Introduction

The publication of these lectures makes publicly available virtually all of the lectures by Harvey Sacks on conversation and related topics in social science. Most of the lectures in this larger corpus were originally delivered to classes at the University of California – first to sociology classes at the UCLA campus, and then (beginning in Fall 1968) to classes in the School of Social Science at the Irvine campus of the University.

Although Sacks produced copious analytic notes, many of which served as materials for his lectures, what is presented here are the lectures themselves, transcribed from tape recordings. Almost all of Sacks' lectures were initially transcribed by Gail Jefferson, although most of the material for Fall 1964–Spring 1965, in that it antedates either her contact with Sacks and this work, or her undertaking to transcribe the lectures, was intially transcribed by others. With one exception (Sacks, 1987 [1973]), it is also Jefferson who has edited those lectures which have previously been published, as well as the lectures published here.[1] As noted in her introductory notes to the several

My thanks to Paul Drew, John Heritage, Gail Jefferson, Michael Moerman and Melvin Pollner for sensitive responses to a draft of an earlier version of part of this introduction (prepared for the 1989 publication of the 1964–5 lectures), and for suggestions which I have in some cases adopted without further acknowledgement. I am further indebted to John Heritage and Michael Moerman for their generous and helpful comments on a draft of the present introduction/memoir, and to Gail Jefferson for calling to my attention what she took to be lapses in accuracy or taste.

[1] Of the lectures published here, the set for 1964–5 were published in a special issue of the journal *Human Studies*, 12, 3–4 (1989), and of those, the following had been previously published elsewhere, edited by Gail Jefferson:

Fall 1964–5, lecture 5 has been published under the title 'You want to find out if anybody really does care' in Button and Lee (1987: 217–25).

Winter 1964–5, lecture 14 has been published under the title 'The inference making machine: notes on observability' in van Dijk (1985: 13–22).

Other than the 1964–5 lectures, the following lectures have been previously published, also edited by Jefferson:

Spring 1966, lecture 18 (and related material in Fall 1965, lecture 7), under the title 'Hotrodder: a revolutionary category,' in Psathas (1979).

Spring 1966, lecture 13, under the title 'Button-button who's got the button,' in Zimmerman and West (1980: 318–27).

Spring 1966, lecture 24 (with excerpts from Fall 1967, lecture 14; Winter 1970, lecture 2; and Spring 1970, lecture 3), under the title 'On members' measurement systems,' (Sacks, 1988/89).

'lectures' and in Appendix II in her editor's notes to the previous publication of the 1964–5 lectures in the journal *Human Studies*, those lecture-texts have been pieced together from several sets of lectures which Sacks gave during the 1964–5 academic year, to make a more coherent and readable document. These early 'sets' of lectures are full of gaps, and it is not always clear just when some lecture was given. Accordingly, the reader should bear in mind that this presentation of Sacks' early lectures cannot be used to track the development of themes over time, to trace what topics or themes appear to have been related in Sacks' thinking, etc.

Otherwise, it needs to be said at the outset with respect to the present edition that the editorial undertaking has been monumental and its execution heroic. This the reader can only partially see, for what has not been included is, for that reason, not apparent, nor is the work of sorting and collating what is made available in these volumes. This work has, as a matter of course, involved divergences of several kinds from the texts of these lectures which have circulated in various forms of reproduction over the years. These are largely stylistic in nature, and are clearly designed to render the text more accessible, more readable, and more consistent in stance, point of view, diction, etc.[2] On occasion, however, these textual adjustments could be misread as taking a stand on an analytic matter which Sacks otherwise addresses, could be given a 'political' reading, or could appear to have a 'political' upshot, and it would be well for the reader to be alerted to such possibilities.

By 'taking a stand on an analytic matter which Sacks otherwise addresses' I mean to call attention to such adjustments in diction as one in which Sacks follows an excerpt from a group therapy session by referring to one of the speakers as "this fellow Dan" (in the originally circulated transcript of the lecture), a reference which is in the present edition rendered as 'the therapist.' Sacks takes up the issue of the description of persons, and category-ascriptions such as 'therapist/patient,' on several different occasions in these lectures and in several papers. Because of the options available for formulating persons, particular choices of descriptors or identification terms served, in Sacks' view, to pose problems for analysis, and could not properly be invoked or employed in an unexamined way. Accordingly, no particular claims should be under-

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Winter 1970, lectures 1 and 2, under the title 'Some considerations of a story told in ordinary conversations,' (Sacks, 1986).

Spring 1970, lecture 1 (with excerpts from Winter 1970, lecture 2; Spring 1970, lecture 4; and Spring 1971, lecture 1), under the title 'On doing being ordinary,' in Atkinson and Heritage (1984).

Fall 1971, lectures 9–12, under the title 'Some technical considerations of a dirty joke,' in Schenkein (1978).

In addition, extracts from a number of lectures have been assembled by Jefferson as 'Notes on methodology,' in Atkinson and Heritage (1984: 21–7).

[2] Cf. the editor's notes by Gail Jefferson (Jefferson, 1989), and, in the present edition, footnotes at Fall 1964–Spring 1965, lecture 2, p. 18; Spring 1966, lecture 04.a, p. 281; Winter 1969, lecture 7, p. 120; as well as Appendix A to lectures for Fall 1964–Spring 1965, and Appendix B to lectures for Spring 1966.

stood as implied by occasional references to participants by such category terms in the current text (cf. the editor's Appendix A, Spring 1966).

By 'political' I mean, in this context, a relative positioning by Sacks of himself, his undertaking, his colleagues, his students, other contemporary intellectual undertakings, the established contours of the disciplines (sociology, linguistics, anthropology, etc.) and their groupings (e.g., the social sciences), the physically present class to which he was ostensibly addressing himself[3] and the like. Deployment of the pronouns 'we,' 'you,' 'they' and the like can serve to express varying sorts of solidarity and differentiation, and different ways of 'partitioning the population' (as he used to put it).[4] This was a matter to which Sacks was sensitive, having written a paper in graduate school only a few years earlier on Durkheim's use of 'we' in *The Elementary Forms of the Religious Life*, an echo of which appears in lecture 33 of the Spring 1966 lectures. Where the text suggests such alignments, readers should exercise caution.

It must also be recalled that the omission of some lecture sets in the present edition, and the transposition of some lectures from one set to another, requires that caution be used in basing an analysis of the appearance and development of themes, ideas and discussions of data fragments on this edition alone. The full texts of prior versions of all the lectures will be available through the Sacks Archive at the Department of Special Collections of the UCLA Library.

These cautions aside, it should be said that one cannot really retrieve Sacks' 'voice' from the text as presented here. In the interest of readability and of the accessibility of the content, what was sometimes a real challenge to discursive parsing – even to his closest friends and colleagues – has been smoothed out. Gone are the often convoluted phrasing, the syntax that might or might not come together at the end, the often apparently pointillistic movement from observation to observation – sometimes dovetailing at the end into a coherent argument or picture, sometimes not. The very long silences, of course, were lost in the transcribing process.

But Sacks himself treated his habits and manners, his attitudes and convictions, as 'private' (as he puts it in response to a question as to whether he is 'convinced' that single events are studyable after the general introductory lecture, Fall 1967, "That's such a private question"), and not really relevant

[3] Cf. the lecture of April 2 in the Spring 1971 lectures, on Sacks' notion that he was really talking to colleagues, friends and 'students' wherever they might be who were interested in his current work and not necessarily to the class actually in the room.

