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Harold Garfinkel and Edward Rose in the early years of ethnomethodology

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Abstract

This article documents the beginning of the intellectual companionship between the founder of ethnomethodology, Harold Garfinkel, and Edward Rose, who is most often associated with his program of "ethno-inquiries." I present results from archival research focusing on the contacts and collaborations between Rose and Garfinkel in the years 1955–1965. First, I describe the review process for Rose and Felton's paper, submitted to the American Sociological Review in 1955, which Garfinkel reviewed and after Rose's rebuttal recommended for publication. The paper induced Garfinkel to write an extensive commentary that has remained unpublished. Second, I discuss the 1958 New Mexico conference sponsored by the Air Force, which was an opportunity for Rose and Garfinkel to work together on topics related to common-sense knowledge and scientific knowledge. Third, I give an overview of the ethnomethodological conferences in 1962 and 1963, supported by an Air Force grant written collaboratively by Rose and Garfinkel. Here I focus primarily on Rose's research on "small languages," which stimulated many discussions among the early ethnomethodologists. Rose's work and exchanges with Garfinkel demonstrate the former's affinity for miniaturization as a research approach and search for ways to empiricize topics of sociological theory in locally observable settings.

KEYWORDS
anthropology, Edward Rose, ethnomethodology, Harold Garfinkel, sociology

1 | INTRODUCTION

In September 1972, Edward Rose wrote in his notebook: "For fifteen years I've been talking off and on with Garfinkel. Out of our conversations, out of the talks he has had and out of his head has come an approach called Ethnomethodology. I am in the background of all that and for purposes of teaching I link myself with the approach and with Garfinkel. Well, there is something to say about this involvement for me and about my non-involvement and differences with Garfinkel."[1] This is the last page in the notebook; nothing else follows. Thus, the primary and overarching task of the present paper is to show that there is indeed "something to say" about Rose's intellectual companionship with Garfinkel in the pioneering years of ethnomethodology that has so far escaped systematic scrutiny. Concurrently, I will show that many of the long-standing preoccupations and shared interests of Garfinkel and Rose emerged as a response to research conducted in human and social sciences at the time, and that the collaboration of these two scholars reflects and reformulates many topics that are both relevant and essential to behavioral sciences.

Since their first documented meeting in 1957, Garfinkel and Rose "did a lot of talking together … about finding things out about people" (Rose, 1992, p. 23), and Rose counted these conversations as "just about the best [he] entered into in all [his] life."[2] On the grounds of Rose's significant involvement in those early years, Pierce J. Flynn includes him as a member of the "first generation" of ethnomethodologists, and remarks that he "has remained a 'fellow traveller' along ethnomethodology's paths" (Flynn, 1991, p. 40). Garfinkel and Rose continued working and talking together for 20 years and remained in touch afterwards.[3] The topics of their numerous talks—taking place mostly in Boulder and Los Angeles—ranged from common sense and shared understandings to conversational structures and invented languages and to ethnography and the social sciences in general. Rose also helped in shaping Garfinkel's ideas on the research launched after the publication of *Studies in Ethnomethodology* (Garfinkel, 1967), known as the ethnomethodological studies of work (Garfinkel, 1986, 2022). In this paper, I focus on the first 10 years of this remarkable and for the most part unknown collaboration in the crucial era that saw the establishment and early development of ethnomethodology as an original approach to the study of social order. This development was grounded in Garfinkel's and Rose's significant involvement in the behavioral sciences of the time. Providing an account based on archival materials from the Garfinkel Archive (Newburyport, Massachusetts, USA), complemented by documents from the Edward Rose Papers collection at the University of Colorado Boulder and from the Harold Garfinkel Papers collection at the University of California Los Angeles, I aim to open up a discussion on the continuing relevance of and possible opportunities that may lie in revisiting some of Rose's central topics and ideas, as they were worked out in his sustained engagements with Garfinkel and other early ethnomethodologists such as Egon Bittner and Harvey Sacks.

1.1 | A SHARED INTEREST IN CULTURE, LANGUAGE, AND SOCIETY

Edward Louis Rose (1909–2002) was an American sociologist who is most often associated with the program of "ethno-inquiries" that he devised and developed over nearly 50 years in his own work and with his students at the University of Colorado Boulder (see Carlin, 1999a, 1999b, 2002, 2006, 2009; Carlin & Watson, 2019; Slack, 2000; Watson, 1992, 1997). Although Rose was a witness to a large part of sociology's history, his route to sociology was not a straight one: he first studied psychology in the 1930s and only later moved on to anthropology and sociology (although his doctoral degree was formally obtained in economics, at Stanford in 1942). Throughout his career, he was consistently interested in culture (e.g., Rose, 1948; Rose & Felton, 1955), which subsequently led him around the year 1950 to become profoundly concerned with language (see Carlin, 2002). Although there might be some interesting affinities with linguistic anthropology and ecological psychology, Rose's interests are largely independent of and predate the emergence of these fields.[4] In a note from June 1955, Rose wrote that he is "studying language in the attempt to understand culture."[5] Similarly, in 1962, during a talk to fellow early

ethnomethodologists, Rose introduced his interest in language by saying that "the best cultural records, the best general cultural records that go beyond, say, one activity, and they're comparable between activities" are available "in the languages, in the intentions and meanings of the language that people happen to use. This is a good record, because it's recorded, available."[6] Rose's preoccupation with language culminated in his etymological study of "natural sociology" (Rose, 1960a) and research into "small languages" (Rose, 1967), continuing later in his abiding interests in "putting society into words" (Rose, 1973), "the wording of the world" (Rose, 1992; Watson, 1992), and the "semantic structure of languages" (Hanson et al., 2001). An equally important element of Rose's *oeuvre* is his ethnographic research, which is most powerfully represented by the studies of Larimer Street (Rose et al., 1965). Here, although slightly less apparent, the link to ethnomethodology was also significant, as Rose explained in a 1965 letter to Ralph H. Turner (supporting Garfinkel's promotion to a full professorship): "I have just completed … a detailed study of Larimer Street, the skid row of Denver, in which I deliberately followed Garfinkel's leads on ways to search for natural interpretations of complex social orders."[7]

Similarly, Garfinkel attributed some ideas and notions to Rose as well. Indeed, we need to keep in mind that Garfinkel would often generously give credit to his students and collaborators, and that his claims may be a sign of affinity rather than of an intellectual debt; nevertheless, Rose's appearances in Garfinkel's texts illustrate the long-lasting relationship of the two scholars. Already in 1959, Garfinkel acknowledges Rose as the originator of the phrase "pretense of agreement" (Garfinkel, 2019[1959]), which captures that "much … of what is being talked about is not mentioned, although each [interlocutor] expects that the adequate sense of the matter that is being talked about is settled" (2019[1959], p. 37). Widely known within ethnomethodology is the notion of "Rose's gloss" (see Carlin, 1999b, p. 61; Carlin, 2006), outlined by Garfinkel and Sacks in their collaborative paper as a deliberate procedure using the property of "circumstantial particulars" (such as noticings) whose "definiteness … *consists* of their consequences," used "to find out definitely what [one] *has* been doing" all along (Garfinkel & Sacks, 1970, p. 366).[8] Furthermore, in an important chapter by Garfinkel and Wieder (1992), Rose is mentioned in connection with the notion of *immortal ordinary society*: "Our use of 'society' was learned from Edward Rose, who uses 'society' in his lectures to collect whatever he needs, at hand, in any witnessable local setting, whose parties are doing some human job. These jobs can range in scale from an exquisitely transient silence that can precede a refused invitation to the spectacle of a developing freeway jam" (Garfinkel & Wieder, 1992, pp. 203–204). In an earlier version of this text, dated May 20, 1990 (Wieder is not yet listed as a co-author), this note on Rose's use of "society" is followed by a footnote in which Garfinkel wrote: "For his encouragement and friendship over the years, and for his generosity in making his work available to me in his lectures, in his writings, in his work in progress, with his students, and in his profoundly original and courageous research, I am indebted to Edward Rose of the Department of Sociology, University of Colorado."[9]

The early collaboration of Garfinkel and Rose in the 1950s and 1960s described in the present article naturally did not happen in a vacuum: both scholars were significantly entrenched in and closely familiar with the current developments in behavioral sciences. Harold Garfinkel was a graduate student at Harvard from 1946 to 1952, supervised by the most influential sociological theorist of the time, Talcott Parsons. Garfinkel and Parsons furthermore worked together after Garfinkel finished his PhD (see Rawls & Turowetz, 2019); for example, Parsons explicitly acknowledged Garfinkel's "very able" assistance in revisiting the pattern variable scheme (Parsons, 1979/1980, p. 12). From 1951 to 1953, while holding a prestigious position at Princeton (Rawls, 2013, p. 304), Garfinkel developed his interest in information science, reflected in the book *Toward a Sociological Theory of Information* (Garfinkel, 2008[1952]), in which he proposed a novel theory of information. In March and June 1952, Garfinkel organized conferences at Princeton, in which "[m]any famous scholars participated, including Parsons, [Alfred] Schütz, and [Kenneth] Burke, along with Gregory Bateson, Oskar Morgenstern, John Van Neumann, Herbert Simon, Carl Deutsch, and Norbert Wiener" (Rawls, 2020, pp. 356–357). Like Garfinkel, Edward Rose was deeply embedded in the behavioral sciences of the time. Rose's earliest published work was based on his laboratory experiments with rats, conducted with the psychologist Edward C. Tolman. In the early 1930s, Rose studied in Frankfurt with one of the founders of Gestalt psychology, Max Wertheimer.[10] In 1960, Rose co-founded the Institute of Behavioral

Science at the University of Colorado Boulder, and was actively involved in organizing the yearly *Conferences on World Affairs* that were attended by prominent scholars in humanities and social sciences.[11] Rose was in contact with Herbert Blumer, Robert K. Merton and Erving Goffman (as was Garfinkel: see Rawls, 2022), along with other leading figures in American sociology (Carlin, 2002).

