Introduction

Many years ago, Bergner (1981) argued that the very nature of modern social science means that there are many starting points leading to multiple perspectives, so that there is no prospect of a single coherent and comprehensive scientific paradigm prevailing in the field as a whole, or even separate but related ones within each of its constituent disciplines in the manner proposed, for example, by Parsons (Parsons and Shils 1951). Bergner claims that this stems from the commitment of social scientists to neo-Kantianism, with its insistence that all inquiry is perspectival and constitutive, each perspective potentially generating knowledge of some aspects of the world while always obscuring others. In fact, Bergner did not take full account even of all the sociological approaches prevailing at the time he was writing, though he does briefly mention one of the more radically distinctive ones: ethnomethodology. And it is by no means clear that there ever was, even less that there is today, general adherence to a neo-Kantian philosophical position in social science. Many ethnomethodologists, as well as representatives of some other approaches, would dissent from that position.

Of course Bergner’s conclusion – that social science is irredeemably plural – may yet be true.¹ But it should be noted that representatives of the various competing approaches within social science are usually in favour of pluralism only as a matter of convenience: generally speaking, they do not celebrate it but are tolerant of diverse perspectives because this is required for the survival of their own approach. In the words of one ethnomethodologist: ‘many sociologies if necessary, but preferably only one’ (Sharrock 1974). There are, of course, those who champion pluralism, but, as Goldthorpe (2016: 127) has insisted, there is surely a point at

¹ As an illustration of the diverse character of ‘sociologies’ today, one could compare Goldthorpe’s (2016) Sociology as a Population Science with Fox and Alldred’s (2016) Sociology and the New Materialism; two books that share very little in common. Goldthorpe champions a longstanding tradition – investigations of population regularities based on survey research, for example on the topic of social mobility – whereas what Fox and Alldred propose, as their title indicates, is of newer provenance. Goldthorpe offers a well-defined conception of sociology that is very narrow in comparison with what currently comes under that disciplinary banner. The kind of sociology prompted by ‘the new materialism’ is more wide-ranging, but is still at odds with most conventional sociological work, not least that promoted by Goldthorpe.
which pluralism becomes a liability rather than an asset. And it must be suspected that this point was reached some time ago in the case of sociology, whose labelled varieties are now beyond worthwhile count.

Towards the end of the twentieth century, Huber (1995) had sounded a warning that the subject lacked a disciplinary core. Nearly twenty years earlier, Hindess (1973: 9) had written about ‘the chaotic and frequently incoherent state of modern sociology, reflected in its theoretical anarchy and the coexistence within it of radically heterogeneous and often incompatible positions’. In fact, concerns about the discipline’s lack of coherence can be traced back to the beginning of the twentieth century (see, for example, Käsler 1983), or even into the nineteenth century, with Simmel’s distinctively narrow definition of its focus challenging Comte’s and Spencer’s much broader ones, and in its turn being rejected by his contemporaries, not least by Durkheim (Frisby 1981: 56–61).

Of course, the differences among sociological approaches vary in depth and significance. In the case of ethnomethodology there is, I will suggest, a deep divide from most other versions of the discipline. This is not to say that it shares nothing at all with them, but the differences are much more striking than the similarities. My task in this book will be to examine these distinctive features, their implications for its relationship with more conventional forms of social science, and the arguments that underpin its radical stance.

Among those who deplore the proliferation of paradigms, of whom I am one, there is often a tendency to assume that it stems from spurious causes: self-promotion on the part of individual social scientists, the force of their ideological convictions, an excessive concern with ‘innovation’, mere following of fashion, etc. However, it seems to me that, to some degree at least, the divided state of social science arises from fundamental, unresolved problems. This is also the view of many ethnomethodologists, and I believe that much is to be learned, both positively and negatively, from their diagnoses and proposed solutions. Hence this book.

The history and character of ethnomethodology

Ethnomethodology was invented by Harold Garfinkel: both the name and the distinctive approach to the study of social life to which it refers (see Garfinkel 1968: 5–11). There is a contrast here with Comte’s invention of sociology, since – while he was the first to use that term (in print in 1839) – in many respects what he proposed was not sharply different from what is to be found, for instance, in the writings of Saint-Simon and Condorcet. Moreover, histories of sociology often go back well beyond even these writers.2 In the case of Garfinkel, however,

2. For example, in his classic account of the development of sociology, Aron (1969) begins with Montesquieu; and some commentators trace it further back to the Scottish Enlightenment, to medieval social and political thought, or even to the writings of Aristotle and Plato.
ethnomethodology is a line of thinking and a form of practice that, in many respects, is strikingly at odds with nearly all previous types of sociological inquiry.\(^3\)

At its most basic, the term ‘ethnomethodology’ refers to the study of how processes of social interaction generate the intelligible world we all experience, and largely take for granted. It stemmed originally from Garfinkel’s recognition that, in some contexts at least, lay people operate with similar concerns and methods to social scientists, and that it is on the basis of these that they make sense of the world and, simultaneously, make the world intelligible for one another. Thus, when he was employed in a study of jurors, Garfinkel observed that they deliberated in a practical way about methodological issues in much the same manner as social scientists: they were concerned with what counts as reliable evidence, what can legitimately be inferred from the evidence available, how competing explanations should be assessed, and so on. He also noted that, in determining what conclusions they should reach, jurors drew not just on what they had been told about relevant legal matters but also on background knowledge about society: about what normally happens in particular sorts of situation, types of people and their characteristic concerns and behaviour; and so on. And, importantly, they routinely assumed one another to have such knowledge. Another key point was that this process took place over time, so that people’s practical reasoning operated both retrospectively and prospectively in making sense of ‘what must have happened’, ‘who must have been responsible’, ‘what is the correct legal judgement’, and so on. Furthermore, Garfinkel notes that they drew on this knowledge in essentially \textit{ad hoc} ways suited to the requirements of the situation. As part of this he concluded that legal rules and other principles are actively used by people to make sense of particular contexts, and that this involves simultaneously making local sense of the meaning of the rules or principles themselves (see Garfinkel 1967a: ch. 4; Hill and Crittenden 1968: 5–11).\(^4\)

Garfinkel formulated the name ‘ethnomethodology’ on analogy with ‘ethnobotany’, ‘ethnophysiology’, and ‘ethnophysics’: anthropological sub-disciplines concerned with documenting the distinctive cultural knowledge and practices of people in ‘pre-scientific’ societies about plants, bodies, and physical phenomena, respectively.\(^5\)

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3 Some have argued that there is strong continuity between ethnomethodology and ‘classical sociology’, for instance Hilbert 1992, but I find this argument is unconvincing. Nevertheless, there are some shared themes; indeed, I will point to some interesting parallels with Simmel’s ‘formal sociology’ in Chapter 2.