[4] For example, in lecture 6 of the 1964–5 lectures an alignment may seem to be implied in which Sacks identifies himself with the physically present students in criticism of "all the sociology *we* read," whereas the text of the lecture as previously circulated had read "all the sociology *you* read . . ." (emphases supplied).

See Appendix A to the Fall 1964–Spring 1965 lectures for the editor's account of Sacks' use of personal pronouns such as 'you,' 'I' and 'we' in the lectures, and of her editorial practices for changing some of these references in preparing this edition.

to what he felt merited the attention of others in what he had to say. It is that which these volumes present. As quickly becomes apparent from the texts of the lectures, we have yet to take the full measure of the man.

These series of lectures present a most remarkable, inventive and productive account of a strikingly new vision of how to study human sociality. With but a few exceptions, the students who sat in the rooms in which the lectures were delivered can hardly have known what they were hearing. The lectures were addressed to non-present students, to those who might come to know what to make of them. That audience continues to grow.

Under what circumstances were these lectures delivered and recorded? What is their intellectual and scientific context? What is most notable in them? These matters cannot be dealt with comprehensively here, but a brief treatment, in a mixed genre which might be termed an 'introduction/memoir,' can help provide an overview and some setting for what is increasingly recognized as a startlingly original and important address to the social organization of mind, culture and interaction.

I

Sacks received his AB degree in 1955 after three years at Columbia College. In later years, Sacks would reminisce with partly feigned and partly genuine awe about the faculty at Columbia – Jacques Barzun, Meyer Schapiro, Lionel Trilling, various students and former students of Franz Neumann such as Julian Franklin and Peter Gay (and Neumann himself, who, however, may well have not been teaching undergraduates when Sacks was there), although it was never entirely clear with which of these 'eminences' Sacks had himself studied.[5]

Although he did not officially 'major' in sociology, Sacks' education was influenced in an important way by C. Wright Mills. The influence was not channeled primarily through course work; most important to Sacks was that Mills secured for him a faculty-authorized access to the stacks of Butler Library and turned him loose on his own. But Sacks would later say that from Mills he had learned 'audacity.'

In spite of the predominantly socio-cultural cast of the faculty who figured most centrally in Sacks' later reminiscences, the two closest college friends with whom Sacks kept in touch later on were both biologists.

Upon graduation from Columbia, Sacks was awarded a scholarship at Yale Law School where he earned his LLB in 1959. While at Yale, he participated in the group around Harold Lasswell, and became more interested in understanding how the law as an institution worked, how it *could* work, than

[5] I recall an account of how students would celebrate if they achieved grades of 'A' from Trilling or Schapiro, but it was unclear, at least to me, whether Sacks himself had been one of those students.

in making it work as an attorney himself.[6] He went looking for intellectual resources with which to pursue this interest and turned first to Cambridge, and to the work of Talcott Parsons in particular (although formally he was enrolled as a graduate student in Political Science at MIT, and was employed as a research assistant in the Department of Economics and Social Sciences there). But what he found in Cambridge that was most consequential for the subsequent development of his thinking was not Parsons (and not Chomsky, some of whose lectures at MIT he attended), but rather Harold Garfinkel.

Garfinkel was spending a sabbatical leave from UCLA at Harvard, where he had himself earned his Ph D a number of years earlier. Sacks and Garfinkel met at Parsons' seminar in Cambridge, and were immediately attracted by each other's seriousness. Their intellectual relationship was sustained until the early 1970s. However, in 1959–60, when it became clear to Sacks that the solutions to the problems he had set himself were not to be found in Cambridge, he followed his law school teacher Lasswell's advice, and decided to pursue graduate work in sociology at Berkeley.

Berkeley appealed on several grounds. Laswell had suggested that Sacks pursue his interests through the continuing study of labor law and industrial relations. An attractive locale was furnished by the Institute of Industrial

[6] Sacks once recounted a story which provides some insight into the appeal which Garfinkel's work must have had for him when he later encountered it.

He was engaged in a discussion with several other law school students arguing through some problem in case law which they had been set – a problem in torts, if I remember correctly. The issue was whether or not a person on the ground was entitled to recover damages incurred from the overflight of his property by an airplane. At one point, in a kind of mimicry of the 'how many hairs make a bald man' paradox, the students coped with the argument that no damages could be collected if the plane was being piloted in a proper and accepted manner by seeing how far they could press the definition of what was 'proper.' What if it were flying at 2,000 feet? At 1,000 feet? At 250 feet? At 5 feet? Sacks reported that when the last of these proposals was offered, it was dismissed as 'unreasonable,' as frivolous, as violating the canons of 'common sense.' But, he pointed out, that could as well have been said about the penultimate one, but wasn't. What struck him, then, and puzzled him, was that the 'legal reasoning' which was the much heralded instrument in whose use they (students of the law) were being trained rested on, and was constrained by, an infrastructure of so-called 'common sense' which was entirely tacit and beyond the reach of argument, while controlling it. And, in that legal reasoning was something on which the entire legal structure rested (and not just particular areas, such as torts, contracts, crimes, etc.), how the law as an institution actually worked, what made it work the way it did, what restrained its reasoning from pressing the law in other directions, was shrouded in mystery. Undoubtedly, this was only one of the puzzles about how the law could work which engaged Sacks' interest, but it is one for the solution of which Garfinkel's work on methods of commonsense reasoning and practical theorizing, then in progress, would have been an attractive resource.

The issue prompted by this law school incident gets articulated explicitly for its bearing on working with recorded conversational materials at the beginning of lecture 1 for Fall 1971; cf. volume 2 of the present edition.

Relations at Berkeley, and in particular by Philip Selznick whose interest in organizations and bureaucracy was complemented by a developing interest in legal institutions. (Indeed, several years later Selznick was to establish the Center for the Study of Law and Society at Berkeley, and Sacks was to be among its first graduate fellows.) But Berkeley was attractive on other counts as well. Aside from its having developed one of the strongest sociology departments in the country, Sacks was attracted by the presence of Herbert Blumer, whose SSRC monographic critique (1939) of Thomas and Znaniecki's *Polish Peasant in Europe and America* Sacks had found penetrating. (Sacks lost interest in Blumer soon after arriving in Berkeley, and did not study with him at all.)

It is worth pausing a moment to recall where some of the relevant American social sciences stood during these formative years of the late 1950s and early 1960s, at least as they appeared to graduate students, to *some* graduate students, to *the* graduate students who figure in this account.

There had not yet been the rise to professional visibility of a radical sociology. C. Wright Mills' *Sociological Imagination* was still a daring manifesto, his *Power Elite* still a model inquiry. Theory was predominantly (as it was then called) 'structural-functionalist' and especially Parsonian. 'Empirical' sociology was still predominantly 'Columbia-oriented' rather than 'Chicago-oriented;' data analysis was multivariate, not regression-based. Blau and Duncan's *The American Occupational Structure* was still half a decade to a decade away. And social psychology was in large measure a choice between 'small groups' of the Bales variety or of the Michigan group dynamics variety, a substantial dollop of 'public opinion' or 'attitudes' research, with a minority voice somehow identified – often wrongly – with symbolic interactionism: Blumer at Berkeley being the most visible – or vocal – representative, Goffman (*The Presentation of Self in Everyday Life*, published in the United States in 1959) just beginning to be recognized, Becker still largely unknown.

In anthropology, the Gumperz/Hymes special issue of the *American Anthropologist* was not to appear until 1964, ethnoscience and componential analysis were just coming into their own, the ethnography of communication was just beginning to recruit its hoped-for army to canvas the world.

