Experimentation in the Social Sciences: Cultural Dope or Reflexive Agent? A Reflexive Critique of Ethnomethodology

Nigel Pleasants

Introduction

In this paper I seek to undermine the ethnomethodological picture of the skilful, reflexive, interpretive agent. Neither critic nor follower has ever seriously questioned whether the 'experiments with trust' reported in Garfinkel's (1984) Studies in Ethnomethodology really demonstrate what he claims. That is to say, Garfinkel's interpretation of the evidence produced by these 'experiments' has not been challenged, and this is precisely what I will do here. My strategy centres around a critical comparison between the famous 'experiments with trust' and Milgram's (1974) inversely infamous 'obedience experiments'. This may seem like an unlikely comparison considering that Milgram's work is usually consigned to the bad old days of positivistic social science. But I will argue, firstly, that Milgram's interpretation of his experimental subjects behaving as 'cultural' and 'judgemental' 'dopes' of institutionalised authority is also a more apt interpretation for the behaviour of Gartfinkel's subjects. Secondly, I will endeavour to defend Milgram's interpretation of his subjects' behaviour against ethnomethodologically-inspired critical reinterpretations of his experiments by Harré and other 'post-empiricist' social theorists. Thirdly, invoking recent sociological work on experimentation in the natural sciences, I seek to show that a priori objections to experimentation in the social sphere are unwarranted. Finally, I go on to question how consistently ethnomethodologists have implemented their central principle of the 'essential reflexivity of accounts' (Garfinkel, 1984: 7). I conclude with a brief comparison of Garfinkel's and Wittgenstein's 'reflexive' practice.

Although I am critical of ethnomethodology in this paper, my main target is contemporary 'critical social theory' (see Pleasants, 1998/9), which has been strongly influenced by ethnomethodology. I would like to stress at the outset that my critique of ethnomethodology differs markedly from that which is typically deployed by social theorists - for example, Gellner's accusation of 'subjectivism'; Giddens's and Habermas's charge of relativism and paralysis of the critical will. I do not criticise ethnomethodological practice as such; my aim, rather, is to 'deconstruct' the 'ontological picture' of the individual which is based upon Garfinkel's early studies.

The term 'ontological picture' alludes to Wittgenstein's frequent use of the word 'picture' in a quasi-ideological sense, where thought is 'held captive' by deeply entrenched metaphysical representations. Ontological pictures that Wittgenstein was particularly keen to deconstruct include: the Kantian picture of transcendental rules 'hidden in the medium of the understanding' (Wittgenstein, 1968: §102); the Cartesian picture of an inner mental space in which individuals read their exclusively private ideas and perceptions; and the empiricist picture of a subject passively receiving sense-data. In contemporary social theory, the ontological picture of the 'reflexive agent' overlays these earlier pictures.

Ethnomethodology and Social Theory

Ethnomethodology was born out of a radical dissatisfaction with established social theory and social science. Garfinkel's principal objections concerned what he saw to be a spurious claim to scientificity, and a deeply felt antipathy to the way in which the individual is portrayed by social theorists as 'a judgemental dope of a cultural or psychological sort, or both' (1984: 67). These 'social science theorists' are chronically 'misled about the nature and conditions of stable actions' (ibid.: 66-7). As a consequence, individuals become the cultural and judgemental 'dopes' of theorists' representations.

Yet although he is vehemently opposed to 'Constructive analysis',[7] Garfinkel's statement of intent nevertheless sounds like the orthodox social theoretical desire to reveal and illuminate the mechanisms and strata of social phenomena, 'the rule governed activities of everyday life' (ibid.: 35). Ethnomethodological investigation, he says (ibid.: 38), 'should tell us something about how the structures of everyday activities are ordinarily and routinely produced and maintained'. The picture of the individual agent/actor which Garfinkel presented in his ethnomethodolgical studies has exercised enormous influence on subsequent social theory. His picture of the individual as an active, skilful, interpretive, reflexive agent, has effectively replaced the passive, socially and environmentally determined subject portrayed in classical social theory. Garfinkel (ibid.: vii) proposed that: 'practical sociology's fundamental phenomenon' is the locally situated everyday activities through which individuals create social order as 'an ongoing accomplishment of the concerted activities of daily life, with the ordinary, artful ways of that accomplishment being by members known, used and taken for granted'.

The first social theorists to take cognisance of 'the reflexivity of the actor' (Heritage, 1984: 31)[8] were Harré and Secord (1972), who endeavoured to incorporate the ethnomethodological picture of action and social order into 'post-empiricist' philosophy of social science. Harré and Secord (ibid.: 12) translate 'reflexivity' into 'self-monitoring', and claim that 'the self-monitored following of rules and plans' is 'the social scientific analogue of the working of generative causal mechanisms' studied by natural scientists. Following Harré and Secord, the 'reflexivity of actors' becomes, with Giddens (1984: 3), and Bhaskar (1979: 104), 'the reflexive monitoring of action' - which differs from Harré's and Secord's formulation in emphasising the predominantly 'practical' and 'tacit' form of this postulated process.[9] Similarly, Habermas (1991: 127) endorses what he takes to be Garfinkel's identification of 'the invariant features of the interpretive procedures used by participants in communicative action'. McCarthy (1994: 71), (Habermas's translator and expositor), suggests that there are 'deep affinities between Garfinkel's account of the routine grounds of everyday activities and Habermas's account of the structure of communicative action'.

Thus ethnomethodology is widely credited with providing for social theory insights into the organisation of the 'lifeworld', and with demonstrating the

[7] 'Constructive analysis' refers generically to the practices of professional social theory and social science (Garfinkel and Sacks, 1986: 162).

[8] Garfinkel does not use the phrase 'reflexivity of the actor', and I should note that although Heritage's account of Garfinkel and ethnomethodology is extremely popular amongst social theorists, his 'cognitivist' reading has been criticised by Button and Sharrock (1993: 21n7). Nevertheless, I think 'reflexivity of the actor' (where 'reflexivity' is not necessarily understood as a cognitive process) *is* an appropriate description for the picture of the individual presented in Garfinkel's *Studies*.

[9] See Pleasants (1996) for a critique of the concept 'tacit knowledge' in social theory.

centrality of individual interpretive agency to it. Garfinkel's 'breaching experiments' are presented by him, and accepted by social theorists, as evidence of the skilful, interpretive procedures used by individuals in the process of creating and maintaining social order.

Experiments with Social Reality

According to recent studies of experimental practice,[10] natural science inescapably depends on scientists' artful manipulation of materials. This activity is described by Rouse (1987: 101-2; 1996: 128-32) as the creation of 'phenomenal microworlds'. Experimental work, says Rouse (1996: 129), 'places a premium on introducing and monitoring controlled disturbances into previously stable and well-understood settings'. This description of experimentation in natural science applies equally well both to Garfinkel's and Milgram's experiments.

Garfinkel's celebrated 'breaching' experiments with trust appear to hold out the possibility of a revolutionary approach to the study of social action. The results certainly seemed to herald the discovery of genuine knowledge regarding the 'micro foundations' of social science - findings[11] which meet the strict criteria of scientific knowledge, namely: replicability, universality and predictability.

The 'experiments' involved interventions in, and disruptions of, the normal course of events in selected regions of social life, which yielded dramatic and unexpected results - 'massive effects', in Garfinkel's (1963: 220) words. The most dramatic results were those obtained from scenarios in which experimenters attempted to treat members of their family as 'anthropologically strange' (Garfinkel, 1984: 9). In another experiment, student subjects were tricked into thinking that they were assessing a new counselling programme for the Department of Psychiatry. (An experiment with medical school students was very similar and produced similar findings [ibid.: 58ff]). The counselling programme required subjects to discuss 'some serious problem on which he would like advice' (ibid.: 79) with an unseen 'counsellor' (the experimenter). Subjects were told that 'most people want to ask at least ten questions' (ibid.: 80) and that each question would receive only a "yes" or "no" answer from the experimenter-counsellor - the 'answers' having been predecided randomly. The most striking finding of the experiment was the extent to which subjects interpreted the 'counsellor's' responses as reasonable and helpful advice. It was found that the course of subjects' monologue was shaped by their interpretation of the import of the 'counsellor's' responses to the questions asked.