Despite the published discussions of Rose's work and other textual traces of his relevance to ethnomethodology, little is known about the details and development of his collaboration with Garfinkel. By focusing on the first 10 years of this relationship (1955–1965),[12] this paper aims to start filling this gap. In particular, I will look closely at three main topics: Garfinkel's critical note on Rose and Felton's article from 1955, the work Rose and Garfinkel did together at an Air Force conference in Summer 1958 in New Mexico and finally their collaboration in organizing a series of ethnomethodological conferences in 1962 and 1963 (attended by Garfinkel, Rose, Harvey Sacks, Egon Bittner, Saul Mendlovitz, Craig MacAndrew, David Sudnow, Emanuel Schegloff, Evelyn Hooker, and others). Describing these events is an important contribution not only to the foundations of ethnomethodology, but also to the history of behavioral sciences more broadly.

2 | 1955: ROSE AND FELTON'S PAPER IN AMERICAN SOCIOLOGICAL REVIEW AND GARFINKEL'S CRITICAL NOTE

In the spring of 1955, serving as an assistant editor of the *American Sociological Review* (ASR), Garfinkel reviewed Rose and Felton's paper, which had been submitted for publication in the journal. At this time, Garfinkel was conducting some of his "breaching experiments": in contrast to conventional experiments in social psychology, these exercises were used as tutorials to make visible the taken-for-granted aspects of the social world shared in common by all participants. Rose and Felton's paper reported the results of an experimental study involving the creation of culture and the transfer of inventions within small groups (Rose & Felton, 1955). This seems to have been the first opportunity for Rose and Garfinkel to learn about each other's work and get into contact, albeit indirectly through the ASR editor Leonard Broom. Garfinkel was critical of the manuscript, but in the end supported the publication of a revised version. As Rose recounted more than four decades later, "Garfinkel had blasted Rose for producing in the laboratory mere simulations of culture," however, Rose had "asserted that they had produced the real thing, habits and capabilities of members of distinct societies" (Rose, 1997, p. x). A commentary written by Garfinkel (manuscript available in the Garfinkel Archive) was to be published alongside the paper, but in the end Rose and Felton's paper was published without Garfinkel's note. In this section, I provide a more detailed account of this early exchange, not only because this occasion was the first (albeit mediated) encounter between Rose and Garfinkel, but also for its continuing relevance to important methodological and epistemological issues of experimental work in the social sciences (see, e.g., Hollander & Turowetz, 2017; Kendrick, 2017; Schegloff, 2004; Ting & Fitzgerald, 2020).

The first version submitted by Rose and Felton,[13] and sent by Broom for review to Garfinkel, immediately opens with the claim that "[c]ulture can be produced in the laboratory" (p. 1). They give an overview of experiments that aimed to test "the relation of invention, culture and habit to social isolation and mobility" (p. 1), which are described as "miniature histories where culture could develop in rudimentary societies persisting through a limited series of brief historical epochs" (p. 1). Four experiments were designed to constitute "various combinations of closed and open societies" (p. 1) and related "cultural developments" through mobility: experiments 1 and 2 moved people from closed societies to open societies, experiment 3 included a move from a closed society to another closed society, and experiment 4 moved people from an open society to a closed society (reversing "the ostensible course of history," p. 3). Not unlike the settings of the small-group research that was influential at the time (e.g., Bales, 1950), the experiments were conducted by locating three groups of three persons in separate rooms, presenting them with two Rorschach inkblot cards, for 8 minutes each. First, for 2 minutes, concepts that were drawn for the card were recorded individually, and then discussed for 6 minutes within the group. Each "epoch"

thus lasted for 16 minutes. Results obtained from the experiments were statistically analyzed and led to several findings, summarized generally in this way: "as mobility follows isolation, borrowing displaces invention; as a new order of isolation succeeds an old order of isolation, both borrowing and invention are maintained; and as isolation displaces mobility, both borrowing and invention disappear" (pp. 8–9). According to the authors, the results "give reason to question any bland presumption that social mobility leads inevitably to cultural creativity" (p. 11).

In his initial review of the first version of Rose and Felton's article, Garfinkel states that the paper "breaches an elementary rule of procedure of scientific theorizing" (p. 1).[14] He points out that "the authors use the behavioral events of the experiment in a synecdochical relationship to the sociologist's meaning of culture change," while "the experimental events should be described in terms that are literal with respect to the possible events that the model of the experiment proposes" (p. 1). Garfinkel criticizes the "error and gimmick" that the events of the experiment are treated "as symbolic" of the events entailed in sociological terms such as culture borrowings and invention (p. 1). Ultimately, according to Garfinkel, "it becomes necessary to discount what Rose and Felton found unless one is willing to accept the model of the experiment as an adequate translation of the model of cultural invention, transmission, etc. … But the problems of establishing an experimental sociology consist precisely in not swapping a word for the hard tasks of showing formal correspondence" (pp. 2–3).

After Broom sent Garfinkel's review to the authors, Rose wrote a rebuttal in which he explains that the anonymous reviewer "somehow gained the impression that our experiments produced some behavior in the laboratory that is *like* culture in the field" and maintains that rather than presenting an "analogy," the experiments "actually produced culture in the laboratory."[15] Rose claims that they were "dealing with culture, however crude, imaginative, and insignificant," not "something *like* culture." Terms like isolation, mobility, society or epoch were used to describe "the operations of the experiment," not to draw "a comparison between the happenings of the experiment and happenings in the field" (p. 1). He also attached another paper entitled "The Production of Culture in the Laboratory," which he believed would indicate more clearly what the purpose of their experiments was.

Rose's rebuttal and the attached paper led to Garfinkel's second reading and reconsideration of Rose and Felton's manuscript. On March 16, 1955, Garfinkel sent in a new review, admitting that his "criticism of the synecdochic relationship between the meanings of the terms that describe the experiment and the meanings of these same terms in sociological discourse must be put aside" (p. 1). He recommended the paper for publication, though with significant revisions that were subsequently introduced by the authors mostly in the introductory parts of the paper, where they made clear that it was not their aim to "bring about an image, but rather the actualization of culture out of experimental transactions" (Rose & Felton, 1955, p. 383). Garfinkel also made an agreement with Broom to write a commentary, synthesizing his two reviews, to be published alongside Rose and Felton's paper.

Over the next several months, Garfinkel worked on this commentary together with Paul K. Rowan, also discussing it with Aaron Cicourel on April 29, 1955. A 100-page folder at the Garfinkel Archive includes rough notes, partial drafts and transcripts of discussions among Garfinkel, Rowan and Cicourel related to this endeavor. At the beginning of July, he submitted a nine-page commentary, signed by himself and Rowan, outlining two alternate readings of the paper—the reading in which his first review was grounded, and the reading induced by Rose's rebuttal. The note states that "[r]eading according to different rules resulted in two radically different interpretations of the paper" (p. 2), as presented in the three paragraphs above in the present paper. Garfinkel and Rowan write that respecting the claims made in the rebuttal letter, "the new reading reveals a very neat, specific, and clear experimental field, also, the fact is that the meaning for them and other sociologists of 'the effects of society on culture' is to be found in their model of the experiment and nowhere else" (p. 4). Therefore, what seemed to be "a quarrel over the custody of some key sociological concepts now reappears as the problem of electing one from at least two meanings of the concept of 'minimal operating definition'" (p. 5). In their commentary, Garfinkel and Rowan thus acknowledge and welcome Rose and Felton's proposal that concepts entrenched in sociological theory, such as "culture" or "innovations," can be revisited and illuminated if they are attended to as locally accomplished phenomena that are produced in and as the details of the group's work within the experimental setting.

According to Garfinkel and Rowan, the first meaning of the concept of "minimal operating definition" has to do with abstracting from the "fullness of the data" and depends upon "the order and clarity in the corpus as well as upon the soundness of the abstracting procedure" (p. 5). An unclear corpus of abstracting operations would make it difficult to choose "an acceptable replication but [also] to compare, contract, criticize or otherwise relate their findings to others" (p. 6). The second meaning of the concept of minimal definition is connected to that of essential definition, but such definitions are not available for sociological discourse, and "[u]ntil this task is done the logical status with respect to the rules that govern usage of terms like culture remains that of barely refined common sense" (p. 6). Garfinkel and Rowan elaborate this idea by pointing out that for many "nuclear concepts" of sociological terminology "we are dependent for their meanings upon a background of matters known or assumed to be known in common and more or less taken for granted. The meanings are intended but unstated. Their successful usage is dependent upon an active interpretive contribution of the audience" (p. 7). Garfinkel purports that Rose and Felton have not used a minimal definition as an essential definition, but that they "merely elected a presupposition by asserting a preference for Tylor's definition of culture" (p. 8).

