

NOTES

- 1 For some discussion of these theoretical developments, see Coulter, 1973a.
- 2 For further details on Russell's relationship to Wittgenstein, see Blackwell (1981).
- 3 Also see Wittgenstein (1968: paras. 80–4).
- 4 Ryle had also elaborated a similar analysis, Ryle (1949).
- 5 See, e.g. Searle (1969: 30) on 'promising' as analysable into both 'function-indicating device' and 'propositional content' even for an expression such as: 'I promise to come', where the former is specified as the component: 'I promise' and the latter as the ('elided') component: 'I will come'.
- 6 See for instance, Fodor and Katz (1963).
- 7 For a brief review and discussion, see Coulter (1983a). For a more elaborate overview, although informed by an adherence to 'linguistic pragmatics' as a point of reference, see Levinson (1983).
- 8 See various contributions to Tyler (1969). Cognitive anthropology, and the general issues that are involved here, are given detailed consideration by John Lee in chapter nine when he exclusively addresses the relationships between language and culture.
- 9 See Coulter (1974).
- 10 See McCawley (1981: 98–104) on standardly recognised properties of quantifiers.
- 11 For a fuller discussion of these properties of account construction in terms of the logic of categories, as well as for their relationship to *de re* and *de dicto* reportage, see Jayyusi (1984: Ch. 6).
- 12 See Coulter (1975).
- 13 See Coulter (1979b).
- 14 For some analysis of categorical open-texture as an argumentative resource, see Coulter (1990).

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Epistemology: professional scepticism

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1 Introduction

In our view, the epistemological arguments over 'objectivity and relativism', the relationship between 'commonsense and pure reason', the issue of 'a paramount and multiple realities', the relationship between 'objects and appearances' and other related epistemological issues in sociology and the human sciences seldom get beyond first base, not least because it is hard to get the lines of division identified well enough for there to be agreement on what are indeed the points of difference. Here we attempt a first base treatment of these issues by reverting to consideration of them in terms of Schutz's argument, and other basic phenomenological considerations. We do this because reasoned presentation of the issues in simple terms may help with the uphill struggle that, as Margolis (1986) observes, confronts anything that looks like a 'relativist' position – and we add, any which might be construed as 'subjective' in approach – because it will be typically presented by the opposition as blatantly stupid. Our objective is to display how Schutz, then Garfinkel and ethnomethodology, transforms the formulation of epistemological criteria into the topic of describing the properties of social organisation.

2 Examining social reality

It is a serious mistake to set philosophical scepticism on all fours and head to head with common sense understandings as though one straightforwardly and directly challenged the other. It is a usual characteristic of that scepticism that it seeks to operate at another level than the one on which our ordinary claims to knowledge get made. The epistemological sceptic, who denies that we can ever really know anything, has no interest in getting into dispute with someone who, say, claims to know where to find a good Chinese restaurant in a strange town, over whether they can in fact find such a restaurant. The philosophical sceptic is typically willing to grant that people do in the ordinary sense in which they make their claim, know what they say they do. The epistemological sceptic's case is that even when our ordinary standards of

knowledge have been fully satisfied so that everyone would normally agree that, indeed, in this case it is right to say that someone knows something, still there is reason to question whether, at a *stricter, more demanding, level*, we should want to allow that this is truly knowledge. Descartes (1971 edn), the initiator of modern philosophical scepticism, consistently maintained that he had no intention of raising doubts with any practical effect, that his were philosophical doubts, raised within the context of a distinctive form of investigation, the 'pure enquiry', which aimed to determine what could be established as certain by the wholly unaided power of thought, through the application of pure reason.

The way disagreements over 'objectivity' in sociology are expressed is such, it seems to us, that in important ways they often reproduce the mistake just described, with ethnomethodology being one unfortunate victim of confusion on this point. Reducing the diversity of sociological views to a dichotomy, the contrast between sociologies conceiving 'social reality as objective', and those conceiving 'social reality as subjective', provides a handy but potentially misleading categorisation. It *has been* seriously misleading insofar as it has encouraged the idea of ethnomethodology as being of the 'social reality is subjective' school which can be dismissed peremptorily because it results in – if it does not depart from – a scepticism which conflicts with the patent objectivity of social phenomena – perhaps even natural, physical phenomena – as we ordinarily experience these. Saying that social reality is 'subjective' supposedly means that people can do anything they like, that the only thing stopping them from flapping their arms and flying, for example, is that they do not try to fly strenuously enough, but they could do it if they really believed in their power to do so. Indeed, they will have succeeded if they really believe they have. The individual will is sovereign and can dictate the nature of reality. If this is what is meant by saying that social reality is subjective, then the simple exposition of the point of view can be relied upon to reveal its absurdity. A sturdy sense of reality tells us that it is nonsense to suppose that anyone can fly like a bird. The law of gravity and other laws of physics tell us they cannot do this. It is an impossibility. The applicability of the laws of physics is an objective matter, something very different from a matter of personal preference or of even the most determined conviction.

The mistake which is being made, then, is in pitching the opposition between the views of social reality as 'objective' and as 'subjective' at the level at which this could make a difference to what ordinarily observably goes on in the everyday world. At that level the 'subjective' predilection is notably disadvantaged, for its vindication would apparently require that people do the impossible – at least, that they do what we generally regard as impossible, and do it at will. Because of this we will rigorously dissociate ethnomethodology from this idea of what saying 'social reality is subjective' means for this idea is indeed implausible. Continuing to use it as though it contrasted with our everyday use of 'objective' will make it seem that we are indeed setting out to

dispute (amongst other things) the laws of physics. At the very least, continuing compliance in such a usage gives other sociologies the opportunity to arrogate to themselves a position to which they are not entitled, namely, that making them the distinctive and staunch defenders of the reasonable and plausible. Who, just by taking thought, can add one cubit to their stature?

In the light of the role that phenomenology has played as inspiration to ethnomethodology, some adversion to Husserl's own project is relevant and spells out this point. It was surely not Husserl's (1970a and 1970b) aim to make the truths of logic, mathematics and science 'subjective' in the sense that they could be treated in a cavalier way, with people arbitrarily deciding, if they felt like it, to accept that two plus two equals four, but refuse to assent to two plus three equals five. If the objectivity of mathematics is manifest in the fact that two plus two *does* equal four, regardless of whatever personal preferences any one might have, then Husserl does not contest the objectivity of mathematics.

The same applies to the findings of science. Husserl is not out to challenge or restrict the universality of the law of physics as we *ordinarily* understand those. In other words, it is integral to those laws that they apply across the board, not in random ways, here but not there, to you but not to me. If *this* is what the objectivity of the laws of physics consist in then, again, Husserl leaves this intact. The crux of Husserl's project was, as it has been for many twentieth-century philosophers, to understand the nature of logic, to comprehend the inexorability of its supposed laws. Husserl concurred in a widespread view that these were iron laws, ones which 'are more strict, more coercive, more general, and *in their sense* more unalterably "objective" than any of the generalizations of science or everyday common sense' (Eddie 1987: 37).

This is hardly the premise which should commit a project to the conclusion that everything is open to a 'take it or leave it' treatment, that matters are within the arbitrary discretion of each individual. On the contrary, Husserl's project is to understand, *not contest*, the 'objectivity' of (especially) logic, but also mathematics, science and so forth, and the target of his opposition is not the truths of mathematics or the findings of science, but (amongst others) the 'objectivist' interpretation of these. For Husserl the problem with objectivism is its *philosophical starting point*. It tends to take the findings of science and the suppositions of common sense at face value to the extent of assuming with them that the external world is already there, already given, but this, for Husserl, is to take for granted the very thing that ought to be up for *philosophical* inspection. Not, however, because there is a need to take a sceptical attitude toward the existence of the external world but because of the philosophical necessity to demand the full and explicit justification of assumptions, meaning that one cannot take the assumption of the givenness of the external world as the unexamined basis for a philosophy. A thorough philosophy must distance itself from the assumption, *suspend* (not deny) it, the proper task being to understand the sense of the supposition, to understand what it

consists of. Starting from there, it ought to be clear that at the level of our ordinary experience the phenomenological investigation *makes no difference* to the ordinary experienced objectivity of social or natural phenomena, even in their character as exhibitions of the inexorability of the logical 'must', or invariance in the application of natural laws.