These experiments (and others) are taken to provide evidence of two fundamental ethnomethodological phenomena: (1) an omnipresent order of norms, rules, methods and procedures constituting the corpus of socially shared knowledge of how to 'go on' in social life - 'all actions', Garfinkel (1963: 198) maintains, have a 'constitutive structure' and a 'normative order'. And (2) the 'interpretive work' (Garfinkel, 1984: 31) required of individuals to access and maintain that order of normality. The 'counselling' experiments are said to demonstrate the interpretive work of individual subjects - they 'catch the work of "fact production" in flight' (ibid.: 79).

The experiments are presented as an empirical demonstration that real individuals are not like the 'cultural dopes' portrayed in

---

[10] Along with the 'constructionist' sociology of Collins (1985), Latour and Woolgar (1986), Knorr-Cetina (1983), and Mulkay (1985), ethnomethodology has been at the forefront of ethnographic investigation of experimental practice - see in particular, Lynch, Livingston and Garfinkel (1983).

[11] Garfinkel (1984: 69) himself describes the results of his investigations as 'findings'.

classical social theory. However, I suggest that Garfinkel has merely replaced the classical sociological picture of the 'cultural dope' with his own 'ontological picture' of 'knowledgeable', 'reflexive' agents actively making sense of their situation.

Cultural Dope Versus Reflexive Agent

It is my contention that Garfinkel overlooks the extent to which his own practice (as experimenter) is 'reflexively' implicated in bringing about the effects he observes in the counselling experiments. Garfinkel mentions that some subjects entertained the possibility of trickery, but, even so, found this suspicion difficult to maintain in practice, and could be seen to search actively for ways in which the counsellor's answers 'made "good sense"' (ibid.: 91). I suggest that subjects found it difficult to act upon their suspicions primarily because of the power and authority relations structuring the experimental situation. Expert knowledge in general, and experimental research in particular (both of which were primary features of the counselling experiment), carries the legitimating authority of scientific knowledge acquisition. My alternative account of the experiments is that the experimenter-counsellor exercises 'scientific' authority over his subjects, gaining their compliance in virtue of his occupancy of the role of 'expert' and 'authentic scientist'. This trust and compliance in the experimenter is precisely what Milgram (1974) engineered in his obedience experiments.

Rather than agreeing with Lynch (1993: 140) that Garfinkel's 'experiments' were more like 'practical jokes' than 'social-psychological experiments', I want to suggest that a comparison with Milgram's experiments is both apt and instructive.[12] This comparison is particularly apt considering that some of Milgram's fiercest critics have used ethnomethodologically-inspired arguments against his interpretation of the obedience experiments.

Garfinkel's and Milgram's experiments were very much alike in that both constructed a 'phenomenal microworld' and both had to deceive their subjects on the true aim of the experiment and identity of the experimenter. But whereas Garfinkel claimed that his experiments show individuals to be 'reflexive', 'interpretive' agents, Milgram interpreted the results of his own experiments in precisely the opposite way, as evidence that people are much more likely to obey deferentially than they are to interpret, 'reflexively', directly perceivable features of their situation. Milgram's experiment brings to the fore the relations of power and authority in an experimental situation which I contend were crucial, though unremarked, factors in Garfinkel's studies.

I am not saying that Milgram's experiments support Bloor's (1992: 269) assertion that 'the actor must be some form of cultural or judgmental dope'. This merely replaces one 'ontological picture' with another, which is equally over-generalised. Similarly, for the same reason, I do not support Milgram's (1974: 132) postulation of an 'agentic state', which is a causal-dispositional explanation of his subjects' 'obedience'. But I do suggest that the scenarios constructed by Milgram closely match the ones engineered by Garfinkel in his 'counselling' and 'medical school' experiments, and that within those situations we do see (the majority of) both sets of subjects acting as 'cultural' and 'judgemental' 'dopes'.

In Milgram's experiments subjects believed that they were participating in a learning programme, in which they were instructed to administer to a 'learner' (the experimental stooge) an electric shock each time he answered incorrectly. The shocks increased in severity, up to a maximum of

---

[12] Milgram (1974: 209n) also suggests that his experiments resemble Garfinkel's, both in terms of methodology and phenomena revealed.

450 volts. Even though the learner was heard to scream in pain and protest that he had a weak heart, the majority of subjects (65 %) - encouraged by a white-coated experimenter - proceeded to administer the highest level of shock, categorised as 'DANGER - SEVERE SHOCK' on the control panel. Subjects were told politely, but firmly, that they must continue with the experiment, and that the experimenter assumed full responsibility. Subjects were not coerced into acting in the way they did: 'obedience' was 'willingly assumed in the absence of any threat of any sort' (ibid.: xiii).

Apart from similarities in the experimental scenarios created by Garfinkel and Milgram, it is pertinent to examine ethnomethodologically-informed criticisms of Milgram because these critics claim that, in addition to Garfinkel's demonstration of the reflexive, interpretive nature of individual agency, Milgram's experiments (when properly reinterpreted) actually confirm rather than negate this picture. I will focus on objections raised by Harré (1979; 1993) and Mixon (1972b; 1989).

Harré's (1993: 25) critique of Milgram invokes the ethnomethodological picture of the 'creation and maintenance of small-scale social order', as 'an artful achievement of active human agents'. Harré and Mixon both claim that the meaning attributed to subjects' actions by Milgram is quite different to the meaning actually experienced by the subjects themselves. They claim that, despite appearances to the contrary, Milgram's subjects were in reality much more like Garfinkel's 'reflexive agents' than the 'judgemental dopes' that Milgram made them out to be. According to Harré and Mixon, Milgram's subjects did not 'obey blindly'; on the contrary, they proceeded on the basis of their interpretation of the situation. It is alleged that, as skilful, reflexive agents, subjects would have interpreted (correctly) that they would not be asked by a 'scientific' psychologist to administer potentially fatal electric shocks to another person.

Mixon (1989: 40) asserts: 'to suppose that in ordinary circumstances 65 per cent of the population can be expected to obey an illegitimate command to harm and kill is quite simply a delusion'.[13] His confidence in the truth of this assertion depends on very strong assumptions about what subjects 'must' have known about their situation, and also on a question-begging use of the concept of 'legitimacy'. As with Bloor, Mixon's position is completely hostage to what he insists 'must' be the case - '"must": that means we are going to apply this picture come what may' (Wittgenstein, 1976: 411).

In support of his (1972b: 158) claim that Milgram's subjects knew that 'people are not harmed in psychological experiments', Mixon points out that in our society psychologists are not authorised to issue commands which knowingly visit harm on people, hence Milgram's experimenter's commands were illegitimate. This is, of course, quite correct in the de jure sense, but Milgram's (obedient) subjects did nothing to indicate that they thought the experimenter's commands were 'illegitimate'. On the contrary, they (ex hypothesi) did what they did precisely because they perceived the experimenter as an authoritative expert and the laboratory as a legitimate setting. In contrast, Mixon (1989: 27) says that the 'hideous commands so many obeyed' in wartime Germany did emanate from legitimate authority; they were, he says, 'commands authorised by the Nazi state'. But it is precisely the question of how perpetrators of these atrocities came to perceive their actions to be legitimate that Milgram sought to investigate via his

[13] Button (1991: 6), an ethnomethodologist, also believes that in 'practical everyday' life, psychologists' 'random orders to administer people with electric shocks would be regarded with some scepticism'.

experiments.[14]

Appealing to the 'background expectations' which he assumes to be continually operative, Mixon (1972b: 157,158) claims that subjects really knew that 'safeguards [were] in place', and that 'people are not harmed in psychological experiments'. The question-begging nature of these propositions is evident in the way that they are phrased: 'the assumption of safeguards must to some degree confound the interpretation of any action involving supposedly dangerous consequences' (ibid.: 169 - my emphasis). This is an extremely intellectualist, and heavily counterfactual interpretation, claiming to know what subjects really believed.