5 On the nature of ethnoscience, see Sturtevant 1964. The method it employs was sometimes labelled componential analysis; see Goodenough 1956.
So, the term ‘ethnomethodology’ was intended to refer to the common-sense knowledge and methods relevant to social affairs deployed, and assumed, by members of a society. It is worth noting at this point, however, that there is a significant difference between those anthropological sub-disciplines and ethnomethodology, in that the former do not imply that how people make sense of plants, bodies, and physical phenomena determines the nature of those phenomena, whereas Garfinkel claims that the methods ethnomethodology studies produce the very phenomena to which these methods relate. However, there was, and is, a significant tension within anthropology between a view of the sciences as a superior source of knowledge, with anthropology itself being one of the sciences, and attachment to a form of cultural relativism in which, at the very least, Western ideas, including those produced by science, are to be suspended in order to try to understand the belief systems of non-Western societies in their own terms. As we shall see, there is a parallel issue within ethnomethodology concerning its own relationship to conventional professional and lay understandings of the social world.

While Garfinkel developed ethnomethodology over the course of the late 1940s and 1950s, it was not until the 1960s that a significant body of researchers began to adopt it, at a time when dominant sociological approaches were coming under challenge from several directions, leading to a sense of fundamental crisis (Friedrichs 1970; Gouldner 1970). This was also a time when the number of sociologists was increasing significantly, against a background of broader sociocultural changes in Western societies (Bell 1976; Martin 1981; Denzin 1989). Looking back, Wieder et al. (2010: 128) report that: ‘Given the struggle to understand what was later to be more widely known as EM, there was a sense of being involved in something new, something revolutionary, and something that offended established sociology – all this, perhaps, being in keeping with the spirit of the 60s’ (see also Mehan and Wood 1975).

In the mid-twentieth century, a variety of approaches arose that suspended the assumption, previously widely taken for granted, that social phenomena – institutions and structures – exist and have particular characteristics independently of the perceptions and actions of people in the society concerned; indeed, that they determine those perceptions and actions. Also challenged was the idea that lay people’s perspectives routinely involve misconceptions that need to be corrected by social science; on the grounds that they are either only proto-sociological in character; or are the product of ignorance and ideology. Instead, it came to be

6 Interestingly Wieder (1977: 2) distinguishes between ethnomethodology, concerned with the formal features of accounting practices, and ethnoscience, which focuses on the substantive content of the accounts people provide about life in their society. For critical discussions of the differences between ethnoscientific and ethnomethodological, from an ethnomethodological perspective, see Wieder 1970 and Wootton 1975: ch. 2.
argued that cultural variation involves not just differing values and attitudes but also fundamental differences in perception and cognition; so that the task of sociology, for some, became exploring and documenting this diversity. Indeed, it was often proposed that people belonging to different groups live in different ‘worlds’, or experience different ‘realities’; and the challenging task was to understand each of these in its own terms. Particularly significant developments here, besides ethnomethodology, were Berger and Luckmann’s (1966) *The Social Construction of Reality*, which reformulated the sociology of knowledge partly along phenomenological lines (see also Psathas 1973); the increasing importance of symbolic interactionism and the development of labelling theory in the sociology of deviance (Becker 1963; Pollner 1974); Winch’s Wittgenstein-inspired critique of social science, and exploration of the problem of understanding other cultures (Winch 1958, 1964; Lerner 2002; Hutchinson et al. 2008); and the emergence of various kinds of constructionism (Weinberg 2014).

An important influence on many sociologists at the time was Kuhn’s (1970) analysis of the history of science, as punctuated by the emergence of a succession of incommensurable paradigms. As a result, natural science was no longer to be seen either as the progressive accumulation of facts or as the testing of hypotheses against incontrovertible evidence. Rather, Kuhn portrays it as operating within a host of untested assumptions on which it necessarily relies, these even determining what would count as sound evidence. His work prompted the development of a relativist version of the sociology of science, concerned with explaining ‘what passes for’ scientific knowledge (Barnes 1974; Bloor 1976). The fact that natural science displayed ‘incommensurable’ paradigms was widely taken to challenge any sharp epistemological distinction between scientific and other kinds of understanding, and any insistence on the superiority of the former. At the same time, this view of science opened up what was a largely new terrain for sociology, bordering on and potentially taking over territory from both philosophy and psychology; and it was also used to legitimate the diversity of approaches emerging in the field.

The sites where ethnomethodology initially became established were various campuses of the University of California in the 1960s and 1970s. Garfinkel taught at UCLA, stimulating several generations of students there, some of whom went on to carry out ethnomethodological work themselves and to teach on other
The radicalism of ethnomethodology

West Coast campuses, and elsewhere. Among the most important were Harvey Sacks and Emmanuel Schegloff, whose work on conversation analysis came to provide an influential model for one kind of empirical work that ethnomethodologists could do. Others – such as David Sudnow, D. Lawrence Wieder, Egon Bittner, Melvin Pollner, and Don Zimmerman – carried out ethnomethodologically-informed ethnographies. As this brief account implies, while Garfinkel published several articles, some subsequently being included in his seminal book *Studies in Ethnomethodology* (Garfinkel 1967a), much of his influence took place through teaching, and this was true later in his career as well. In this and other ways, at first ethnomethodology was to a large extent based upon a predominantly oral culture. Also important, was the circulation of unpublished materials by Garfinkel, some of which came to be published towards the end of his life (Garfinkel 1948/2005, 1952/2008, 2002).

