CHAPTER 1

Ethnomethodology

Ethnomethodology can be described briefly as *a way to investigate the genealogical relationship between social practices and accounts of those practices.* It generally is considered to be a subfield of sociology, although it also is represented in communication studies, science studies, anthropology, and philosophy of the social sciences. Its connection to twentieth-century traditions in social theory and sociological methodology is both deep and ambivalent. It is deep because ethnomethodology offers a way to address a set of themes that have unquestioned pride of place in theories of social structure and social action. They include action, order, rationality, meaning, and structure, among others. These themes are also prominent in methodological debates about the relationship between commonsense reasoning and scientific analysis. The connection is ambivalent because ethnomethodology's orientation to these foundational topics puts it at odds with most of the established theories and methodologies in the social sciences.[1]

Ethnomethodology's relationship to sociology is difficult to describe and comprehend. Many ethnomethodologists work in sociology departments, so in that sense they are sociologists, but one of their most infuriating research policies has been to place "professional" sociological methods for generating knowledge of social structure alongside the "lay" know-how that is substantively part of the society that sociologists study. As a matter of research policy (if not personal conduct), ethnomethodologists treat the "family concerns" of professional sociology with studious indifference.[2] By treating lay and professional methods as part of the same domain of study, they distance themselves from the disciplinary form of life in which they and their sociologist colleagues conduct their professional affairs. The infuriated

[1] These issues are addressed at length in Graham Button, ed., *Ethnomethodology and the Human Sciences* (Cambridge University Press, 1991).

[2] The policy of ethnomethodological indifference is discussed in Harold Garfinkel and Harvey Sacks, "On formal structures of practical action," pp. 337–66, in J. C. McKinney and E. A. Tiryakian, eds., *Theoretical Sociology: Perspectives and Developments* (New York: Appleton-Century-Crofts, 1970), repr. in H. Garfinkel, ed., *Ethnomethodological Studies of Work* (London: Routledge & Kegan Paul, 1986), pp. 160–93. Also see Benetta Jules-Rosette, "Conversation avec Harold Garfinkel," *Sociétés: revue des sciences humaines et sociales* 1 (1985): 35–39. I elaborate on the policy of ethnomethodological indifference in Chapter 4.

reaction by the colleagues down the hall can be no less understandable and no less well founded than the high rage expressed by family members toward the students who performed an ethnomethodological "breaching experiment" by pretending to be strangers in their family households: "Why must you always create friction in our family harmony?" "I don't want any more of *that* out of you, and if you can't treat your mother decently, you'd better move out!" "We're not rats, you know."[3]

What would motivate such a disconcerting and apparently disrespectful "attitude" toward the profession?[4] Different answers can be given in different cases, but for many of us, it has to do with the combination of an intense interest in the topics that sociologists address and a profound disappointment with what happens to those topics when they are subjected to professional sociological analysis. This disappointment is due not only to the widely acknowledged "looseness" of sociology's analytic procedures or the inconclusiveness of the predictions based on them. In my judgment, it has more to do with the way that sociological perspectives and methods have been designed to give unified treatments of an entire roster of topics: families, religions, riots, gender relations, race and ethnicity, class systems, and the like. Of course, plenty of room is left for the empirical study of these topics, but the dominant trend in late-twentieth-century sociology has been to subsume the entire roster of social phenomena under an overarching conceptual framework that defines a set of analytic categories that apply at the "level" of society as a whole.[5] Consequently, familiar facets of family life, religious experience, economic activity, and daily life generally become thinned out when they are treated as cases to be analyzed by referring to a stock of sociological variables and using statistical procedures to determine sources of variation from norms. Sociology also includes so-called qualitative and micro approaches, but often the tendency is to treat these as preliminary or degraded modes of analysis, and the descriptive orientation in such studies is often dominated by an effort to show how the observable

[3] Harold Garfinkel, *Studies in Ethnomethodology* (Englewood Cliffs, NJ: Prentice-Hall, 1967), pp. 48–49.

[4] For rhetorical purposes, I am presenting the story as though all ethnomethodologists evinced the same (extreme) attitude toward their colleagues. Actually, persons who identify themselves as ethnomethodologists have worked out various accommodations to their disciplinary situations. These can be likened to the different "stances" that inmates in an asylum take toward the institution and its staff: openly rebelling, going along with, or making the best of the situation. See Erving Goffman, *Asylums* (Garden City, NY: Doubleday, 1961).