But Husserl's phenomenology *does* lead toward what is very widely – and, surely, rightly – called 'subjective idealism', does it not? Well it may, so long as it is borne in mind that in this context 'the subject' is a 'transcendental ego' that we *ultimately* encounter only through the rigorous and persistent application of the method of phenomenological reduction, through the suspension of ever-deeper layers of presupposition, and that this ego is very different from the empirical persons we encounter in the course of, and as the focus of, sociological and psychological studies. It is only at this remote level, and *only at this level*, that it is really possible to say that (social) reality is subjectively constituted in this sense, a level which is very remote from that at which people ordinarily talk of the objectivity of the law of gravity, the truths of mathematics or the impersonality of social arrangements (and a level to which many phenomenologists – including Alfred Schutz – have declined to follow Husserl). The undeniable fact that ethnomethodology has drawn more or less directly upon phenomenology does not entail that it must, in order to be sufficiently consistent with Husserl's work for its own legitimate purpose, be itself identified as a form of 'subjective idealism'. Insofar as ethnomethodology operates at the level of *sociological* investigation, it is a long way indeed from the level at which such a characterisation would be relevant, and, therefore, any reading of it which tries, on the grounds that phenomenology is, sooner or later, a *metaphysical* subjectivism, to project ethnomethodology as 'subjectivist' in this way at the level of our commonplace experience, is misguided.

3 Common sense and pure reason

The adoption and adaptation of Alfred Schutz's work made a crucial contribution to ethnomethodology's initial formulation, and it is, therefore, relevant and helpful in the development and clarification of the theme of the 'objectivity' of social phenomena to return to Schutz's arguments and their connection with the Cartesian method of systematic doubt, the basis of modern philosophical scepticism.

In Cartesian terms, true certainty exists only where there is no *conceivable* possibility of doubt (given, of course, that one recalls that this standard is seriously applicable only in the context of the 'pure enquiry'). Possession of certainty on this scale requires that every *conceivable* possibility of doubt be eradicated – which in turn requires, of course, that every conceivable possibility of doubt be identified, and so the attempt must be actively made to doubt everything to determine whether doubt proves to be impossible in the sense of being inconceivable.

Alfred Schutz's reflections (Schutz, 1962, 1964 and 1966) take note that the organisation of *practical action* cannot proceed on the basis of systematic doubt.² Some other auspices must be identified as those under which action-in-society takes place. The systematic application of the method of doubt would (at least on Cartesian assumptions) result in complete paralysis, and not just to practical action but even to philosophy itself, as the arguments of the sceptic show.

If a philosophically rigorous demand for certainty is unrelentingly applied, action will be interminably postponed whilst all conceivable possibilities of doubt are identified and thoroughly investigated. The Cartesian method is, then, utterly corrosive, for it opens up endless possibilities of doubt and at the same time undercuts the very procedures which could close them down. Within the context of ordinary affairs there is sometimes the possibility that something is not as it might appear to our eyes, a possibility which the Cartesian method ruthlessly generalises, but in *that* context (i.e. that of everyday affairs) we *can* very often resort to closer or more careful visual inspection to determine whether something is indeed as it appears to be. The Cartesian method, however, denies us legitimate confidence even in our eyesight:³ for we can on its terms conceive that our eyes might systematically deceive us: we can doubt not only appearances but also the very efficacy of eyesight itself and, given that, the most scrupulous visual inspection will be of no avail in dispelling the possibility that all things might be quite otherwise than they look to us. The point is generalisable beyond eyesight: any and all of our ordinarily acceptable ways of checking things out can be shown to be exposed to unresolved, and probably unresolvable possibilities, of doubt.

The simple fact about life-in-society, though, is that it does not exhibit the total paralysis that is to be expected if everyone were engaged in the endless pursuit of Cartesian certainty. Action goes on, things get done. Doubts occur, but (relative to the Cartesian possibility of their utter ubiquity) only occasionally, and are typically short lived, being speedily resolved.

To achieve explicitly formulated contrast with the Cartesian frame of reference we can identify the auspices under which social action is conducted as those of 'the natural attitude', an attitude which is most centrally characterised by its orientation to the possibility of doubt. The 'natural attitude' does not involve the total suspension of the possibility of doubt, but differs from the pure, systematic philosophical doubt in that it cannot cast doubt comprehensively, but only selectively, from within the assumption of the givenness of the external world as a whole. The philosophical doubt can indeed put the very existence of the external world itself into question, but the natural attitude can only question the existence of this or that thing within the context of assuming the existence of the external world as a whole.

The kind of caution we are about to give should not need making, but experience proves that unless – and even when – such clauses are explicitly entered the whole sense of these arguments will be misconstrued. We heavily

stress that we are not proposing that consideration of the orientation under which daily life is conducted *must* begin from the Cartesian problematic, only that in this instance it has *happened* to do so. Without implying that 'the natural attitude' provides the inexorable place to initiate reflection on the character of conduct, by differentiating some of its kinds with respect to their orientation to the possibility of doubt, we nonetheless maintain that *given* the starting point of an interest in Cartesian issues, *then* it is entirely natural to pay attention to the differential possibilities of doubt, being forcibly struck with the way in which action in society requires taking things for granted.

Those acting under 'the natural attitude' turn away from the general possibility of doubt, allowing doubts only where they are specifically occasioned, where there is reason for them: the specifically motiveless, for-its-own-sake, doubting of the philosophical sceptic is actively excluded. It will be helpful if it is borne in mind that talk is at a very broad and abstract level of orientation here, that we are making comparison at the level of 'attitude' where this is absolutely not to be identified with the specific opinions of particular individuals, the psychological sense of attitude, but refers rather to the general orientation, or frame of reference, of conduct. 'Attitude' in this sense is manifested in the way people react to, and treat, situations, the fluent way in which they go about most of their activities, seldom if ever hesitating, only very occasionally showing any disposition to check anything out, to confirm that something is indeed what it appears to be. Anyone who would call the appearances of everyday phenomena into question 'just to see' runs every risk of getting a dismissive response for the futile, foolish, offensive and/or time-wasting character of their effort (unless, of course, they have specific grounds for doubting appearances.⁴ The point, here, though, is not to develop the contrast of the 'natural attitude' with the Cartesian method of doubt, but to introduce a consideration of the 'scientific attitude' as an instance of the natural attitude. It is, of course, the contrastive treatment of 'common sense' and 'science' which provides the problematic crux of much sociological agonising, and Schutz's reflections on common sense are often seen as germane to this. Science may be more extensively sceptical than common sense, but these two fall together within the natural attitude, for both raise doubts against the background assumption of the external world. *Both* lack the capacity to put the world as a whole into question.

Much agonising occurs over the counter-posing of common sense and scientific understandings. *On the supposition* that 'common sense' and 'science' are both modes of knowledge and that they have one and the same object of knowledge (viz. the external world) the question is: where the two conflict (as they allegedly very visibly do) which of them is to be adjudged correct? The argument is then joined, with some (for example, Gellner, 1985, contributions by MacIntyre, Lukes, and Hollis in Wilson, 1970, and Hollis and Lukes, 1982) being thoroughly confident that the frame of reference of science must be taken as the setting for all our deliberations, that its speci-

cations identify what is really there and that, therefore, any respects in which 'common sense' (or any other scheme of knowledge) fails to agree with those specifications, then so far does it fail to represent reality.