Garfinkel’s writings, published and unpublished, have constituted a rather equivocal stimulus – not least because of his increasingly enigmatic mode of expression – in response to which readers, both ethnomethodologists and others, have frequently offered competing readings. The early death of Sacks, and the fact that most of his writings were in the form of lectures, unpublished in his lifetime, but again available in mimeographed form, may, over and above intrinsic merit, have facilitated the development of a thriving movement, while also leading to accusations that ethnomethodology constituted a ‘sect’ or a ‘cult’ (Coser 1975). At the same time, the highly successful development of conversation analysis as a research programme – by Sacks, Schegloff, Jefferson and others – had the effect of raising the profile of ethnomethodology considerably within social science and beyond – notably in linguistics and even in psychology. Furthermore, while conversation analysis was subjected to considerable criticism, its empirical and constructive character, and the cumulative nature of its findings, matched some of the ideals to which many mainstream social scientists were committed; and arguably did so more effectively than other approaches.

As I noted earlier, ethnomethodology survives today more successfully than most other radical new approaches announced in the 1960s and 1970s – such

8 For details regarding personnel, albeit not entirely accurate, see Mullins 1972: ch. 8 and Flynn 1991: ch. 1.
10 For diverse views about Garfinkel’s style of writing on the part of ethnomethodologists, see Wilson 2003: 491; Rawls 2006:5; Watson 2017: 16–18. For the view of a far from neutral outsider; see De Mille 1990: 80.
11 Schegloff (1992a, 1992b, 1999; Prevignano and Thibault 2003:chs 2, 9) has provided detailed accounts of the development of conversation analysis.
12 On the failure of mainstream sociology cumulatively to develop knowledge, see Rule 1997.
as ‘conflict sociology’, ‘humanistic sociology’, ‘existential sociology’, and ‘ethogenics’. While it has remained relatively marginal to the discipline overall, in both institutional and intellectual terms, clusters of ethnomethodologists still operate in the USA, the UK, Germany, France, Australia, and elsewhere, forming part of a loose global network. Indeed, these clusters probably represent amongst the most durable networks to be found within social science over the past 50 years.\(^\text{13}\)

One could argue that part of the reason for the endurance of ethnomethodology, despite never having been dominant in most of the fields in which it has had a presence, is that it displays a continual strain towards purity. Generally speaking, a radical identity in any social movement must be actively sustained both by drawing sharp dividing lines that mark it off from competing approaches, and by strongly criticising and marginalising members who are judged to deviate from its ethos. Both these processes can be found within the history of ethnomethodology. For example, like Garfinkel, Erving Goffman was also teaching in the University of California during the 1950s, and some of his students later became influential ethnomethodologists, notably Sacks, Schegloff, and Sudnow. Initially, the boundary between ethnomethodology and his work, and that with symbolic interactionism more generally (represented by Blumer at Berkeley), were blurred. Garfinkel, Sacks, Goffman, and Blumer all shared a concern with closely examining processes of social interaction \textit{in situ}, a focus on the role of communication within these processes, and a resistance to the dominance of conventional quantitative methods. However, ethnomethodologists came increasingly to insist on the radical difference between their own approach and the work of Goffman and the symbolic interactionists (Zimmerman and Wieder 1970; see Chapter 2).\(^\text{14}\)

The strain towards purity has also involved criticising internal variants that were judged to have deviated from the central principles of ethnomethodology. For example, the distinctive positions of Cicourel and his students, of Blum and

\(^\text{13}\) These clusters are partly coordinated via the International Institute for Ethnomethodology and Conversation Analysis: see http://www.iiemca.org/_wp_live/. See Psathas 2010, and the editor’s and author’s introductions in Garfinkel 2002. Psathas (2008) discusses two ‘schools’ of ethnomethodology beyond the West Coast: those at Boston University and in Manchester (UK). Conversation analysis has grown increasingly independent, and has become even more widely institutionalised: see Sidnell and Stivers 2013: Intro.

\(^\text{14}\) On these differences see Dennis 2011. See also Gallant and Kleinman 1983; Rawls 1985. Fine (1993: 61) has noted a different outcome from that of ethnomethodology in the case of symbolic interactionism; he writes that ‘Once considered adherents of a marginal oppositional perspective, confronting the dominant positivist, quantitative approach of mainstream sociology, symbolic interactionists find now that many of their core concepts have been accepted. Simultaneously their core as an intellectual community has been weakened by the diversity of interests of those who self-identify with the perspective.’ For an illuminating analysis of radicalism by an ethnomethodologist that, unfortunately, does not apply it to radical movements within social science, see Bittner 1963. However, Maynard and Clayman (2018) apply his analysis to the position adopted by some of their fellow ethnomethodologists.
McHugh, and (later) of Pollner came to be viewed by many ethnomethodologists as beyond the pale. There has also been marginalising of work on ‘institutional talk’ (Schegloff 1991, 1992a; see also Hester and Francis 2000, 2001), and even conversation analysis itself has been criticised, in the highly influential form practised by Schegloff and his associates, as well as that more recently characteristic of Heritage and his collaborators (see Lynch 1993, 2016a, 2016b; Lynch and Bogen 1994; Livingston 1987; Lynch and Macbeth 2016). There have also been discussions in which competing versions of the approach have been distinguished within the work of Garfinkel himself: the ‘protoethnomethodological’ versus ‘postanalytic ethnomethodology’ (Lynch 1993), ‘classical’ versus ‘radical’ (Wilson 2003, 2012), and ethnomethodology 1.0 and 2.0 (Pollner 2012a).15

In light of the fundamental challenge it poses to conventional sociology, heightened by this strain towards radical purity, it is perhaps not surprising that the reaction of most mainstream sociologists to the emergence of ethnomethodology, where they took any notice of it at all, fell short of enthusiastic adoption. It ranged from cautious interest in what usefully might be learned from it methodologically (see some of the contributions to Hill and Crittenden 1968), through attempts to incorporate elements of it into more conventional sociological approaches, whether through a merger with symbolic interactionism (Denzin 1969) or through integration with some other form of social theory (for example Giddens 1984; see Tucker 1998), to hostile rejection, treating it as suffering from ‘sterility’, ‘inadequacy’ and ‘banality’ (Coleman 1968), as ‘an unfruitful approach to sociology that impedes its development’ (Blau 1969: 128), or as the ephemeral product of West Coast fashion (Gellner 1975).16

Subsequently, attitudes towards ethnomethodology within the discipline have generally amounted, at best, to grudging tolerance; though this has not prevented it having considerable influence. In particular, it has shaped some of the many forms of discourse and narrative analysis that have come to be practised across a variety of substantive areas, as well as the development of actor network theory and related approaches.17 There have also been effects on the practice of ethnography, and on other kinds of qualitative research, within and beyond sociology; particularly in encouraging a focus on the detailed analysis of patterns

15 See also Liberman’s (2013: 1) reference to ‘old school ethnomethodology’. Some ethnomethodologists have adopted a more catholic stance, accepting the diversity of ethnomethodology; see for instance Maynard and Clayman 1991; Psathas 1995.