[5] This approach derived from Talcott Parsons's theories of social action and social system. The dominance of Parsons's "structural functionalist" program began to wane in the late 1960s, and many sociologists today treat it as defunct. To a large extent, however, the Parsonian conception of science and basic orientation to social order remains very much alive in the glossaries of concepts and in treatments of theory and method in contemporary sociology textbooks.

details of the cases studied "reflect" the behind-the-scenes operation of abstract social "forces." There is a clear and understandable reason for sociology's dominant theoretical and methodological orientation: It instantiates an *image* of a unified science inherited from logical–empiricist philosophy of science, and it conveys a *sense* of a progressive and authoritative approach that incorporates the essence of natural science inquiry.

Debates about whether sociology should be scientific have gone on for at least a century, and those who opposed scientism in sociology lost the battle long ago, even though their position continued to survive as a minority view. However, ethnomethodologists opened a new front in this classic battle by closely studying the relationship between sociological practice and everyday language, and by so doing they provided a kind of empirical rebuttal to sociology's scientism that could no longer be dismissed as "merely" a philosophical argument. Since the early 1970s still another battlefront was opened up when constructivist sociologists of science, and a few ethnomethodologists, turned their attention to the "actual" practices in the natural sciences and found (or at least claimed) that the logical–empiricist view of scientific inquiry was an idealized and substantially mistaken version of scientific practice. These studies were far from definitive, and many of them remained respectful of established sociological traditions, but they raised new kinds of trouble for analytic sociology's conception of science. First, they "problematized" many of the assumptions about scientific theory and method that are entrenched in sociology's pedagogy, and second, they challenged the tendency in sociology to address "social aspects" that were analytically distinct from the "concrete" and "internal" details of the practices studied.

For now, I shall defer a more extended discussion of how the sociology of scientific knowledge confronted established sociological conceptions of scientific theory and method. In the rest of this chapter, I present a brief history of ethnomethodology, in order to introduce some of the distinctive research policies that inform the discussions in later chapters. As mentioned in the Introduction, my overall purpose is to set up a critical convergence between the treatments of scientific practice and ordinary language use developed by ethnomethodologists and sociologists of science. This, in turn, will lead to a set of recommendations for "respecifying" some of the central topics of social theory and epistemology.

Garfinkel's invention of ethnomethodology

Unlike many other developments in sociology, ethnomethodology can be traced to a definite origin. Harold Garfinkel is universally acknowledged as the "founding father" of the field, although he occasionally has joked that he

has sired "a company of bastards." He coined the term *ethnomethodology* in the mid-1950s, but it did not become familiar until the mid-1960s when several of his and his students' works were published. During a symposium held in the late 1960s, Garfinkel recounted the story of how he came up with the term ethnomethodology while he was preparing a series of reports for a multidisciplinary study of jury deliberations at the University of Chicago:

> I was interested in such things as jurors' uses of some kind of knowledge of the way in which the organized affairs of the society operated – knowledge that they drew on easily, that they required of each other. At the same time that they required it of each other, they did not seem to require this knowledge of each other in the manner of a check-out. They were not acting in their affairs as jurors as if they were scientists in the recognizable sense of scientists. However, they were concerned with such things as adequate accounts, adequate description, and adequate evidence. They wanted not to be "common-sensical" when they used notions of "common sensicality." They wanted to be legal. They would talk of being legal. At the same time, they wanted to be fair. If you pressed them to provide you with what they understood to be legal, then they would immediately become deferential and say, "Oh, well, I'm not a lawyer. I can't be really expected to know what's legal and tell you what's legal. You're a lawyer after all." Thus, you have this interesting acceptance, so to speak, of these magnificent methodological things, if you permit me to talk that way, like "fact" and "fancy" and "opinion" and "my opinion" and "your opinion" and "what we're entitled to say" and "what the evidence shows" and "what can be demonstrated" and "what actually he said" as compared with "what only you think he said" or "what he seemed to have said." You have these notions of evidence and demonstration and of matters of relevance, of true and false, of public and private, of methodic procedure, and the rest. At the same time the whole thing was handled by all those concerned as part of the same setting in which they were used by the members, by these jurors, to get the work of deliberations done. That work for them was deadly serious.[6]