Nevertheless, says Garfinkel, "the difficulty … is not Rose and Felton's alone, but is one in which we all share" (p. 8), possibly extending his critique to the overall state of sociology in the 1950s. It stems from two different senses of minimal definitions which come from two "radically different theories of knowledge" (p. 8). At this point, Garfinkel draws from Felix Kaufmann's distinction between the correspondence theory of logical empiricism and the revised coherence theory of knowledge (Kaufmann, 1944; see also Garfinkel, 2022[1960]). The sense of Rose and Felton's paper depends on the reader's subscription to the former or to the latter. Under the auspices of the correspondence theory "for relating a symbol system to a system of objects" (p. 8), claims of Rose and Felton's paper are "entirely legitimate—the bird may be a strange one, but there can be no doubt that it belongs in this roost" (p. 8). On the other hand, subscribing to Kaufmann's revised coherence theory of knowledge would mean that the first necessary task consists of clarifying commonly held propositions about culture. This has to be accomplished "by addressing those pretheoretic decisions which are the terms in which the proper subject matter of sociology is defined" (p. 8). Garfinkel and Rowan conclude that this "requires not experimentation but the most radically empiricist kind of methodological study in which among other things the actual rules of daily sociological perspective and practice are investigated and clarified" (pp. 8–9).

Broom responded that the journal needs a shorter version of the comment, which Garfinkel also prepared, but in the end—for reasons unknown[16]—the text was not published. Garfinkel listed it in his annual departmental report in 1956 as "being revised for submission to the [ASR] for publication as a letter,"[17] but nothing suggests that it was ever published later. It also remains unclear whether Rose had received or responded to this final note: in the last available letter on this matter by Rose, sent to Broom on March 31, 1955 (i.e., before the note was written), he only remarks that he "shall be looking forward to reading Professor Garfinkel's comments."[18] In any case, the communication described in this section prefaced and initiated a long-lasting collaboration and friendship between Rose and Garfinkel, which started taking more distinct shape over the next few years, as I describe in the next section.

3 | 1957–1958: AIR FORCE CONFERENCES IN NEW MEXICO

Rose and Garfinkel's shared interest in culture, exemplified in the previous section, was further developed and elaborated over the following years. This section focuses on Rose and Garfinkel's extended collaborations linked to two large conferences sponsored by the Air Force in Albuquerque, New Mexico. The conferences in the summers of 1957 and 1958 brought leading sociologists and psychologists to work on the current problems in behavioral sciences and were attended by well-known researchers such as P. F. Lazarsfeld and J. Bruner, among many others. Following up directly on the issues outlined in the previous section, the proceedings from the 1958 conference

contain Rose's contribution on culture (Rose, 1962) and Garfinkel's paper on common-sense and scientific rationalities (Garfinkel, 1960; reprinted in Washburne, 1962; and included as Chapter 8 in Garfinkel, 1967).

The first in-person meeting between Rose and Garfinkel seems to have happened at the First Interdisciplinary Research Conference at the University of New Mexico in 1957. The archival materials give no indication as to whether they discussed their earlier mediated contacts around the ASR paper 2 years before (described in the previous section). Nevertheless, it is documented that on July 31, 1957, Rose provided comments and suggestions on Garfinkel's ongoing research on "20 Questions with Random Answers," namely on possible variations of the experimental design in Garfinkel's study (see Garfinkel, 1967, 2019[1959]). Three weeks after their meeting in New Mexico, Garfinkel wrote a letter to Rose informing him of his upcoming travel from Los Angeles to Washington, assuring Rose that he would spend 3 days (September 2 to September 5) in Boulder on his way back, and noting that such an arrangement would give them "several days to get a program of experiments solidly worked out."[19]

A folder of typewritten documents and Garfinkel's handwritten notes entitled "Rose—Garfinkel: 'Common Understandings'" from the Garfinkel Archive suggests that on September 3, 1957, Garfinkel and Rose were discussing some of the work that had been developed and presented by Rose, William A. Scott, Michael Wertheimer and others at the conference in New Mexico in the summer of 1957. The folder includes Wertheimer's first draft of a paper entitled "Values in Person Cognition"; an undated proposal for a study of "Values and Evaluating Processes" involving Rose, McCord, Scott and Wertheimer; Rose's manuscript of "The Organization of Microcultures" (published as Rose, 1960b) and Rose and Willoughby's manuscript on "Culture Profiles and Emphases" (published as Rose & Willoughby, 1958). The rest of the folder consists of Garfinkel's handwritten notes pertaining to the next upcoming tasks, as well as several pages of notes about possible variations of his experiments on random answers to questions. This meeting also set the basis for the collaboration between Garfinkel and Rose at the Air Force conference the following year, in summer 1958.

Leading up to the second conference, in January 1958, Rose wrote to Garfinkel: "I am extremely anxious to have you join us in New Mexico for at least a short while. I am in the precarious situation of wishing to discuss some of the matters that we worked on at the end of last summer, and although I can make it clear that your work and thinking on the 'documentary method' and other approaches to group understandings and achievements are very important, it seems to me far more useful for you to say these things yourself."[20] Rose also wrote to Ralph D. Norman, one of the conference organizers, about Garfinkel's interest in participating. Norman wrote to Garfinkel on February 4, 1958, and asked for his formal application to attend the conference. In his response, Garfinkel wrote: "Much of the work that Ed Rose and I would hope to do together this summer is an extension of a program we outlined together last fall in Boulder after we met in New Mexico during my visit with Ratoosh, Scodel, and Minas. Since then Ed and I have been working separately, and have scheduled several meetings during the spring to pull together what we have and get set for a concerted push. If you find that an eight week appointment cannot be arranged, perhaps an appointment for a shorter period may be possible."[21] Ultimately, Garfinkel was present for 7 weeks of the 8-week conference (from June 21 to August 9, 1958). Sharing an office with Rose at the conference premises, they "did a lot of talking together … about finding things out about people" (Rose, 1992, p. 23).

The project conducted by Garfinkel, Rose, and Scott was called "The interrelations among cognitive structures, communication and consensus in group interaction" and co-funded by the Society for Investigation of Human Ecology. In a brief two-page proposal written and circulated before the conference, Garfinkel's interests are described as "the dimensions of common sense; interpretative and expressive work and social involvements," while Rose's focus is labeled as "tests of consensus; presumptions of consensus and other behavioral bases of culture."[22] The document captures the interests that Rose and Garfinkel shared and chronicles interesting overlaps in their individual work at the time. It is worth reproducing a longer passage on "the aims" of their work scheduled for the New Mexico conference, while we must keep in mind that the list is itself a product of group consensus and in addition to the interests of Rose and Garfinkel, it also reflects Scott's psychological viewpoint:

> Preliminary discussions have suggested inclusion of the following sub-problems in our focus of concern: (1) The meaning and uses of consensus and presumptions of consensus; (2) The nature of communication processes directed toward consensus; (3) Characteristics of the communication process which facilitate or impede consensus; (4) Characteristics of individuals' cognitive structures which facilitate or impede consensus; (5) Consequences for individuals' cognitive structures of attitude change attendant upon group consensus; (6) Consequences for the communication process of achieving, or failing to achieve consensus.[23]

In terms of empirical research, the projected aim of the collaborative work was to bring together experimental studies with research on "large natural collectivities." Echoing issues that were at the center of Garfinkel's critical note on Rose and Felton's paper in 1955 (see the previous section), the authors of the proposal state that "[i]t should be possible to translate variables and relationships from the domain of experimental groups to that of natural collectivities, and vice versa. To the extent that this can be done, it may permit better coordination between theories of social organization and research findings from experimental microsystems."[24]

Garfinkel brought his Audograph recording machine to the 1958 conference in New Mexico and solicited 200 FL-60 recording discs from the organizers.[25] Nine recordings (from July 1, 6, 8, and 9) are available in the Garfinkel Archive as digital files. The first meeting on July 1 gathered Rose, Garfinkel, Scott, Henry W. Riecken, Richard M. Montague, Melvin L. De Fleur, and Robert A. Ellis. The discussion starts with the differences between Garfinkel and Rose's sociological approach and Scott's psychological approach to the problem of consensus, agreement, and common understandings. Whereas Scott begins with "the fact of agreement between persons," Garfinkel and Rose are interested in "how the having agreement is the outcome of a concerted kind of judgmental, perceptual interpretive work that the persons themselves are engaged in…."[26] Garfinkel brings up the example of games with rules, such as chess, as a situation treated by several persons as more or less equivalent. The main point is that "the object of agreement … is the situation in its sense"[27] and not the perception of the situation by one or the other person. Rose adds that "many consensual factors are predictable before an interaction begins, on the cultural basis, and it's these predictables that establish the consensual practices."[28] These very "presumptions" of shared understandings, explicitly conveyed only in a "negligeable part," are the main issue of interest for Rose and Garfinkel.[29] Rose offers "conversation, or *a* conversation" as a concrete example of "consensual work" and points out that "most of the words used in conversations, even in violent arguments, are taken for granted."[30] Moreover, Rose suggests that "all conversations … are characterized by a tremendous amount of imputational work, whereby the conversationalists do not stop—in fact that would wreck the conversation—to *test* the sense of what is implied by any person or what is imputed by the other person."[31] Garfinkel then speaks about his breaching tutorials, in which students were instructed: "have an ordinary conversation with someone, and in the course of the conversation do *not* take for granted that you know what the other person is talking about, and indicate clearly that you do not know what he's talking about, that he's not entitled to assume that you know what he's talking about, so *ask* him then to clarify his terms,"[32] that is, not taking for granted the common knowledge of what is being talked about (e.g., "changing a tire"; see also Garfinkel, 1967). Toward the end of the meeting, Garfinkel formulates his and Rose's interests as studying "the things that anyone knows,"[33] and mentions "a normative feature to what one is expected to know," that is common knowledge as grounds for taking a "corrective action" and the "administration of sanctions."[34]

Given the proximity of their shared interests, and their differences with the other conference participants, Rose and Garfinkel worked together over the following weeks, and the next recordings include only the two of them. On July 8, they were following up on a draft that they had prepared the day before, starting with Kaufmann's (1944) notion of "corpus" and its possible transfer from the field of scientific knowledge to the domain of everyday interaction.[35] Garfinkel and Rose further discussed the "body of knowledge" or a "set K," referring to "any set of propositions that may be correctly used as the grounds of further inference and action."[36] This set is constituted by a set of rules (R) "that define the conditions of membership in the set" (p. 1). Set K can be divided into subsets K_S