Mixon (1989: 30) insists that, because the experimenter showed no concern for the condition of the learner, subjects would interpret this as confirmation that the shocks were not real; or as assurance that no real harm was being done. This belief about the import of the experimenter's reactions (or lack of them) is supposed to have counteracted the evidence of subjects' sense-perception and their knowledge of the learner's physical condition (his weak heart, the effects of high voltage electric shocks, etc.). Yet it seems quite clear from the observed distress of subjects and from their de-briefing interviews, that they really did believe that they were administering (real) electric shocks to a vulnerable victim. This interpretation is only confirmed by the subsequent moral outrage of Mixon, Harré and many other social theorists. In Harré's (1979: 106,104) opinion, 'the most morally obnoxious feature of this outrageous experiment' is the behaviour of Milgram and his assistants - not that of the 'otherwise kindly citizens' participating in the experiment.[15] Similarly, Mixon questions the ethical propriety of exposing subjects to the kind of distress so vividly described by Milgram (quoted by Mixon, 1989: 32):

> I observed a mature and initially poised businessman enter the laboratory smiling and confident. Within 20 minutes he was reduced to a twitching, stuttering wreck, who was rapidly approaching a point of nervous collapse. He constantly pulled on his earlobe, and twisted his hands, at one point he pushed his fist into his forehead and muttered: "Oh God, let's stop it".

Yet it is clearly inconsistent for critics to maintain, as do Harré and Mixon, both that subjects did not believe they were causing the learner any real harm and that Milgram reprehensibly put subjects into a situation which caused them such palpable distress. Why were subjects so distressed if they either realised that the electric shocks were not real, or believed the experimenter's assurance that: 450-volt shocks 'may be painful' but 'there is no permanent tissue damage' (Milgram, 1974: 21)? Harré (1979: 105) claims that subjects 'believed their actions were not going to affect the learner at all, other than in the beneficial way of improving his capacity to learn'. But Harré does not tell us how he knows that this is what Milgram's subjects really believed.

Harré neither observed Milgram's

[14] The contemporary relevance of Milgram's work has been chillingly highlighted once again by a war-crimes trial. Erich Priebke admitted taking the leading role in the killing of 335 Italian civilians. He pleaded in his defence that his actions constituted 'a legitimate reprisal' for Resistance attacks on German soldiers. The court accepted his plea of 'obedience as mitigation', and he was adjudged not to have 'acted in a cruel or premeditated way because he was obeying orders' (*The Guardian*, 6 August 1996: 3). (This judgement was later overturned).

[15] Milgram's (1974: 45) report on a subject called Mr. Batta hardly depicts a 'kindly citizen': 'after the 150-volt level, Batta has to force the learner's hand down on the shock plate, since the learner himself refuses to touch it'. The Nazi War-crimes defendant referred to in note 8 is also described by his lawyer as a 'good citizen'.

experiments, nor has he attempted to replicate them himself. The basis for his claim regarding the beliefs of Milgram's subjects is nothing other than a theoretical derivation from his 'conception of man as actor' and 'self-directing agent' (Harré and Secord, 1972: 313). This conception accords primacy to 'the role of actors' interpretations and beliefs' because they are 'the central determining factor of action' (Harré, 1979: 101,104). The upshot of this theoretical commitment is that, rather than paying attention to the 'beliefs and interpretations' of subjects in Milgram's experiment, Harré elects to infer, solely on the basis of his 'ontological picture', what subjects must have believed.

Although Harré has not conducted any empirical research which might have given him insight into the beliefs and perceptions of subjects in obedience experiments, Mixon (1989: 28) does claim to have managed '(successfully) to simulate the conditions in the study that led to the extreme tension and stress exhibited by many of Milgram's subjects'. But, Mixon's (1972b) study differed from Milgram's in one crucial respect - Mixon avoided the need for deception because his 'experiment' was performed with volunteers in a 'role-playing simulation'. Mixon explains that 'the role player is told to imagine particular things and certain consequences and to behave as if they are real' (ibid.: 147).

In his 'replication' of Milgram's study Mixon (1989: 28-9) also 'debriefed' his actors, apparently revealing that the actors 'could not understand why [the experimenter] behaved the way he did, how he could know without looking that the "learner" was all right'. According to Mixon, this provides the key to seeing what Milgram's subjects really believed. Thus subjects would take their definition of the situation from the experimenter; that is, they would believe his assurance that the shocks might be 'painful but not dangerous' (ibid.: 30). Mixon (ibid.) maintains that 'the experimenter's verbal and nonverbal behaviour' would be enough to convince subjects that the shocks 'are not harmful'. Hence subjects allegedly never really believed Milgram's deception. Mixon does not say whether he thinks that (merely) 'painful' shocks are harmless; or whether he thinks that Milgram's subjects believed that (merely) painful shocks are harmless; or whether he thinks they thought that the shocks were not painful, or perhaps that the 'shocks' were not real.

Harré and Mixon both believe that experimentation is a wholly inappropriate method in social psychology because it presupposes erroneous ontological and metaphysical commitments (Harré, 1993: 102). Experimental investigation of human action is said to be governed by a positivistic philosophy of science and a mechanistic conception of human beings, whereas the role-playing method privileges individual agency and interpretive skills. Thus the rationale to role-playing simulation is that individuals are treated as 'actors'. In contrast, orthodox experiments, such as Milgram's, treat individuals as mere 'organisms' (Mixon, 1972a). According to post-empiricist philosophical realism, which Harré helped to found, role-playing simulation 'represents the closest analogue of experimentation in natural science' (Greenwood, 1991: 126).

However, justification for the methodology of simulation and role-playing is inseparable from the 'ontological picture' of individuals as essentially reflexive, interpretive actors. This being so, a simulated experiment which is designed to show, contra Milgram, that subjects always act interpretively and knowledgeably, can hardly count as an independent empirical test. Mixon's methodology is a prime example of 'ontological gerrymandering' (Woolgar, 1988: 98), presupposing the very assumptions that it claims to investigate.

Harré's and Mixon's certainty that they know, contra Milgram, what his subjects really believed and experienced, emanates directly from their ontological picture of the individual rather than empirical investigation. Obsessive theoretical commitment to an 'actor' model of human action is responsible for persuading Mixon that he had successfully recreated, through his simulations, the experience of Milgram's original subjects. One of Wittgenstein's 'grammatical reminders' is quite apposite here - he remarks that: 'looking up a table in the imagination is no more looking up a table than the image of an imagined experiment is the result of an experiment' (1968: §265).

In the next section I take a closer look at the supposed 'interpretive skills' displayed by Garfinkel's subjects.

The Grammar of Interpretation

Garfinkel's account of how subjects managed to construct "good sense" (1984: 91) out of a small number of randomly pre-determined "yes" or "no" responses attributes to individuals the possession of active, skilful, interpretive abilities. Examples of subjects' postulated 'interpretive work' include the following: 'subjects heard the experimenter's answers as answers-to-the-questions'; 'subjects saw directly "what the adviser had in mind"'; 'much effort was devoted to looking for meanings'; 'throughout there was a concern and search for pattern'; etc. (Garfinkel, 1984: 89-91). These examples present a picture of autonomously reasoning individuals driven by an inherent propensity to find meaning, and make sense. But how could anyone derive significant meaning from an arbitrary "yes" or "no" response elicited by a question at the end of a complex description of some state of affairs? Why did subjects not realise that the situation in which they found themselves was not the one defined to them by the experimenter?[16]

Adopting the same strategy as Harré and Mixon, but from an opposed perspective, I contend that Garfinkel's counselling experiment is better described as exhibiting the phenomenon of conformity rather than skilful interpretation. Harré and Mixon challenge Milgram's interpretation of his experiment, asserting that although his subjects appear to obey 'blindly', in fact they were able to penetrate the deception by interpreting the actions and responses of the experimenter. Similarly, but from the opposite standpoint, I suggest that Garfinkel's subjects only appear to be engaged in 'interpretive work' through the interpretive work of Garfinkel's presentation. Another way of interpreting the counselling experiment is to see Garfinkel's subjects as merely responding, somewhat unreflectively, to 'the constitutive order of events of everyday life' (Garfinkel, 1963: 209).