In linguistics, Chomsky's *Aspects of the Theory of Syntax* was not published until 1964, outsiders were just registering the import and impact of his review of Skinner's *Verbal Behavior* and his *Syntactic Structures* (1957). Linguistics was just beginning to establish a track record for its significance to other disciplines.

Throughout his stay in Berkeley, Sacks remained in touch with Harold Garfinkel (now returned to his home base at UCLA) whose program of ethnomethodological studies was being developed in a series of writings which were privately circulated for the most part in mimeographed form. (It should be recalled that it was not until 1959 that Garfinkel's 'Aspects of the problem of commonsense knowledge of social structure' was published – and not in a broadly accessible outlet at that; not until 1960 for 'The rational properties of scientific and commonsense activities,' also not in a source generally read by

sociologists; not until 1964 that 'Studies of the routine grounds of everyday activities' appeared in *Social Problems*; and not until 1967 that *Studies in Ethnomethodology* was published.) It was largely through Sacks that these manuscripts came to be circulated in Berkeley, largely among graduate students in sociology. Of course, Sacks did not only circulate Garfinkel's manuscripts; in discussions among the students he added the special directions of his own thinking, in some respects converging with Garfinkel's, in other respects quite distinctive.

At the time, Garfinkel was co-principal investigator with Edward Rose of the University of Colorado on a research grant which supported a series of conferences in Los Angeles in which Sacks took part. So Sacks' engagement with Garfinkel's manuscripts in northern California was complemented by more direct, personal engagement in the south. At the same time, other developments were in progress in both north and south; in the north, for example, Selznick had brought into his new Center for the Study of Law and Society a number of graduate students in the social sciences, and especially sociology. During the 1962–3 academic year, this group included Sacks, David Sudnow and the present writer, whose activities separately and together were to contribute to future developments, but are not directly in point here.

In 1963, Garfinkel arranged for Sacks to move to Los Angeles. He was to have an appointment as Acting Assistant Professor of Sociology at UCLA, with the first year off. During that year, 1963–4, Garfinkel and Sacks[7] were to serve as Fellows at the Center for the Scientific Study of Suicide in Los Angeles, under the sponsorship of its director, Edwin Schneidemann. As it happens, my own work prompted a move from Berkeley to Los Angeles during the summer of 1963, and Sacks and I continued both a work and a personal relationship during that year. I can therefore describe, at least in brief compass, his primary intellectual preoccupations during the year. A great many of them had his involvement with the Suicide Prevention Center as a point of departure, thereafter taking the often surprising directions which his distinctive mind imparted to them. In diverse ways, these interests show up in his first ventures in teaching, the 1964–5 lectures which provided the point of departure for the further development of the work, presented in the subsequent lecture series published here.

One line of these concerns focussed on an examination of psychiatric, and especially psychodynamic, theorizing, which furnished one primary theoretical handle on the phenomenon of suicide at the SPC, and which, more particularly, was key to the so-called 'psychological autopsies' which were conducted following suicides and which were of very great interest to both Sacks and Garfinkel. Thinking about psychodynamic theorizing led Sacks (as it had led me; cf. *Berkeley Journal of Sociology*, 1963) to a concern with dialogue, and in particular with Platonic dialogue as a form of discourse designed to control conduct. That, in turn, led him to a more general interest

[7] And Erving Goffman, visiting on an occasional basis from Berkeley.

in Greek philosophy, and particularly in Greek logic (on which he was reading, among other sources, Kneale and Kneale's *The Development of Logic*, 1962).

From the Freudian theorizing, from a prior interest in 'children's cultures' set off by the work of the Opies (1959), and from a persistent attention to the problems posed by the apparent facts and achievements of socialization, there developed an interest in the behavior of children. This interest Sacks pursued largely through examination of source books on children's games (an interest prompted as well by the work of O. K. Moore on games as 'autotelic folk models;' cf. Anderson and Moore, 1960), of the studies and protocols of Barker and Wright (1951, 1954), in observational studies Sacks acquired from Roger Brown, and other sources.

And Sacks pursued a number of other scholarly interests, in biblical studies and interpretation, in translation, in archaeology, etc. In a very different vein, Sacks came across stenographic transcripts, and then the tapes, of the telephone calls to the Suicide Prevention Center of, or about, suicidal persons. All of these themes may be found in the 1964–5 lectures, but it was the last of them which provided the proximate source for the focussed attention on talk itself – perhaps the most critical step toward the development of conversation analysis.

Throughout the 1963–4 academic year, Sacks and I continued the discussions and explorations entered into in Berkeley during the preceding year and a half. This is not the place for a substantial account of those activities (on-site explorations of the possibilities of field observation at the Los Angeles International Airport, in the reference room of the UCLA library, at neighborhood 'Okie' bars in Venice, and elsewhere; long discussions on the UCLA campus where I was a visiting scholar, at the beach in Venice where he lived, or at the apartment at the fringe of Beverly Hills where my wife and I lived). But it may be of interest to describe what seemed to me at the time something quite new, and seems to me now in retrospect the first appearance of what would eventually become, after a number of major transformations, what is now called 'conversation analysis.'

It was during a long talking walk in the late winter of 1964 that Sacks mentioned to me a 'wild' possibility that had occurred to him. He had previously told me about a recurrent and much discussed practical problem faced by those who answered phone calls to the Suicide Prevention Center by suicidal persons or about them – the problem of getting the callers to give their names. Now he told me about one call he had seen/heard which began something like this:

A: This is Mr Smith, may I help you.
B: I can't hear you.
A: This is Mr Smith.
B: Smith.

After which Mr Smith goes on, without getting the caller's name. And later,

when Mr Smith asks for the caller's name, the caller resists giving it. On the one hand, Sacks noted, it appears that if the name is not forthcoming at the start it may prove problematic to get. On the other hand, overt requests for it may be resisted. Then he remarked: Is it possible that the caller's declared problem in hearing is a methodical way of avoiding giving one's name in response to the other's having done so? Could talk be organized at that level of detail? And in so designed a manner?

A month or two later, I arrived home at our apartment in the late afternoon, to find Sacks waiting for me there. A transient difficulty with Garfinkel had led him to realize that, if not on the present occasion then at some future time, he might have to fend for himself in the academic marketplace and had better have some written work to show. So he had drafted the sketches of two papers. I left him talking to my wife in the living room and retreated to my study and read the sketches. One of them was about a methodical way of avoiding giving one's name. As the reader who turns to the 1964–5 lectures will soon discover, this is where Sacks' lectures began (not only in the composite version assembled for this publication, but in the original as well).

Why might this episode, and these observations, be treated as the beginning of what would come to be called 'conversation analysis'?[8] Because

[8] In the 'General Introduction' lecture for Fall 1967, (p. 621), Sacks introduces the work to be presented by describing "When I started to do research in sociology . . ." It is unclear what Sacks means to refer to: when he went to Cambridge? to Berkeley? sometime during graduate school? in Los Angeles? are these the right terms to locate the reference?

In a way, the 1963 paper 'Sociological description' is not incompatible with the account offered in this Fall 1967 lecture, except for the description (p. 622) of starting "to play around with tape recorded conversations," which surely did not happen until the year at the Suicide Prevention Center. Until then, friends of Sacks will remember occasions of sitting 'with him' in some public place and suddenly realizing that Sacks was no longer in the same interaction, but was overhearing a nearby conversation, and often taking out the omnipresent little multi-ring notebook and jotting down a fragment of the talk and some observations about it. The virtues of "replay[ing] them . . . type[ing] them out somewhat, and study[ing] them extendedly" (Fall 1967, ibid.) were realized against a long experience of such overhearing and notetaking. (One shared experience which may have alerted Sacks to the payoffs of taking materials like the SPC tapes seriously was my experience during 1962–3 in Berkeley at the Law and Society Center of tape recording psychiatric competency and criminal insanity examinations for subsequent analysis.)