16 For other external commentaries from sociologists with diverse commitments, see Wilkins 1968; Gouldner 1970: 39–5; Goldthorpe 1973, 1974; Coser 1975, 1976; Grabiner 1975; McNall and Johnson 1975; Maroules and Smelser 2006. For replies by ethnomethodologists to some of the attacks, see Benson (1974), Mehan and Wood (1976); Zimmerman (1976).

17 See Latour 2005. For relevant discussion of the relationship of ANT and some other approaches in Science and Technology Studies to ethnomethodology, see Lynch 1993; Sharrock and Button 2011.
of social interaction, with a view to identifying processes of social construction and/or the operation of social forms of one kind or another (see, for example, Holstein and Gubrium 2008; Silverman 1993a; Atkinson 2015; see Chapter 5 in this volume). It has also been influential in interdisciplinary fields like Communication Studies, the investigation of Human-Computer Interaction, and Science and Technology Studies.\(^\text{18}\) However, in this book I will examine ethnomethodology primarily against the background of its relationship to the discipline of sociology.

### Sociological background

In order to understand the significance of Garfinkel’s ideas we need to consider the state of US sociology in the 1940s and 1950s, a period punctuated by the Second World War.\(^\text{19}\) Sociology was in the process of expansion and institutional diversification over this period (see Odum 1951), with other centres, notably Harvard and Columbia, taking over the Chicago Department’s previously dominant position (see Bulmer 1984). Garfinkel initially studied business accounting at the University of Newark,\(^\text{20}\) where he became friends with several individuals who were later to become influential in their fields – Melvin Tumin, Herbert McClosky, and Seymour Sarason – as well as coming to know already established figures in sociology like Paul Lazarsfeld and Philip Selznick, who were then at Columbia University. He began his graduate studies at the University of North Carolina, where a distinctive brand of empirical sociology was being taught, broadly in the spirit of the Chicago tradition but with a strong emphasis on contributing to the solution of social problems.\(^\text{21}\)

Garfinkel later moved to Harvard, where Parsons, with others, had established the Department of Social Relations: an interdisciplinary unit that drew together sociology, anthropology and psychology (Schmidt 1978). Parsons led a strong push for systematic sociological theory, and also for the clarification and development of its relations with other disciplines. This was promoted not just in his individually produced books at the time – *The Structure of Social Action* and *The Social System* – but also in interdisciplinary collaborative work concerned with developing various aspects of his theoretical framework (Parsons and Shils 1951; Parsons et al. 1953).

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\(^\text{18}\) The interdisciplinary connections of conversation analysis are particularly wide: see Sidnell and Stivers 2013.

\(^\text{19}\) On the effects of this on sociology, see Parsons and Barber 1948.

\(^\text{20}\) Pollner (2012b) has explored the possible influence of this on the subsequent emergence of ethnomethodology, notably through the concept of ‘accountability’, as well as the ‘selling’ of ethnomethodology as a business.

\(^\text{21}\) While at North Carolina he wrote a short story about racial discrimination which was published – Garfinkel 1940 – and reprinted in a couple of collections. See Doubt 1989 and Hama 2009.
Other important developments were the growth of research on small groups (the study of jurors in which Garfinkel participated was part of this) and on decision-making in organisations, both of which were stimulated by practical as well as theoretical concerns. Garfinkel was involved in one of the major post-war projects relating to these areas, involving psychologists and economists as well as sociologists, run by Wilbert Moore at Princeton, an older-generation student of Parsons (see Rawls 2008).

The ethos of the environment in which Garfinkel was trained was one of optimism about the development of sociology and about its significant social role. Moreover, the conception of science involved had moved on considerably from the earlier Chicago focus on case studies of groups involved in social problems, on community studies more generally, and on the statistical documentation of social trends. There was a much more explicit concern with developing and testing sociological theories, and with investigating forms of social organisation at micro and macro levels through both theoretical and empirical work, the latter often quantitative in character.

Garfinkel’s relationship to prevailing forms of social science seems to have been increasingly ambivalent. Much of his early work was concerned with problems in carrying them out, for instance in coding (for sociological purposes) records produced by bureau officials and others. On the basis of these investigations, Garfinkel raised fundamental questions about what was being achieved and how the study of social order, social organisation, and practical decision-making was being pursued (Garfinkel 1948/2005, 1952/2008). His doubts were centred on his concern with ‘the problem of meaning’, this having developed especially through a reading of the phenomenological writings of Schutz and Gurwitsch. In order to explore this he deployed informal experiments, perhaps modelled on some of those in small group research at the time (on which see Strodtbeck and Hare 1954), to reveal the practices in which people engage so as to make sense of situations. This was achieved by intentionally disrupting these practices (Garfinkel 1963: 187 and passim; 1967a: chs 1–3).

22 After noting the ‘immense sums of foundation money’ that had gone into social science, he comments that ‘it is common knowledge that in the overwhelming number of researches that are methodologically acceptable, and, paradoxically, precisely to the extent that rigorous methods are used, dramatical discrepancies are visible between the theoretical properties of the intended sociological findings of inquirers and the mathematical assumptions that must be satisfied if statistical measures are to be used for the literal description of the intended events’ (Garfinkel 1967a: 101–2).