Others (for example, Winch, 1958 and 1970, Collins, 1982 and 1983, Collins and Cox, 1976 and 1977, Feyerabend, 1975, 1987 and 1988) are less than convinced by this. It seems deeply unsatisfactory because it degrades whole groups of persons relative to scientists and to those who, though not themselves scientists, overweeningly pride themselves on embodying a 'scientific culture'. The only way out of this often appears to be to maintain that *even though* the specifications of science and (for relevant example) common sense differ considerably, both must be considered as cognitively adequate, which means that some form of relativism appears necessary. We are not going to follow out the 'relativism' controversy here.⁵ We only mention it at all because Schutz's characterisations of 'common sense' are often treated as though they initiate arguments which lead us in a relativist direction. However, Schutz's arguments *fall entirely outside* the framework of choice just outlined, *for they simply do not involve attempting to match the substantive specifications of common sense with those of science to begin with*. The kind of substantive matching which we are talking about is the sort that is famously exemplified by the 'two tables' problem. Here is a table: common sense tells us that it is a solid object, made of wood, but science tells us that it is not solid, that the table is made of atoms and consists, in large proportion, of empty space – which of them is right? This practice of counterposing 'what common sense would say' about a particular object and 'what science would say about the same object' finds much favour in sociology, where it is often automatically assumed that what science would say about it would be the right thing.⁶ Whatever the actual utility of putting what common sense says against what science does, it certainly is not the case that Schutz is setting out on another exercise of the same kind, though one which might reverse the usual verdict, finding that what common sense says is to be preferred over (or at the very least held to be just as good as) what science says. Schutz is not interested in matching the specifications of science and common sense at all, in invidiously comparing what they respectively have to say on any particular topic. Already we have noted that Schutz's investigations are at the level of 'attitude' and it is at this level that 'common sense' and 'science' are to be counterposed, as variants of the natural attitude. Consistent with the characterisation of the natural attitude in terms of its level of doubt so 'science' and 'common sense' may be contrasted with respect to the extent to which they accommodate doubt, albeit within the common restriction on doubting the existence of the world as a whole.

What Schutz does invite us to consider are the common-sense and scientific outlooks in terms of the possibility that they might be substituted for each other, especially that the scientific outlook could be substituted for the 'common-sense' one *in conduct*.

The idea that there is a superior correspondence between science's specifications and the 'real world' feeds into what we will call the 'canonical conception of reality'. Assuming that science is the optimal *cognitive* instrument, it will be natural to suppose that, since there is a substantial cognitive element in *practice*, then the optimal organisation of action will involve the incorporation of not merely the specifications that science provides but, more fundamentally, the adoption of science's procedural standards as maxims of practical conduct, rather than simply as procedures for theorising. This canonical conception typically assumes that those engaged in practical conduct could actually make such a substitution (and thereby achieve more effective action), but Schutz sees reason to doubt this. He holds that the attitudes of common sense and scientific theorising are not interchangeable.

The world of scientific theorising is constituted through *modification* of the practical, common sense one. Naturally, given the terms of Schutz's comparison, one at the level of attitude, instituted with reference to the Cartesian frame of reference, the scientific attitude is also to be characterised primarily with respect to the possibility of doubt, and in that connection it falls between the Cartesian one of methodic doubt and the 'practical' one of suspending virtually all possibility of doubt save that occasioned by things failing to turn out as it had been taken for granted that they would. The scientific attitude is exempted from common sense's prohibition on doubt 'for its own sake', and is licensed to raise and pursue doubts where there is no pragmatic necessity for them, to raise them 'just to see' where these might lead. It is difficult to formulate these comparisons both briefly and in a way which does not allow (let alone invite) the reading into them of rather more than is either intended or allowable.

The presentation of the difference between 'common sense' and 'science' as one at the level of attitude is not misread as an attempt to insinuate (if not say right out) that this difference is essentially one in the personalities of individuals. The impression that this is what we are saying may be reinforced by the way we continue the case, but even our arguments so far should not foster such impressions. That it will do so perhaps testifies to the fact that within contemporary sociology there is an overdeveloped sense of the threat 'individualism' poses, with correspondingly over-zealous searching out of instances of the supposed offence (Althusser, 1971, 1976 and 1979, and Foucault, 1979).⁷ The comparison at the level of 'attitude' we already said does not apply in a psychological sense, and it ought also to be apparent that much that we have said about 'attitude' could easily be recast as remarks about 'norms of conduct', a phrase which identifies non-individual, socially provided requirements. The emphasis is, throughout, surely upon *the differential legitimacy* of asking otherwise unmotivated questions in the respective contexts of (say) scientific theorising and business practice, in the laboratory and the school room.

When we go on to elaborate further on the contrast of common sense and

scientific theorising, by describing the common-sense outlook as ego-centred, we will, if our previous comments are taken in unduly 'individualist' terms, be compounding our imagined offence, falsely presenting the individual's standpoint as though it were the centre of the universe. Again, though, to say that the 'common-sense' outlook is ego-centred is a long way from saying that people are more than ordinarily egotistical, or that in any way they overestimate the extent to which things revolve around them. It is a *very long* way from saying that the individual in the common-sense outlook views the world with him/herself at its centre, *to saying* that this same individual imagines that everything revolves around him. The fact that someone speaks of a place as 'my home town' indicates, in plain English, that this is the town in which they grew up and/or in which they live, and their speaking of it as '*my* town' indicates the place that it has in their relevances, accounts for the differential, perhaps the preferential, interest that it has for them over other towns. 'My town' does not say that the town belongs to the speaker, but that the speaker belongs to the town. In other words, the user of the saying is not expressing conviction that the home town's affairs are organised for and around him: though it is the centre of his life, *he is not the centre of it*. There is, then, no attribution of heightened possessiveness to the locution 'my town', nor any hint that we treat this expression as manifesting an unacceptably monomaniac standpoint to its user. It is, therefore, in very much the same sense to talk of 'my town' that we speak of 'my world' when we take up Birtner's (1973) contention that the social world is, in the common-sense attitude, fundamentally and irreducibly encountered as 'my world'. This formulation highlights the fact that persons structure their experience around the focal point of their particular situation, that they view things from their individual 'here and now' (with – of course – its associated history and prospects), that it is the world within which they are at home and within which events have their meaning relative to how they fit into their relevances.

To re-emphasise: that someone views events from their centre does not mean they are unduly insensitive to others, for it is an elemental feature of the ego-logical orientation that it is relativised, not absolute. It involves recognition that others comparably structure experience, placing themselves at the centre of their (so to speak) system of coordinates. We hasten to add that individually developed co-ordinates are not being invoked to eliminate socially provided ones, as will shortly be seen. However, the task in hand is to continue the 'scientific'/'common sense' contrast.

The attitude of scientific theorising certainly contrasts with common sense on the dimension of ego-centredness. It goes to the opposite extreme. It is constituted through the displacement of the ego-centred frame of reference, which is accomplished through the adoption of a conception which idealises the theorist as one who examines matters as 'from no particular point of view' or, in other words, from the standpoint of eternity. Such an idealisation certainly makes a difference to the way events are viewed, for its adoption means the

methodic elimination from the portrayal of events the things which give them their very character when regarded in terms of an individual's common-sense coordinates. The scientific theorist, then, operates within a different kind of frame of reference to that which is employed in common-sense situations, with the scientific discipline providing an impersonal standpoint from within which things are to be viewed, within which problems are to be formulated and their solutions sought. The investigator takes as problematic those matters which are recognised as such by the discipline, which have not already been resolved within its frame of reference. In exchange for exemption from the prohibition on pragmatically unmotivated doubt, the scientific investigator is subjected to exceptional requirements of logical consistency and semantic clarity. To put the contrast as starkly and simply as we can, the common-sense outlook is directed toward practicality, dominated by the need to *get things done*, and the acquisition of knowledge is overwhelmingly subordinated to this. The result is that the practical actor's 'stock of knowledge at hand' will be a heterogeneously organised collection of (pre-eminently) recipes for effective conduct. The natural attitude is certainly not suitably designed for the pursuit of *systematic knowledge*, whilst the scientific attitude provides a far superior environment for the single-minded pursuit of this, providing a setting within which individual inquiries are conducted within a coordinating framework and their results may therefore be incorporated in a unified whole. The common-sense and scientific attitudes are not so much in conflict with one another, as they are incongruous, to the extent that the systematic substitution of either for the other would be disruptive.

This stark contrast of the practical and scientific attitudes is devoid of all implication that the worlds of common sense and science are hermetically sealed against one another, so much so that it would be impossible for any 'common-sense conceptions' to be dislodged by 'scientific findings'. We nonetheless maintain that the fact that piecemeal transplantation of scientific ideas into common sense can and does occur, it just fails to bear upon the argument here, which has been about whether there might be a *thoroughgoing* substitution of the scientific for the common-sense attitude.