But it is worth noting that Sacks did not set out to study conversation or language in particular. His concern was with how ordinary activities get done methodically and reproducibly, and the organization of commonsense theorizing and conduct which was relevant to those enterprises. Clearly, he found talk, or what was being done through talk, of interest before coming upon taped materials – else he would not have been jotting overheard bits in notebooks. But the taped material had clear attractions when it became available as a resource, and the talk invited being dealt with as an activity in its own right. But that was something that turned out from experience, not something that had been aimed at, or 'theoretically projected.'

there is the distinctive and utterly critical recognition here that the talk can be examined as an object in its own right, and not merely as a screen on which are projected other processes, whether Balesian system problems or Schutzian interpretive strategies, or Garfinkelian commonsense methods. The talk itself was the action, and previously unsuspected details were critical resources in what was getting done in and by the talk; and all this in naturally occurring events, in no way manipulated to allow the study of them. And it seemed possible to give quite well-defined, quite precise accounts of how what was getting done was getting done – methodical accounts of action.

This was just the start of a long train of quite new things that Sacks was to provide. It was only a little over a year later that the eventually published version of 'An initial investigation . . .' (1972a) was completed. It is hard now to appreciate how startlingly new and unprecedented that paper was at the time. If one recalls the publication history of Garfinkel's work (and that Goffman's *Behavior in Public Places* was published in 1963, and *Relations in Public* was not to be published until 1971), a sense of its uniqueness when it was published in 1972 might be somewhat more accessible. Its utter originality in 1964–5 when it was being written, and the originality of the materials in the first of these lectures which were delivered around the same time, may be better grasped by reference to this other work. With the current wisdom of hindsight, of course, our sense of this early work of Sacks' is readily assimilated to the direction we now know such studies took. But the originality was not only startling in 1964 and 1965; it had the additional headiness – and vertigo – of indeterminateness: How might one proceed? What sort of discipline was this or might it be? Once a previous sense of plausibility about the depth and detail of organization in conduct and apperception of the world were set aside, what constraints on inquiry were defensible? To what level of detail was it sensible to press?

During the summer of 1964, I left Los Angeles for the mid-west, wondering what ever Sacks would do about lecturing to UCLA undergraduates, and wondering as well how our contact could be sustained. The latter problem was solved in part by a variety of resources that allowed me recurrent trips to California during the 1964–5 year (though less so in ensuing years), and in part by a practice which also satisfied my curiosity in the first respect. Sacks would tape record his lectures and send them to me, and (if I remember correctly) to David Sudnow who was spending the year in St. Louis, doing the field work for his dissertation, later to appear as the book *Passing On* (1967). At irregular intervals I would receive in the mail a little orange box with a yellow label, containing a three-inch reel of tape, enough for the 50-minute lectures (more or less) which Sacks was delivering. The lectures were for me, then, a rather special form of monologic telephone call interspersed with our dialogic ones (which were not recorded), and then, after Gail Jefferson started transcribing the lectures, they were a sort of long letter series.[9] It turns out that

[9] At the time they were being delivered, I encountered the lectures term by term, like long analytical letters from Sacks. I had little overall view of them and of their

they became Sacks' most successful and prolific form of scientific communication.

When he wrote papers, Sacks imposed standards of formality and precision that were extremely hard for him to meet to his own satisfaction. Most of the papers he published under his own name alone were work-ups of lectures.[10] Most of the papers he drafted on his own as papers he was never sufficiently satisfied with to publish. The exceptions, 'An initial investigation . . . ' or 'On some puns, with some intimations' give some idea of what Sacks thought a finished piece of work might look like.

Aside, then, from his collaborative publications, the lectures are the vehicle by which most of his work was made available. Perhaps it was the explicitly and necessarily informal and limited character of the occasion that could allow him to get 'the stuff' out the best he could, with no pretense to finally getting it 'just right.' Those who have seen some of his successive versions of the 'same pieces' will know how great a change could overtake some piece of work under the guise of getting it just right.[11] But the quality of what was delivered in those lectures, and in those which followed, and the

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overall development, of long term changes in the work reflected in them, etc. This was largely because such changes would have come up in, or (without necessarily being explicitly discussed) informed, our conversations with each other in the interim between shipments, or could not be recognized for the changes they represented until later developments. Largely, then, my reading was marked by my being struck, charmed, and often amazed at what Sacks' sleight of hand could materialize out of a bit of data, the twist he could impart – no, discover – in it, the tacit understandings he could, by a flash of insight, show we ('casual' readers or onlookers) had furnished it. Sometimes the 'twist' assumed the proportions of a whole analytic topical area – e.g., storytelling structure. I came to the reading of each new 'package' with a kind of avid curiousity about what sorts of new things – whether unexpected observations about a moment or whole new analytic issues – were tucked into those pages, and the reading proceeded from flash to flash. It was like watching one's athletic friend show what he could do.

Preparation of this publication and this introduction has afforded me the occasion for a larger overview, or series of overviews – of each set of lectures and of the set of sets. In them I am brought to recall or to discover in retrospect larger scale movements and changes, emerging and waning themes. Of course, this is refracted through my own experience and intellectual colleagueship with Sacks. I have tried to strike a balance between that kind of perspectival account and a less personalized overview and setting-into-context.

[10] Cf. for example, the paper on story-telling (1974). The paper on puns (1973) is an exception here, having never been fully worked up as a lecture before being prepared for the Georgetown Round Table, in whose proceedings it was published. 'Everyone has to lie' (1975) was adapted from a lecture, but the materials for the lecture were initially drafted as a paper, under the title 'The diagnosis of depression,' which was never published in its original 'paper' format.

[11] See the initial two lectures of the Spring 1966 term presented in this edition with Sacks' first effort at revision, at pp. 236–46 below, for a sample. In this case, a virtually identical version of the same material was eventually published as 'On the analyzability of stories by children' (Sacks, 1972b).

special vision that underlies it, did not require getting it 'just right' to be apparent.[12]

Although he continued to tape a variety of research and teaching activities, Sacks stopped recording his lectures in 1972 for a number of reasons. Some of his lectures at the Linguistic Institute of 1973 at the University of Michigan were recorded, as were some of the sessions of the joint seminar we taught, but these were not recorded by Sacks, and were not reviewed for transcription by him.

Harvey Sacks was killed in an automobile accident in November 1975 while on his way to the campus of the University of California, Irvine where we were to meet to formulate a program which we were discussing establishing at the Santa Barbara campus of the University. One can hardly imagine what the next years of Sacks' intellectual life would have produced, especially in an academic environment fully supportive of the enterprise which had already developed.

II

The 'first installment' of these lectures – the ones delivered during the 1964–5 academic year – can be furnished with two sorts of intellectual reference points – ones in Sacks' own intellectual development and ones in the intellectual context around him.