23 There is an interesting parallel, for example, between Garfinkel’s experiment on pre-medical students designed to engender consternation and confusion and Asch’s (1951, 1955) experiments on conformity (Clayman and Maynard 1995: 5). However, Psathas (2012) also points out that Garfinkel’s breaching experiments were a practical implementation of Husserl’s notion of ‘free variation’. These ‘experiments’ attracted considerable attention. Here was a sociologist instigating social disorganisation, rather than studying it with a view to finding a
Introduction

Perhaps reflecting Garfinkel’s ambivalence towards prevailing forms of social science, the exact relationship between ethnomethodology and its source discipline, as well as with other related disciplines, is rather uncertain (Dennis 2003; see Chapter 3 of this volume). While he has often claimed no more than that ethnomethodology opens up a new realm of phenomena for investigation, one that had previously been largely overlooked, his arguments have generally been taken to carry profound implications for social science and its relationship to the people and activities it studies. On the one side, it is often held to undercut social scientists’ stereotypical treatment of lay people’s understandings of the social world as generally false and/or irrational, and as in need of replacement by sound social scientific evidence. On the other side, Garfinkel’s work implies that, contrary to the claims of many social scientists to rely solely on ‘logic’ and ‘evidence’, in pursuing their work they actually deploy more or less the same kinds of reasoning and data as the people they study, and rely upon common-sense knowledge, this raising questions about their claim to a scientific approach. Furthermore, their explanations typically treat commitment to particular values and norms as causal factors explaining people’s behaviour. Yet, if the meaning of rules and principles cannot be specified independently of their use, as Garfinkel’s own work suggested, then (he argued) these could not serve as causal factors. And this connects to the more general problem of the relationship between the concepts that lay people employ and those that social scientists produce in order to explain their actions (see Winch 1958; Louch 1966; Hutchinson et al. 2008).

What all this makes clear is that, at a minimum, ethnomethodology involves suspending the assumption that what conventional social science produces is superior to practical common-sense knowledge and skills. This suspension is partly a product of the way that ethnomethodology shifts the focus of inquiry on to the role of such knowledge and skills in social life. In this way, it distances itself from what Garfinkel later began to refer to as ‘constructive analysis’, ‘formal analysis’, or ‘the world wide social science movement’. He came to argue that ethnomethodology is simply ‘incommensurable’ and ‘asymmetrically alternate’ to this (Garfinkel and Wieder 1992). However, as already noted, many ethnomethodologists have explicitly challenged the claims to validity and value that conventional social scientists make for their work. For instance, Mehan and Wood (1975: 63) declare that ethnomethodology has ‘found social science out’. To quote another
The radicalism of ethnomethodology

particularly strident example, it has been suggested that what social science produces is no different from ‘journalism or [ideas put forward by] political parties and pressure groups, technology enthusiasts or conspiracy theorists or; indeed, the person standing next to you at the bar’ (Button et al. 2015: 137). These latter authors propose that ‘ethnomethodology would do away with social science’ (p. 135). Meanwhile, others have insisted that ‘there is no such thing as a social science’ (Hutchinson et al. 2008, emphasis added; Hammersley 2017a).

A radical project

There is little doubt, then, that ethnomethodology presents a fundamental challenge to conventional social research, and that the sorts of investigation it proposes are very different from the mainstream, despite the latter’s diversity. There are several aspects to its distinctiveness.

In the 1960s, ethnomethodology formed part of a broader movement concerned with the sociology of everyday life, in which the work of Goffman also played a central role (Truzzi 1968; Psathas 1980). Where other interactionist work at the time focused on the study of deviance, on forms of occupational work, or on distinctive types of local community, Goffman was interested in relatively universal processes of face-to-face interaction. In effect, this new movement sought to promote a change in ‘news values’: it announced a focus on issues that, in conventional sociological terms, were trivial. While Simmel had earlier opened up the study of everyday life, in this respect he had had few followers within the discipline.

Furthermore, the new field was often treated by its exponents as having theoretical priority: it was insisted that studying the nature of mundane processes of social interaction provides an essential foundation for social science; that this area had been neglected as a result of sociologists’ concern with producing abstract theoretical accounts of social structures, or with addressing high-profile social problems. Also at fault, it was argued, was sociologists’ reliance upon highly structured methods of data collection that cut them off from what normally happens in natural settings.

As this suggests, there was also often a methodological critique of mainstream social science. In one of the most influential early presentations of ethnomethodology, Zimmerman and Pollner (1970: 80–1) open their discussion as follows:

In contrast to the perennial argument that sociology belabors the obvious, we propose that sociology has yet to treat the obvious as a phenomenon. We argue

24 Subsequently, the study of everyday life has recurrently been revived, in a variety of forms: Sztompka 2008; Pink 2012; Kalekin-Fishman 2013; see also Human Studies, 3:1, 1980; Sociology 49:5, 2015.
Introduction

that the world of everyday life, while furnishing sociology with its favoured topics of inquiry, is seldom a topic in its own right. Instead, the familiar, common-sense world, shared by the sociologist and his subjects alike, is employed as an unexplicated resource for contemporary sociological investigations. (pp. 80–1)

In line with Garfinkel’s early writings (see Garfinkel 1962), Zimmerman and Pollner insist on the need for a ‘principled respect for the distinction between the world of common-sense as a resource and the world of common-sense as a topic’, implying thereby that conventional sociology is unprincipled in this regard (p. 84).

While Zimmerman and Pollner do not spell out exactly why sociology’s reliance upon unexplicated resources is a problem, it was not difficult at the time to understand the challenging implications of their argument. These are signalled when they write that: ‘sociology apparently is in the position of providing a professional folklore about the society that, however sophisticated, remains folklore’ (p93). The predominant methodological views within sociology in the 1950s and 1960s were what could be broadly described as positivist, and a key element was the idea that scientific knowledge is founded on empirical givens accessed by scientists employing objective methods. As already noted, this necessarily implied a contrast with the informal and ‘subjective’ perception and cognition of non-scientists, of ‘ordinary folk’. To show, as Zimmerman and Pollner go on to do (pp. 86–92; see also Cicourel 1964), that, in practice, sociologists rely upon common-sense knowledge and members’ methods for making sense of the world, amounts to a damaging critique of this position: it documents the failure of sociology to live up to the precepts that were its scientific credentials. This was, in effect, an internal critique of conventional sociology, but it was also the case that ethnomethodologists themselves insisted that, in studying the social world, researchers must not simply trade on the background knowledge of it they have as participants, that this knowledge must be the focus of inquiry if that inquiry is to be rigorous.