(scientific corpus) and K_{CS} (common sense corpus). Rose and Garfinkel review Kaufmann's proposal that one can decide upon the sense of a proposition not by examining its content but rather by looking at the rules governing its use.[37] Later on, discussing "rules about rules," Garfinkel proposes that

> the only place I can find, that you can possibly go … to discover the rules, is to look actually to the ways in which persons in fact go about making sense of an environment known in the manner of common sense. And the preferred procedure for me has been to examine this using the phenomenological method. … it's a matter of literal description, after first preparing the content by taking any content whatsoever and treating it in its entire content but suspending any reference now to its truth or its falsity or its value or its correctness or anything of the sort.[38]

He then concludes that "the attitude is the attitude of phenomenological reduction, the method is description,"[39] and "the authority is in the appearances themselves."[40] Garfinkel and Rose also discuss how their kind of work relates to behaviorism ("it couldn't be more away from behaviorism," Garfinkel asserts), and the centrality of studying cultural events for sociology. A further set of topics relates to the "convincing character of a story,"[41] and what specifically constitutes credibility on the grounds of common-sense knowledge. Credibility and "believability" are discussed by Rose and Garfinkel as a point of comparison between scientific and common-sense knowledge. Rose suggests that believing contents of a corpus of knowledge can be translated to "acts of believing" (contrasted with "acts of demonstrating") or convincing another person to believe.[42]

Toward the end of the day's recording, Rose summarizes and formulates their program: "let's set out to do two things; let's set out to define … the rules of common-sense knowledge, and then the rules of any general body of knowledge."[43] He suggests that they would "need a few objects to look at" that would be "different enough": science, common sense, mystical topics, and a game or a play.[44] Going back to the issue of credibility and incredibility, Garfinkel suggests looking at "validity" in common-sense terms.[45] Rose brings up the question of different bodies of knowledge providing different "versions," and Garfinkel invokes Alfred Schütz, who takes "common-sense knowledge, and the attitude of common sense, as the point of departure," and in effect "all alternative bodies of knowledge, corpuses, are produced by transformations upon the presuppositions making up the attitude of everyday life."[46] Finally, Rose proposes that there are "really two fields of knowledge: one is spiritual, makes everything possible … [and] in our culture, the scientific-testing knowledge. There's almost a dramatic battle between them…,"[47] and one "can have operating two schemes of credibility."[48]

On the next day, July 9, Garfinkel and Rose review their discussions and common interests to formulate an agenda for the remaining weeks, which they spend working on individual papers for the edited conference volume (Washburne, 1962). They agree that it would be possible to make the rules of praxeological operations clear or explicit as the body of "how-to-do-it knowledge," demonstrating that certain rules were operative in a cultural production, through an experimental program, as opposed to a program of theorizing.[49] Rose explains that he had "been wondering about, really without realizing, (about the) how-to-do-it propositions regarding culture, I think this is about all I've been doing, and I've been really careful about what aspects of culture you try to do. … And I've wondered if in having done all these things you then have a cultural product," recalling his "little experimental studies"[50] already known to Garfinkel (see the previous section). He suggests that he could carefully review this experimental work with regard to turning "ideas in culture" into rules and demonstrations, whereupon he "might succeed … in finding the rules of cultural knowledge."[51] "Having done that," continues Rose, "I would have something to contribute to this … problem of what is common-sense knowledge. Because my opinion at this point now is that … cultural knowledge … is simply one of the products of common-sense operations."[52] In this statement, although the ideas were still being worked out and were far from clearly formulated, we can see how Rose's and Garfinkel's respective perspectives converge, and that Rose had fully realized the significance of Garfinkel's ideas for the development of his own sociological interests.

As I have managed only to sketch out here, the archival documents from 1957 to 1958 show that Garfinkel and Rose were engaged in a close collaboration, mutually reinforcing, advancing, and consulting their ongoing—albeit separate—research projects that were dealing with culture and common sense from a sociological perspective. The conferences in New Mexico were followed by a lively exchange of correspondence and multiple visits by Garfinkel to Boulder, especially to the annual Conferences on World Affairs, co-organized by Rose. In 1961, Rose and Garfinkel started to consider a conference focused specifically on ethnomethodology, where they would invite a limited number of seriously interested people—as Garfinkel wrote in a letter to Rose: "Indeed my inclination is to invite only those who have interests that are focused enough to have stuff in the works that they can and ought to and want to write about. … I see the conference as one devoted to detailed talk about specific issues to which talk is directed by actual accomplished work or work in progress."[53] The next section describes this collaborative effort of Garfinkel and Rose, and its outcomes, in more detail, concluding with a closer look at Rose's studies of "small languages" (see Carlin & Watson, 2019) that he had presented to his fellow early ethnomethodologists.

4 | 1962–1963: ETHNOMETHODOLOGY CONFERENCES AND "SMALL LANGUAGES"

The 1962–1963 series of conferences funded by an Air Force Office of Scientific Research (AFOSR) grant constitute an important and nearly unknown milestone in the development of ethnomethodology, providing an occasion for frequent meetings and research collaboration between Garfinkel's early coworkers and students. Rose would recall these encounters decades later in *The Werald:* "We met variously in Berkeley or at UCLA, or sometimes in Boulder, Colorado. We had a series of those meetings that went on and on over the course of quite a few years" (Rose, 1992, p. 325). He also word-paints the groundbreaking and pioneering character of their conversations: "We were these twisted people who somehow found one another in those early times. We talked past one another. We didn't understand one another as we talked, but we talked nonetheless. Some of the talk got into the record. And some of the talk got to be Ethno-Methodology" (Rose, 1992, p. 330). The conferences took place in February, June, August, and November of 1962 and in February, June, and October of 1963.[54]

The series of meetings was a result of a grant proposal prepared in December 1961 for the AFOSR, entitled "Methods of Validation of Judgements in Decision Making" and scheduled for the period between March 1, 1962, and August 31, 1963.[55] Although the proposal was a collaborative effort by Garfinkel and Rose, it was Rose who was identified as the principal investigator, and the grant was submitted through the University of Colorado in Boulder and its Bureau of Sociological Research, Institute of Behavioral Science. Garfinkel's role is specified as assisting Rose "in the planning and directing of the project, in recruiting still other participants, and in editing a monograph of papers prepared for the project" (p. 8).[56] Other people listed who "have already expressed definite interest in the project and who would be willing to collaborate in this enterprise" are Egon Bittner, Aaron V. Cicourel, Craig MacAndrew, and Harvey Sacks. The text of the project proposal begins in a way that is similar to the discussions held in New Mexico in 1958 (see the previous section): "Unstated assumptions and institutionalized facts, methods, and propositions and other properties and procedures of common sense will be reviewed for their contributions to … practical decisions and actions. Considerations will be given to the ways in which the reasonable and practical thought of 'ordinary persons' is refined and corrected by 'expert persons'", taking into account "laboratory and field studies revealing the interpretive work going into cultural and semantic productions" (p. 2). Common sense is outlined as "a social phenomenon, employing such social instruments as shared understandings and institutionalized facts, arguments, proofs and conclusions" (p. 3), while the main topic of the proposed inquiries is "[t]he involvement of the social science job and its results with common sense proceedings and particularly with the practical decisions of everyday life" (p. 6). Foreshadowing later principles, such as "ethnomethodological indifference" (Garfinkel & Sacks, 1970) and the "non-ironic position towards member's practices" (Laurier et al., 2019), the proposal stresses that the applicants are "not engaged in a debunking or unmasking enterprise" (p. 7), but

rather bringing "some clarification of the data and of the methods and theory of the behavioral sciences and … of their relationship to the practical undertakings of everyday life" (p. 12).

Independent of the actual outcomes of the proposal evaluation, a "planning meeting" was scheduled to take place at UCLA from February 16 to 18, 1962. A couple days before the meeting, Rose wrote to Garfinkel that "[t]he Grant came through."[57] At the beginning of the first meeting, Rose therefore offered some introductory and mostly organizational remarks on the project already funded as AF-AFOSR-62-278, promising a line-up of several meetings over the next 18 months. In addition to Garfinkel and Rose, Craig MacAndrew, Saul Mendlovitz, and Harvey Sacks were present at the first meeting.[58] The point of departure—as in the New Mexico discussions of Garfinkel and Rose several years earlier—is common sense: "the problem is what *is* common sense" and one of the aims is to "assemble common sense,"[59] specifying the "way of talking of the member of society … acting like a sociologist."[60] The meeting was rather informal, with no scheduled paper presentations, although some materials seem to have been circulated in advance: a programmatic statement by Garfinkel and also an initial version of Sacks' paper on sociological description (Sacks, 1963). In this context, the group dedicated a lot of time to discussing features of a "language that can't be beat"—a phrase borrowed from George Tarjan and reused as a proxy for the problem of developing a descriptive ethnomethodological language that is "delivering the procedural account" in each case.[61] Another central topic of discussion in the meeting was the "et cetera clause" (Garfinkel, 1962, 1967), pointing to the essentially vague and unfinished character of everyday practical knowledge, whose importance was discovered by Garfinkel shortly before—as Rose remarks, "I haven't seen you for a year and suddenly you show up with an et cetera principle. So I gotta … see what you have on your mind."[62]

Even though the group shares a general point of view and serious interest in Garfinkel's ethnomethodology, numerous disagreements and misunderstandings occur. For instance, Rose suggests that an ethnomethodological attitude requires setting "aside some time to pause and look at what others are doing around you and especially to look at yourself"[63]—that in principle it is a reflective procedure. One could then propose a "meta-ethnomethodologist who has watched this withdrawal and you naturally have to return to get your audience, or to show— to get the results of this work. Now I describe this as a way—as a sociological description of how you proceed to be both within the culture that you're examining, and apart from it."[64] Harvey Sacks did not agree with this proposition, stating that "what we ought to be doing is taking the categories and setting up the procedures that produce them,"[65] "in order to say 'he's being reflective', 'he's being non-reflective', in our discussion what we are doing is taking the—when somebody is talking about reflective and getting at what it is that they *do* to talk this way, what's involved in it—that's our description."[66] Indeed, the tapes of the first conference aptly illuminate the sense of Rose's retrospective commentary about the way they "talked past one another" and "didn't understand one another as [they] talked, but … talked nonetheless" (Rose, 1992, p. 330).