Some 'grammatical' reflections on the meaning/use of 'interpretation' are called for. The activity of interpretation, I suggest, is closely associated with the aim of 'discovery'. Thus one attempts, through interpretation, to discover 'the meaning' of a poem; similarly, anthropologists and historians interpret strange practices in distant cultures, and theorists interpret difficult or controversial texts - in an effort to discover their meaning. In these cases, interpretation is a highly reflective, and necessarily intentional process (though not dependent on discovering 'the true

[16] It should be recalled that in the 'family-breaching' experiments, where the experimenter pretended to be a stranger in his or her home, most of the 'subjects', just like Milgram's subjects, did *not* 'search for meaning' nor try to 'make sense' (only two out of 44 families interpreted their situation 'as a joke from the beginning' [Garfinkel, 1963: 226]). On the contrary, most subjects simply reacted with outrage and exasperation at the senselessness of their predicament, closely followed by the demand that such deviant activity cease immediately.

meaning'). The majority of Garfinkel's subjects did not engage in this kind of interpretation. On the contrary, subjects in the counselling and other experiments attempted to 'normalise' events in the experiment by making them fit into their customary way of being. This is hardly an example of skilful, 'reflexive' accomplishment. A more appropriate description of (the majority of) Garfinkel's subjects, I suggest, is that they behaved 'conservatively' and 'obediently' (like Milgram's subjects). I would reserve the epithets 'interpretive' and 'reflexive', in this context, for the small number of subjects that discovered the deception. This is indeed quite properly described as a 'skilful accomplishment'.

I turn now to a consideration of the experimental status of Garfinkel's and Milgram's studies.

Real Experimentation, or Aids to a Sluggish Imagination?

Garfinkel equivocates on whether the breaching experiments were real experiments revealing genuine discoveries or, rather, vivid enactments of what 'anyone like us knows' (1963: 212). Most subsequent ethnomethodologists tend to opt for the latter characterisation, agreeing with post-empiricist realist philosophers that experimentation with social phenomena is radically misconceived. Garfinkel's (1984: 49,38) account of his studies is ambiguous, oscillating between, on the one hand, presenting them as experimentally based discoveries: 'there were several entirely unexpected findings'; and on the other hand as: 'not properly speaking experimental', just '"aids to a sluggish imagination"'.

Drawing upon the studies of experimentation cited above (note 4), I propose that there is no principled reason to deny experimental status either to Garfinkel's or to Milgram's investigations. If Garfinkel's and Milgram's studies are to be denied experimental status they must be seen to lack whatever it is that constitutes authentic experimentation. Authenticity depends on whether or not a putative candidate for experimental status is, in key respects, 'the same as' a true experiment. Such a judgement will depend on the criteria for what is to count as 'the same' or 'different'. Hence the question is: 'how do we compare these experiences; what criterion of identity do we fix for their occurrence?' (Wittgenstein, 1968: §322). Notice that Wittgenstein's formulation emphasises that it is we, (that is, we who are engaged in a comparative exercise), who must decide the criteria of identity. We should not assume that objects or experiences somehow embody their own criteria of identity independently of our interpretive work.

One of the most interesting 'findings' of ethnographic studies on experimentation is described by Mulkay (1985: 134) as follows: 'the sameness/difference attributed to two or more experiments depends on interpretive work carried out by the scientists concerned'. Natural scientists' judgements of sameness/difference 'often derive from, or at least vary with, their views about the scientific phenomena under investigation' (ibid.). If this is how natural scientists establish experimental validity, then I suggest that it begs the question to decide that investigations of human activity cannot be experimental due to 'ontological' differences between the social and natural realms. The claim that neither Garfinkel's nor Milgram's studies were properly experimental depends on the 'interpretive work' which is needed to show how and why they differ from the real thing.

My discussion above of Harré and Mixon depicts some of the 'interpretive work' through which they deny experimental status to Milgram's study. In opposition, I have myself engaged in 'interpretive work' (appealing to the 'interpretive work' involved in denials and affirmations of replication) in order to reach a quite different assessment. Disputes in the natural sciences often revolve around the question of whether a phenomenon has been discovered, recorded and measured or, on the contrary, whether it was produced by the experimental procedure itself (see Collins, 1985; Latour & Woolgar, 1986). This question of 'artefactuality' is precisely the bone of contention in Milgram's study - and also informs my reinterpretation of Garfinkel's counselling experiment. My point is that the 'ontological picture' of the reflexive individual is not simply a representation of human reality; it is, rather, the product of the interpretive work of its purveyors.

I have not sought to provide a comprehensive defence of Milgram's experiments, nor would I want to defend all of his interpretations and conclusions. Instead I have sought to confine myself to the rebuttal of a priori arguments against the very possibility of experimentation in the social sphere. Experimental investigation has many more applications than the forms of experimentation to be found in physics and chemistry laboratories. Both Milgram's and Garfinkel's 'experiments' are, I believe, marvellously ingenious and do indeed advance our understanding of certain aspects of modern social life. But I would not describe this contribution to understanding (pace Milgram himself) as 'scientific'. Milgram's experiments consisted in an investigation into the effects of scientific authority itself (they had an essentially 'reflexive' purpose). The kind of understanding afforded by these experiments is not to do with the positivist goal of precise measurement, prediction and control; it is, rather, more akin to hermeneutical insight. We are provided with a reflective glimpse into our own modern form of life.[17]

I go on now to examine ethnomethodology's self-conception of its own theoretical practice.

The Reflexivity of Accounting

Central to Garfinkel's picture of social and psychological reality is the notion that 'the activities whereby members produce and manage settings of organised everyday affairs are identical with members' procedures for making those settings "account-able"' (Garfinkel, 1984: 1 - my emphasis). This image of the identity between social order and members' methods for making the various features of social order knowable to one another is the ethnomethodological phenomenon of 'reflexivity'. The primary reality for ethnomethodology is conceived praxiologically, as members' 'accounting practices' (ibid.: 9), through which social life is organised. Garfinkel 'respecified' the concept of social structure so as to signify features of social life that are 'known' to all competent members.

The notion of 'reflexivity', and the use of 'reflexive' as a prefix in descriptions of action, often seems to carry a bewildering array of subtly shifting meanings. Some of the confusion is due to a failure to distinguish two different forms of reflexivity at work in Garfinkel's classic text. These two forms, 'the essential reflexivity of accounts' (ibid. 7) and 'the reflexivity of the actor' (Heritage, 1984: 31) signify rather different theoretical concerns (see Czyzewski, 1994). The 'reflexivity of accounts' is a 'reminder' to theorists that all accounts of an activity are reflexively tied to the practices in which such activity is made accountably-observable. Thus, the principle of 'ethnomethodological indifference' means that ethnomethodologists treat all accounts of social structures and processes, whether those of lay members or professional analysts, as no more nor less than members' situated accounting work (Garfinkel and Sacks, 1986: 166).

[17] See Miller (1995) on the 'hermeneutical' significance of the obedience experiments.

However, whereas the 'reflexivity of accounts' points to a contextual embeddedness which no account can escape, the 'reflexivity of actors' is a theoretical generalisation referring to the nature of individuals as such and their relation to 'objectivity'. This theoretical proposition is neither 'reflexive' nor 'indexical'; on the contrary, it is easily assimilated into post-empiricist 'ontological pictures' of human nature - for example, (Giddens, 1990: 36) asserts that: 'there is a fundamental sense in which reflexivity is a defining characteristic of all human action'. And it is not only 'constructive' social theorists who express Garfinkel's 'findings' in this 'ontological' idiom; many ethnomethodologists exemplify the same trait. For example, Jayyusi (1991: 236) says that Garfinkel 'uncovers, through his "breaching experiments", the irremediably normative foundations of intersubjectivity and of the very possibility of concerted action and intelligible discourse'. But the ontological claim about actors' inherent reflexivity clearly contradicts the principle of the reflexivity of accounts, in that the latter principle prohibits all ontological, universalist and ('constructive') theoretical propositions. Although both forms of reflexivity are present in the 'classical' phase of ethnomethodology, most subsequent social theorists and many ethnomethodologists (Czyzewski, 1994) concentrate almost exclusively on 'the reflexivity of actors', thereby neglecting the 'reflexivity of (their own) accounts'.

There is clearly considerable tension between the two uses of reflexivity because, according to the principle of the 'essential reflexivity of accounts', propositions concerning the 'reflexivity of actors' must also be irreparably 'indexical'. This being so, Garfinkel's and Sacks's (1986: 160) treatment of Durkheim's proposition that '"the objective reality of social facts is sociology's fundamental principle"' applies equally to the ethnomethodological notion of actors' reflexivity. The indexicality of Durkheim's 'fundamental principle' means that, 'according to occasion', it may be understood variously as: 'a definition of Association members' activities, as their slogan, their task, aim, achievement, brag, sales pitch, justification, discovery, social phenomenon, or research constraint' (ibid.). Similarly, by the same standard, ethnomethodologists' 'fundamental principle' - the reflexivity of actors - is their 'slogan', 'achievement', 'sales pitch' etc.

