is doing 60mph. To say that the bike is going both 40 and 60 mph makes no sense, since it is saying that the bike is both going and not going 40 mph. What is someone who says this saying? How is it other than a contradiction? One could perhaps try to rescue this by saying that Pollner's proposals would entail that there are two bikes, each going a determinate speed in their own realities, but if one is prepared to say things like that, why suppose—as Pollner presupposes throughout—that there is only one courtroom, only two witnesses etc.?

There is no need to suppose that Pollner provides any serious challenge to the fundamental assumptions (about reality) of our 'natural attitude' in a way which threatens to destabilise them, his challenges are, rather to the numerical and calculational systems in current use—to realise his proposal we could possibly change the idea that objects have a unique speed (indeed relativity theory is sometimes understood as doing just that), but this would also require that in respect of everyday affairs we make changes to the organisation of the number systems that we use in calculating speed, and this would involve all kinds of very complicated consequences, leaving us perhaps inclined to see what the further benefit of going to all the trouble that would be involved (altering the odometers in cars etc. etc, etc.) would yield any real benefit, even with respect to traffic courts. Given we accept that systems of calculation are contingent, we certainly can't claim—and wouldn't

want to be seen dead doing so—that there is any metaphysical or physical necessity which rules out all possibility of developing number systems that would allow a bike to be rated as going at 40mph and 60mph simultaneously, but,

a) we cannot see that such a system would necessarily conflict with our present understandings, since in all likelihood the sense of the expression 'mph' would be changed (as happens when we accept, as loyal British subjects, that our beloved Queen has two birthdays! We do not suppose that she was born twice, unlike everyone else who has only one birthday.) and
b) the proposal involves inviting us to set aside our ways of telling whether a report on e.g. a speeding vehicle is correct, and then tries to persuade as that we have no *real* way of telling whether one person's claim or another is the correct one. The argument is simply a circular one, and derives not from finding any specific failings in our usual ways of making and assessing claims (because the discussion itself relies to a considerable extent on those) but from the usual sceptics tactic of asking us to put our practices into doubt even though there are no genuine grounds for doubt. As hinted above, Pollner is much closer to Evans-Pritchard than he is to Winch, another victim of confusedly

> engaging in 'misbegotten epistemology' and traditional scepticism on the assumption that he is framing an empirical inquiry.

Pollner's position is closer to Evans-Pritchard in that it treats notions of 'error', 'delusion' and the like as if they were secondary terms, ones which are applied *after* an experience's authentic nature has been formulated. If one witness in traffic court says that a motor-cycle was travelling at 30mph and another witness says it was travelling at 50mph why not accept that both reporters are honest recorders of their experience, and accept, then, that the bike was travelling at two speeds? The idea that each witness did see what they testified to overlooks the nature of 'see' as, in Ryle's terminology, an achievement verb, one which intends in such contexts, saw *correctly.* This highlights how Pollner's argument short circuits the function of the traffic court, for the issue is not the sincerity with which a witness delivers testimony, but the capacity of the testimony to contribute to a determination of the speed that the motor bike was travelling. In other words, the quality of the experience was is a function of what determinately took place, and the latter is determined by other evidential input in addition to the testimony of the two witnesses into the courtroom's adversary procedure—the nature of the facts and the relevant experiences are determined together. The witness's testimony only establishes the speed of the bike if the witness is accepted as

e.g. having indeed been able reliably to determine and honestly to report the speed of the bike—otherwise, the witness's experience is/was guesswork, misjudgement, delusion etc. There is no logical connection between the sincerity of the speaker and the correctness of their claim.

Rather than an empirically inspired new radicalism, Pollner's efforts are merely another application of traditional scepticism, an attempt to raise a doubt where there is no (ultimately) intelligible basis for doubt. There are, of course, plenty of doubts in the courtroom: e.g. whether a witness is honest, whether a witness could possibly be correct in what they claim, whether a witness is well enough equipped to understand what they are attempting to testify to &c. Deciding these matters is what the courtroom practices are for. Pollner, however, wants to ask whether courtroom proceedings (conducted on the basis of 'the natural attitude') are capable of getting *anything* correct. Pollner's views originate in an attempt to raise the standard of proof for courtroom proceedings, raise the standard to a level which such proceedings cannot attain. Of course they cannot, for this is the sceptic's art, to insinuate standards which are unattainable and use these to indicate the presence of (possible) doubt. Pollner treats 'the natural attitude' as an ensemble of hypotheses which, considered as such, cannot be empirically grounded, for they are presupposed in the determination of what is and what is not empirical evidence. Therefore the proceedings in courtrooms cannot be *truly* justified by evidence, for they too must be circular. As we say, the ostensible

space for 'doubt' comes not from any issues in the courtroom but from the treatment of the courtroom as an example of a procedure based on the natural attitude which is, on Pollner's understanding, a circular operation. Pollner overlooks, of course, the extent to which courtroom proceedings are a (so to speak) grammar of action rather than an ensemble of empirical hypotheses (whereas they provide the standards for deciding what can—for legally admissible purposes—count as an empirical hypothesis and what could comprise evidence for or against it, providing in various and complicated ways, the scaffolding which *gives sense* to factual claims, the validity of testimony and the like. The witnesses' rival claims can only be rival claims because both presuppose the same system for the determination of speed, one in which only a single velocity can be assigned—its not an empirical (im)possibility that is in prospect here but rather an adjudication between two applications of the same measurement system.