In this emphasis on rigour, ethnomethodology differed from many of the other new approaches to sociology that emerged in the 1960s and 1970s, including those that made up the study of everyday life. Indeed, ethnomethodologists put forward an alternative conception of rigorous inquiry, and of what the task of sociology should be. This was initially modelled on phenomenology, to a considerable extent, but appeal was also sometimes made to a specific natural science model, what Sacks (1984: 26) referred to as ‘primitive sciences’, such as nineteenth and early twentieth-century biology (Lynch and Bogen 1994). In both cases what was emphasised was the need for careful, detailed description of phenomena.

25 It should be mentioned that there were, in fact, important parallels between positivism and phenomenology in the early twentieth century. And these were brought to a sophisticated synthesis in Felix Kaufmann’s Methodology of the Social Sciences (1944; see Huemer 2003), a book to which Garfinkel makes frequent reference. It is also worth noting that Husserl was himself influenced by the neo-positivist work of Avenarius and Mach: see Moran 2012: 187.
Also influential for some ethnomethodologists have been the philosophical writings of Wittgenstein, and of other philosophers belonging to what is sometimes referred to as the tradition of ‘ordinary language philosophy’ (two other early key figures were Ryle and Austin). There are, of course, tensions amongst these models and sources, but it is of significance that both Husserl and Wittgenstein challenged conventional philosophy in ways that are not entirely dissimilar to the manner in which ethnomethodology challenges conventional social science.  

So, ethnomethodology insists that rigorous analysis of the social world must focus on description of how phenomena come to be constituted in and through processes of social interaction, as phenomena of particular kinds that have determinate relations with one another. While the parallel with phenomenology is clear; it should be underlined that Garfinkel’s motive for adopting this orientation did not stem primarily from philosophical concerns: it resulted specifically from his identification of fundamental problems in the practice of conventional social science. He argued that these derived from the fact that the meanings of social actions are context-dependent, and therefore in an important sense ad hoc. And he set out to investigate this ‘indexicality’ of action-meanings, and how this was practically remedied, doing so through empirical investigation of what is publicly observable, rather than by employing phenomenological investigations of personal experience.

This emphasis on the indexicality of meaning led to a further sense in which ethnomethodology was radical, what might be referred to as ontological occasionalism. In the account provided by Zimmerman and Pollner (1970), and even more in Garfinkel’s later work, social phenomena – indeed all phenomena – are treated as generated entirely through processes of social interaction on particular occasions; having no existence independent of these processes, and deriving their character entirely from them. Thus, in a later article, Pollner (1979: 249) writes that ‘social meanings are continuously created and recreated through the situated praxis which presupposes, preserves, and uses those meanings. Thus, “table” is a gloss for the cognitive, practical, and interactional work through which “table” is enacted.’

26 The influence of Wittgenstein seems to have derived to a considerable extent from the work and teaching of Wes Sharrock at Manchester. It can be seen particularly strongly in the writings of one of his early students, Jeff Coulter, and in that of some of his other colleagues. Wittgenstein has also been a particularly important influence on the work of Mike Lynch.

27 There are only a few parallels with the traditional philosophical doctrine of ‘occasionalism’ (on which see Pyle 2003: ch. 5; Lee 2016), but it is a convenient term in this context. The most obvious parallel is the emphasis on how events are produced through action on specific occasions. Another, I suggest, is the offering of an account that stresses a single source of experienced events, and one that is fundamentally at odds with what is routinely assumed by most participants involved in those events. This is a contentious claim I will discuss in the Conclusion.
Introduction

There may also be a further, related, sense in which ethnomethodology is radical. Rawls (2006: 82) has claimed that it undercuts virtually the whole of Western philosophical thought, challenging its focus on individual, collective, or transcendent minds as the source of knowledge and morality. She writes that

Instead of treating logic and reason as the starting points for a theory of social order; theory of knowledge, or conception of justice, beginning with the individual mind, or shared conceptual structures (culture), Garfinkel argues that logic and reason are characteristics of situated orders and depend entirely on the details of these orders for their production. On this view, the mutual commitment to constitutive orders of practice is at the heart of everything that is considered essential to the human being: reason, self, and sociality. Garfinkel offers the study of social interaction as the key to unlocking the great philosophical questions. As he says, ‘once we get beyond Kant’, reason, will, and moral reciprocity are revealed as questions of social order. They are not characteristics of the mind, however transcendent, and they are not values.28

In effect, the claim is that ethnomethodology represents a ‘Copernican Revolution’ to trump that of Kant, in the sense that not only is thought grounded in action but the relationship between actor and action is reversed; it is social practices that constitute the actor, rather than their being produced by decisions on the part of individual actors in light of intentions, motives, norms, values, etc. In other words, the identities of actors, and mental phenomena generally, do not precede action but are a product of it.29 On this interpretation, ethnomethodology entails a whole philosophy, one that is at odds with the assumptions about the social world built into what Husserl (1965, 1983) calls ‘the natural attitude’. There is a parallel here, perhaps, with the work of Heidegger, who argues that human being should not be understood ‘primarily as a site of mental activity or a metaphysical substance, but rather as a “unity of living-through [Er-lebens]” whose fundamental relation to the world [is] activity, not theoretical contemplation’ (Gordon 2010: 73).30

28 Rawls (2002) makes a similar claim, in more general terms in her introduction to Garfinkel’s Ethnomethodology’s Program, presenting him, along with Wittgenstein and Wright Mills, as confronting ‘the problematic legacies of empiricism and neoKantianism’, all three writers insisting that ‘treating intelligibility and social order as fundamentally a matter of matching concepts with reality or logic was the problem’, and independently embracing ‘the social as an alternative approach to the great philosophical questions of knowledge and meaning’ (p. 3).

29 There is, of course, a parallel here with some versions of structuralism and post-structuralism that announce ‘the death of the subject’.