The central 'reflexive' problem that I want to pose for ethnomethodology is this: what is the basis for, and status of, the claim that individuals are not cultural or judgemental dopes? This proposition is not just a policy recommendation about how to conduct empirical studies; its main function is to negate the image of social reality promulgated by classical social theory. In Garfinkel's Studies it is clearly not just an assumption; on the contrary, it is presented as the central finding. Thus, speaking retrospectively, Garfinkel (1991: 17) remarks that 'these phenomena were not suspected until the studies established their existence [and] provided the methods to study them'. Garfinkel's (ibid.: 10) 'respecification' of action and social structure as the 'local production and natural, reflexive accountability of the phenomenon of order' looks very much like a 'constructive' theoretical representation - which is exactly how ethnomethodology has been received by post-empiricist social theorists such as Harré, Giddens, Habermas and many others.[18] I do not think, as some indignant ethnomethodologists have implied (Button and Sharrock, 1991: 138), that critical social theorists have entirely misinterpreted Garfinkel and ethnomethodology on this. It is difficult not to read Garfinkel as offering a more accurate picture of the real nature of

[18] Habermas (1991: 100) refers to the 'more realistic picture [that] is drawn by ethnomethodologists'.

social order and the qualities of individuals than that given by classical social theorists.

Lynch (1992: 285) claims that Garfinkel's celebrated remarks on the 'cultural dope' are misconstrued if understood to be stating that '"the human agent" is active rather than passive'. In fact, says, Lynch (ibid.), Garfinkel is asking a question, namely: '"how is an investigator doing it when he is making out the member of society to be a judgemental dope?"'. Taking Lynch at his word, it is appropriate therefore, in the interests of 'reflexivity', to turn this question round and ask: How is Garfinkel (and other ethnomethodologists) doing it when he makes out the member of society to be a skilful, reflexive actor? How does the social theorist make accountable, what 'interpretive work', models, assumed knowledge, is s/he using, in order to present individuals as knowledgeable, reflexive agents? To what end, and for what purposes, does the theorist portray individuals in this way? Why has no ethnomethodologist pursued this line of enquiry? The reason for this absence, it seems to me, is that despite disclaimers to the contrary, ethnomethodologists invariably do hold ('unofficially') what Lynch (ibid.) calls a 'general theory of social action'.

If it were the case that Garfinkel's Studies do not formulate the theoretical generalisation that individuals are active, reflexive agents, but merely assembles some examples of practical action in which, in some respects, from a certain point of view, at a given time and place, some individuals are seen to act in a way which is incompatible with viewing them as cultural dopes - if only this were being claimed, then Lynch's objection would stand. Moreover, if this were the case, that is, if ethnomethodologists had no a priori conception of 'the agent', then the ethnomethodological approach would be quite compatible with holding that in some cases people might well behave like cultural and judgemental dopes - as in the kinds of situation contrived by Milgram for instance.

However, it is evident that Garfinkel's references to 'members' and 'members' accounting practices' are not restricted to certain kinds of situations. The theoretical tone is set on the first page of Studies in Ethnomethodology, where the 'central recommendation' is that 'the activities whereby members produce and manage settings of organised everyday affairs are identical with members' procedures for making these settings "account-able"', and that these 'practices consist of an endless, ongoing, contingent accomplishment. Whenever Garfinkel mentions his key concepts: 'members', 'accountability', 'local production of order', 'practical accomplishment', 'reflexivity', etc., there is no limitation of scope, nor examples of phenomena which do not fit the typifications.

It has often been observed that ethnomethodology embraces a theoretical and methodological position which is very close to that of the later Wittgenstein. One of Garfinkel's principal aims was to escape from the metaphysics and transcendentalism of classical social theory. Because Wittgenstein's 'reflexive' critique of his own Tractarian philosophy was similarly motivated, I end this paper with a brief examination of his 'reflexivity' in relation to Garfinkel's.

Wittgenstein's Reflexivity

In the Tractatus Wittgenstein struggles to say 'how things are' in the world. He thinks the doctrine of solipsism contains an important truth: 'what the solipsist means is quite correct; only it cannot be said' (1988: 5.62). Nevertheless, Wittgenstein (ibid.: 5.64) goes on to try to indicate what it is about solipsism that is so profound: 'solipsism, when its implications are followed out strictly, coincides with pure realism. The self of solipsism shrinks to a point without extension, and there remains the reality coordinated with it'. Returning to the topic later, in the Blue Book, Wittgenstein (1972: 60-1) defines solipsism in much the same way as before - the solipsist is someone "who says only his own experiences are real", and "only I really see (or hear)". But Wittgenstein no longer thinks that this expresses a profound truth about 'how things are', whether sayable or only showable. The significance of solipsism now is seen to reside in the idiosyncrasy which makes an individual (philosopher) want to say such a thing.

Wittgenstein (1972: 57) points out that the solipsist has introduced a new 'notation', a new 'form of expression'. This is not objectionable in itself, so long as the speaker indicates how the new notation is to be used, and how it connects with the old way of speaking. For example, how is the distinction between real/false, simulated/natural, etc. to operate given that these tasks are managed perfectly well in the old notation? Wittgenstein (1972: 59) says: 'why shouldn't we grant him this notation?' - but immediately adds: 'in order to avoid confusion he had in this case better not use the word "real" as opposed to "simulated" at all, which just means that we shall have to provide for the distinction "real" / "simulated" in some other way'.

This new, 'reflexive' stance is very much like the ethnomethodological policy of treating theorists' (but not, as we have seen, their own) categories and concepts as a 'topic' rather than a 'resource'. The policy is exemplified by Latour's and Woolgar's Laboratory Life (1986: 179), in which they note that scientists use the epistemological categories of 'realist, relativist, idealist, transcendental relativist, sceptic, and so on' as resources in promoting the scientificity of their own theories and, at the same time, as resources to criticise the artefactuality, confusion, and incompetence of competitors' work in the field. Thus, just as Latour and Woolgar do not accept the idea that 'reality' (as a kind of 'super-order' [Wittgenstein, 1968: §97]) explains why some theories are successful and others not, equally, Wittgenstein now thinks that the solipsist has not said anything about 'how things are', but rather, is 'reflexively' exhibiting something (quite odd) about their own practice.

Like the solipsist, Garfinkel's statements are presented and received as a new discovery about the true nature of social reality: 'ethnomethodological studies contribute to deeper understanding of the nature and role of rules, rationality, and agency in social life' (Pollner, 1991: 371). But on Wittgenstein's (1972: 57) later view, theoretical generalisations, including those formulated by Garfinkel, are above all symptomatic of the desire for 'a new notation'. The exceptionless generality of Garfinkel's use of his key terms - the practical accomplishment of order is 'everywhere, always, only, exactly and entirely, members' work' (1991: 11) - exemplifies what Wittgenstein regards as a 'metaphysical' use of language. Another of Wittgenstein's examples of a metaphysical proposition is the empiricist's claim that "a man's sense data are private to himself" (1972: 55). Garfinkel's assertions on 'practical accomplishment', 'reflexive interpretation', etc., actually embody the same grammatical form as the empiricist's claim, and are therefore, on Wittgenstein's view, metaphysical propositions. Rather than referring to an enquiry-independent reality, metaphysical propositions express certain attitudes and dispositions of the writer or speaker.

Garfinkel's insistence that all aspects of social order are 'everywhere, always' skilful and reflexive accomplishments of interpreting-agents looks like an empirical proposition, but it is being used in the same exceptionless way as the empiricist's claim on the essential privacy of sense data. Garfinkel, and other ethnomethodologists, 'are going to apply this picture come what may' (Wittgenstein, 1976: 411). As Wittgenstein says, there is nothing per se wrong with idiosyncratic notations. However, such notations are rarely just an unusual way of talking; the user of a 'metaphysical' notation is irresistibly tempted to make exaggerated claims about reality. Thus ethnomethodologists are prone to reading skilful, reflexive interpretation into situations where no such thing is going on. Like psychoanalysts who claim to have discovered the previously unknown phenomenon of 'unconscious thought' (Wittgenstein, 1972: 57), ethnomethodologists conflate their creation of a new mode of representing human action and social order with psychological and social reality itself.