To provide a detailed account of the ethnomethodological conferences of 1962 and 1963, I would need to discuss them separately. For the purposes of this paper, in the remainder of this section, I will focus chiefly on the meeting in June 1962, which brought together Garfinkel, Rose, Sacks, MacAndrew, and Evelyn Hooker,[67] and a conference in February 1963, at which Rose, Sudnow, Schegloff, Bittner, and Charlie Slack presented their work. They were scheduled in a slightly more formal way, with each participant (except for Garfinkel) presenting a paper with further time allocated for a discussion. On both occasions, Edward Rose presented his own work, especially the research on language in society.

In June 1962, Rose starts with an outline of his academic career and how he arrived at an "overcriticism of theory,"[68] explaining that his "way out is to worry about the development of the subject matter, rather than the development of ideas about the subject matter."[69] He also describes his earlier interest in culture (see above) and the methodological approach of "discovering social data that can be handled at least by the crude sort of methods that are accessible to my thought, not being … any more than a relatively simple methodologist."[70] In a quest to find empirical materials on culture, Rose became interested in languages, being the best and most directly available records of culture. He says that "one of the most accessible records to myself, but to anyone, is simply the historical dictionary, with the record of when certain ideas, labelled ideas now, first came into any language,"[71] describing that

he had "spent now about 15 years or so on the study of such records, not exclusively, but such historical records as historical dictionaries, with the intent to essentially discover, within such dictionaries, within such records, what you might call the cultural formations that they seem to reveal."[72] This work was at the time already available in print, and at the conference Rose only summarized some of his main findings (Rose, 1960a), pointing to his continuing interest in translation among languages, and in using computer programs and simplified languages for these purposes.

The largest part of Rose's talks and of the discussion was dedicated to his research on "small languages," underway at the time, and published only several years later (Rose, 1967; Rose et al., 1964, 1965, 1966). Rose's work is not a contribution to linguistics but to a sociology of knowledge (Rose et al., 1964, p. 2). He is not focusing on a study of endangered languages or existing small-language communities, but on the development, learning, and production of invented languages by two groups of students in a quasi-experimental[73] setting, where "small" means simply "manageable."[74] The studies were primarily devised to examine the "natural logic" of translation work and Rose considered this work to allow for "a direct look at how people are handling labelled ideas … and ideas coordinated to the language,"[75] producing "a record of how persons try to display to others what is on their minds, of how they wonder about and try to deal with what is on the minds of others, and particularly of how they manage to treat certain ideas as shared understandings" (Rose et al., 1964, p. 2). According to Rose, such small languages are "invented" while at the same time "natural," for they "can be comprehended only in terms of the rules of common sense: without stating them all, went by them."[76] Although he also admits that "the notion of inventing a language is ridiculous,"[77] he proposes starting with taking "the stunt of inventing a language, and not doing it yourself, but the further stunt of getting other people to, and let it be a language that is in fact a natural language, and *not* a perfect orderly logical language."[78]

Rose's studies of small languages were informed and inspired by Wittgenstein's "primitive language games," which he developed and discussed in *Philosophical Investigations* (2014, e.g., p. 6e).[79] As Rose explains at the February 1963 conference, his work on "small languages" started with a sentence from Wittgenstein: "What is the meaning of a word?" and he says that he took "that particular question seriously, and used it as a device for getting on with sociological exploration. And I end up, surprisingly, with something that's pretty much like some of the imagined situations that Wittgenstein gets into…" Rose reminds the audience about the passages where Wittgenstein imagines a "tribe" with a primitive language consisting of just a few words. Then he continues: "I systematically forget Wittgenstein and proceed to try to … engage in some experimental production of precisely that."[80] The question for Rose is "what do you get when you get people to come together and hit sticks together. So I don't have to engage in so much imagination, in fact I don't have the skill to go on with that. It's easier to—" At this point, Garfinkel proposes a continuation of Rose's turn at talk: "Have them come together and hit sticks, right?" And Rose agrees: "Have them come together and hit the sticks. See what they make of that." He then summarizes that the groundworks of his research are "not too different from those of Wittgenstein."[81]

The basic idea of Rose's investigation of small languages is that "two small groups of persons composed what may be called small languages that were put to use in translation and in several modes of discourse" (Rose et al., 1964, p. 1). Rose's social setting devised for the production of a small language is connected to the notion of a *small world*, which provides a "convenient working instrument" (Rose et al., 1964, p. 11).[82] Once they have designed the small world together, such as an imaginary fallout shelter or the interior of a suitcase with several objects, the student group splits in two smaller groups. These two groups have to first invent a language to describe the small world and then gain fluency in the language. Rose's previous studies of historical languages and natural sociology served him as the grounds for "laying down certain rules" to be followed by the students participating in the experiments: for example, at least some words should "carry more than one meaning."[83] In sum, as Rose disclosed, "some of what is expected of natural language I used as advice,"[84] thereby turning the issue of language production into a rule-governed, formally describable, and instructable matter. At the conference in June 1963, which was probably held in Boulder, two of Rose's students shared their experiences from participating in the research, including reciting poetry in a language called Fislongzuzakzurk.[85] Then the two groups engaged in a "conversation,"

as Rose called it, mediated by written messages delivered from one room to another in the two invented languages. Their task was to try to translate the other group's language as they sent messages back and forth, attempting to produce a meaningful exchange, and constructing dictionaries for their languages, assigning word equivalences, and describing the grammar as they progressively developed it. The sessions were audiotaped and transcribed in accordance with Rose's efforts to conduct "sociological investigations that retain and celebrate members' accounts" (Carlin & Watson, 2019): most of the three published volumes (Rose et al., 1964, 1965, 1966) consist of verbatim transcriptions of the groups' actual talk at work.

In a presentation at a conference organized with the support of the AFOSR in June 1962, Rose outlined a possible generalization of his small language studies: "… and it's going to some, I would say, larger theory, in terms of human experience, at least it's something to think about, that it's a series of small worlds, or scenes, or situations and so on, with a language appropriate to *them*. There's a matter of interest as to where the language, let's say, translates from one world to the next, that were parts of it will grow out of this scene, and into the next and so on. I'm not at that level of operation yet."[86] Then Rose goes on to state that his "interests tie up pretty much with Harold [Garfinkel]'s, especially his demonstration work that he's been doing for a number of years on—because there are actual problems of worrying what's indeed on the other person's mind, that's what it amounts to as one of these simple problems."[87]

Around the same time, Rose also discussed his preliminary findings from the small language experiments during his one-on-one meetings with Garfinkel in Boulder. In October 1962, they were talking about Garfinkel's paper on Parsons, which brought them to the topic of mock-ups (elaborated by Garfinkel and Sacks in April 1963 at the Conference on World Affairs in Boulder; see also Rawls & Lynch, 2019). Rose says here that in the "small language studies, I claimed they're not mock-ups, I claimed that we are making languages, but more than that, the test of that being not mock-ups is that meaning can be conveyed through them as through any other language, and I claim they have properties of natural languages … and this whole technique of *miniaturization*, that's what it's based on, is not … a mock-up-ization."[88] Garfinkel agrees and responds that in such miniaturization, when one compares it with phenomena in "real settings," there are things that could be added as "more to say," but not contradicted. To build a mock-up, one needs to already know what the original event or object is, to be able to use the mock-up as a "practical guide" or an illustration. Contrary to such miniatures, there are specific contradictions built into the mock-up that are known to be contradictions (i.e., not corresponding to the actual event or object) and can be "put aside" when one is encountering an actual, real-world instance of the thing that is mocked-up.[89] What seems to be reemerging here are some of the central topics that were already present in Garfinkel's critique of Rose and Felton's paper in ASR (see above): topics related to the nature of sociological research, theorizing, and its terminology, with regard to actual cultural events as they are produced in society. It pinpoints some more general features of Rose's intellectual acquaintance with (and differences from) Garfinkel and allows us to proceed to the final section of this paper.