30 The quote within this quote is from Heidegger 1962: 73. See also McHoul 1998. This radical philosophical interpretation seems to be confirmed by Lynch (2016a: 1) who writes: ‘Harold Garfinkel and Harvey Sacks emphasized that ethnomethodology and conversation analysis differed fundamentally, not only from sociology but also from intellectual traditions dating back to ancient Greek philosophy.’ However, for many ethnomethodologists, including Lynch, Wittgenstein would be a more appropriate reference point than Heidegger.
The radicalism of ethnomethodology

It should be clear from this discussion that ethnomethodology’s claims to radicalism are beyond question, albeit in epistemological and ontological rather than political terms. Of course, whether such radicalism is desirable, and whether ethnomethodologists’ arguments are justified, is another matter. I will address this question in later chapters.

Ethnomethodology and research methodology

Perhaps not surprisingly, ethnomethodology is sometimes assumed by those first coming across the word to represent a new social research methodology; or, alternatively, it is confused with ethnography. While some ethnomethodological work has been ethnographic, a great deal has not been, and the two terms certainly do not have the same meaning. The issue of whether ethnomethodology offers a new methodology is less clear-cut. On the one hand, ethnomethodologists would certainly resist the idea that their work offers a new method of doing what mainstream social scientists attempt to do, for reasons already explained. Furthermore, they would deny that ethnomethodological research amounts to following a single method or set of procedures. In fact, it has taken a variety of forms, and has used diverse kinds of data. These include: Garfinkel’s (1967a, 1967b) breaching experiments; participant observation (Bittner 1967a, 1967b; Sudnow 1965, 1967; Zimmerman 1970a, 1970b; Wieder 1974; Anderson et al. 1989); the detailed analysis of audio or video recordings of naturally occurring social interaction (Heath and Luff 1992; Sacks 1992a, 1992b; Goodwin 2000; Schegloff 2007), and the work of many other conversation analysts; self-description of social experience (Sudnow 1978; Robillard 1999; Anderson and Sharrock 2018); the use of documents (Anderson 1978; McHoul 1982; Davies 1995; Watson 2012) or some combination of these (see, for instance, Sormani 2015).

More fundamentally, many ethnomethodologists resist the idea that their work employs specialised techniques at all; they insist instead that their task is simply to document the observable ways in which social phenomena are generated, and that the primary requirement for this is a suspension and explication of the natural attitude. However, this should not be taken to mean that the task is easily achieved. There are now quite a lot of introductions to ethnomethodology and conversation analysis, many of which are specifically concerned with assisting readers in carrying out this type of research themselves (for example Leiter 1980; Benson and Hughes 1983; Livingston 1987; Francis and Hester 2004; Hutchby and Wooffitt 2008; ten Have 2008). Moreover, in the case of conversation analysis, this has usually been treated as involving the production of audio or video recordings, and the use of technical transcription conventions to represent social interaction, as well as learning a distinctive form of analysis; one which requires

31 For a discussion of their relationship, see Pollner and Emerson 2011.
viewing social interaction in ways that are significantly different from how it is treated by participants – not least, paying attention to what they largely take for granted.

Despite many ethnomethodologists’ coolness towards research methodology (see, for instance Sharrock and Anderson 1986: 107–8), it is important to remember that, to a large extent, it was methodological concerns that were the driving force behind Garfinkel’s work. His starting point was a recognition that sociological research was failing to live up to its methodological ideals (Garfinkel 2006). And this also seems to have been the stimulus for Sacks (1963), who is undoubtedly the second most important figure in the movement. Moreover, as we have seen, ethnomethodologists often claim that their work is rigorous in a way that conventional social science is not.

Perhaps not surprisingly, given its history, there have been relatively few sustained efforts on the part of outsiders to understand the assumptions that underpin ethnomethodology, and to take seriously their implications for social science.32 This is the task I have set myself in this book. While I stand ‘outside’ ethnomethodology, I am sympathetic to the spirit of the movement, and I have endeavoured to understand its principles as well as I can. Engaging with it is important because, as I indicated earlier, the problems it has identified in mainstream social science are genuine and serious ones that deserve more attention than they are typically given. Writing about the work of Harvey Sacks, Silverman (1998: ix) has commented: ‘Whether or not we follow the path that Sacks sets out is perhaps less important than whether we respond to the questions that [he] poses about social science’. He goes on to suggest that ‘after more than twenty years, they are … still vitally important and still largely unanswered’. Another twenty years on, this remains true. At the same time, I have not hesitated to criticise what I find to be weaknesses in the case for ethnomethodology, and to identify problems with the alternative approach(es) to studying the social world proposed and practised. I believe that this sort of critical engagement is essential if we are to have any chance of resolving the divisions within sociology, and within social science more generally.33

### Overview of chapters

The chapters in this book largely take the form of separate essays, examining various aspects of ethnomethodology and its sources; though they reflect a consistent stance towards it, and towards social science more generally (a

32 For one previous example, see Atkinson 1988, and the response: Watson and Sharrock 1991. See Hammersley 2017d.

33 Few of my criticisms are new – most can already be found in the literature – but I have sought to spell them out clearly and fully, and to show how they relate to one another.
broadly neo-Kantian one). The fact that they are separate essays means that there is occasional overlap, but I hope that this will be, at most, only mildly irritating for any resolute reader who starts at the beginning and persists to the end. I found that writing the book in this manner was the only hope I had of completing it.

In Chapter 1 I try to clarify the views of Alfred Schutz, who was a major influence on Garfinkel’s development of ethnomethodology, and more broadly on what came to be called phenomenological sociology in the 1960s and 1970s. I note the reception of his work by Garfinkel and others, but suggest that in important respects its purpose and nature have been misinterpreted. I examine the context in which Schutz began his studies in Vienna in the 1930s, and in particular his relationship to Austrian economics, along with the influence of logical positivism, notably through his friend Felix Kaufmann. Schutz’s aim was to resolve a problem that had been at the heart of this economic tradition: to ground its basic theoretical principles. And he identified much the same gap in the interpretive sociology of Max Weber. He argued that Weber’s development of the concept of ideal types was an effective way of understanding economic theory, and a sound basis for sociological analysis, but that Weber had failed to secure its roots. Schutz drew on the work of Bergson and Husserl in an attempt to clarify the nature of the lifeworld that underpins social and economic action. In this chapter I consider whether his work represented a fundamental challenge to what widely came to be seen in the 1960s as the positivism of the dominant sociological tradition; or, alternatively, whether it should be regarded as displaying strong elements of positivism itself, as has sometimes been suggested. I also address the question of whether Schutz regarded his work as part of social science or of philosophy, and therefore whether he was, in fact, aiming to build a phenomenological sociology, as seems to have been assumed by Garfinkel and many others.

