

Preface

The pieces presented in this collection were all written over the last decade or so. They comprise essays, reviews, extended notes, and preliminary reports representing what we hope are reasonably accessible summaries of various things we have mused on or argued over during that time. None was written with the express purpose of publication and certainly not with the thought they might make an integrated package. Moreover, they remain in various states of almost-near-completion. Whilst we recognise their unbuffed, ill-fashioned character, it is unlikely much more work will be done on them. That being the case, now seems an appropriate time to release them to the world. Although they are an assortment, we have clustered the pieces into broadly defined topic areas. It may be some will feel individual items have been mislocated. We are relaxed about that because there is no cumulative master narrative being laid out here. Other than the loose topical clustering and a tendency to move focus from the most general and introductory to the more specific and hence specialized viewpoint, there is no organising internal logic at work. One outcome of this, of course, is that many of the essays find us traipsing across the same or very similar terrain. All we can do is beg our readers' indulgence.

Part I is deliberately pedagogical in nature and is aimed at those coming new to Sociology or Ethnomethodology. It assumes no more than a reasonable introductory knowledge of what Sociology is about (or claims to be about). The aim is not to say or show anything novel but to repeat things others, especially Garfinkel and Sacks, have said before. However, it does try to do this in a somewhat different way. Our hope is to encourage those learning Sociology to see *and grasp* the problems of abstraction and formal description for themselves through examination of the detail of actual pieces of sociological reasoning rather than just acquiring a superficial knowledge of those problems and an ability to parrot arguments about their causes. Building on a body of work in the Philosophy of Science, we develop a simple heuristic for tracing how sociological reports transform data collected about aspects of the social and reduce that data to

sociological phenomena which can then be analysed. The point is to show novice sociologists how they might sense assemble the practical reasoning which supports any particular sociological report which they happen to be reading. All we want is to provide a guide for those have no experience of doing sociological reasoning themselves and so we hope no more is made of these pieces than what they aim to be, working notes for a possible teaching strategy. We are clear, then. There is not much news in Part I for established conventional professional sociologists. More than likely they already know the problems from the inside. Neither is there any for fellow ethnomethodologists. The message is old hat for them. Beside introducing the heuristic, Part I also illustrates its use with types of sociological analysis other than the formal mathematical modelling used in the initial demonstration.

Part II is devoted to some misunderstandings found in the application of social epistemology to the practise of the natural and social sciences and assumes a little knowledge of the range of pathways that particular domain has followed since the (mostly phony) wars over science in the late 1980s and the related emergence of Feminist Standpoint Theory. These misunderstandings have resulted in overly extravagant claims about what sociological findings might mean for an understanding of the epistemological status of the natural and social sciences. Part III takes an interest in more parochial matters but places them in a similarly broad context. It traces the trajectory of Institutional Ethnography from Dorothy Smith's radical re-interpretation of Sociology's social character to its current realisation as the embodiment of familiar ethnographic techniques found in conventional sociological work.

We expect Part IV will mostly be of interest only to those friends and colleagues who have been wondering what lies behind our occasional public interjections regarding debates over Ethnomethodology's current progress and direction. Here the essays try to pick their way through a number of often counterposed positions in order to uncover the sources of what seem to us to be unnecessary assertions and very likely to be unsuccessful interdisciplinary and cross disciplinary projects related to them. Part IV assumes, then, more than a little familiarity with the current state of Ethnomethodology.

Although the collection presented is a motley, we think some common threads can be discerned. The first is what we have referred to as the sense assembly of the sociological. This is not surprising because it is a concern we have returned to time and again over the years. On each occasion we were motivated by a simple but straightforward question. "How can a particular sociological description be framed as an instance of practical reasoning?" Alas, the answers have

always proved less simple and less straightforward to frame than the question and so we have never been able to shape a fully satisfactory response.

The problem manifested itself once again when we took up an interest in applications of forms of mathematics within sociological reasoning, especially attempts to apply mathematical models from Physics to represent problems in sociological theory and analysis.¹ When reading examples of mathematical sociology, we became more and more aware of a disjunction between the monothetic character of the sociological phenomena described by the mathematics and the polythetic nature of the social experience depicted in the data from which the phenomena were derived. This we called “the problem of the abstraction gap” and the strategies for its practical management we described as its “praxeology”. Such management strategies were provisions made in the construction of a sociological investigation and its reporting to prevent difficulties arising from the processes of isolation and simplification which generate abstraction gaps together with attempts to ameliorate the situation when they did. We suggested their role was to secure or sustain the plausibility of the reasoning structure developed in the report. The broad metaphor we used to draw these issues out was the conception of writing sociology as a production process and the writer and readers as complementary ‘recipient-designed’ writer/reader social actors.