In consequence, there are, perhaps, misapprehensions as to what is going on when ethnomethodologists 'look at the data' and what the purpose of such exercises might be, especially in those sectors where there is almost a militant empiricism about resort to data.

Of later Garfinkel

On Winch's understanding, philosophy is only *a priori* to the extent that (the grammar of) our language plays a shaping role in what it is possible

(intelligibly, informatively) to say, and it is *the language,* not philosophical doctrines—or sociological approaches, for that matter—which *facilitates*—in advance, so to speak—what it makes sense to say about empirical instances of social action. Inattention to this means that what is going on in the consultation of empirical instances is lost sight of (such that, for example, there seems a strong strand in Garfinkel's later thought (Garfinkel, 2002) that often features the idea that our 'ordinary language' cannot capture the nature and nuances of the activities he is trying to describe i.e. we don't have a word for it (in English)—though this involves only the adoption of Greek and Latin terms, and these, too, surely qualify as part of natural language; as expressions with perfectly 'ordinary' uses amongst Greek speakers and indeed, as possible extensions of English. Talking about the examples as 'data' can help obscure the fact that the materials sampled are not the sole or even necessarily main materials for the exercise, where the prime 'materials' are rather one's own, commonplace, understandings (ones which are commonplace in one's own life, or that one has learned are commonplace in the lives of some others) whose application is invoked and focussed through the example. What is going on is the spelling out—explicating—of what is being brought to bear (through the medium of an enculturated understanding of the language, of how to conduct oneself intelligibly) through consideration of the instance.

There are at least two conflicting ways in which ethnomethodologists can think of their inquiries: as either a beginning or an ending. One can—and Garfinkel sometimes seems to—think of ethnomethodology as a first step in the direction of a genuine sociological science, one which differentiates itself from sociology-at-large (or Formal Analysis (FA), as Garfinkel nowadays terms it) in being the only branch of sociology that addresses itself directly to actual and observable occurrences in and of the social order. By contrast, FA is perceived as typically addressing observable social activities (a) in forms that have been processed and reconstructed by sociological methods and/or (b) are construed as a function of the preconceived interests that sociological theorising has in turning to the social world, i.e. sociologists *project* their procedural forms onto their data, rather than exploring the data for itself.

Thus, one might think of ethnomethodology as attempting the beginning of a reconstruction of sociology, where the current investigations open up new directions of inquiry which, if cultivated, will produce much more striking and powerful results.

One might. Garfinkel himself does not consistently indicate that this is the direction he forsees, being inclined to destabilise any seemingly settled understanding of his work, to repudiate some of his own prior stances, and the attachment other ethnomethodologists might show to them. Looked at that way, ethnomethodology has a subversive, rather than a constructive role,

is ultimately subversive even of its own apparently constructive contributions.

Garfinkel talks of ethnomethodology as an 'alternate' sociology, but this too might be understood in different ways. The possibility being explored here is that the 'alternate' registers ethnomethodology's interconnection to the enterprise of Formal Analysis. Rather than having an independent platform from which to launch itself as an autonomous form of sociology, it may be better understood as existing in response to and reaction against Formal Analysis. As, that is, countering the theoretically and methodologically top heavy renderings of social affairs with displays of the way in which the observable doings making up the society's practical life elude representation by those renderings, with recovering what is otherwise readily recognisable and eminently well known to practitioners from beneath the overload of professional interpretation.

Another of Garfinkel's turns of phrase acquiring latter day popularity is that of 're-specification'. This term can also fit the idea of an umbilical affiliation of ethnomethodology to Formal Analysis. Re-specification is a matter of taking the topics and problems of sociology done in formal analysis, and turning them into topics of ethnomethodological inquiry. This was reflected in Garfinkel's 'studies of work' programme—take sociological topics and themes and ask, 'Who in society has the work of dealing with this problem as their daily work?', then go and study how those people, as part of

that work, encounter and deal with these problems. Rather than, for example, worrying about sociology's problems of measurement, investigate, instead, people whose job it is (within the state, in organisations, in educational organisations, *wherever*) to measure social phenomena and understand what measurement is for them, and how they achieve it.