Doubtless there are many different lessons which could be drawn from Garfinkel's 'classroom demonstration' or, as they are otherwise known, his 'experiments' (Garfinkel, 1967), but the ones which are often drawn – that, as Giddens (1976), Craib (1984) and Gilbert and Mulkey (1984), allude to, these demonstrate the fragility of social order, for example – often have little real relevance for ethnomethodology, and certainly do not draw the most useful conclusions. Amongst the lessons which can, we think, most usefully be taken from some of the experiments are those which implement the case just developed. For brief example mention of two such exercises will suffice. The first involving the relentless interrogation of the conversational partner, the second requires students to act as though they were lodgers in their own homes. Both of these can be understood as simple, economic, above all

unpretentious ways of illustrating incongruity between the common-sense and the scientific attitudes.

Garfinkel's students were sent out to engage people in conversation (Garfinkel, 1967: Ch. 2). On any possible occasion in the conversation on which they can see the opportunity they are to demand clarification of the other's remarks, persisting in that demand until all possible ambiguities or obscurities are eliminated. These attempts did not get very far, the conversational 'subjects' quickly becoming impatient and irritated, denying any need for further clarification of their remarks despite being challenged, and even terminating the conversation. This exercise can be seen as a way of introducing into the context of the common-sense outlook a requirement appropriate to that of scientific theorising, namely that of semantic clarity for its own sake. The implementation of that requirement does not, however, result in better organised, more rationally conducted conversation but in the disruption, even destruction, of the conversation itself.

Comparably, the 'experiment' with students casting themselves in the role of lodger within their own homes (Garfinkel, 1967: Ch. 2) can be seen as inviting the adoption of something akin to the de-personalised standpoint that the scientific attitude imposes, removing the student from involvement in the specific context. The students did not, however, find that this distancing from their involved, personalised standpoint gave them a more objective comprehension of those same circumstances but, instead, deprived them of some essential features. The capacity to see what was really happening before their eyes as events in a household – more, their own home – required reference to knowledge of the circumstances, of persons and the history of their mutual relationship which were acquired by full participation in the household.

Like Garfinkel, we decline to promote these simple illustrative devices to anything more than that, claiming for them only that they offer *prima facie* support for the view that the scientific and common-sense attitudes are incongruous, at least to the extent that the attempt at the progressive, and eventually complete, substitution of the former for the latter is an impracticable project.

4 Paramount and multiple realities

Though we have previously set aside the 'relativism' question and have implied that Schutz's treatment of the common sense/science issues gives no reason to develop that as an issue, it is nonetheless probably as well to spell out why these arguments do not support relativism.

There are still important points of clarification about the nature and implication of Schutz's argument which are essential to the forestalling of relativistic interpretations. The fact that Schutz's thoughts are largely devoid of these will be the more readily appreciated if it is seen that the move is being made from 'epistemological' to 'organisational' mode, and that first Schutz, then

Garfinkel and ethnomethodology, are attempting to respecify topics by transforming them into ones which involve describing properties of social organisation, rather than formulating epistemological criteria. It will not remotely help the comprehension of the work to read it as if it attempted to formulate properties of social organisation so that *they can serve as epistemological criteria*, either securing or undermining ordinary claims to knowledge, let alone vindicating or invalidating the philosophical sceptic's challenge. Wittgenstein (1978) remarked that (legitimate) philosophy could only leave everything as it is, meaning (particularly with reference to the sciences and mathematics) that philosophy *could not possibly* either enhance or diminish the support for these. It was beyond the capacity or competence of philosophy to make any difference to the findings of science and the results of mathematics. A comparable thing might be said about ethnomethodology, that it too leaves everything as it is, is capable of making no alteration to the cognitive value of either common-sense understandings nor scientific theorisation: these are neither more nor less certain as a result of ethnomethodology's investigations than they were before.

One of the difficulties with which one must contend in contemporary sociology (and social thought throughout the human sciences more generally) is that strong contemporary prejudice condemns political quietism (and, in accord with Durkheim's argument about strong, widely shared sentiments, that prejudice is quick to be outraged), with the result that remarks like Wittgenstein's and our reiteration of them are likely to be singled out as evidencing that such approaches to philosophy and sociology are reprehensible invitations to quietism. Our remarks here, and the related arguments concerning values and moral judgement in general that are made by Lena Jayyusi in chapter 10, are certainly out of sorts with those views of philosophy and sociology which make it incumbent upon these pursuits to achieve the revaluation of phenomena, to reveal that they are more or (more usually) less than they are cracked up to be, but those same remarks are (if read *without prejudice*) quite neutral about the desirability of political quietism. The critical supposition is that 'leaving everything as it is' contributes support to the political *status quo*, but the conclusion that Wittgenstein's (or ethnomethodology's) approach does this omits to notice what is being said, which is that (properly executed) Wittgensteinian philosophy and ethnomethodological inquiry *make no difference* to these things, so they certainly cannot make them stronger or weaker, more or less defensible etc. than they otherwise might be. Just as it is outside of the competence of these approaches to re-evaluate cognitive schemes, so it is equally beyond their reach to re-evaluate other kinds of legitimacy, hence it would be less the fulfilment than the violation of their conviction that they must leave everything as it is if they were to make phenomena out to be more legitimate than they otherwise would be.

It ought, then, to be obvious from the outset of our argument that any

version of ethnomethodology which seeks to see it as substituting an inferior 'socially constructed object' for 'the real thing'⁸ has somewhere along the line gone off the rails. One point at which such derailing can occur is in taking Schutz's discussion of 'multiple realities' as leading in the direction that relativists like them to go, which is that of setting up premises for arguments about a plurality of cultural systems, with the implication that what we are confronted with is a plurality of autonomous and competing 'realities'. Schutz's own thoughts on 'multiple realities' have little to do with this, and if taken *in conjunction* with those on the reciprocity of perspectives and the interchangeability of standpoints definitively show how far he was from going that way. Sure enough, Schutz does talk of 'multiple realities' and does identify the world of daily life (as comprehended through common-sense understandings) as 'paramount reality', but this does not indicate either an incipient relativism *nor* (in the opposite direction) an inclination to grant incorrigibility to common-sense understandings.

It is important to recall that Schutz is not concerned with 'common sense' as a determined collection of specifications, that his notion is a *formal* one: 'common sense' is the social correlate of the individual's demonstrable disposition to take things for granted. At the level of the premisses of conduct (so to speak) it is inevitable (*given* the origins of the argument in Cartesian issues) that anyone who would act *must* leave a multitude of things unexamined, *must* be taking all of these for granted. To reiterate, the Cartesian method of doubting everything that can possibly be doubted must result in the paralysis of action, in its endless postponement pending the resolution of the vast multiplicity of possible doubts, but the very existence of social life entails that *action does occur*, which means that possible doubts must be being extensively disregarded. The capacity of persons to take things for granted on this scale is enabled through inheritance of the socially distributed stock of knowledge, the acquisition of a set of received recipes for practical life that are socially sanctioned and which delimit the possibility of doubts by institutionalised insistence that there are matters which cannot legitimately be inquired into. Those who attempt to raise doubts with respect to these matters will typically find that their attempts reflect not on their intended targets, but react back upon the questioner, casting doubt on their practical competence, even their sanity. Others will be mutually supportive in rejecting such doubts. Obviously, in order to illustrate one may speak from one's own pre-theoretical point of reference of things which *amongst us* are taken as certainties and which stand outside *legitimate* examination, but this manner of presentation must not be mistaken for an analytical parochialism.

Things that might be pointed to as having 'common-sense' status in such illustration are not, thereby being awarded any universal and generally incorrigible character, for *analytically speaking*, talk of 'common sense' merely intends the fact that *amongst any given collection of persons organised into anything that can meaningfully be called a collectivity*, there will be a

corpus of matters which those persons will find 'obvious', as 'going without saying' and as 'beyond doubt and investigation'. What those matters will be will vary, of course, from one collectivity to another. It will not do, either, to suppose that these matters are the equivalent of Durkheim's sacred concepts, *protected* from investigation by their sanctity, for they are instead prevalently matters of utter mundanity, such that inquiry into them embodies less the transgression of boundaries into the forbidden than it does the investment of energy into the pointless, the time wasting and the unnecessary. In other words, the perceived futility of such inquiries is what ensures that they reflect negatively upon their maker.