As Garfinkel elaborates, the jurors conducted themselves predominantly as practical reasoners with no professional credentials or technical expertise for collecting and assessing evidence, performing methodic demonstrations, or making judgments about matters of fact and opinion. Nevertheless, in their own way they addressed familiar "methodological" concerns during

[6] In Richard J. Hill and Kathleen Stones Crittenden, *Proceedings of the Purdue Symposium on Ethnomethodology* (Purdue, IN: Institute for the Study of Social Change, Department of Sociology, Purdue University, 1968), pp. 6–7. This excerpt was reprinted in Harold Garfinkel, "On the origins of the term 'ethnomethodology,'" pp. 15–18, in Roy Turner, ed., *Ethnomethodology* (Harmondsworth: Penguin Books, 1974). Also see John Heritage, *Garfinkel and Ethnomethodology* (Oxford: Polity Press, 1984), p. 45.

their deliberations, and the way they did so was substantively related to the verdict they negotiated. By analogy with the various "ethnoscience" approaches in social anthropology, Garfinkel proposed that "ethnomethodology" would be a way to study these largely untutored methods of practical reasoning.

While pursuing a Ph.D. in Harvard's Department of Social Relations in the late 1940s, Garfinkel had been exposed to some of the early efforts by anthropologists to develop "ethnoscience" approaches. A few years later, while working on the Chicago jury project, he read a review of research in those fields and hit on the idea of developing an "ethnomethodology." The ethnosciences – ethnobotany, ethnomedicine, ethnophysics, and so forth – were designed to elicit culturally specific taxonomies of plants, animals, medicines, color terms, and other semantic domains and to map them against a backdrop of relevant scientific knowledge.[7] So, for instance, ethnobotany is the study of "native" classifications of plants, that is, culturally specific systems of names for differentiating types of plants, including the systematic axes and hierarchical relationships between plant categories. After eliciting these classifications from native informants, the ethnobotanist compares them with the taxonomies developed by contemporary botanists. The differences between native and scientific taxonomies can then be interpretively related to characteristic patterns of native custom, ceremonial practice, and kinship organization. By analogy, ethnomethodology would be the study of the ordinary "methods" through which persons conduct their practical affairs. The relationship between ethnomethodology and the other ethnosciences should, however, be treated with some caution, since there are some striking differences:[8]

1. Garfinkel characterized the jurors' methodology in terms of broad epistemic distinctions.[9] The jurors' methods for assessing the veracity of

[7] The ethnosciences retained some of Durkheim and Mauss's interest in native classifications. See Emile Durkheim and Marcel Mauss, *Primitive Classification,* trans. J. W. Swain (London, 1915). Claude Lévi-Strauss's more general discussion of the "sciences of the concrete" draws on ethnoscience research in *The Savage Mind* (Chicago: University of Chicago Press, 1966), chap. 1. Later, some of these approaches became integrated into cognitive anthropology. See Charles Frake, "The diagnosis of disease among the Subanum of Mindanao," *American Anthropologist* 63 (1961): 113–32; Harold Conklin, "Hanunóo color categories," *Southwestern Journal of Anthropology* 11 (1955): 339–44; William C. Sturtevant, "Studies in ethnoscience," *American Anthropologist* 66 (1966): 99–131; Stephen Taylor, ed., *Cognitive Anthropology* (New York: Holt, Rinehart and Winston, 1969).

[8] See Harvey Sacks's remarks on the difference between ethnomethodology and ethnoscience in Hill and Crittenden, eds., *The Purdue Symposium on Ethnomethodology,* pp. 12–13.

[9] What Garfinkel developed can be described as an "epistemic sociology." See Jeff Coulter, *Mind in Action* (Oxford: Polity Press, 1989), pp. 9 ff. The idea of "epistemic sociology" runs the risk of overintellectualizing the juror's work, but it does enable us to see that Garfinkel was respecifying the central topics in epistemology by identifying them as commonplace discursive and practical activities. This theme is picked up in Garfinkel's and his students' studies of work in the sciences (see Chapter 7).