It is easy to discern the influence of Harold Garfinkel and Harvey Sacks in this formulation. Their discussions of modes of sociological description and related suggestions about the reconstructed nature of sociological accounts and “the unsatisfied programmatic distinction between and substitutability of objective for indexical expressions” [Garfinkel 1967, p 4] had already pointed to the possible significance of the abstraction gap we had identified. However, they and we lacked a framework by which its features might be made visible and so analysable in terms of their local ‘production processes’. This deficiency was remedied when we encountered what were for us at the time two unrelated clusters of ideas, though now we now see them as intimately connected. The first was the claim made by James Bogen and James Woodward [Woodward and Bogen 1988] and later revised and extended by Woodward [Woodward 2009] that the natural sciences do not describe their data. They describe their phenomena. The process of scientific investigation in the natural sciences is a process of data acquisition and

¹ Throughout this collection, we shall reserve capitals to designate disciplinary, sub-disciplinary bodies of practise (Physics, Sociology, Arithmetic, Epistemology etc.) and clearly identified unified bodies of work (Standpoint Theory, Critical Theory, Institutional Ethnography, Newtonian Mechanics, Quantum Mechanics, Ethnomethodology etc.). We use lower case designations (mathematical sociology, applied mathematics, social epistemology, quantitative and qualitative sociology etc.) to particular modalities of practise.

transformation followed by analysis. In the transformation phase, experiential data are turned into theorised phenomena. The second was a remarkable set of studies by Mark Wilson [Wilson 2017; Wilson 2017; Wilson 2019]. In these studies, Wilson reconstructs the practical reasoning of Classical Mechanics. In so doing, he points to a range of practices which he groups under three heads: constructing well posed problems; avoiding difficult or undoable physics; preserving internal coherence by effacing unnecessary or irrelevant features or steps. Professional good practise in Physics consists in deploying these strategies and their tactics in recognised ways and under normatively regulated conditions.

For Bogen and Woodward as well as Wilson, data reduction is key. To be processed by analytic methods, the manifold of experience captured in and as data must be rendered down. This rendering is a response to the challenge of choosing among the problematic possibilities of description when assembling everyday experience. The term the mathematical sciences use for this resolution is 'characterisation'. Characterised data are data turned into phenomena. Characterisation involves the selection of a minimal set of parameters to be used in the description of an object or process and the stipulation of the variables and measures applied to those parameters. Analysis is the generalisation over equivalence classes constructed from characterised data.

Related to the first thread is a second; the epistemic status of sociological descriptions and the metaphysical claims which can be made on their basis. Whilst visible elsewhere, this thread actually provides the tonal palette for Part II. In previous work [Anderson and Sharrock 2019], we have argued for the treatment of sociological representations as "keyed descriptions" with their defining conceptualisations being "convenient fictions" organised by tropes. The need to reiterate the advantages of this instrumental treatment was brought home to us recently by a couple of comments made by David Albert. He prefaces these comments with a little story in which he imagines he is teaching Classical Mechanics. Having outlined the nature of the 6^N -dimensional phase space and how it can be represented in a single curve, he points out the mathematical advantages of this form of representation. After the lecture, two students approach him. The first exclaims: "I now understand that we *really* live in a 6^N -dimensional physical space—not the 3-dimensional one I had always taken for granted". The second, somewhat more cautiously, says: "I now understand there is simply no *fact of the matter* as to whether our world consists of N particles moving around in a 3-dimensional space or a single particle moving around in a 6^N -dimensional space". Albert makes it clear both students have failed to understand his lecture and the nature of models in Physics. He goes on:

What I should say to these students—what I should *explain* to these students—is that phase space, as it is employed in classical mechanics, is an explicitly and self-consciously “roundabout” way of talking about systems of classical particles. It is useful for all sorts of practical and theoretical and calculational purposes—but it is not meant to be taken as a *direct* or *literal* picture of what is going on. [Albert 2022]

The import of Albert’s comment for us lies in the state of affairs it points to. If, in a discipline as rigorous and systematic as Physics, those learning the discipline and going on, no doubt, to have successful careers in it, can simply fail to grasp the constructed nature of their discipline’s representations and the reasoning processes behind them, how much more likely is it to be the case in Sociology?²