The world of daily life, comprehended under the auspices of common sense, is picked out by Schutz as 'the paramount reality' but, again, this only indicates the organisational position which 'the world of daily life' occupies amongst the various 'finite provinces of meaning'. The initial objective of distinguishing amongst 'finite provinces of meaning' is to put the spotlight on the episodic nature of the flow of experience. The differences which talk about 'multiple realities' is designed to capture are those between waking and dreaming, between walking the streets and being engrossed in a theatrical performance, between engaging in practical pursuits and theoretical reflection. Over any period of time the individual can alternate between episodes of these kind, passing from wide awakeness into sleep and dreaming then awakening again, moving from attentiveness to the daily world into an imaginative day-dream and so on.

The first point Schutz is making is that the transition between these episodes is abrupt, with the respective spheres of the wide-awake world, the dream, the fantasy and so on being self-contained. *Second*, he emphasises that *within* each sphere an 'accent of reality' is assigned to the things experienced there. *Whilst we are undergoing them* the things which happen in the dream seem real to us, just as those which take place before our eyes on the stage of the theatre do. *Thirdly*, the transition from one episode to another is typically via the world of daily life: we enter the world of dreaming from the waking world, and we return eventually to the latter; we go to the theatre from the streets, are caught up in the world of the play but eventually the play will end and we return to the streets and the concerns of daily life (remembering where the car is parked, wondering if we left the umbrella in the theatre, etc.). *Fourth*, the standards of reality which are applied in daily life pre-empt those in the other 'provinces of meaning'.

Thus, to repeat, *whilst we are dreaming* the occurrences in the dream are real to us, the dream of winning a vast amount of money engenders the euphoria we should doubtless feel if such an event occurred in real life, but, however real the events in the dream might seem, upon waking they will be (regrettably, in this case) consigned to the category of 'only a dream'. Try telling the bank manager that you have dreamed you won a million pounds and that you would like to use this million to clear off your overdraft. Simi-

larly, try telling the police complaints desk that you have just seen the murder of Julius Caesar and that you are fortunately able to name the killers: Brutus, Cassius and so on. However strongly the accent of reality may have been placed upon the events in the play, *in the context of daily life* these events did not really take place. It is by virtue of its two characteristics, as the environment for other provinces of meaning and the role of its standards as arbiters of reality, that common sense is dubbed 'the paramount reality'. This is not to offer any endorsement of its position on Schutz's behalf, merely to note the typical manner in which relations amongst waking, dreaming, fantasising and theorising (to name but a few) are dealt with by the members of society.

One further point is necessary in connection with Schutz, pertaining to the vital but easily neglected qualification which he sets upon the assumptions of 'the reciprocity of perspectives' and 'the interchangeability of standpoints'. *Given* that Schutz is attempting to reconstruct the generic properties of the social world out of the structure of individual experience, and *given* that the argument involves the 'relativising' of the environment to the extent that the world is, for each individual, *my* world, then the risk is that this will be taken to involve the decomposition of the social world into a vast multiplicity of ego-centred realities, each substantially and irreconcilably different because of the exigencies of individual positioning and experience. However, to project such a possibility of proliferation is to neglect the fact that Schutz is throughout responsive to the pre-theoretical givenness of the world as a social world, as an intersubjectively available 'one and the same' world for different persons. The aim was certainly not the decomposition of the social world into unrelated subjectivities but, rather, one the experiential underpinning of the *socially sanctioned* unity of the world, of the mutual *demand* that we recognise the commonality of circumstance.

The provision Schutz makes for this is, first, in terms of the socially distributed stock of knowledge. It is necessary to distinguish between actually identifiable persons and the theoretically reduced creatures that are devices of Schutz's own theorising. For the purposes of exposing the structure of socially organised experience Schutz envisages a drastically reduced consciousness, a pure stream of experience which is unstructured and into which socially provided structures will have to be installed. The wisdom of employing such a method of exposition may be debatable, but the fact that it is employed should not lead us to suppose that the experience of actual persons is *conceivable independently of socially provided structures*, for it is not.

Any actually encountered person will, of course, be the possessor of a handed-down body of knowledge in terms of which their circumstances and courses of action will be conceived. Hence, it is just not possible to conceive the real members of society as a collection of mutually independent standpoints. Further, Schutz builds into his analysis of the structure of experience two 'theses', which are those of 'the reciprocity of perspectives' and 'the interchangeability of standpoints'. These are assumptions that the respective

standpoints of ego and alter will be complimentary, that the way things seem to A will not be strictly identical with the way they are for B, given that the two occupy different standpoints, but that they will reciprocate each other, to the extent that differences between them can be discounted against their separate locations.

This first assumption of the 'reciprocity of perspectives' is reinforced by that of 'interchangeability of standpoints' which is that if A and B were to change position then what A would find from his/her new vantage point would be identical with what B had previously found in that position *and vice versa*. These are, note, assumptions built into the common-sense attitude, their presence evidenced in the multifarious and multitudinous ways in which the flow of activity in society simply presupposes orientation to a commonly known environment. There is not, on Schurz's part, any attempt to make unduly optimistic assumptions, to dispose of, let alone, minimise, possibilities of divergence and dissent, for the reciprocity of perspectives and the interchangeability of standpoints are assumptions and *are not guaranteed*. They hold good *until further notice* – they can, and do, break down. Conduct which begins on the presumption of a commonly known environment may find that its presumption must be reviewed. Furthermore, and this is the point at which the above mentioned vital qualification needs to be entered, these assumptions are ones which have legitimate application only when it can be assumed that *biographical differences can be set at zero*. If this qualification is overlooked then it will appear that Schurz's characterisation gratuitously overestimates the homogeneity of experience in the face of cultural diversity but, to the contrary, Schurz makes ample provision against such overestimate: allowance for the diversity of experience and culture is built into the socially distributed stock of knowledge itself, as citation of the cases of children and strangers unequivocally illustrates.

5 Objects and appearances

The challenge of scepticism is not absent from contemporary sociology,⁹ but is still strong there, perhaps more prominent than ever. Cartesian scepticism directed itself toward the identification of necessary certainties and an exercise in that spirit will be moved to despair if it cannot find any certainties, for Cartesianism holds a 'foundationalist' conception of knowledge. A sound edifice of knowledge can be erected only if it stands in secure foundations: if there can be doubts about even its founding assumptions, then the whole construction is affected. However, 'foundationalism' is now in poor repute and the idea of the search for certainty itself is inimical. The reaction against Cartesianism means nowadays not a disappointed resignation to the fact that there are – can be – no certainties of the kind it seems, but an enthusiastic, even joyous acceptance of the absence of certainty, the abandonment of whose schemes of thought have been informed (or infected) by Cartesian aspirations.

This is celebratory scepticism and, though it may not have the upper hand in contemporary sociology, it is certainly a pro-active force, vigorously cultivating equivocality, irresolution, and doubt, delighting in the ultimate and utter indeterminacy of reality. Though those who follow out the most developed forms of this scepticism are apt to regard ethnomethodology as joining them on the sceptical side, but as doing so only timidly and without taking the possibility of doubt anywhere near far enough.¹⁰

Appreciating that the roots of contemporary sociological scepticism are diversified and more complex, certainly, than can be properly acknowledged in comments as concise as ours, we will incautiously but flatly assert that much of this scepticism is propelled by rejection of the possibility of any 'final interpretation'. The 'achievements' of reason are now 'recognised' to be the production of interpretations, and there is always a multiplicity of these. No matter how convincing any interpretation may appear, some other (equivocally effective) interpretation is *always* possible, the pursuit of any 'final' one being futile because the sequence of possible interpretations is endless. The obligation is, therefore, to reveal interpretations for what they are, unravelling any pretensions they may have to finality, and proliferating the alternatives to them, this activity of course being wholly alive to its own inherent lack of finality and requiring, therefore, the development of the means of its own eventual destruction.