5 | CONCLUDING DISCUSSION

This article gave an account of the early collaboration and intellectual companionship of Harold Garfinkel and Edward Rose in the late 1950s and early 1960s. Presenting results of a study of archival materials from the Garfinkel Archive in Newburyport, supplemented by findings from the Harold Garfinkel Papers collection at the University of California Los Angeles, and from the Edward Rose Papers collection at the University of Colorado Boulder, I have focused chronologically on three occasions of the Garfinkel–Rose collaboration. *First*, the review process for Rose and Felton's paper, submitted to the American Sociological Review in 1955, which Garfinkel reviewed and after Rose's rebuttal recommended for publication. The paper induced Garfinkel to write an extensive commentary, which has remained unpublished, outlining two alternate readings of the manuscript. Despite his initially critical position toward Rose and Felton's claims about producing culture in a laboratory, Garfinkel changed

his opinion on the grounds of the rebuttal and admitted that there is some merit to their study, which is based on a definition of culture as a locally achieved phenomenon. *Second*, the two conferences in New Mexico sponsored by the Air Force in 1957 and 1958, which Rose and Garfinkel both attended. The latter event in particular was an opportunity for them to work together intensely on topics related to common-sense knowledge and scientific knowledge, spelling out rules of the conduct of everyday life and working out novel sociological approaches to their study. *Third*, the series of ethnomethodological conferences in 1962 and 1963, supported by an Air Force grant written collaboratively by Rose and Garfinkel. Rose was officially the principal investigator and Garfinkel the actual leader and major protagonist. Much of these conferences were tape-recorded, and I have focused primarily on Rose's work on "small languages" which seemed to be stimulating for the discussions among the ethnomethodologists. It consisted of studies of invented languages that were designed to have features of natural languages and were developed by Rose's students in semiexperimental settings to explore the issues of meaning-making and common-sense procedures of translation between different languages.

All of these topics of collaboration point to a shared interest in language and culture, common-sense knowledge and common-sense procedures as preconditions for social life. Rose and Garfinkel also appear to have shared a distrust of sociological theory and a preference for empirical work, just looking at how society is actually enacted "in-and-as-the-work-of-the-streets" (Garfinkel, 2022, p. 57)—quite literally so, as became visible in Rose's later ethnographic studies of urban homeless communities, which remain outside of the scope of this paper (e.g., Rose et al., 1997[1965]). There also seems to be a resemblance between Rose's and Sacks' ways of doing sociology, exhibiting a "cognate analytic mentality" (Carlin, 2002, p. 42) despite significant differences and disagreements: both Sacks and Rose share a "primitive science" approach to the study of social phenomena (see Lynch & Bogen, 1994) which is based on sociological naturalism, direct observation, and the absence of a grand theory. Rose's sociology remains down to earth and is based on a radical ethnographic empiricism: "I am basically attracted to what might be called sociological naturalism: I like to watch people to see what they do and what they can do—just as any naturalist may watch other creatures" (Rose et al., 1964, p. 16). Rose attempts to strip sociological work down to "plain" and "even naïve" depictions of "the social spectacle," which includes "human capability" and aims to produce "a sociological record of what takes place … when it is said that language is created" (ibid.).[90] This approach may have been a valuable and productive backdrop for Garfinkel himself to develop, revisit and fine-tune, in conversations with Rose, his own ideas, which originally came from theoretically driven and phenomenologically grounded positions.

Rose's work and his discussions with Garfinkel demonstrate the former's affinity for miniaturization as a research procedure—this can be vividly seen in his "small languages" project, but also in the earlier laboratory studies on the production of culture. Rose's approach to the study of social phenomena consisted of finding ways to empiricize, or even—using more recent vocabulary—"respecifying" topics of sociological theory (such as culture and language) in locally observable settings and small groups. As I have shown in my discussion of Garfinkel's review of Rose and Felton's ASR paper, realizing that terms such as "society," "culture," or "innovation" are not used by Rose in a synecdochic way but literally, was important for Garfinkel in changing his mind about the evaluation. Garfinkel also aligned himself with a specific use of the term "society" that he attributed to Rose, as a term "to collect whatever he needs, at hand, in any witnessable local setting, whose parties are doing some human job" (Garfinkel & Wieder, 1992, p. 204). One can therefore see much continuity in Rose's work in the mid-1950s and early 1960s, throughout the period of his early collaboration with Garfinkel as discussed in this article. This work, as was the case with other experimental studies of the time, was concerned with creating "a real world in the laboratory" (Festinger, 1999, p. 384). The later developments in ethnomethodology since the early 1960s, including Sacks' creation of conversation analysis, ultimately moved away from "laboratory" research procedures and toward detailed studies of practical actions in the real world, aiming to describe its aspects in their full phenomenal constitution. Rather than being a resource, the laboratory gradually became a topic for ethnomethodology; Rose began favoring ethnographic methods over experiments in the mid-1960s, thereby getting closer to the proposed "sociological naturalism." In any case, as I hope to have demonstrated in this paper, Rose's contribution and participation in the earliest years of

ethnomethodology were important, as acknowledged in the following years by Garfinkel and others. Many of Rose's particular topics of interest—such as the links between language, society and the world, common understandings, cultural innovations, history and social change, and language acquisition—remain as relevant as they were 65 years ago. Finally, I hope that this article also provides a further invitation to engage with Rose's inspiring writings, creative studies, and profoundly original take on the nature of sociological research.

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DATA AVAILABILITY STATEMENT

Data sharing is not applicable to this article as no new data were created or analyzed in this study. The article is based on archival materials, available on-site after registration at the institutions listed in the footnotes and/or in the reference section.

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ENDNOTES

[1] University of Colorado Boulder Libraries, Rare and Distinctive Collections, Edward Rose Papers, COU:1389 (henceforth the "Edward Rose Papers"): Box 21 with "Looking Glass + Small Language Research."

[2] Garfinkel Archive, Newburyport, Massachusetts, USA (henceforth the "Garfinkel Archive"): Letter from Edward Rose to Harold Garfinkel, May 15, 1996.

[3] The archival materials do not shed light on the specific circumstances under which the intensity of their meetings and formal collaborations diminished. Approximately in 1998, Rose wrote on the folder of his 1976 hand-written manuscript on *The Rules of an Ethnographical Method*: "Note that this [was] written when Rose and Garfinkel were still talking together" (Edward Rose Papers: Box 22). Although there are signs of transient disagreements in their friendship there is no indication of a serious falling-out. It seems that Rose and Garfinkel gradually parted ways after Rose's retirement in 1977 (Carlin, 1999a; though his scientific and pedagogical work continued while he was an emeritus), and as Andrew Carlin informed me in a personal communication (e-mail, May 18, 2022), it became increasingly difficult for Rose to leave Boulder due to family reasons.

[4] As Andrew Carlin pointed out in a personal communication (e-mail, May 18, 2022), Rose's experimental studies of culture in the 1950s "were a product and blend of his training and experience in psychology and anthropology."

[5] Edward Rose Papers: Box 29 with "Social and Cultural Research Papers."

[6] Garfinkel Archive: "17. #1 of 5, Ethnomethodology Conference 6-8-62, Hours 1, 2, 3, 4 of 13, Ed Rose, Craig McAndrew Side A_7in_Scotch_2.04.22.mp3," Edward Rose, 7:55–8:28.

[7] Edward Rose Papers: Letter from Edward Rose to Ralph H. Turner, December 12, 1965. The acknowledgment in the report itself reads: "Intellectually we are indebted particularly to Harold Garfinkel and Harvey Sacks for their notions on counting and to Robert Potter and David Riesman for their thinking on middle-class sociability. We have had in mind the interests of Erving Goffman in much of our work, particularly his thoughts about encountering" (Rose et al., 1997[1965], p. iii).

[8] Note that there is a similarity between "pretense of agreement" and "Rose's gloss," which is a practice—as formulated by Garfinkel and Sacks—"that makes deliberate use of the property that definiteness of circumstantial particulars *consists* of their consequences" (1970, p. 366). The similarity is more apparent from an earlier formulation of "pretense of agreement," given in a paper "Features of common sense knowledge, the attitude of daily life, and the method for common sense thinking and conduct," presented in July 1958 at the Air Force conference in New Mexico, where Garfinkel worked intensely with Rose (see the section "1957–1958: Air Force conferences in New Mexico"). On page 78, Garfinkel gives this explanation of "pretense of agreement": "… persons operate in using this corpus with the expectancy that *the other person will furnish the intended sense of the utterance*; the expectancy is that the other person will 'understand'" (my emphasis).

[9] Garfinkel Archive: Harold Garfinkel, "Two incommensureable, asymmetrically alternate technologies of social analysis," draft manuscript from May 20, 1990, here p. 75.

[10] As Carlin (2002, p. 44) notes, *gestalt*-related topics were an important resource for Rose in his original approach to sociology. This is particularly interesting with regard to the recently reinvigorated interest in connections between Garfinkel's ethnomethodology and *gestalt* phenomena (e.g., Eisenmann & Lynch, 2021; Garfinkel, 2021; Meyer, 2022; Mondada, 2014; or Hutchinson et al., 2022, and the special issue they introduce).

[11] Both the institute and the conferences still exist today: see https://ibs.colorado.edu/about/ and https://www.colorado.edu/cwa/ (accessed on October 27, 2022).

[12] This was not the end of their collaborations, as Rose and Garfinkel shared interests and worked together after 1965 as well; for example, they both participated in the famous "Purdue Symposium" (Hill & Crittenden, 1968) and the round table discussion on phenomenological sociology (Hinkle, 1977).

[13] Garfinkel Archive: Edward Rose and William Felton, "Experimental Histories of Culture."

[14] Garfinkel Archive: Letter from Harold Garfinkel to Leonard Broom, February 6, 1955.

[15] Garfinkel Archive: Letter from Edward Rose to Leonard Broom, February 15, 1955.

[16] I have approached ASR with an inquiry about an editorial archive that could potentially answer these questions, but I have not been successful in locating any available archives of that kind reaching back to the 1950s. According to the editors, ASR only has insight into journal communications occurring since 2010, when they moved online and went paperless; the editorship has also been moving from institution to institution, and all the relevant paperwork and correspondence is only preserved as part of dispersed individual collections of former ASR editors and staff. I thank one of the anonymous reviewers for suggesting this line of investigation, and I am grateful to Art Alderson for his quick and helpful response on behalf of the ASR editors.