Conclusion

Although I have not argued the case positively here, I believe that the most useful service that ethnomethodology can provide is the deconstruction of ontological pictures, not the provision of new, 'improved' ones. My argument has been directed at the ontological picture of the 'reflexive agent' bequeathed (albeit unwittingly) to contemporary social theory by ethnomethodology. The 'dopes' of social theory today are no longer structural and cultural 'puppets' - they are active, creative, skilful, 'reflexive' dopes. I say 'dopes' because the 'actors'/'agents' in contemporary social theory are just as much a theoretical abstraction as their classical ancestors (cf. Lynch, 1992: 285). Yet the construction of ontological pictures is a practice which is quite incompatible with ethnomethodology's radical principle of the 'essential reflexivity of accounts'. This principle, when followed through, leads to critical reflection on the construction and maintenance of all ontological pictures, including ethnomethodology's own favourite picture.

In my view we should not say either that individuals are essentially cultural and judgemental dopes, or that they are essentially reflexive, interpretive actors. Rather, we should look and see at which times, in which places, doing which kind of activities, which kind of people (etc.) are behaving 'reflexively', 'dopily', or some combination thereof. I emphatically reject the view, which is so enthusiastically promulgated by post-empiricist critical social theorists, that portraying individuals as reflexive agents is ipso facto a critical intervention in social and political affairs (see Pleasants, 1998/9). Equally, I reject the converse of this position, namely the charge that any portrayal of individuals behaving non-reflexively is ipso facto conservative reactionism.

Acknowledgements

I would like to thank the organisers for their invitation to speak at the 4th Mind and Society Seminar, at which I received marvellous hospitality. I would also like to thank all the participants - most of whom disagreed strongly with what I had to say - for many stimulating conversations. I am extremely grateful for the financial support which I received from the British Academy and Hughes Hall, Cambridge. Special thanks again to Mark Peacock.

Bibliography

Bhaskar, R. (1979). *The Possibility of Naturalism*. Brighton: Harvester Press.

Bloor, D. (1992). Left and Right Wittgensteinians. In Pickering *Science as Practice and Culture*. University of Chicago Press, pp. 266-82.

Button, G. (1991). Introduction: Ethnomethodology and the Foundational Respecification of the Human Sciences. In G. Button (ed.) *Ethnomethodology and the Human Sciences*. Cambridge: University Press, 1-9.

Button, G. and Sharrock, S. (1991). The Social Actor: Social Action in Real Time. In Button *Ethnomethodology and the Human Sciences*, pp. 137-75.

Button, G. and Sharrock, S. (1993). A Disagreement over Agreement and Consensus in Constructionist Sociology. *Journal for the Theory of Social Behaviour*, 23, (1), 1-25.

Collins, H. (1985). Changing Order: Replication and Induction in *Scientific Practice*. London: Sage.

Czyzewski, M. (1994). Reflexivity of Actors versus Reflexivity of Accounts. *Theory, Culture & Society*, 11 (4), 161-8.

Garfinkel, H. (1963). A Conception of, and Experiments with, "Trust" as a Condition of Stable Concerted Actions. In O. Harvey (ed.), *Motivation and Social Interaction*. New York: Ronald Press, pp. 187-238.

Garfinkel, H. (1984). *Studies in Ethnomethodology*. Cambridge: Polity.

Garfinkel, H. (1991). Respecification: Evidence for Locally Produced, Naturally Accountable Phenomena of Order, Logic, Reason, Meaning, Method, etc., in and as of the Essential Haecceity of Immortal Ordinary Society (1) - an Announcement of Studies. In Button *Ethnomethodology and the Human Sciences*, pp. 10-19.

Garfinkel, H. and Sacks, H. (1986). On Formal Structures of Practical Actions. In H. Garfinkel (ed.) *Ethnomethodological Studies of Work*. London: Routledge, pp. 160-93.

Giddens, A. (1984). *The Constitution of Society*. Cambridge: Polity.

Giddens, A. (1990). *The Consequences of Modernity*. Cambridge: Polity.

Greenwood, J. (1991). *Relations and Representations*. London: Routledge.

Habermas, J. (1991). *The Theory of Communicative Action vol. 1*. Cambridge: Polity.

Harré, R. (1979). *Social Being*. Oxford: Blackwell.

Harré, R. (1993). *Social Being (2nd edn.)*. Oxford: Blackwell.

Harré, R. and Secord, P. (1972). *The Explanation of Social Behaviour*. Oxford: Blackwell.

Heritage, J. (1984). *Garfinkel and Ethnomethodology*. Cambridge: Polity.

Jayyusi, L. (1991). Values and Moral Judgement: Communicative Praxis as Moral Order. In Button *Ethnomethodology and the Human Sciences*, pp. 227-51.

Knorr-Cetina, K. (1983). The Ethnographic Study of Scientific Work: Towards a Constructivist Interpretation of Science. In K. Knorr-Cetina & M. Mulkay (eds.) *Science Observed*. London: Sage, pp. 115-40.

Latour, B. and Woolgar, S. (1986). *Laboratory Life*. Princeton University Press.

Lynch, M. (1992). From the "Will to Theory" to the Discursive Collage: A Reply to Bloor's "Left and Right Wittgensteinians". In Pickering *Science as Practice and Culture*. University of Chicago Press, pp. 283-300.

Lynch, M. (1993). *Scientific Practice and Ordinary Action*. Cambridge: University Press.

Lynch, M., Livingston, E. and Garfinkel, H. (1983). Temporal Order in Laboratory Work. In Knorr-Cetina & Mulkay *Science Observed*. London: Sage, pp. 205-38.

McCarthy, T. (1994). Philosophy and Critical Theory: A Reprise. In D. Hoy & T. McCarthy, *Critical Theory*. Oxford: Blackwell, pp. 7-100.

Milgram, S. (1974). *Obedience to Authority*. New York: Harper & Row.

Miller, A. (1995). Constructions of the Obedience Experiments: A focus upon Domains of Relevance. *Journal Of Social Issues*, 51 (3), 33-53.

Mixon, D. (1972a). Behaviour Analysis Treating Subjects as Actors rather than Organisms. *Journal for the Theory of Social Behaviour*, 2 (1), 19-31.

Mixon, D. (1972b). Instead of Deception. *Journal for the Theory of Social Behaviour*, 2 (2), 145-77.

Mixon, D. (1989). *Obedience and Civilization*. London: Pluto Press.

Mulkay, M. (1985). *The Word and the World*. London: Allen & Unwin.

Pickering, A. (ed.) (1992). *Science as Practice and Culture*. University of Chicago Press.

Pleasants, N. (1996). Nothing is Concealed: De-centring Tacit Knowledge and Rules from Social Theory. *Journal for the Theory of Social Behaviour*, 26 (3), 233-55.

Pleasants, N. (1998/9) *Wittgenstein and The Idea of a Critical Social Theory: A critique of Giddens, Habermas and Bhaskar*. London: Routledge (forthcoming).

Pollner, M. (1991). Left of Ethnomethodology: the Rise and Decline of Radical Reflexivity. *American*

Sociological Review, 56, 370-80.

Rouse, J. (1987). *Knowledge and Power: Towards a Political Philosophy of Science*. New York: Cornell University Press.

Rouse, J. (1996). *Engaging Science: How to Understand its Practices Philosophically*. New York: Cornell University Press.

Wittgenstein, L. (1968). *Philosophical Investigations*. Oxford: Blackwell.

Wittgenstein, L. (1976). *Cause and Effect: Intuitive Awareness*, Philosophia, 6 (3-4), 409-25.

Woolgar, S. (1988). *Science: the very Idea*. Chichester: Ellis Horwood.

Discussion of Pleasants

RW You eventually made the distinction , ... between reflexivity of actions and reflexivity of actors. I think that is an extremely crucial and central distinction.... You seem to privilege the reflexivity of actors, especially in the first part of your paper. It struck me that there is a real problem with that, at least in my reading of Garfinkel and what's central to his work, there is only a conception of reflexivity in terms of accounts and accounting practices. That they elaborate the circumstances that they are a part and are elaborated by them and all that litany. That is in my opinion very important and crucial. What I do not see, is much concern with this business of reflexivity of actors, and I think you gave the game away a little bit by having to resort to Heritage, (laughter) in order to warrant your concern in this respect. But it is to be noted that Marek Czyzewski pretty much demolished Heritage's characterisation of ethnomethodology in terms of that version of reflexivity a couple of years ago in *Theory, Culture and Society*.