The third thread runs through almost every element but is most easily glimpsed in Part IV. It concerns Ethnomethodology’s ambivalence towards conventional Sociology and the tensions generated thereby. Ethnomethodology emerged from conventional Sociology as a response to dissatisfaction with the central tenets of that discipline’s methodological stance. As a result, setting up a well-known sociological position on some phenomenon as the foil against which an ethnomethodological treatment of what appears to be the same issue can be laid out, has become a standard opening move for investigations. Usually, this is couched as an allegation about features of “the setting” being deployed as resources rather than taken as topics. Although Garfinkel is often more guarded, he is nonetheless very clear. For him, Ethnomethodology is an alternate sociology not an optional investigative technique within conventional Sociology. It has been formed and framed to do things conventional Sociology does not want to do and could not do even if it wanted to. The two are incommensurable in some sense; a position which led to him defining Ethnomethodology’s attitude to conventional Sociology as “indifference”.

Given the principle of indifference and its foundation in Ethnomethodology’s incommensurability with conventional Sociology, it makes no sense to berate some instance of Sociology for not being ethnomethodological. Yet, this is what the use of the foil often amounts to. One way to resolve this tension is to treat sociological practise ethnomethodologically; that is, as cases of practical reasoning. In doing so, we are enjoined to set aside the claims professional Sociology makes about its theories, methods and findings and look solely at what it takes to get it

² This is not to say all physicists are in this position or that the nature of the discipline’s representations has not been understood. However, for many the implications are both puzzling and distasteful. Unlike Albert, their reactions when queried on them is more likely to be that summarised by David Mernin’s sardonic “If I were forced to sum up in one sentence what the Copenhagen Interpretation [of Quantum Mechanics] says to me, it would be “Shut up and calculate!” [Mernin 1989]

done as the practical, routine, generally accepted and acceptable going concern it is. Points can be marked where the practice of the discipline departs from the claims made about it, but such marking is simply a note about the practical, not an opportunity for excoriation and critique. Given the principle of indifference, it could not be.

It would seem obvious that Ethnomethodology can adopt precisely the same ethnomethodological attitude to instances of ethnomethodological reasoning itself and so treat them under the same principle of indifference. Indeed, we ourselves have occasionally attempted this. However, and this is a crucial point, all species of Ethnomethodology should exhibit adherence to *the same* fundamental principles, a condition which does not apply vis a vis Ethnomethodology and Sociology. Whilst it is clear that it is not a requirement of ethnomethodological analysis of ethnomethodological reasoning, commentary can draw out when and where adherence to those principles seems to have lapsed. A just balance must be struck, however, between Ethnomethodology's aspiration to analytic indifference and its desire for the achievement of disciplinary probativeness.

A further thread is the difficult question of Ethnomethodology's potential therapeutic function with regard to the domains in which it carries out investigations. Although it seems pretty clear Ethnomethodology might be able to play that role for Sociology, it can only fulfil the necessary diagnostic and remediation requirements by setting aside its critical stance and with it the oft adopted insistence Sociology should re-specify itself in line with ethnomethodological principles. No doubt such a change in tack would present challenges, obligations and constraints but they are likely to be vastly different to and more straightforward to accommodate than will be the case with many other arenas of disciplinary and professional practise and, in particular, the natural sciences.

Our final thread is possibly the most elusive but perhaps the most important. Its shadow presence can be felt in our repeated use of words like "depiction" and "construal" in relation to sociological descriptions and their construction. These terms are functions of our attempts to operate a systematic suspension (though, having been shooed out of the front door, these beasties have a way of creeping back in a rear window when you aren't looking) of epistemological commitments such as empiricism's myth of the given, the Kantian fudge of the synthetic apriori and Phenomenology's vision of a transcendental Ego grasping noumenal essences. This suspension leaves our sociologising oriented towards a space of descriptions (to paraphrase Wilfred Sellars) with no place for naturalistic observation, analytic realism or eidetic reduction. Such a sociology has no truck with 'fundamental', 'foundational', 'grounding' or 'necessary' or any other

epistemologically privileged representations except as social objects subjected to its analyses. Instead it accepts the imposition of an overriding concern with the praxeological character (the 'how') of the organisation of description and the methods deployed to manage and resolve the problematic possibilities of adequate description under whatever canons of adequacy may be in play on the occasions being examined. This radical equality of sense assembly is, of course, reflexive. It could not be otherwise. For this form of sociology, since the only way to determine the adequacy of accounts is from of the context of their deployment and the determinations made by those engaged in the courses of action unfolding therein, the adequacy of its own descriptions is given by the consistency and coherence of the application of its adopted structuring by keys. This is not the whirligig of relativism nor the regress of scepticism but a commitment to the probativeness *within* modes of analysis. Given a mode and its keys, things can be settled. But only for that mode.

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