testimony and correctly interpreting the evidence did not closely compare with technical research procedures in jurisprudence, nor did they even dimly resemble standard designs for experimental or other scientific methods. The jurors' deliberations over "facts," "reasons," "evidence," and so on were expressed in a natural language shared with professional jurists and scientists, but the jurors' methodic practices did not closely parallel the methodological techniques in the natural or human sciences. Garfinkel's ethnomethodology was thus not, strictly speaking, an ethnoscience, since it was not precisely based on the model of scientific methodology.[10] Instead, he treated the jurors' commonsense methods as phenomena in their own right. In this, he was influenced by Alfred Schutz's efforts to explicate the phenomenology of the social world.[11]

2. Garfinkel was not proposing to develop *taxonomies* of ordinary methods, nor was he trying to delimit a semantic domain common to particular scientific fields and native cultures. The phenomenon for ethnomethodology was not the system of *names* that jurors used to refer to methodological matters; instead, it was how they accomplished their "methodological" determinations over the course of their deliberations. For Garfinkel, such "methods" include the entire range of lay and professional practices through which social order is produced. By conceiving of these methods as subject matter, Garfinkel was proposing an encompassing approach to the study of social actions and not a study of a particular domain of native classifications (of plants, animals, colors, kin relations, etc.).

3. The "natives" on the jury were not members of an exotic culture; they were variously educated, English-speaking members of the same society that Garfinkel inhabited. For him to study the terms and procedures according to which jurors conducted their deliberations did not require a tutorial in a specialized language, nor did it require anything like the sort of ethnography of distinct horticultural or medicinal arts that might inform a study of ethnobotany or ethnomedicine. The jurors' practical reasoning was intuitively transparent, so much so, in fact, that it was easily disregarded by the other researchers in the Chicago jury project. The researchers wanted most of all to get "behind" the apparent surface of the deliberations to find out what really motivated the jurors. Garfinkel related an anecdote about the jury project that concisely illustrates this:

> In 1954 Fred Strodtbeck was hired by the University of Chicago Law School to analyze tape recordings of jury deliberations obtained from a bugged jury room. Edward Shils was on the committee that hired him. When Strodtbeck

[10] In some respects ethnomusicology would be more comparable because no single genre of modern music provides the measure of native musical practices.

[11] See Alfred Schutz, *Collected Papers,* vols. 1 and 2 (The Hague: Nijhoff, 1962, 1964). I give a more elaborate account of Garfinkel's relation to phenomenology in Chapter 4.

> proposed to a law school faculty that they administer Bales Interactional Process Analysis categories, Shils complained: "By using Bales Interaction Process Analysis I'm sure we'll learn what about a jury's deliberations makes them a small group. But we want to know what about their deliberations makes them a jury." . . . Strodtbeck replied and Shils agreed: Shils was asking the wrong question![12]

Strodtbeck's reply that "Shils was asking the wrong question" alerts us to a dilemma for sociological studies of practical actions that remains contentious to this day. Bales Interactional Analysis is a method of "content analysis" that was once widely used in social psychology.[13] When using the procedure, the analyst observes an interactional event (either live or on tape), and while doing so he or she "codes" the utterances by selecting among an inventory of categories, for example, deciding that a particular speaker's utterance "gives support" to another party in the conversation. Strodtbeck recommended Robert Bales's procedure as a way to reduce the hours of tape-recorded deliberations to a manageable, statistically analyzable data base. Shils's complaint acknowledged that Bales's coding scheme would enable the researchers to construct the sort of data that conventional sociological methods were designed to "handle" but that by using it they would fail to address a more contextually specific set of "methods" that the jurors used. Had Shils and Strodtbeck taken the complaint seriously, they would have been left with no methodological basis beside their own commonsense, prescientific understandings of the "raw data" recorded on their tapes. Nevertheless, Garfinkel took Shils's complaint very seriously, and by doing so he courted the difficulties that Strodtbeck's reply avoided.

4. No developed natural or social science provided a comparative basis for defining and assessing the jurors' methods. Ethnobotanists can often presume a relatively complete scientific inventory of the plants in the region studied, or short of that, they can begin to produce one by conducting laboratory analyses of specimens supplied by native informants. With the scientific taxonomy in hand, the botanist can then consult the native inventory of plant categories to find out whether it includes names for each scientifically validated species. By doing so, the botanist can discover whether the natives assign different names to morphologically different male and female plants of the same species and whether their taxonomy includes distinctions or entire axes that the scientific classification system lacks. In cases in which natives use an herb for medicinal purposes or they prohibit the

[12] Harold Garfinkel, Michael Lynch, and Eric Livingston, "The work of a discovering science construed with materials from the optically-discovered pulsar," *Philosophy of the Social Sciences* 11 (1981): 131–58, quotation from p. 133.