In his own thinking, these lectures come after his paper 'Sociological description' (1963), written in 1962–3 in Berkeley[13] and during the same period as 'An initial investigation . . .' (1972a) which was finished in June 1965.[14]

Several features of these early papers which serve as landmarks in Sacks' intellectual terrain, and of the early lectures, display some of the most potent influences on his thinking at that time. There is first of all a wide-ranging responsiveness to Garfinkel's thematics, broadly acknowledged in a footnote to 'Sociological description' (1963: 1), and in recurrent notes in the early writings and lectures. A thorough treatment of the influences here, I daresay the reciprocal influences at work here, remains to be written. At a different level, there is the transparent allusion to the later Wittgenstein embodied in

[12] Still, readers should bear in mind the in-progress status which this work had for Sacks. While still alive, he expressed a willingness to have the lectures published, if the publication could be done without much editing, not only because he did not want to spend the time, but also to avoid masking the work-in-progress nature and status of the effort. It should be a way of getting 'a lot of stuff' noticed, without suggesting what should in the end be fashioned from it. The lectures were not meant to look finished.

[13] See the discussion below of the Spring 1966 lectures, and of 'possible description' in particular.

[14] Cf. the initial footnote to the version published in Sudnow (1972b).

the invention (ibid.) of the 'commentator machine' as a grand metaphor for (variously) the relationship of social science discourse to the social world which is its object, of commonsense or lay talk about the world to ordinary enactments of it, etc.

Perhaps less expectable in the contemporary academic setting, in which studies of discourse and conversation are often set in contrast to transformational grammar, is the echo of generativist studies in the form of some of this early work, and especially in the form of its problem development. Take as a case in point 'On the analyzability of stories by children' (1972b, revised from the first two lectures for Spring 1966, but first worked up as lectures 1 and 2 for Fall 1965).

The data for that set of lectures and publication, it will be recalled, were taken not from ordinary conversation, but from the response of a young child to a request by an adult for a story. Most relevantly for the present discussion, this had the consequence that there was no ensuing talk by a co-participant which could be examined to reveal an understanding of the 'story' which was 'indigenous' to the interaction, along the lines exploited in later conversation-analytic work. In its place, Sacks relied on his understanding of the text being examined ("The baby cried. The mommy picked it up"), and the understanding which he attributed to his audience – understandings not overtly provided for by the text itself (for example, that 'the mommy' is the mommy of that baby, although the story as told by the child was expressed as '*the* mommy picked it up').

The problem, as Sacks developed it, was to build 'an apparatus' that would provide for such hearings or understandings, and would serve both as a constraint on them and as a research product to which they could lead. This form of problematics, of course, echoes the commitment to build a syntactic apparatus which would provide for the alternative parsings of a claimedly ambiguous sentence such as 'Flying planes can be dangerous' (Chomsky, 1957). The reader is first asked to recognize that alternative 'structural interpretations' can be assigned to this sentence, and then to be concerned with the construction of a syntax that produces such an ambiguity and provides for its disambiguation. To be sure, this form of problem development and statement is invoked by Sacks on behalf of a quite different intellectual and scientific enterprise, but the formal similarities in the problematics seem clear enough.[15] (And connections appear in other guises as well, for example, in the

[15] See the comments on the Fall 1965 lectures for further discussion of the relationship to generative grammar studies.

In this regard as well, John Heritage has called to my attention an exchange involving Sacks and others at the Purdue Symposium on Ethnomethodology, in which he remarks in response to several inquiries (Hill and Crittendon, 1968: 41–2),

> One of the things that is obvious from the kind of analysis I have given you is that there can be a set of rules which can reproduce the problems in the data with which you started . . . [Query: How do you become satisfied with a solution?] . . . I have a set of rules which give me back my data.

extensive paragraph numbering system which is used to organize 'An initial investigation . . .' as well as the 'Introduction' of 1965 printed in this volume), a format hardly familiar to sociologists at all, but in common practice in linguistics at the time, though Sacks may well have come to it initially through his study of the law).

I think it characteristic of Sacks' relationship to work which he respected that it would enter into the warp and woof of his own thinking and would shape the way he did his work. And this is so not only in this formative stage of his work. Later on (in the work published in Schenkein (1978) but delivered as lectures 9–12 in Fall 1971), for example, his argument that the obscenity in a dirty joke is not its point, but is rather a form of 'circulation control' on knowledge which is packed or tucked in elsewhere, not overtly labelled or featured as the point of the joke, brings to bear a form of analysis developed by scholars of classical Greece such as Milman Parry (1971) and Eric Havelock (1963) in work on the role of the Homeric epics in an oral culture and its transformation in the passage from an oral to a literate culture.

Another case in point is furnished by Cressey's work on embezzlement (1953), which served Sacks (in 'An initial investigation . . . ,' 1972a) not to constitute the problem or suggest the shape of a solution, but as a way-station in the substantive analysis. Cressey had proposed as a key to understanding embezzlement that its perpetrators all had 'a nonsharable problem.' In Sacks' effort to come to terms with the assertion by some avowedly suicidal persons that they had 'no one to turn to,' he proposed as a proximate solution that these persons found that what troubled them would, if recounted to the ones they would properly turn to (e.g., spouse), undermine the very relationship that made them 'turnable-to;' that is, precisely, they had a 'nonsharable problem.' But for Sacks this merely served to pose a problem: how to formulate the terms of the 'search for help' that yielded these persons as the candidates to be turned to, and therefore yielded the result that a problem not sharable with them left the searcher with 'no one to turn to.' And that recasting of the problem led to the central contribution of that analytic undertaking – the formulation of 'membership categorization devices' and their features.[16]

Sometimes Sacks would cite such sources. More often, the shape of the problem formation or solution, or the analytic resource, had simply entered into the currency of his thinking, and its source was lost sight of, especially in the context of lectures to undergraduate courses. The lecture format is, in this regard, 'informal.' Although published work which is, taken as a whole, remote from his concerns is often quoted directly and/or cited by name (e.g., Freud, Gluckman, Von Senden), more intimately related work is often not,

[16] In the paper presenting this work (Sacks, 1972a), the analytic ordering given in the text here is reversed. The paper begins with the most formal and general posing of the issues of categorization, and only eventually arrives at the more proximate, situated problem, as a 'derivation,' i.e., the dilemma presented when what qualifies another as the proper person to turn to will be compromised by the very turning to them.

as for example (to cite an early instance in the text which follows) in the discussion of 'common knowledge' in lecture 3 of the 1964–5 lectures (as printed herein), for which Garfinkel clearly was relevant. In the preparation of these lectures for the present publication, that practice has not been addressed; it is a characteristic feature of the form in which Sacks' work was shaped for presentation.

As unexpected as may be the appearance in Sacks' early lectures of echoes of the analytic style of transformational grammar, even more striking is the apparent lack of specific influences from the work of Erving Goffman. This is especially surprising since, during the years at Berkeley, Sacks took Goffman more seriously than he did virtually any other member of the faculty.

At a very general level, of course, Goffman's analytic enterprise had undertaken to establish the study of face-to-face interaction as a domain of inquiry in its own right, and his work was very likely central in recruiting Sacks' attention to face-to-face interaction as a focus for the concern with practical theorizing and commonsense reasoning which animated the ethnomethodological enterprise. Surely Sacks' work, and work which it inspired, have been important to whatever success and stability this area of inquiry has achieved. And Sacks could treat Goffman's work as setting a relevant domain for students for pedagogical purposes; in the first of the Fall 1967 lectures, Sacks recommends readings in Goffman's work as the most relevant sort of preparatory reading for the course, and the most indicative of the general stance of the course, while explicitly differentiating his own work from it.