So I think you are looking at different characterisations of reflexivity; one of which is massively present in Garfinkel's work, another which is arguably present, and if so peripherally so. Also, it was a strange move to deal with perhaps a rather early and unformed study of Garfinkel, - *The Breach Experiments*. I have always tended to see this stuff in terms of what Kenneth Burke calls "a perspective by incongruity". I think Garfinkel was trying to do something along those lines. But none of Garfinkel's propositions about the nature of action are very well elaborated in that study. I think that his central concerns with reflexivity, that of reflexivity of accounts and accounting practices, is to be found much better in his later studies. In the 'pulsar studies', you could show his notion of reflexivity operates massively to organise what his analysis is about. So I'm saying that you have needlessly brought in the notion of reflexivity of actors. I understand how you may have done that, not just owing to the Heritage problem, but you focus far too much on the property of actors in the early part of your paper, and there is no strong warrant for treating Garfinkel's work in those terms.

NP What the paper is about is my saying 'yes' to social reflexivity of accounts and 'no' to reflexivity of actors. That in a nutshell is my attitude to ethnomethodology. The "essential reflexivity of accounts".., the most important reflexive relation between accounts and what accounts are about - not so much this reflexivity of accounts as operative out there in the world between other individuals - but between the theorist and what the theorist is trying to say. That's what I'm really interested in. But the main contention we have is whether in Garfinkel's early texts or in a subsequent ethnomethodology, is there this conception of the reflexive agent - as a theoretical generalisation - as an ontological picture...

RW Is it present? Is this a Straw Man that you have addressed?

NP Well! I thought there are a number of quotations I read from Garfinkel here that (suggest) there is one.

There are two possibilities, one that there is this picture of reflexive agent in the early texts (of Garfinkel), and then that's dropped and the work moves on. In response to that I refer to the quotation from Graham Button's book in *The Respecification Article* (1991), where he says something like: "These phenomena were not suspected until the studies established their existence." This is the language of: 'Here is a phenomena out there in the world and we have found a way to talk about it.' I am not really sure how I can argue that there is not this strong (exception) to the reflexivity of the actor in Garfinkel. I know Heritage is probably the most ideal type of someone who does take this view, and the only reason I mention him is because the 'reflexivity of the actor', the phrase which summarises it, is his phrase. I simply quoted him. It is for me an accurate characterisation of a very important aspect of ethnomethodology. What I'm concerned with is not so much a critique of ethnomethodology, as such, but the influence ethnomethodology has had ... on critical social theory - Giddens, Habbermas, Harre and a number of others. This picture of the reflexive agent is the one most people understand is in ethnomethodology.

On the question of intention - I think in Mike Lynch's last book, you see it very clearly; I think he wants to say that this conception is an abstraction - it is the thing that grand sociological theory is made of. That said, but in many other places you fall back on proto-ethnomethodology language where you talk about actors ... that are all inherently reflexive interpreters and there is no such thing as a cultural dope. So I'm not quite sure how to prove or establish my claim that it is not a straw man. Although I do accept that there is more to ethnomethodology than this. This is the bit I want to get rid of.

JC Well, the first thing I'd like to say is that I wish we had had critics of your calibre twenty-five years ago. Although, I think Rod Watson is right; you are reading Heritage's Garfinkel into Garfinkel ... There are many arguments in your paper which should stand and be addressed independently of that. What concerns me is that you see ethnomethodology as wedded to some sort of ontological picture of the agent, as skilful (and) artful. There is something to it, but when you characterise the depiction of agents as practical actors, it seems to me that that is not really an ontological claim. It's a common-sense observation that we begin with, that above all people face practical exigencies. If you deny that, it seems to me, you are not denying a theoretical argument, you are just talking nonsense. You have to accept that as the basis of any kind of depiction of human affairs - it just seems to me uncontentious.

Now the trick is to put that to sociological work - to make that sociologically sensible. To follow through on what that common-sense depiction would mean for doing sociology,... which is notoriously absent from a great deal of classical social theory. Hence the cultural dope stuff; because they (classical social theorists) don't take seriously the local practical exigencies of conduct in a rigorous way. That's why so many of us are interested in

Harold's achievements. But that is an ontology I don't really think we need buy, because that presupposes we have a theoretical starting point. And the issue for ethnomethodology is that we are not interested in theorising the problem. We are interested, like Wittgenstein, (in) starting from everything before our eyes, without need for theoretical lenses. And then following through the implication remorselessly no matter where they take us, that human beings have to face practical circumstances, and really confront what that amounts to for doing sociology.

NP I often find there is a bit of a disjuncture between what ethnomethodologists do; which I'm impressed with and I don't want to criticise at all, and what they say about what they do. The kind of ethnomethodology which grabs my attention, is this kind of social theoretical discourse where ethnomethodology pits itself against other forms of constructive classical social theory. And there, ethnomethodologists are tempted, by wanting to enter into that discourse to challenge those who received pictures, (and then) we start seeing more positive depictions of interpretive, skilful, infinitely creative people. I think there is a difference between characterising the practical exigencies with which people are confronted any moment and depicting that as a reflective interpretive achievement. I recall in your own book..., where you are talking about cognitive psychology and the way they want to intellectualise something as ordinary as recognition. You know - we have to recognise something as an apple before we see it - and you say that's nonsense. And if you take that view, then you are taking out a distinction, rather like Wittgenstein arguing against real and simulated. So it is only on certain occasions that we actually go through the activity of recognising.

JC The accomplishment. It is not an act! Recognising is not an act.

NP Well it is sometimes - when we are recognising!

JC We try to recognise, that's all!

NP It seems to me that there is a direct symmetry between your complain about postulating a continual process of recognition - when we are merely seeing - and the tendency to always postulate that: what people do is interpretive, skilful, creative.

And what I don't like about that is that you take away the possibility of noticing when people are not behaving interpretively; reflexively; skilfully and creatively. As, for example, the majority of the subjects in the Milgram experiments.

Some of those subject (about 35%) who didn't buy the deception - these were skilful, reflexive interpretations; the ones that went ahead to (apply) 450 volts, these were cultural dopes. So I say this is an empirical question and shouldn't be an apriori characterisation of activity as such.

WS I'd just like to interject that you don't hate the reflexive interpretive actor anywhere near as much as I do! (Laughter)

DF I liked very much your disparagement of post-empirical social theory (laughter).

But I was unhappy about the notion that we could have a distinction between cultural dopes and reflexive actors; as though it was an empirical distinction. I agree that one would not necessarily want to have an ontology of these things; I am not sure I would want an empirical theory either. That seems to be what you are offering us.

NP No, I just want us to acknowledge there are at least two possibilities, and of course many more.

DF Yes, but the Milgram experiments are actually very poorly described. The problem with Milgram is not deciding whether his subjects were cultural dopes or reflexive agents, but with the very poor ethnographic quality of the description of the experiments, such that you can never make that judgement. If we had Milgram here I'd want him to tell us much more about what was going on. That seems to me the problem. If you start to play around with these things as though they are empirical categories; that seems to buy into a whole bunch of things that I wouldn't want to buy into. As an ethnographer you certainly have no basis for making those kinds of judgements in the descriptions that Milgram gives you because they are so poor.

DR What hinges here is the notion of a methodology or a method. It is after all ethnomethodology and the presumption (is) somehow any method entails a strong ontological claim. I find it bizarre that you would suggest that in the Milgram experiment there is a category of person there, the people that refused the argument. And those people are somehow behaving skilfully, because skilfully in that conception does involve some kind of ontological claim and that (other) people are somehow behaving as cultural dopes in contrast.

It seems to me that a sensible ethnomethodological position would not argue that at all. It would argue that methodologically one would want to see all accomplishments here as skilful - whether or not they decide they want to support Milgram's push to 'obedience' or they decide they want to refuse it. In either instance there is a skilful, in one sense but not in another, behaviour going on. It may seem that skilfulness itself entails some sort of ontological claim, I would argue it doesn't. It is simply a methodological choice.