[13] Robert Bales, *Interactional Process Analysis: A Method for the Study of Small Groups* (Reading, MA: Addison-Wesley, 1951).

eating of a plant, laboratory analysis can be used to assess the "actual" biochemical composition and physiological effects of the plant substances. In contrast, under a research policy that Garfinkel later termed "ethnomethodological indifference," he and his students made no assumption that a particular set of methodological prescriptions – whether drawn from sociology, a particular natural science, or formal logic – can act as a standard for defining a rationality operating "beneath" native methods. This policy articulated a decisive break with the established Weberian and Parsonian theories of action that dominated sociology at the time. Although Max Weber's action theory is quite complicated, and it prescribes a legitimate societal place for different types of nonrational as well as rational action,[14] it proposes a descriptive method that uses an idealized "standpoint" of an omniscient scientific observer:

> When we adopt the kind of scientific procedure which involves the construction of *types,* we can investigate and make fully comprehensible all those irrational, affectively determined, patterns of meaning which influence action, by representing them as "deviations" from a pure type of the action as it would be if it proceeded in a rationally purposive way. For example, in explaining a panic on the stock exchange, it is first convenient to decide how the individuals concerned would have acted if they had *not* been influenced by irrational emotional impulses; then these irrational elements can be brought in to the explanation as "disturbances." Similarly, when dealing with a political or military enterprise, it is first convenient to decide how the action would have proceeded if all the circumstances and all the intentions of those involved had been known, and if the means adopted had been chosen in a fully rationally purposive way, on the basis of empirical evidence which seems to us valid. Only then does it become possible to give a causal explanation of the deviations from this course in terms of irrational factors.[15]

The first-person plural pronouns in this passage identify the narrator's voice with the hypothetical scientific observer's viewpoint. In order to "make fully comprehensible" actions in complex historical fields, "our" descriptions would have to have extraordinary scope and specificity. A sociology constructed along these lines would aspire to be a science of all sciences, capable of rendering complete judgments about singular events in countless circumstances. Just imagine what it would take to "decide how the

[14] See Stephen Kalberg, "Max Weber's types of rationality: cornerstones for the analysis of rationalization processes in history," *American Journal of Sociology* 85 (1980): 1145–79.

[15] Max Weber, "The nature of social action," pp. 7–32, in W. G. Runciman, ed., *Weber: Selections in Translation* (Cambridge University Press, 1978), quotation from p. 9. Also see Weber, *Economy and Society,* ed. and trans. G. Roth and C. Wittich (Berkeley and Los Angeles: University of California Press, 1978), p. 6; and Jürgen Habermas, *The Theory of Communicative Action,* vol. 1, trans. Thomas McCarthy (Boston: Beacon Press, 1981), pp. 102–3.

action would have proceeded" in the battle of Waterloo "if all the circumstances and all the intentions of those involved had been known."[16] It can fairly be said that Weber's "we" is inserted in the grammatical place occupied by a omniscient deity in more explicitly theological discourse.[17]

Although Talcott Parsons distinguished his voluntaristic theory of action from positivistic and utilitarian versions of rationality, his theory of social action similarly retained the judgmental position of an idealized scientific observer. Like Weber, Parsons acknowledged that actions could be governed by socially accepted, legitimate standards other than those verified by empirical science. As he defined it, "intrinsically rational" action adhered to scientifically verifiable standards, but this form of action was only a subclass of a much broader domain of normatively guided social action. Parsons never let go of the theoretical leverage provided by an *idealization* of a scientific observer's knowledge of the conditions and choices relevant to actual situations. Parsons used the *imaginable possibility* of obtaining verifiable empirical knowledge of the situation as a standard for defining and distinguishing among the various subjective elements and normative standards composing a conceptual framework for action.[18]

Garfinkel completed his Ph.D. dissertation under Parsons's guidance,[19] and his earlier studies were heavily influenced by Parsons's action theory, but his account of jurors' reasoning entirely recast the relation of the scientific observer to the social actions being analyzed. In contrast with long-standing traditions in sociology, economics, and psychology, Garfinkel advocated neither a man-the-scientist model of rational action[20] nor a virtual "scientific" standpoint from which to assess the rationality of any practice. What was distinctive about his agenda was *not* that he wanted to study ordinary methods of practical reasoning but that he disavowed the privilege of an academic or administrative science. The traditions of social study he rejected also investigate commonsense knowledge and practical reasoning, typically by using formal models and prepackaged analytic techniques, but Garfinkel decided to make a topic of commonsense knowledge of social

[16] These complexities can be appreciated by reading John Keegan, *The Face of Battle: A Study of Agincourt, Waterloo and the Somme* (New York: Viking, 1976).