Ethnomethodology (and Wittgensteinian philosophy) also are seen as initial moves in this direction. They are anti-Cartesian certainly, and are imagined to have contributed their share to the erosion of 'reality' and to the supposed realisation that *there is only interpretation*. Ethnomethodology has established that 'social reality' is (merely) an ensemble of interpretations, but it does not take this insight seriously enough (in the judgement of its would-be surpassers, for example: Blum and McHugh 1986, McHugh *et al.*, 1975, Woolgar, 1988, Silverman and Torode, 1980), to throw itself into the constantly self-destabilising proliferation of interpretations which is the *sine qua non* of adequately self-conscious contemporary theorising. In other words, ethnomethodology has started on, but failed to go very far down, the road to the realisation that theorising is essentially a self-expressive activity, which – unless it is conducted as a continuously self-deflating self-scrutiny – will become gratuitous assertiveness, intrusion into the other's interpretive space. In the manner of all those who regard one set of values as a provisional but partial step toward their own, those who subscribe to contemporary scepticism perhaps consider themselves to be paying ethnomethodology a compliment by allowing that it is on the right side of the sceptical divide – just – but failing to subject itself to a sufficiently searching, self-doubting scrutiny.

However, compliments of this sort may not be gracefully received, and may even be flatly rejected with a tart comment like Bittner's, because they have as their source 'the pallid ideology of cultural relativism' (Bittner, 1973).

Bittner, appraising the situation in field-work studies of a quarter of a

century ago, wrote with continuing relevance, for though he might have been expected to welcome the 'turn to subjectivity' which was then taking place in sociology, giving a new centrality to 'the actor's point of view', he proved highly critical of the way this turn was being taken, declaring that it implemented an 'abortive phenomenology'. Abandonment of the previous ideals of 'objectivity' should not mean the relaxation of strong standards of investigation and analysis in favour of a relaxed, casual, perhaps self-indulgent approach to these, though this was the direction in which the movement away from aspirations to 'objectivity' was taking. The opportunity was there for the maintenance of standards of rigour, but in the service of realism, a commitment to capturing the phenomena under investigation through scrupulous study and accurate description had the opportunity to replace the previous, 'positivist' commitment to the supposed requirements of scientific method. The opportunity might however be missed for,

although the attacks on positivism were mounted from positions that involved strong commitments to philosophically well-grounded and rigorous scholarship, the arguments of the attack were often invoked as the aegis for studies of a loose, impressionistic and personal nature. (Bitner, 1973: 117)

Bitner wanted to hold these studies to what they were apt to put up as objectives for themselves, but from which they were tending to deviate because they were short-circuiting the process of understanding 'the actor's point of view':

if the fieldworker's claim to realism and to respect for the actor are to be given serious credence, then it will have to be made clear when they are a function of a natural attitude of the actor but of a deliberately appropriated 'natural attitude' of the observer. (Bitner, 1973: 119)

Fieldwork methodology was recommended because it brought researchers into close contact with those whose setting and life was being investigated, and supposedly ensured that the researcher would be more intensively, vividly aware of the actor's point of view, and therefore able to give the most faithful rendition of this, but, if Bitner's judgement was right, immersion in the field was producing only an attenuated characterisation of the actor's sense of social reality. Indeed, the very nature of fieldwork itself could be the very thing producing this attenuation with the fieldworker's relation to the phenomena encountered in fieldwork experience being conflated with those encountered from within the social worlds under study. The world of daily life comes to appear to the fieldworker 'merely' as a corpus of exhibits, with the consequence that:

[the fieldworker] tends to experience reality as being of subjective origin to a far greater extent than is typical in the natural attitude. Slipping in and out of points of view [the fieldworker] cannot avoid appreciating the meanings of objects as more or less freely conjured. Thus [the fieldworker] will read signs of a future from entrails of animals,

believe that the distance objects fall is a function of the square of time, accept money in return for valuables, and do almost anything else along this line; but the perceived reality of it will be that it is so because someone is so seeing it, and it could be and probably is altogether different for someone else, because whatever necessity there is in a thing being what it seems to be is wholly contained in the mind of the perceiving subject. Hence, without it ever becoming entirely clear, the accent of the fieldworker's interest shifts from the object to the subject. (Bitner, 1973: 122)

Again we have the spectacle of an ethnomethodologist setting himself well apart from a viewpoint which many would be convinced must be his own, but the history of ethnomethodology's public life, at least since the appearance of *Studies in Ethnomethodology* in 1967, is full of comparable ironies. The decomposition of social reality into a phenomenon within the mind of the subject is a failing (to those who allow this is a failing) for which ethnomethodology is regularly criticised. The double irony is that remonstrations like Bitner's receive scant attention when these criticisms are made.¹¹

A united front of ethnomethodologists is too much to hope for, and is not in any case really needed. Evident disagreement between them on the issues under review at least reinforces our general case – a shift of attention to 'the subject' *at the expense of* 'the object' is not what all ethnomethodologists see as the inexorable outcome of their point of departure, though the prospect of unwittingly making such a transition/transgression is no doubt one ethnomethodologists run. Unless the distinctive standpoint which is identified in investigation as 'the actor's point of view' is understood to be rooted in the natural attitude, and itself interpreted against its background, then in all probability they will fall foul of that risk.

Though the passion for fieldwork has now abated, acknowledgement of the contingency of social phenomena now engenders similar difficulties. First Bitner again, and at some length:

For the fieldworker, as noted earlier, forever confronts 'someone's social reality'. And even when [the fieldworker] dwells on the fact that this reality is to 'them' incontrovertibly real in just the way 'they' perceive it, he knows that to some 'others' it may seem altogether different, and that, in fact, the most impressive features of 'the' social world is its colourful plurality. Indeed, the more seriously [the fieldworker] takes this observation, the more [the fieldworker] relies on his sensitivity as an observer who has seen firsthand how variously things can be perceived, the less likely he is to perceive those traits of depth, stability, and necessity that people recognise as actually inherent in the circumstances of their existence. Moreover, since [the fieldworker] finds the perceived features of reality to be perceived as they are because of certain psychological dispositions people acquire as members of their own cultures, he renders them in ways that far from being realistic are actually heavily intellectualized constructions that partake more of the character of theoretical formulation than of realistic description. (Bitner, 1973: 123)

Now dulled, the passion for fieldwork has been succeeded by a fascination amongst sociologists more generally with the contingent character of social

arrangements, a fact which in all likelihood has indeed been understated by many social theories and which is, therefore, rightly given corrective acknowledgement. Unfortunately, over-compensation is not unknown, and the successor to Bittner's anxiety is the worry that, in seeking unequivocal demonstration of the contingent nature of some phenomenon, the 'traits of depth, stability and necessity that people recognise as actually inherent in the circumstances of their existence' may continue to be excluded from the picture.

Surely, though, phenomenology itself recommends paying attention to the appearances, leaving the question of how Bittner's strictures can be applied in the name of that cause? If phenomenology aims to close altogether the gap between reality and appearance, then what is the source of Bittner's dissatisfaction with the view that reality is as it appears to individuals?

Once again large and important differences hinge much upon how one understands slogans such as 'there is nothing behind the appearances'. 'Behind the appearances' there were supposedly objects, so if someone claims there is nothing behind the appearances then they will be understood as saying that there are no objects, only appearances. Hence, one is saying that appearances have displaced objects, that there are *only* appearances. The 'only' here can rapidly acquire, if it does not from the very first posess, distinct overtones of diminishment: objects have been replaced not by appearances but by *mere* appearances: reality has certainly been down-graded if it has been reduced from solid objects to mere appearances. This move, the disposal of objects in favour of appearances, surely encourages the view that objects are matters of appearance, of being 'merely' how they appear to particular observers?