[17] Garfinkel Archive: Letter from Harold Garfinkel to Leonard Broom, undated, annual report of activities for "the year July 1, 1955, to the present."

[18] Edward Rose Papers: Letter from Edward Rose to Leonard Broom, March 31, 1955.

[19] Garfinkel Archive: Letter from Harold Garfinkel to Edward Rose, August 23, 1957.

[20] Garfinkel Archive: Letter from Edward Rose to Harold Garfinkel, January 27, 1958.

[21] Garfinkel Archive: Letter from Harold Garfinkel to Ralph D. Norman, February 10, 1958. The persons listed as Garfinkel's companions in summer 1957 were psychologists working at the time on decision-making in two-person nonzero-sum games (e.g., Scodel et al., 1959). One of the spring meetings mentioned took place on April 20, 1958, in Boulder: in addition to Rose and Garfinkel, the psychologists William Scott and Michael Wertheimer were present, and they prepared an initial prospectus of their plans for the summer.

[22] Garfinkel Archive: Harold Garfinkel, Edward Rose and William Scott: "UNM Study Proposal," p. 2.

[23] Garfinkel Archive: Harold Garfinkel, Edward Rose and William Scott: "UNM Study Proposal," pp. 1–2.

[24] Garfinkel Archive: Harold Garfinkel, Edward Rose, and William Scott: "UNM Study Proposal," p. 2. I thank Hanna Svensson for suggesting that Garfinkel's "trust paper" (Garfinkel, 1963) could be seen as an elaboration of the interest in "consensus" outlined in this proposal. As we know from Rawls (2019), Garfinkel was working on this topic in the late 1950s and early 1960s, and in addition to the chapter published in 1963 also produced two other versions that essentially deliver the same argument (Garfinkel, 2019[1960]; Garfinkel, 2019[1962]).

[25] Garfinkel Archive: Letter from Harold Garfinkel to Ralph D. Norman, April 14, 1958.

[26] Garfinkel Archive: "000730 Side 1.mp3," Harold Garfinkel, 5:13–5:30.

[27] Garfinkel Archive: "000730 Side 1.mp3," Harold Garfinkel, 27:56–28:03.

[28] Garfinkel Archive: "000730 Side 2.mp3," Edward Rose, 1:10–1:23.

[29] Garfinkel Archive: "000730 Side 2.mp3," Harold Garfinkel, Edward Rose, 22:18–22:30.

[30] Garfinkel Archive: "000731 Side 2.mp3," Edward Rose, 4:19–5:18.

[31] Garfinkel Archive: "000731 Side 2.mp3," Edward Rose, 5:39–5:58.

[32] Garfinkel Archive: "000731 Side 2.mp3," Harold Garfinkel, 9:54–10:18.

[33] Garfinkel Archive: "000731 Side 2.mp3," Harold Garfinkel, 21:15.

[34] Garfinkel Archive: "000731 Side 2.mp3," Harold Garfinkel, 23:54–24:11.

[35] Felix Kaufmann (1895–1949) was a phenomenologist studying the methodology of social sciences, a close colleague and friend of Alfred Schütz. Lynch (1993, p. 135) argues that many of Schütz's views on the natural and social sciences were influenced by Kaufmann's philosophy of social science, which also puts forward "conceptions of language and rule-governed action [that] antedated Wittgenstein's devastating criticisms of the logical positivistic conceptions of language, meaning, and action" (*ibid.*). Kaufmann developed the notion of "corpus" of a science as an organized set of knowledge that is produced by acting in accordance with established rules. Garfinkel (and Rose) borrowed this notion from Kaufmann and transposed it from the domain of scientific methodology into the domain of ethno-methodology, that is, the ordinary practices of members of society, regardless of whether they are engaged in doing "lay" or "professional" sociology (see Garfinkel, 1967, p. vii). Socially sanctionable rule-governed actions and related conceptions of "fact" in the established corpus of knowledge then constitute the basis for decision-making in a "common sense situation of choice" (Garfinkel, 2019[1959], p. 10).

[36] Garfinkel Archive: "Notes on Conference 7/7/58 Ed Rose and H. Garfinkel, Albuquerque—Concept of Body of Knowledge," p. 1.

[37] Garfinkel later refined and developed this argumentation in a paper distributed in 1959 at the Fourth World Congress of Sociology in Italy (Garfinkel, 2019[1959], pp. 7–10), which indicates the overall importance of Kaufmann's work in the first two decades of Garfinkel's career (see also Lynch, 1993, pp. 133–141; Hammersley, 2019). This paper was later revised and published as Chapter 3 in *Studies* (Garfinkel, 1967).

[38] Garfinkel Archive: "000737 Side 1.mp3," Harold Garfinkel, 06:04–06:50.

[39] Garfinkel Archive: "000737 Side 1.mp3," Harold Garfinkel, 10:02–10:06.

[40] Garfinkel Archive: "000737 Side 1.mp3," Harold Garfinkel, 10:46–10:48.

[41] Garfinkel Archive: "000736 Side 2.mp3," Harold Garfinkel, 01:58–02:00.

[42] Garfinkel Archive: "000736 Side 2.mp3," Edward Rose, 09:55–11:25.

[43] Garfinkel Archive: "000738 Side 1.mp3," Edward Rose, 18:40–18:54.

[44] Garfinkel Archive: "000738 Side 1.mp3," Edward Rose, Harold Garfinkel, 18:58–19:17.

[45] Garfinkel Archive: "000738 Side 1.mp3," Harold Garfinkel, 20:30–20:35.

[46] Garfinkel Archive: "000738 Side 1.mp3," Harold Garfinkel, 22:16–22:46.

[47] Garfinkel Archive: "000739 Side 1.mp3," Edward Rose, 13:03–13:22.

[48] Garfinkel Archive: "000739 Side 1.mp3," Edward Rose, 13:58–14:02.

[49] Garfinkel Archive: "000733 Side 1.mp3," Edward Rose, Harold Garfinkel, 01:23–03:22.

[50] Garfinkel Archive: "000733 Side 1.mp3," Edward Rose, 03:34–04:04.

[51] Garfinkel Archive: "000733 Side 1.mp3," Edward Rose, 04:05–04:36. The approach that Garfinkel and Rose discuss here does not consist of formal analysis of semiotic components, but of (as Garfinkel would later put it) "praxeologizing" the common-sense knowledge as a set of rules, producing a description that could be "'mis'-read—as instructions" (Garfinkel, 2002, p. 149).

[52] Garfinkel Archive: "000733 Side 1.mp3," Edward Rose, 04:44–5:01.

[53] Edward Rose Papers: Letter from Harold Garfinkel to Edward Rose, October 23, 1961.

[54] Furthermore, in April 1963, Garfinkel, Egon Bittner, and Harvey Sacks participated in the *Conference on World Affairs* in Boulder with talks on "mock-ups" (see Rawls & Lynch, 2019) by Rose's invitation, presenting "a series of rather carefully

prepared papers and discussions," as Rose wrote in a letter to Charles E. Hutchinson, the chief of the Behavioral Sciences Division of the AFOSR (Harold Garfinkel Papers Collection [LSC.1273], University of California Los Angeles: Letter from Edward Rose to Charles E. Hutchinson, July 8, 1963). Part of the transcribed discussions in Boulder were also related to a book planned as a result of the AFOSR grant.

[55] This work was closely related and informed by ongoing research on small-group decision-making at the time. Garfinkel was not only aware of, but also directly involved in, the early research on complex decision-making and information processing in the early 1950s, as discussed at the beginning of this article with regard to his engagements at Princeton.

[56] Garfinkel Archive: "Methods of Validation of Judgements in Decision Making," December 19, 1961.

[57] Edward Rose Papers: Letter from Edward Rose to Harold Garfinkel, February 8, 1962.

[58] Egon Bittner could not attend due to other obligations; and Garfinkel decided not to invite Aaron Cicourel for personal reasons—alluded to also in Cicourel's own account of his collaboration with Garfinkel in the late 1950s and 1960s (Cicourel, 2016, pp. 114–115). Craig MacAndrew was a psychologist interested in alcoholism who worked from a phenomenological basis and briefly collaborated with Garfinkel in the early 1960s (see MacAndrew & Garfinkel, 1962). Saul Mendlovitz had a background in law and worked with Garfinkel in the mid-1950s on the jury study reported in *Studies* (Garfinkel, 1967, pp. 104–115).

[59] Garfinkel Archive: "88. #1 of 10 Ethnomethodology Conference—Harvey Sacks, Ed Rose, Craig McAndrew, Saul Mendelowitz, HG—Hours 1,2, 2-16-62, 2-18-62 Side B_7in_Scotch_1.03.06.mp3," Harvey Sacks, 7:33–7:56.

[60] Garfinkel Archive: "88. #1 of 10 Ethnomethodology Conference—Harvey Sacks, Ed Rose, Craig McAndrew, Saul Mendelowitz, HG—Hours 1,2, 2-16-62, 2-18-62 Side A_7in_Scotch_1.02.56.mp3," Harold Garfinkel, 22:22–22:27.

[61] Garfinkel Archive: "89. #2 of 10, Ethnomethodology Conference, Hours 3, 4 of 18, 2-16 & 2-18 Side A_7in_Scotch_1.03.09.mp3," Harold Garfinkel, 6:32–6:45. The tape recording does not give an indication whether the notion of "language that can't be beat" is directly related to the notion of an observation language and Schütz's critique of it, although Garfinkel was probably familiar with this aspect of Schütz's work (they were in close contact from the 1940s until Schütz's death in 1959; see, e.g., Psathas, 2009). My thanks to one of the anonymous reviewers for suggesting this possibility.