Goffman's influence on Sacks was at its peak during Sacks' years as a graduate student. While at Berkeley, for example, Sacks satisfied a requirement in one of Goffman's courses not with an empirical study of interaction of the sort chracteristic of his later work, but by writing the so-called 'police paper' (later published as 'Notes on police assessment of moral character,' 1972c), concerned with methods of commonsense theorizing about appearances and moral character, and based on handbooks and manuals of police procedure. The subsequently published version of the paper begins with a handsome acknowledgement of debt to Goffman's writing and lectures, and though the style and 'address' of the work differ in various respects from those of Goffman, the topic plays off of several themes recurrent in Goffman's work at the time, and the exploitation of handbooks and manuals echoes Goffman's use of manuals of etiquette and advice. But after this, Sacks' work diverges increasingly from Goffman's.

To be sure, in later work Sacks addressed himself to more specific interactional topics mentioned in Goffman's work (see, for example, the discussion of 'rules of irrelevance' in Goffman's essay 'Fun in games,' (1961: 19ff.), or the passing mention of turn-taking (Goffman, 1964: 136), but the lines of influence are often not entirely clear. Goffman is reported to have responded to a question years later asking whether Sacks had been his student by saying, "What do you mean; I was *his* student!" Leaving aside the possible elements of generosity, irony and flipness in such a remark (and assuming

that the report is, generally speaking, correct), a serious treatment of the directions of influence and the interplay of ideas between them remains to be written.[17]

That important divergences between Goffman and Sacks began to develop early on can hardly be doubted. These came to a head, both symbolic and practical, over Sacks' PhD dissertation, an episode which cannot be recounted here.[18] For now the upshot must remain this: although in retrospect Sacks seems clearly to have labored in the same vineyard, and although he was not only formally Goffman's student but learned a great deal from him, the degree to which Goffman influenced more specifically the work for which Sacks is known remains an open question. Certainly, such specific influences are not as much in evidence as most readers are likely to expect, either with respect to Goffman's most characteristic substantive concerns – face, demeanor, structures of attention and information, etc., with respect to governing themes – dramaturgic, ethologic, frame-analytic, etc., or with respect to data and method.

III

In mentioning genres of work and particular people who constituted a relevant intellectual ambience for the early corpus of Sacks' work, one name which might be thought missing is that of John Searle. But it turns out that Searle's work constitutes a parallel stream, not a source. Indeed, although his *Speech Acts* was published in 1969, his paper 'What is a speech act?' appeared in 1965, the same year as the first of Sacks' lectures. It is striking to compare the quite different tacks taken in these two approaches to the accomplishment of social action through the use of language, even if only in the brief and superficial way that space limitations compel.

Searle begins not with a particular utterance – either actually spoken or invented. He addresses himself rather to a class of utterances that would satisfy whatever is required for them to effectively – felicitously – accomplish the speech act of 'promising.' It is the type 'promises' that provides Searle his object of inquiry. The solution takes the form of stating the "conditions . . .

[17] Some considerations on the relationship between Goffman's work and conversation analysis may be found in Schegloff (1988). Goffman's most explicit engagement with conversation–analytic work appeared in *Forms of Talk* (1981), the earliest of whose essays dates to 1974.

[18] The upshot was that Goffman found the argument of 'An initial investigation . . .' circular, and no amount of discussion could move him from this view. Nor would he, for quite a while, step aside from the committee to allow its other members to act favorably on the dissertation, as they wished to do. Eventually, however, he agreed to do so, largely at the urging of Aaron Cicourel who, in the end, signed the dissertation as Chair of its sponsoring committee, making possible the awarding of the PhD in 1966.

necessary and sufficient for the act of promising to have been performed in the utterance of a given sentence" (i.e., a general definition of 'promise'), with a later derivation of the rules for performing acts of this class.

Readers may recall the sort of result yielded by proceeding in this manner – the formulation of preparatory conditions, sincerity conditions, etc., followed by "rules for the function indicating device for promising." The focus, then, is on the class or type of act, and the term describing it – 'promising.' It is not on particular utterances or the contexts in which they occur. Indeed, Searle's paper begins by invoking the most general context possible, "In a typical speech situation involving a speaker, a hearer, and an utterance by the speaker . . ."

Sacks' first lecture starts in a significantly different way (and although the original transcripts show a much more uneven presentation than appears in the edited version, in the manner of their opening they do not differ). Sacks begins by offering particular utterances in a particular context. Our attention is focussed from the outset on particular exchanges, such as *A*: "Hello," *B*: "Hello;" or *A*: "This is Mr Smith, may I help you;" *B*: "Yes this is Mr Brown;" or *A*: "This is Mr Smith, can I help you;" *B*: "I can't hear you," which are

> . . . some first exchanges in telephone conversations collected at an emergency psychiatric hospital. They are occurring between persons who haven't talked to each other before. One of them, *A*, is a staff member of this psychiatric hospital . . .

Sacks goes on to offer a variety of detailed considerations about what these utterances, "This is Mr Smith," "can I help you" or "I can't hear you" might be observed to be doing, and how they might be doing it. Then he remarks (lecture 1, pp. 10–11):

> Clearly enough, things like "This is Mr Smith," "May I help you"? and "I can't hear you" are social objects. And if you begin to look at what they do, you can see that they, and things like them, provide the makings of activities. You assemble activities by using these things. And now when you, or I, or sociologists, watching people do things, engage in trying to find out what they do and how they do it, one fix which can be used is: Of the enormous range of activities that people do, all of them are done with something. Someone says "This is Mr Smith" and the other supplies his own name. Someone says "May I help you" and the other states his business. Someone says "Huh?" or "What did you say?" or "I can't hear you," and then the thing said before gets repeated. What we want then to find out is, can we first of all construct the objects that get used to make up ranges of activities, and then see how it is those objects do get used.
>
> Some of these objects [*recall that 'objects' here refers to the utterances which have been examined*] can be used for whole ranges of activities,

> where for different ones a variety of the properties of those objects will get employed. And we begin to see alternative properties of those objects. That's one way we can go about beginning to collect the alternative methods that persons use in going about doing whatever they have to do. And we can see that these methods will be reproducible descriptions in the sense that any scientific description might be, such that the natural occurrences that we're describing can yield abstract or general phenomena which need not rely on statistical observability for their abstractness or generality.[19]

Nor (one might add) do they rely for their abstractness or generality on being stripped of all contextual particulars (in the manner of Searle's "In the typical speech situation . . .") or on the stipulation of general constitutive definitions of verbs for speaking.

The focus in Sacks' work here, and in much of the work of the ensuing years,[20] is not on general constitutive conditions, or even on rules in Searle's sense, but on practices and methods – on how Members, in particular contexts (or classes of context arrived at by examining particular contexts), methodically construct their talk so as to produce a possible instance of an action or activity of some sort, and to provide for the possible occurrence next of various sorts of actions by others.

Although the 1964–5 lectures exhibit some striking early explorations along these lines, a particularly exemplary instance of such an analysis is Sacks' discussion in lecture 4 of Spring 1966, of the utterance by a previously present participant, after a newcomer to a group therapy session of teenaged boys has been greeted, "We were in an automobile discussion," which Sacks undertakes to show to be "a possible invitation." (In later 'takes' of this analysis, the treatment is varied; for example, in Fall 1968, lecture 6, (volume II) he discusses it as 'orientational,' although all the analysis bearing on its 'invitational' aspect is included. This later discussion is rather fuller, more detailed and compelling.)

His undertaking – '. . . to build a method which will provide for some utterance as a recognizable invitation . . .' – may sound like Searle's, but it turns out to be quite different. There are two component tasks. One of these tasks is

[19] The reference to "reproducible descriptions in the sense that any scientific description might be" is an appearance in this first lecture of a theme and argument which Sacks had been percolating for some time, and which was written up at the end of the 1964–5 academic year in a putative introduction to a publication which never materialized. (That introduction is included in this volume, and its argument is recounted below, at pp. xxx–xxxii.)