[17] James Edwards, *The Authority of Language: Heidegger, Wittgenstein, and the Threat of Philosophical Nihilism* (Tampa: University of South Florida Press, 1990).

[18] See Talcott Parsons, *The Structure of Social Action,* vol. 1 (New York: McGraw-Hill, 1937). A useful discussion of Parsons's theory of action and the phenomenological criticisms of it is presented in Heritage, *Garfinkel and Ethnomethodology,* pp. 7–36.

[19] Harold Garfinkel, "The perception of the other: a study in social order" (Ph.D. diss. Harvard University, 1952).

[20] Examples of "man-the-scientist" models of action and reasoning are presented in several of the psychological and cognitive science studies in Denis J. Hilton, ed., *Contemporary Science and Natural Explanation: Commonsense Conceptions of Causality* (New York: New York University Press, 1988).

structures without first setting up a scientific counterpoint to that knowledge.

As I elaborate in Chapters 4, 6, and 7, ethnomethodology's disavowal of classic studies of action, reasoning, and social structure can be extremely puzzling. Garfinkel's proposals seem to divest ethnomethodology of any special entitlement to knowledge about the practices it studies, and it can be baffling to consider what an ethnomethodological pedagogy or an ethnomethodological demonstration of research findings might look like. The entire nexus of methodological concepts and preliminary arguments for establishing a study's evidential claims and conclusions would seem to dissolve in a confused relationship to the fields of commonplace methods described in the research. If methods are to be situated on the side of *what* is described, what can ensure the truth, relevance, and adequacy of any description? These concerns have often been raised about, and against, ethnomethodology, and later I address them at length. But for the present, let me simply mark them as chronic puzzles and complaints about the consequences of Garfinkel's invention.

Early initiatives

Although Garfinkel came up with the term ethnomethodology in the mid-1950s and some of the studies and research policies that later came to be associated with the field were already present in his Ph.D. dissertation,[21] it was not until the mid-1960s that ethnomethodology became a familiar part of sociology.[22] A few of Garfinkel's ethnomethodological studies were presented at conferences and published in the late 1950s and early 1960s.[23]

[21] Garfinkel, "The perception of the other." Talcott Parsons was Garfinkel's dissertation adviser, and the dissertation attempts to develop Parsons's theory of action by infusing it with phenomenological insights largely drawn from Schutz's writings. While at Harvard, Garfinkel was exposed to Aron Gurwitsch's teachings in phenomenology, and he also read some of Husserl's original works. At the time, he did not decisively break with Parsons's program, and it can be argued that the rupture was not complete until the publication of Garfinkel's *Studies in Ethnomethodology*. However, evidence of Garfinkel's "deep disquietude" with the Parsonian system is evident in his dissertation and in various unpublished manuscripts Garfinkel circulated in the late 1950s and early 1960s comparing Parsons's and Schutz's theoretical positions.

[22] An account of some of the early seminars in the 1950s and 1960s is provided in a rather strange account of the history of ethnomethodology: See Pierce Flynn, *The Ethnomethodological Movement* (Berlin: Mouton de Gruyter, 1991), pp. 33 ff.

[23] Some of Garfinkel's early papers include "Aspects of common-sense knowledge of social structure," *Transactions of the Fourth World Congress of Sociology* 4 (1959): 51–65; "The rational properties of scientific and commonsense activities," *Behavioral Science* 5 (1960): 72–83; and "Studies of the routine ground of everyday activities," *Social Problems* 11 (1964): 225–50. The latter two papers were reprinted in *Studies in Ethnomethodology*. As the first reference indicates, Garfinkel presented drafts of some of his papers at conferences in the late 1950s. Somewhat earlier he published a paper in which "ethnomethodology" was not mentioned, although with hindsight it might as well have been. See his "Conditions of successful degradation ceremonies," *American Journal of Sociology* 61 (1956): 240–44.