To construe the slogan 'there is nothing behind appearances' in that way, however, is to continue to speak the language of the very conception we are trying to get away from, to accept their terms in which objects and appearances are distinct, in which one provides the substance that is covered by the other. What we experience are appearances and (presumptively) *behind* those appearances are the objects. Construed in those terms, phenomenological arguments which invite us to confine our investigations 'within experience' thereby perhaps unavoidably deny us access to objects themselves, for they stand outside experience. It is feared that if we do not preserve the separation of objects from their appearances then we will inevitably lose that vital distinction between how things appear to us and how they really are, independently of our perceptions. This kind of argumentation, surely, is familiar enough in recent sociology: where the insistence that this latter distinction is vital not only to the prosperity of sociology but to the continuing strength (and superiority?) of our civilisation.

There is, though, no reason why phenomenology should be understood in the terms of its predecessors' conception, as placing itself within the terms of the distinction they want to make. Why should it continue to speak their language, and why, therefore, should the slogan 'there is nothing behind the

appearances' be meant in a way that makes the naïve mistake of supposing that we can never be wrong in our identification of the thing we are (say) looking at? Instead of *directly* controverting the assertion that there are objects behind appearances, the case is that this received way of talking makes it seem that we should only say that we see the surface of a chair or the front of a house, and never that we see the chair or the house. Seeing-a-house-from-the-front is, in phenomenology's submission, a much more adequate description of our experience than is 'seeing only the front surface of a house' and this latter, inadequate description is forced on us because of the way philosophers have attempted to separate objects from their appearances. Once again, the phenomenological position begins from, rather than goes against, the objectivity we ordinarily find in things. It does not seek to *reduce* objects to appearances in this inimical sense because it does not respect the contrast of 'object' and 'appearance' that was previously in place. The slogan can now be seen to mean something quite different than that objects do not exist, that only appearances do. Rather than putting appearances where objects used to be, one may be seen to be drawing attention to the way in which (so to speak) *objects are found in their appearance*. The 'objects' have been 'relocated' and are to be found from amongst the appearances.

Two very simple but persistently employed examples which are designed to show the difference between the appearances and the object are those featuring the stick-bent-in-water, and the-disk-that-is-seen-to-be-elliptical. The (object?) lesson in both cases is that exclusive reliance upon appearances, the strict confinement of our inquiries within the domain of experience, will deny us a distinction that we should otherwise consider indispensable. It is that between the stick which *appears* to be bent in water but which *in reality* is straight. Similarly, the disk that is seen to be elliptical is a disk which is seen to be elliptical but which is in fact (in itself) round.

The possibility of such instances, of a disjunction between how things appear and how they really are, between how things are in our experience and how they are in themselves, is one we are allegedly deprived of if we give credence to the phenomenological slogan, but this is not so. On the terms of phenomenology it is entirely conceivable that persons will find that things which appear bent when submerged in water will prove straight when extracted from the water, that a disk viewed from an angular perspective will look elliptical but upon closer inspection will be found to have been all along round. The apparent incapacity of 'appearances' to reveal the true character of the stick and the disk – as straight and round, respectively – is not, for phenomenologists, evidence that we need to go *outside* experience in order to determine the actual nature of phenomena – from the phenomenological point of view there is, after all, nowhere 'outside experience' to go.

The idea of a standpoint which is not *someone's* standpoint is equally non-sensical (though this does not, as we have already strongly stressed in our discussion of Schutz, deprive anyone of the capacity to use the standpoint of 'no

particular point of view' as a cognitively invaluable device). Our apparent incapacity to determine *through* appearances, from *within* experience, the difference between something which 'merely appears' to be one thing but is 'in fact' another, is not inherent in the supposed 'restriction' of phenomenology to the merely subjective, perspectival or experiential domain. It is, rather, a product of the distinctly *impoverished description* which will inevitably be given of the character and dimension of the world-perceived-from-someone's standpoint, if the conceptual apparatus of phenomenology's opponents is retained. In phenomenological terms, the stick which appears bent is not necessarily a stick which *is* bent, for the stick *as a properly assembled and described ensemble of appearances* to be precisely a stick-which-appears-bent-but which if we go-over-extract-it-from-the-water-and-then-look-at-it-will-appear-straight which, for us counts as *being straight*.

An alternative possible way in which the stick can appear, is as a stick which looks bent but, allowing for the fact that it is partially immersed in water, we know even without examining further that it would prove to be really straight if we did look at it. However, does this not bring us back to the difficulty which both would-be friends and enemies see affecting the preservation of 'the object' in terms of 'appearances'? Is there not a (fateful) symmetry between 'the stick which appears bent (when in water)' and 'the stick that appears straight (when extracted from water)', and so how are we to determine which – bent or straight – the stick really is? Do we not have to say either that the stick is both-bent-and-straight, or that the stick is in itself neither-bent-not-straight? Do we not have to accept that such determinacy as people find in reality is only that which they have imposed upon it? This, though, remains within the very framework from which phenomenology has withdrawn, one which puts in place a distinction between reality as it is in our experience, and reality as it is in itself. In phenomenology's own terms, however, there is no such distinction between reality as found in experience, and reality in itself.

An irresolvable symmetry appears only if *we withdraw* from assumptions which are otherwise already in place. There is a deep-rooted ambiguity in the presentation of both the case of the disk and the stick, which is that it has been presented as though each one of us was being invited to decide *for the very first time* what shape the disk was, what shape the stick was. To decide for the very first time *for everyone* that is, as though we were without any pre-given basis whatsoever on which to resolve the problem. We are being presented, therefore, not with the problem of deciding *for one particular case* what shape this disk is, or whether that stick is straight or not. It is not, that is, a *problem of perception* to resolve the general problem of the relationship between 'reality' and 'appearances', since it will be only in terms of some pre-given 'solution' to that problem that one is able to determine the character of particular perceptions. The ambiguity of the example results because the inherited ways of deciding what we see are built into their construction: the fact that we know about the effect of perspective on shape, and about the refracting effects of

light in water, is presumed in describing the very situations themselves. When invited to consider the case in which a disk looks round to one observer, and elliptical to another, we are supposing that readers will imagine a familiar situation, one in which a something which, viewed head on, looks round will, when viewed at another angle, look elliptical. We do not suppose they take the example as one in which they are invited to consider the possibilities of the disk looking round and elliptical, as one in which these two are just the first in a long line of equivalent possibilities: that, for example, the disk might look like a triangle to a third party, like a fly to a fourth, like a caterpillar tractor to a fifth and so on and on.

If our supposition about the likely reading of the examples is correct, then this shows that the perceptual possibilities are *already* pretty well restricted, are restricted in terms of a pre-given conception of the possibility of relations between appearances and realities. The apparent choice being constructed on *that very basis*, then the apparent symmetry between the disk's being round and elliptical and the stick's being bent or straight is a fake one, for the basis of choice between them is built into that same pre-given conception: on its terms, the shape that a disk looks when viewed straight decides what shape it really is, and a look at the stick out of the water settled whether or not it is straight (if, in fact, there is any real doubt about the stick's shape).

We have come back by another route to Bitner's point about aborted phenomenology. Many philosophers and sociologists want to challenge the adequacy of our ordinary experience as a source for knowledge of reality, which, by an almost Newtonian law of equal and opposite reaction, inspires others to set out to defend that experience. From the standpoint adopted here, however, *both* sides of that argument are inclined to start with largely unexamined preconceptions about the character of that commonplace experience, and, further, that there is a critical issue not about the adequacy of our ordinary experience but about the possible inadequacy of the *descriptions* of that ordinary experience. The simple examples of the disk and the stick together highlight the difference between partial and wholesale withdrawal from the framework within which 'everyday experience' is gained, suggesting that:

(1) the character of any ordinary person's supposed experience of a disk viewed from an angle, or of a stick partially submerged in water, is described in an over-simplified way, and

(2) that the examples which are supposedly constructed to allow us to distance ourselves from the 'prejudices' of our ordinary experience, and to invite us to critically reflect on these, are examples whose very construction presupposes, and employs for its basic intelligibility, the very apparatus of presuppositions that it ostensibly suspends.