[20] When Sacks does introduce a shift to a rather more general form of undertaking, for example at lecture 3 of the Fall 1968 set, it still has quite a different character than Searle's.

> . . . to construct . . . "a partial definition of an invitation." What makes it partial is that while it's a way of doing invitations, it's not . . . all the ways . . . there are other ways and those would be other partial definitions.

The second task is to have this partial definition provide for the actual case which occasions the inquiry:

> We want to do both: Construct a partial definition of 'invitation,' and one that provides for 'this is a case.'

It turns out that there are other things such an analysis should do, which need not preoccupy the present discussion.

The construction of the method that provides for the data under examination as a possible instance of 'invitation' has two parts. First, Sacks characterizes the 'slot' in which this utterance occurs, and characterizes it in various ways – as (1) just after introductions and greetings, (2) in the arrival of a newcomer to a conversation already in progress, (3) in a situation of a psychiatric neophyte coming to group therapy for the first time and joining more experienced patients, etc. Second, he characterizes one particular aspect of the utterance itself – its formulation of the topic preceding the newcomer's arrival as "an automobile discussion." He shows that that formulation makes relevant the common category membership of the newcomer and the others, but a category membership as "teenaged boys" or potential "hotrodders," rather than as "patients." And in formulating the topic as one for which the newcomer might be competent in common with them (rather than as one for which he is not, as is done by a next speaker who extends the utterance by saying ". . . discussing the psychological motives for . . ."), a possible invitation is done.

What this (here highly oversimplified) analysis provides, then, is not necessary and sufficient conditions for the felicitous performance of an invitation, or rules for its performance, but rather a partial method (Sacks refers to it as a "a partial definition") for doing an invitation in a particular interactional/sequential context.

IV

As noted, the earliest lectures, of 1964–5, include a variety of efforts to develop analyses along these lines. Certain themes recur, only some of which can be remarked on here, to highlight something of an abbreviated catalogue of concerns animating Sacks' work at the time.

Consider, for example, the following sort of issue to which Sacks addresses himself recurrently throughout the 1964–5 lectures (this is not an exhaustive listing):

How to get someone's name without asking for it (give yours), lecture 1.

How to avoid giving your name without refusing to give it (initiate repair), lecture 1.
How to avoid giving help without refusing it (treat the circumstance as a joke), lecture 2.
How to get an account without asking for it (offer some member of a class and get a correction), lecture 3.
How to get people to show they care about you, given few opportunities afforded by routine life, e.g., of the divorced (commit/attempt suicide), lecture 5.
How to introduce a piece of information and test its acceptability without saying it, lecture 6.
How to do a 'safe' compliment, i.e., without derogating others, lecture 8.
How to get help for suicidalness without requesting it (ask 'how does this organization work?'), lecture 10.
How to talk in a therapy session without revealing yourself (joke), lecture 12.

Sacks' analytic strategy here is not a search for recipes, or rules, or definitions of types of actions. He begins by taking note of an interactional effect actually achieved in a singular, real episode of interaction (in the listing above, this often includes an achieved absence – something which did not happen). And he asks, was this outcome accomplished methodically. Can we describe it as the product of a method of conduct, a *situated* method of conduct, such that we can find other exercises or enactments of that method or practice, in that situation or context or in others, which will yield the accomplishment, the recognizable accomplishment (recognizable both to co-participants and to professional analysts) of the same outcome – the same recognizable action or activity or effect.

So in the listing I have offered above, the 'solutions' mentioned in parentheses after some of the 'problems' are not 'general;' they are not practices which whenever or wherever enacted will yield those activities as systematic products. They are situated, contexted. How to describe the relevant contexts, the scope within which the proposed practice 'works'? That, of course, is one of the prime sets of problems in this analytic enterprise. How shall we as analysts describe the terms in which participants analyze and understand, from moment to moment, the contexted character of their lives, their current and prospective circumstances, the present moment – how to do this when the very terms of that understanding can be transformed by a next bit of conduct by one of the participants (for example, a next action can recast what has preceded as 'having been leading up to this'). Clearly enough, these questions are of a radically different character than those which are brought to prominence in an undertaking like that of Searle, or Austin (1962) before him.

The recurrent theme documented above will remind some readers of 'indirect speech acts.' In many items on that list the problem appears to be how to achieve some result without doing it 'directly' (as one says in the

vernacular – and it *is* a vernacular term). The proposed 'solutions' might then be cast, in this vernacular and quasi-technical, idiom, as 'indirect' speech acts, although this is, of course, an idiom not employed within the conversation-analytic tradition. (cf. Levinson, 1983: 356–64 for one account).

One line of inquiry (ibid., 274; Brown and Levinson, 1978; Lakoff, 1973) relates the use of indirect speech acts to considerations of politeness. But Sacks' discussion focusses instead on what might be termed 'strategic/sequential' considerations. He notes that the sorts of next turns made relevant by what might be called direct requests are quite different from the ones made relevant by the conduct whose methodic practices he is explicating. When answerers of the telephone at a psychiatric emergency service ask "What is your name?" they may get in return a request for an account – "Why?" – and may end up not getting the name. When they give their own names, they do not get asked "Why?," because they have not done an action which is accountable in that way. The thrust of the analysis is, then, not considerations of politeness, but contingent courses of action as progressively and differentially realized in the set of turns that make up structured sequences based on what would later come to be called 'adjacency pairs.'[21]

The divergence of these two paths of analysis seems quite clearly related to the materials being addressed. On the one hand, we have single classes of utterances, and eventually (Searle, 1976) not even particular ones necessarily, but the categorical type of action which they are supposed to instantiate, singly and across contexts. On the other hand, we have particular utterances occurring in particular series of utterances, in particular organizational, interactional and sequential contexts, with the source of the utterance in prior talk and conduct accessible and demonstrably relevant to professional/academic analysis as it was to the participants *in situ* and *in vivo*, and with the ensuing interactional trajectory which was engendered by the utterance inviting examination in the light of the set of possibilities from which it might have been selected. One of these sets of materials is the natural setting for the work of philosphy and 'academic' inquiry; the other is rather closer to the natural setting for the workings of talk in the everyday world. Sacks' first lectures make clear what course is being set.

The consequentiality of working with *particular* data, for example, with particular utterances, is underscored elsewhere in these and subsequent lectures, when Sacks directs the problematic of describing a 'method for the production of . . .' to whatever action label one would assign to an utterance such as "I'm nothing." Sacks asks (lecture 9, p. 67): how does someone "properly and reproducibly" come to say such a thing, *this* thing? What is someone doing by saying this thing, and how do they come to be doing it?

At the time that Sacks was launching inquiry along these lines, a common reaction was that an utterance of this sort was 'just a manner of speaking.'

[21] Sacks deals with these themes from a different stance subsequently in Winter, 1967, cf. the lecture for March 9 in particular, and the discussion of the varying tacks he takes below at pp. l–li of this introduction.

That the *particular* way of speaking, the phrasing, was almost accidental (a stance suitable to the view that an utterance is an enactment of a sentence which expresses a proposition, where it is the underlying proposition – perhaps accompanied by its 'function indicating device' – which finally matters, not the particulars which happen to give it expression on any given occasion). But Sacks saw it as the outcome of a procedure, as announcing 'a finding' by its speaker. He asked what that procedure was, and how it could arrive at such a finding, in a fashion that other participants would find understandable, and even 'correct.' He took seriously the particular form in which conduct appeared – the participants had said *this thing*, *in this way*, and not in some other way. He insisted on the possibility that that mattered – that every particular might matter. None could be dismissed *a priori* as *merely* (a word he particularly treated with suspicion) a way of talking.