[62] Garfinkel Archive: "92. #4 of 10, Ethnomethodology Conference, Hours 7,8, 2-15-62 & 2-18-62 Side B_7in_-Scotch_1.03.51.mp3," Edward Rose, 6:24–6:31. In a paper published 2 years later and reprinted as Chapter 2 in *Studies*, Garfinkel (1967) notes that "[t]he *et cetera* clause, its properties, and the consequences of its use have been prevailing topics of study and discussion among the members of the Conferences on Ethnomethodology that have been in progress at the University of California, Los Angeles, and the University of Colorado since February, 1962, with the aid of a grant from the US Air Force Office of Scientific Research" (Garfinkel, 1964, p. 248). The topic is also tackled by Sacks (1963).

[63] Garfinkel Archive: "93. #5 of 10, Ethnomethodology Conference, Hours 9,10 of 18, 2-16-62 & 2-18-62 Side B_7in_Scotch_1.03.36.mp3," Edward Rose, 13:26–13:36.

[64] Garfinkel Archive: "93. #5 of 10, Ethnomethodology Conference, Hours 9,10 of 18, 2-16-62 & 2-18-62 Side B_7in_Scotch_1.03.36.mp3," Edward Rose, 13:42–14:12.

[65] Garfinkel Archive: "93. #5 of 10, Ethnomethodology Conference, Hours 9,10 of 18, 2-16-62 & 2-18-62 Side B_7in_Scotch_1.03.36.mp3," Harvey Sacks, 31:35–31:38.

[66] Garfinkel Archive: "93. #5 of 10, Ethnomethodology Conference, Hours 9,10 of 18, 2-16-62 & 2-18-62 Side B_7in_Scotch_1.03.36.mp3," Harvey Sacks, 31:46–32:02.

[67] Evelyn Hooker (1907–1996) was an American psychologist who was one of the first to argue that homosexuality is not a mental disorder (Hooker, 1957). The work was later developed through ethnographic studies of homosexual communities, which she reports on in the 1962 ethnomethodological conference.

[68] Garfinkel Archive: "17. #1 of 5, Ethnomethodology Conference 6-8-62, Hours 1,2,3,4 of 13, Ed Rose, Craig McAndrew Side A_7in_Scotch_2.04.22.mp3," Edward Rose, 2:14–2:16.

[69] Garfinkel Archive: "17. #1 of 5, Ethnomethodology Conference 6-8-62, Hours 1,2,3,4 of 13, Ed Rose, Craig McAndrew Side A_7in_Scotch_2.04.22.mp3," Edward Rose, 3:13–3:24.

[70] Garfinkel Archive: "17. #1 of 5, Ethnomethodology Conference 6-8-62, Hours 1,2,3,4 of 13, Ed Rose, Craig McAndrew Side A_7in_Scotch_2.04.22.mp3," Edward Rose, 07:18–7:38.

[71] Garfinkel Archive: "17. #1 of 5, Ethnomethodology Conference 6-8-62, Hours 1,2,3,4 of 13, Ed Rose, Craig McAndrew Side A_7in_Scotch_2.04.22.mp3," Edward Rose, 8:52–9:07.

[72] Garfinkel Archive: "17. #1 of 5, Ethnomethodology Conference 6-8-62, Hours 1,2,3,4 of 13, Ed Rose, Craig McAndrew Side A_7in_Scotch_2.04.22.mp3," Edward Rose, 9:29–9:57.

[73] Rose himself writes that the productions, in terms of their controlled character, are "like experiments" but "not experiments" (Rose et al., 1964, p. 5).

[74] Garfinkel Archive: "12. #7 of 10 Ethnomethodology Conference 2-23-63 Side A_7in_Scotch_1.05.20.mp3," Edward Rose, 63:39.

[75] Garfinkel Archive: "17. #1 of 5, Ethnomethodology Conference 6-8-62, Hours 1,2,3,4 of 13, Ed Rose, Craig McAndrew Side A_7in_Scotch_2.04.22.mp3," Edward Rose, 48:18–48:25.

[76] Garfinkel Archive: "95. #6 of 10, Ethnomethodology Conference, Hours 11,12, 2-16-62 & 2-18-62 Side B_7in_Scotch_1.03.38.mp3," Edward Rose, 37:56–38:04. This response is offered after Garfinkel makes a distinction between "natural" languages and "invented" or "game-type" languages, and Rose argues that "game-type" is a better notion than "invented." In a review of Rose's book reporting on the results of the small language study (Rose, 1967), Birdwhistell (1968) proposes that he would rather call such languages "codes."

[77] Garfinkel Archive: "12. #7 of 10 Ethnomethodology Conference 2-23-63 Side A_7in_Scotch_1.05.20.mp3," Edward Rose, 59:13–59:16.

[78] Garfinkel Archive: "12. #7 of 10 Ethnomethodology Conference 2-23-63 Side A_7in_Scotch_1.05.20.mp3," Edward Rose, 60:19–60:40.

[79] I thank an anonymous reviewer for bringing up this intriguing question, which prompted me to revisit my research notes from the archives and write this paragraph. Rose and Garfinkel were familiar with Wittgenstein's late philosophy at least since 1960. In March 1960, Garfinkel wrote in a letter to Rose: "… Yesterday I bought all of Wittgenstein's stuff and began to read. … Wittgenstein permits me to get into marvelous details … [his texts are] touching off a flow of fresh and interesting thoughts and I want to keep it going" (Harold Garfinkel Papers Collection [LSC.1273], University of California Los Angeles: Letter from Harold Garfinkel to Edward Rose, March 11, 1960). See also Garfinkel's notes on "language games" from the same year (Garfinkel, 2019[1960]). Harvey Sacks, a close colleague of Garfinkel and Rose in the early 1960s, also valued Wittgenstein's work (see, e.g., Sacks, 1992, p. 26).

[80] Garfinkel Archive: "11. #6 of 10 Ethnomethodology Conference 22363—Sat morning Side A_7in_Scotch_1.03.53.mp3," Edward Rose, 20:40.

[81] Garfinkel Archive: "11. #6 of 10 Ethnomethodology Conference 22363—Sat morning Side A_7in_Scotch_1.03.53.mp3," Edward Rose, 21:30. Rose's empirical study of "small languages" could therefore be seen as a perspicuous case for "respecifying" Wittgenstein's late philosophy, which might be particularly relevant for the ongoing work that is concerned with outlining a "Wittgensteinian ethnomethodology" (e.g., Button, 2008; Hutchinson, 2022; Watson & Coulter, 2008).

[82] It would be possible to say that Rose was tackling fundamentally anthropological problems—accessing a small world and learning a language that provides a fitting description of this world. I thank Peter K. Manning for suggesting this idea.

[83] Garfinkel Archive: "12. #7 of 10 Ethnomethodology Conference 2-23-63 Side A_7in_Scotch_1.05.20.mp3," Edward Rose, 61:51–61:52.

[84] Garfinkel Archive: "12. #7 of 10 Ethnomethodology Conference 2-23-63 Side A_7in_Scotch_1.05.20.mp3," Edward Rose, 63:09–63:14.

[85] The student reciting her poem was Laurel Richardson, who later became a professor of sociology. Interestingly, she reflects on Rose's small language studies in a paper published 35 years later. She writes: "A shoebox, silvered and textured, lay on the seminar table. Each of us looked through a peephole into a 'small werald' Rose had created within the box. I do not remember what was 'in' the world, but I do remember the vividness of the colors, the luminescent blues, especially, and sparkles. … Then, Rose divided us into two groups. 'Imagine each group is a small, closed society inhabiting this small werald', he said. 'You only know about and can only talk to members of your own small society'. … 'Invent a living language to talk about your werald', he instructed us. 'What's a living language?' an ex-divinity student wanted to know. Without a moment's hesitation, smiling, Rose answered, 'A living language is one in which you can write poetry'. By the end of the semester, each group could converse, and I was writing awful poetry in my group's awful Germanic language, 'Fislongzuzakzurk'. At the start of the second semester, Rose brought the two groups to his stellar home on Twelfth Street. … A courier carried messages from one group to the other. Rose told us, 'You do inhabit the same werald, but you have worded it into being differently. Try now to translate from one language to the other; to communicate.'" (Richardson, 1999, p. 84).

[86] Garfinkel Archive: "17. #1 of 5, Ethnomethodology Conference 6-8-62, Hours 1,2,3,4 of 13, Ed Rose, Craig McAndrew Side A_7in_Scotch_2.04.22.mp3," Edward Rose, 47:15–47:44.

[87] Garfinkel Archive: "17. #1 of 5, Ethnomethodology Conference 6-8-62, Hours 1,2,3,4 of 13, Ed Rose, Craig McAndrew Side A_7in_Scotch_2.04.22.mp3," Edward Rose, 48:45-49:09.

[88] Garfinkel Archive: "Garfinkel and Rose Research, October 1967 Side A_7in_Unknown_1.02.57.mp3," Edward Rose, 11:50–12:39. Note that the date on this tape (1967) given in the file name appears to be incorrect; there is another version of the same tape labelled as "October 1962." The one mislabeled as 1967 and quoted here is of better audio quality. Considering the topics of discussion, it seems highly probable that 1962 is the correct year.

[89] Garfinkel Archive: "Garfinkel and Rose Research, October 1967 Side A_7in_Unknown_1.02.57.mp3," Harold Garfinkel, 14:09–15:33.

[90] Such tasks formulated by Rose for sociology seem to be aligned with, and predate, later advice in ethnomethodological studies of work to produce accounts that are not theory-driven and rather "tell-it-like-it-is" (Button & Sharrock, 2009, pp. 65–74).

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