The acceptance that there might be something to these two points, makes enormous differences to the whole agenda. The first, and major, difference is

that it turns attention to the issue of *description*. Of course, the question of what counts as an adequate (or more adequate) description of our commonplace experience is not to be dogmatically resolved in favour of a 'phenomenological' style of approach, but our simple examples and their performative discussion are introduced merely to show that *there is a problem here*. On superficial inspection, there is reason to argue that the versions of 'everyday experience' are, if nothing else, over-simplified, giving truncated descriptions of the perceptions from which we *all* (philosophers, sociologists and everyone else) begin. Though we have given only very simple-minded and very basic examples, we see no reason to expect that, if the examples were 'scaled up' in terms of both complexity and sophistication, the problem would significantly change.

The standard question is put: are phenomena real in the sense that persons take them to be? Because of the pivotal role which scientific knowledge is imagined to play in defining our contemporary concept of knowledge, it is supposed that examination of the case of science will resolve many epistemological issues. This perhaps accounts for the interest which has recently been taken in the sociology of scientific knowledge.¹² It is, for example, regularly asked whether the phenomena 'discovered' by natural science are real *in the way practitioners of science and commentators on the history of science* typically take them to be? Though they may not necessarily be meant to carry such a connotation, the concern to say that they are 'socially constructed', that they are *real in a social or cultural sense*, such descriptions nonetheless carry strong overtones of the suggestion that these phenomena are less real than they are presumed, by scientists, commentators and laypersons, to be.

Ethnomethodology need not step up to defend the conception of the reality of the science's phenomena in the sense in which the sociology of scientific knowledge typically challenges this. The issue is not whether scientists are right or wrong to hold 'realist' conceptions of their work, but whether the *fundamental* sense in which scientists find 'the reality of their phenomena' has anything to do with holding realist views at all. The question whether scientists are right in their 'realistic' construal of their achievements gives way to the question of whether the *scientists' sense of the reality of the phenomena* they deal with has in fact been identified at all. Ethnomethodology prefers to look into the ways in which scientists *encounter* their phenomena, to examine the ways in which they 'come upon these' in the course of their investigations, to see how – for example – their activities in a laboratory comprise – *as far as the scientists are concerned* – the disclosure of a hitherto undiscovered phenomenon (or, alternatively, the routine reproduction of a well established one). (See for example, Garfinkel *et al.*, 1981, Garfinkel *et al.*, 1989, Lynch *et al.*, 1983, Lynch, 1985 and Livingston, 1986.) It is, after all, in the laboratory, the observatory and comparable places of work that scientists satisfy themselves about the *bona fide* character of findings, and ethnomethodology's question is about the ways in which, within such settings and *through their*

disciplinary work practices, scientists determine the 'reality' of what they have found. It is only through (minimally) fuller description of the ways in which scientists conduct investigations and discover phenomena that one will have begun to specify the sense in which the phenomena of science are real or the scientists themselves.

6 Conclusion

If there is anything to the argument we have just made, then it has more severe consequences for the agenda than meaning just that description is given much higher priority on it but that, otherwise, the list remains the same. The implication is that if the problems of description are more seriously treated then the rest of the agenda may be obviated. If a more thorough description of our 'ordinary experience' is given, then it may transpire that standard suggestions about the way in which our commonplace experience might 'misrepresent reality' originate in very bare descriptions of the structure of that experience, and that they cannot, therefore, be regarded as initiating *serious* problems.

Now there is a fundamental difference in judgement. Does one take the problems of description as being merely incidental? Should we simply bypass them, taking it for granted that taking the problems of description seriously would not change the basic problems and that, therefore, to make description crucially problematic would merely lead us into a detour? The challenge to move the issues to the level of description will then simply be declined. It has not been answered, merely set aside. The other judgement is, of course, that there is no way around the problem. Far too much *depends* upon casual presuppositions about the nature of ordinary experience, about the appearance of the world of daily life, for it simply to be granted that these presuppositions are apt. Before making judgements as to whether the (social) world is the way it appears to be, there does seem to be room to ask: but how exactly does it appear to be? An important part of ethnomethodology's peculiarity within sociology is that it takes the answering of that question to be a serious and problematic task.

NOTES

1 Whether Husserl overstates the objectivity of logic or misunderstands its basis for it is irrelevant to the assertion. If he *did* conceive the world and the leading expressions of our knowledge of it 'subjectively', then it was certainly not in the sense that sociologists worry about.

2 Since Schutz's objective was the clarification of the presuppositions of the 'social sciences' it is a natural consequence that he should have argued that the 'social

- premisses of conduct in the central place, not those of the acquisition of knowledge.
- 3 In fact, it only denies us this for philosophical purposes. For all practical purposes we can continue to count on our capacity to 'look and see' at its usual level of reliability.
 - 4 We take it that, in this sense, the presence of the natural attitude in the orientation of the general run of 'everyday' conduct is *indubitable*.
 - 5 See Wilson (1970) and Hollis and Lukes (1982) where the debate is engaged.
 - 6 See Ryle's (1954: Ch. 6) discussion of technical and untechnical concepts.
 - 7 See James (1984) for an examination of this issue.
 - 8 We are reluctant to use the expression 'social construction' to characterise ethnomethodology's view of 'social reality', though not because of deficiencies inherent in the expression itself. It is widely used in a way which is contrastive with 'real' such that, classically, the demonstration that a social problem is a 'social construction' carries the implication that it is not really a problem: cf. Spector and Kitsuse (1977). However, this contrastive use of 'socially constructed' against 'real' or '*bona fide*' is not the only possible employment, and in all probability represents a theoretical short-circuiting of the idea of 'social construction' itself. The demonstration of something's 'socially constructed' character need not be – in our view, should not be – at the expense of its 'reality'.
 - 9 Three concerns illustrate this: deconstructionism (for example, Derrida, 1976 and 1978), Quine's scepticism (Quine, 1961, 1966, 1981 and 1990; see Roth, 1986), reflexivity (for instance, contributions to Woolgar, 1988; Barnes, 1974 and Bloor, 1976).
 - 10 For example, there is much scepticism about meaning; ethnomethodology's early stress upon the 'indexical' character of commonplace expressions is thought to show that these expressions are without determinate meaning. This disregards Garfinkel's (1967) overt insistence upon the way in which the everyday users of indexical expressions find plain sense, determinate meaning in each other's talk, writing and so forth.
 - 11 Ethnomethodologists may be less than unanimous on this point – consultation of *some* of Melvin Pollner's work, for example, would amply support the location that social reality is 'a mere construction', cf. Pollner's *Mandane Reason* (Pollner, 1987). Even so, Pollner's writings do not consistently sustain this view: contrast the above work with his 'The Management of Meaning in Traffic Courts' (Pollner, 1979).
 - 12 Kuhn (1977) and contributions to Knorr-Cetina and Mulkey (1983) are illustrative of this interest.

5

Method: measurement – ordinary and scientific measurement as ethnomethodological phenomena^a

Michael Lynch

1 Introduction: the classic view of measurement

Measurement in science traditionally is defined as an assignment of numbers (or, in some definitions, numerals) to fundamental attributes of an object or event studied (Kyburg 1984). Once such a correspondence between numbers and objective properties is established, the numbers can be manipulated via mathematical operations and the results assigned back to the measured phenomena. The classic conception of measurement can be elaborated to include the entire range of techniques through which geometric models mediate the interpretive relationship between theory and data: 'Using a geometry, the abstract objects and events of physical theory are composed into models which give a picture of reality and which are used to connect theory by experimental and nonexperimental investigation to sense impressions' (Willer, 1984: 243).

This broadened definition of measurement comprehends a more complex array of practices than simply attaching numbers to objective attributes. The construction of an 'interpreted diagram' or model articulates the relevancies under which theoretical expressions are brought into correspondence with empirical properties. A model's constituent symbols and imagery can enable progressive generalisation of a family of models for mapping and measuring diverse phenomena. Despite its emphasis on the dependence of models on theory, this definition retains the classic concept of measurement as a bringing together of symbolic imagery and 'small bits of information which we receive from the world' (Willer, 1984: 247).

The idea that measurement is constructed (and its validity assessed) in terms of an inherent isomorphism between mathematical symbols and empirical data is widely assumed by social scientists. Even critics of measurement practices in the social sciences often avow that the natural sciences are blessed with

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