The next chapter compares the orientations of Garfinkel and Goffman. They were contemporaries who knew one another; and their work is often regarded as similar; being concerned with the study of mundane patterns of social interaction. Some have suggested that their work is complementary and could be integrated into a more comprehensive approach. However, ethnomethodologists have usually insisted that there are fundamental differences between the two. Against this background, I examine both the similarities and differences, doing this via a comparison with the work of a third sociologist, Georg Simmel. It has often been noted that Goffman seems to have been strongly influenced by Simmel. By contrast, there are few signs that Simmel influenced Garfinkel, or ethnomethodologists more generally. Nevertheless, there are some interesting parallels. In particular, they share a concern with the constitutive role that social interaction plays in social life. While recognising that there are important similarities between the
orientations of Garfinkel and Goffman, I argue that the suggestion that their work is complementary and could be integrated fails to take proper account of fundamental differences, and that these shed light on the radical distinctiveness of ethnomethodology. These differences focus not just on the sorts of data used but more particularly on what they are used to do. For Goffman, the aim is to generate conceptual frameworks that illuminate everyday behavior; whereas ethnomethodologists resist the bringing in of new concepts, being concerned instead with explicating the processes by which social phenomena are produced in their own terms. Other differences relate to what is taken to be the context of social interaction, with Goffman treating the interaction order as mediating the effects of outside factors, whereas ethnomethodologists insist that the context of any process of social interaction can only be what is constituted as context within it.

Chapter 3 considers the ambiguous disciplinary status of ethnomethodology, and especially its problematic relationship with the parent discipline: sociology. Sometimes ethnomethodology is presented as a radical internal reform movement, aimed at re-specifying the focus of the discipline and its mode of operation. On other occasions, it is put forward as an ‘alternate’ or supplement to mainstream sociological work, and as having an ‘indifferent’ attitude towards it, treating it as simply another kind of members’ practice that can be studied by ethnomethodologists. Or, again, particularly in the form of conversation analysis, ethnomethodology is sometimes treated as if it were a discipline in its own right, independent not just of sociology but also of the various other disciplines with which it has a relationship, such as linguistics. Finally, some have suggested that it is anti-disciplinary in character, as regards social science, holding that ethnomethodology can best serve as a complement to other forms of practice, including the work of natural scientists and of those developing new technologies. I examine the cogency of each of these positions, arguing that ethnomethodology’s critique of social science, while salutary, seems to imply abolition rather than reform; and I suggest that the proposal of its complementary relationship to conventional social science or forms of practice leaves open the question of why such a supplement is required. I acknowledge that there are strong grounds for claiming that conversation analysis has provided a significant cumulative development of knowledge, but I note questions about whether its ethnomethodological character was essential to its success in this respect, or is even compatible with it. Less successful in developing a cumulative body of knowledge has been the tradition of ‘studies of work’, but this can, like conversation analysis, reasonably claim to have made a practical contribution to some fields, for example to system and software design. Once again, though, it is not clear that this stems from the ethnomethodological character of the investigations. In conclusion, I suggest that the ambiguous status of ethnomethodology is built into its very nature.
In Chapter 4 I present a fuller assessment of what seem to be the key theoretical presuppositions of ethnomethodology. It treats social order as the central focus of sociological (and perhaps even of all social scientific) inquiry, but reinterprets this concept, transforming the way it was understood by Parsons. Indeed, Garfinkel redefines it as the intelligibility of coordinated patterns of action. He also breaks with Parsons’ ‘analytical realism’, and the stance of other sociological approaches that emphasise the need for a theoretical framework if the causes of social order, and of disorder, are to be investigated effectively. Ethnomethodology insists, instead, that order is observable routinely in everyday situations, and that how it is constituted can only be discovered once the natural attitude (and, along with this, the theoretical attitude adopted by social scientists) is suspended so that processes of social interaction can be carefully examined in their own right. Another core element of ethnomethodology is the idea that the meanings of social actions are locally variable and context-dependent (‘indexical’ and ‘reflexive’), rather than being determined by a semantic code, and that they are intelligible because they are self-identifying – their meaning is displayed and recognised by actors via shared methods or practices, in other words they are ‘accountable’. Ethnomethodology also involves some important methodological commitments. As I noted earlier, it often seems to assume that for inquiry to be rigorous it must avoid reliance upon unexplicated resources, appealing only to what is observable or intersubjectively available. Finally, ethnomethodology appears to treat the aim of rigorous social analysis as literal description, rather than explanation or the production of explanatory theory: the task should be to ‘make visible’ members’ methods for the production of social phenomena. I assess each of these claims in detail, concluding that, while they relate to significant issues for social science, the intractability of these problems has been exaggerated. Moreover, ethnomethodology itself does not escape them.

In the final chapter before the Conclusion, I consider the influence of ethnomethodology on qualitative research methodology, which is one of the main areas of mainstream social science where it has had an impact. I focus particularly on the reception of Cicourel’s (1964) book *Method and Measurement in Sociology*, and also on how conversation analysis has shaped the work of many discourse analysts, and some ethnographers. I outline Cicourel's argument that sociology needs to be re-founded methodologically on an empirical theory that respects the complex and contingent character of human action and communication, along the lines suggested by ethnomethodology. He claims that this would facilitate resolution of the fundamental methodological problems that have plagued sociology. The effects of his early work were to support the rise of qualitative research and to encourage reflexive attention to the processes by which data are produced. However, these developments often subsequently tended to go in directions that were at odds with his conception of rigorous analysis. As regards the influence of conversation analysis, I note how this encouraged the use of electronic recordings.
and transcriptions as data, raised doubts about the traditional uses of interviews, and encouraged the micro-analysis of patterns of social interaction. Furthermore, like Cicourel’s work, it facilitated the spread of social constructionism. I argue that these effects have been beneficial in many respects but more negative in others.

The book ends with a Conclusion that summarises and elaborates on my evaluation of ethnomethodology, and considers its implications.