NP To a large extent my arguments and your argument depend on whether Milgram's experiments show roughly what he claimed they showed. In my view what Milgram found (was) that if you put people into a situation like that (of the experiments) most of them behave in a shockingly immoral way, as he put it. As an ordinary ethical actor, someone who has opinions and moral reactions to things, I want to be in a position to say I wouldn't be one of those 65%. I think interpretation is a skilful process and an intentional one. Interpretation entails the desire to discover something, to discover the meaning of something; now I think those 65% who obeyed were not involved in any interpretive exercise at all. They simply obeyed, they did what they were told to do. But those who didn't go on were really acting intentionally, searching, actively interpreting what this situation was about and what they should do, and I want to say they were doing something qualitatively different.

DF But you have got no more grounds

for claiming that than the interpretation you criticise.

You're saying these people must have been behaving intelligently. Well, who the hell knows?

NP We know from the result of the experiment.

RR No we don't. You have ( ) the experiment in the way you have decided to interpret it. Moreover, given that we have good grounds for thinking that the experiment was thoroughly immoral. Why should we trust such an immoral experiment, to tell us anything?

*

*

*

NP We (have) got into a bit of sticky point over the Milgram experiments. How would we like to characterise the Nazi atrocities? Are we prepared to call the S.S. officers, who did what they did because they felt that this is a perfectly legitimate thing to do - given the ideology of, the Nazi characterisation of the people in the world.

Obviously, I think, we agree they shouldn't have done that. Never the less they did. The question then is, how the hell did they come to do that?

Isn't a reasonable characterisation to be to call these people 'cultural dopes'? Or something like that?

JL Two very brief points - One problem may come from the idea: whether people are, or are not, cultural dopes. One can take up the issue of reflexivity, and one can do so, in such a way as to miss the main methodological thrust of ethnomethodology and its platform. Its platform surely is that, and the experiments, or as I like to think of them - demonstrations, show that order, as we understand it, is produced within the scenes and settings that we desire to investigate. That is, to put it in another way, order is self-organised - our scenes and settings are self-organised - this then is what gives us the program. The program is, given that they are self-organising - how can we look at how these scenes are accomplished, to see the self-organising being done. This then gives us the methodological implications to treat what persons are doing as practices, artful and whatever. But, of course, given that step, that it's a methodological (election), you may have some points as to the issue of a relationship between a methodological implication and how people are. But that's a rather different issue. The second thing I don't see the issue of the Milgram experiment having any relationship, because this isn't a demonstration about the nature of order. If I may conclude - what Milgram seems to show (to me) is that people can be bloody immoral, pretty evil. MY GOD, I KNEW THAT ANYWAY! People have slaughtered 200 bloody million people ...

NP I think Milgram produces insight (showing) something much worse (than) the long history we have of evil. That is what Hannah Arendt call the banality of evil!

JL But we've read those histories of Germany - these are ordinary people, like you and me that have done these things. He doesn't tell me any more about that than I knew. These are ordinary decent people in so many respects, but what they did was evil.

To explain how it comes to be, look at the history of Germany.

NP I agree with you - if you can see that without Milgram fine. I think probably I saw it before Milgram as well, but I felt reflecting on what went on in those experiments, I actually had some greater empathetic insight into how Nazism (and others) did it.

JL So did I, because I thought it shows that experimenters can do it as well!

NP Exactly.

*

*

*

WS The problem is you have overlooked the fact that 'cultural dope' is not a person - any kind of person - it is a model in standard sociological theories. The 'cultural dope' is the creature in the sociological theory who is not equipped with enough grasp of practical affairs to put his socks on and get out of bed. (Laughter) This is the 'cultural dope'! For sure ethnomethodologists know there are real dumb people and real smart people, and there is a difference between those. And some ethnomethodologists go in for silly things, somebody not too far from here wrote a paper called 'Irony as a Methodological Convenience', which denounced these kind of tendencies to make-out ordinary things are amazingly skilful; 'artful practices' doesn't mean amazingly skilful things by any ordinary standards. It just means they are kind of ordinarily artful - in ways that you have to learn to do things - that it requires knacks and gifts and skills to get the spoon in your mouth and not dribble your food down you. (Laughter)

The notion - the 'Cultural Dope' - is not the common-sense division between smart and dumb people. It is a characterisation of a problem in theory of, if you follow the Schutzian homunculus and equip the actor with just what the theory allows it, then the sociological actor couldn't get out of bed and go and buy a loaf. The other thing I hear is the rather terrifying thing, that our superiority over the Nazis is that we are more interpretive than them. It doesn't seem to me that interpretive is ethnomethodology we are none of us interpretive; it is not a model of the interpretive actor at all. Sense-making is not interpretation in the hermeneutic sense at all. It is just practices of identifying things, knowing what they are, recognising when you see them and so forth. It's not an interpretive process at all. So it seems to me you have really missed the theoretical argument at that level about what it would take to make those theories work if you wanted to build them. And the only characterisation the (   ) with the cultural dope clashes with anybody you can see on the street. You can just make a contrast with those people and the 'cultural cope' couldn't do that. So what the cultural dope doesn't know is its own culture. That's John's point about the self-organising thing - if a society is self-organising people have to have enough grasp on the culture to get through the ordinary affairs; otherwise they wouldn't be getting through the ordinary affairs, and society wouldn't be like it is. So if you don't ascribe them enough of that grasp on the culture, you can't reproduce the phenomena of daily life that is patently going on before you. So it seems to me that that brings together a lot of the things that have been said.

NP I'm not using 'cultural dope' as a proper name; clearly I'm lifting it out of a certain context; I'm using it rhetorically; I'm using it to make a kind of judgement that I want to make about certain practices Garfinkel and Milgram engaged in. I take your point about the original criticism of the 'cultural dope' in classical social theory. I think it is a lesson that has been well learned. For example the history of Marxism went completely astray by holding to theories of ideology that portrayed a hopeless conception of what it is to be a set of people related to various class interests or whatever. Personally that is how I would like to contrast the 'cultural dope' with theories of ideology: that it was necessary in Marxism, at least, to understand that as you say the structures of capitalism simply wouldn't get reproduced if people were as ideologically bound as the classical Marxists said they were. That is a very important lesson to learn; I think ethnomethodology has had very beneficial effects like that. Unfortunately, I think the beneficial effects have been taken too far by contemporary social theorists. So I'm not trying to make war with ethnomethodology, but there is a part of it I'm unhappy with.

RR A lot of people have been saying it is important not to construe ethnomethodology, qua-methodology, as something else; i.e. qua-epistemology or qua-ontology. The important thing you do point out, among others, is the continual danger that an idea like ethnomethodology will ossify into social theory. Part of the way you point that up, is by noting the form that some of the remarks that ought to be understood as reminders of methodological injunctions can sometimes appear to be something different. And sometimes terms get used without opposition - a statement like "all actions are practical actions", something like that. Very important, a very real danger, it seems to me. But this is an ubiquitous problem, and in a certain sense there is no getting away from it. So if someone is uncharitable enough they can always hear the very best statements along these lines in a way which makes them problematic. So for example Wittgenstein's remarks of the kind you were quoting earlier, can also be heard as problematical and inclining towards ossifying into theory when they get used without opposition, and when they can look like the possible foundation of a jargon. I'm thinking of a term like "everyday", for example: "There is only everyday language." Someone could hear that and say: "Ah, well, that's obviously some kind of metaphysical claim!" Just to point out the danger you rightly point out is ubiquitous, and you are being to some extent uncharitable in some of the sentences you are reading.

NP I completely agree with nearly all that. Precisely the same misinterpretations apply to Wittgenstein. I think the radical program that Wittgenstein and Garfinkel propose about doing away with explanation/metaphysics is extraordinarily difficult to do. It is easy to state, but not so easy to follow. The couple of ethnomethodologists I have been influenced by on thinking about this distinction between reflexivity of accounts and of the actor is the Polish guy mention earlier (laughter) along with Pollner who talks about ethnomethodology should not be settling down in the middle. But both of these writers went on to do the very things they said they ought not to be doing - which is the ontological gerrymandering. Which shows how difficult it is to carry forward the radical promise of the programme, and giving up these metaphysical and transcendental assurances.