The idea of re-specification is that this can be done ubiquitously with Formal Analysis's themes and pre-occupations, and as such can be understood as an ending for the idea of 'a sociology' as the proprietary possession of a profession of investigators. The work available for such a sociology has been, so to speak, handed over to the members of the society, the themes and pre-occupations making up the putative work of such a profession, having their problematic character resolved in *socially organised indigenous practice* not by theoretical and methodological contrivance and fiat. The ethnomethodologist's own work is not itself that of proposing solutions to 'sociological problems' on his or her own behalf, of offering 'an ethnomethodological account' of 'locating lost property', 'accepting patients for treatment', 'joining a queue for service' or whatever else it might be. It is, instead, a matter of producing exhibitions of what it could be—in social life's practical affairs—that sociological theoretical and methodological discourses are talking about, to recover (from the supervision of theorised discourses) the everyday social world as a place recognisable to those who inhabit it.

Providing displays of this kind could function very much as reminders' and 'perspicuous presentations' in Wittgenstein's—and thus Winch's—sense, though clearly, in many cases, these do not serve as reminders of one's own personal practices specifically, but as a means of relaying the understandings that those involved in unfamiliar activities—mathematics, observatory astronomy, martial arts training, truck wheel repair, industrial print production, loan suitability assessment—employ to organise their activities. The function, in either case, can be an emancipatory one, enabling the breaking of an intellectual spell. The spell is that cast by the idea of theory-and-method as essential precursors to understanding, where the only alternative to any given theory has to be some other, and different, theory.

Ethnomethodology's studies illustrate the way that social affairs are *already* understood, prior to the appearance of professional sociologists on the scene, by those who possess a purely practical understanding of those affairs, and who resolve 'problems of social order' for and by themselves through the practical organisation of their affairs, i.e. by arranging their affairs in ways that are satisfactory according to the standards that come with the practice. Exhibitions of the ways of less familiar activities (less familiar, that is, to sociologists), with a strong emphasis upon their irreducible detail and specificity, is a forceful counter to the 'craving for generality' that comes with the aspiration to theory-and-method or which continues to infect

disappointment with, alienation from, and reaction against such 'universalising' aspirations.

The exhibitions make vivid the numerous specific and localised demands which the circumstances of action make in every case, and the distinctive competences that are involved in adequately (in a practical sense) responding to them. The dense and diversified array of conditions involved in putting together intelligible social actions can be left out of, overlooked by, attempts to give theoretical portrayal of action-in-general, but they cannot be disregarded by those who must, in real time, under real circumstances, carry out 'the affairs of society' in the form of their everyday practical affairs.

Empirical investigations in Formal Analysis are designed to capture empirical cases in a way which will yield generalities that absorb the case into a whole genre of activities (e.g. Goffman's example of making confidence tricks, waiter service, hotel reception, medical encounters, moments of socialisation all examples of 'presentation of self'). In such a context, studies of cases are ends to means, rather than as, for ethnomethodology understood as here, ends in themselves. Understanding is thus effected through clarifying how specific activities are embedded in the social settings to which they belong, and the setting-specific practices which they enact. The connections which are made between one action and another, between actions and their social settings, between one setting and another are not to be understood as

instantiating theoretical axioms, but as forged in and by those activities themselves.

Critical Social Theory and the Charge of the Reification of the Contingent

With an eye to the topic of our next and final chapter on Winch and the charge of conservatism, we wish here to briefly address charges, such as those levelled by Jurgen Habermas (1988), that views like those of Winch and the ethnomethodologists are dangerously conservative, in that they serve to reify the *status quo*, thereby intimating that the social order cannot be changed. Such a criticism is really an expression of Habermas' preoccupations and the way they shape his reading of other people's work and, apparently, blind him to all kinds of important aspects of their thought which don't fit within his own—somewhat restrictive—framework for social science—that social science should pursue the goal of completion of the Enlightenment project (as Habermas understands that project, in (post-)Kantian terms.)

In their parallel efforts to bring gross and misguided philosophical and theoretical abstractions back down to earth, Winch and Garfinkel both put the contingency of existing practices front and centre—there are no (metaphysical) necessities involved in them—though one (more than)

suspects that desire for metaphysical necessities lingers on in Habermas.[14] Such necessities as there are should be understood as functions of how our practices are contingently organised (even the necessities of logic and mathematics!) i.e. as stringent requirements of the practice, not the other way around. The 'status quo' is the ensemble of our current practices, but that ensemble has changed in order to arrive at its current state, is likely changing even now in respect of many aspects of any of our many practices, and will no doubt see further subsequent changes, involving the mutation of some of those practices and even the abandonment of others. But these are banalities, not dogmatics, banalities whose recognition fully obstruct the effort to read-off any implication of the immutability of 'the *status quo'.* Whether *Habermas'* political programme offers ways of transforming existing society in the way he aims for is simply immaterial to the issues that Winch and Wittgenstein address. Habermas wants to derive a political programme from a sociological theory, and, in that respect Winch and Garfinkel are a deadly threat to his project. Even there, it is only because they are—very much unlike Habermas—utterly unimpressed with the idea of 'a sociological theory', and regard acquisition of one as supernumerary to the creation of a political programme.

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[14] Exchanging Kantian talk of the "transcendental" for post-Kantian (C.S. Peirce-inspired) talk of the allegedly post-metaphysical *"transcendent"* leaves these authors unconvinced that either metaphysics or their lure have been overcome.