Of course, the fullest version of this sort of analytic undertaking was Sacks' paper 'An initial investigation . . .' (1972a), where the utterance/action in question was 'I have no one to turn to.'' This utterance was also seen as reporting the result of a search, the description of which required developing the terms in which such a search might be understood to have been conducted, namely, 'membership categorization devices.' Early versions of parts of that paper (as well as other papers) can be found in the 1964–5 lectures, for example in lecture 6.

This way of working, then, mixed a kind of naturalism (in its insistence on noticing and crediting the potential seriousness of particulars of the natural occurrences of conduct) with the ethnomethodological concern for the Members' methods for the production of a mundane world and commonsense understandings of it. Sacks asked how the recognizably detailed ordinary world of activities gets produced, and produced recognizably. It was just this way of proceeding – describing procedurally the production of courses of action – that Sacks understood at the time to be the foundation of the sciences as 'science,' and therefore the grounds for optimism about the principled possibility of a natural observational discipline in sociology. A brief account of this view (argued in the 'Introduction' by Sacks, Appendix I in this volume) is in order.

V

Sacks had developed an argument[22] addressed to the question of whether

[22] The argument was written up, probably in the summer or autumn of 1965, after Sacks' first academic year of lectures, as a possible introduction to a contemplated volume entitled *The Search for Help*. This publication, which was never pursued, would have included two papers – 'The search for help: no one to turn to' (later published in Sudnow, 1972b), and 'The search for help: the diagnosis of depression,' never published. That the argument informed his thinking earlier, and entered into the first lectures, can be seen in the excerpt from lecture 1 cited at pp. xxv–xxvi above, and remarked on in n. 19.

sociology could be shown to be a possibly 'stable' natural observational discipline. By this question Sacks meant to address the possibility that social science provided merely stopgap accounts of human action, conduct, behavior, organization, etc., until such disciplines as biology and neurophysiology matured to the point at which they could deal with such problems. (This was a position that Sacks was trying out when I first met him in 1961–2, and could be seen as a kind of riposte to Chomsky's critique of Skinner. I always suspected that Sacks entertained the position as a provocation, in a law school pedagogical way, rather than as seriously tenable, but used it to force a consideration of the arguments necessary to set it aside. The position certainly shook me up when Sacks first confronted me with it in the winter and spring of 1962, for, in common with most sociology graduate students, I had treated such claims as long since undermined by Durkheim and other ancestors.) If sociology, or social science, were such a stopgap and thus 'unstable,' it hardly seemed worth investing much time and commitment in it. So before setting off on a serious research undertaking, it seemed in point to establish that a stable discipline was possible. Sacks believed that the argument he developed had a further pay-off; it showed something of the features the research enterprise and its results should have if it were to be, or contribute to, a stable science. The argument, briefly stated, was this.

Contributions to science, including to sciences such as biology and neurophysiology, are composed of two essential parts. One is the account of the findings. The other is the account of the scientists' actions by which the findings were obtained. What discriminates science from other epistemic undertakings is the claim that its findings are reproducible, and that reproducibility is itself grounded in the claim that the results were arrived at by courses of action reproducible by anyone in principle. Other investigators can, by engaging in the same actions, arrive at the same findings.

Sacks argued that both of these parts of contributions to science are 'science', and not just the findings. For it is the reproduction of the actions reproducing the results which make the findings 'scientific', and the descriptions of those courses of action which make their reproducibility possible. If the results are scientific, the descriptions of the actions for producing them must also be science.

But, he noted, the descriptions of courses of action in scientific papers are not couched in neurophysiological terms, but take the form of accounts of methods or procedures. This form of account of action is reproducible, both in action and in description.

So, Sacks concluded, from the fact of the existence of natural science there is evidence that it is possible to have (1) accounts of human courses of action, (2) which are not neurophysiological, biological, etc., (3) which are reproducible and hence scientifically adequate, (4) the latter two features amounting to the finding that they may be stable, and (5) a way (perhaps *the* way) to have such stable accounts of human behavior is by producing accounts of the methods and procedures for producing it. The grounding for the possibility of a stable social-scientific account of human behavior of a

non-reductionist sort was at least as deep as the grounding of the natural sciences. Perhaps that is deep enough.

This conclusion converges, of course, with the thrust of ethnomethodology as Garfinkel had been developing it, and was undoubtedly motivated, at least in part, by Sacks' engagement with Garfinkel (and informed, perhaps, by Felix Kaufmann (1944) as well). Still, the argument is novel and provides a grounding from a different direction than Garfinkel had provided. For the tenor at least of Garfinkel's arguments was anti-positivist and 'anti-scientific' in impulse, whereas Sacks sought to ground the undertaking in which he was engaging in the very fact of the existence of science. (And, indeed, in the earlier 'Sociological description' (Sacks, 1963) he had written, "I take it that at least some sociologists seek to make a science of the discipline; this is a concern I share, and it is only from the perspective of such a concern that the ensuing discussion seems appropriate.")

VI

I have remarked on two types of problems taken up in the 1964–5 lectures – the reproducible methods by which 'findings' such as "I'm nothing" or "I have no one to turn to" may be arrived at (note in this regard the special claim on Sacks' attention exerted by commonsense uses of 'quantifiers,' starting with the ones mentioned above, but extending to utterances such as 'Everyone has to lie', (Sacks, 1975)), and how to achieve some outcome without aiming for it 'directly'. Several other recurrent themes in these earliest lectures might be mentioned here.

One is an attention to certain 'generic forms' of statement or question, into which particular values can be plugged in particular circumstances. Sacks isolates, for example, the question form 'Why do you want to do X?' (lecture 5, p. 33), or the generic form of statement 'Because A did X, B did Y' (lecture 5, p. 36). Later he focusses on the form, 'X told me to call/do Y' (lecture 10, pp. 76–7). It was very likely the exposure almost exclusively to calls to the Suicide Prevention Center, and the sort of recurrencies which they provided, which led to a focus on regularities so literally formulated. But it was in this sort of problem that the concern with the formats of utterances, often rather more abstractly and formally described, initially appeared.

There is throughout these lectures the repeated use of 'the socialization problem' as a resource for focussing analysis. The question gets posed, 'How does a child learn that X?,' for example, that activities are observable; what properties of competence does socialization have to produce, and how are they produced; how does this learning take place (e.g., lecture 14, pp. 120–1). This form of problem or observation finds expression in Sacks' writing of this period as well as in the lectures (for example, in the remarks in 'An initial investigation. . . .' concerning what is involved in learning how adequate reference is to be done), although it recedes in prominence in the later years of the lectures.

These early lectures of 1964–5 touch on, or give a first formulation of, a variety of themes more fully developed in later work, either of Sacks' or by others.

For example, although many believe that the early lectures were taken up with membership categorization, and that sequential organization is only addressed in later years, we have already seen that the early lectures – including the very first – engage that issue from the very beginning. To cite but one other instance of this early engagement, lecture 9 includes observations on sequence organization (the asker of a question gets the right to do more talk), on what were later (Sacks et al., 1974) called contrasting speech exchange systems (remarks on press conferences and cross-examination), on how the turn-taking systems of different speech exchange systems can affect the forms of utterances (e.g., long questions when there is no right of follow up), and the like.

Or note how the earlier-mentioned recurrent theme concerned with 'how to do X without doing Y' finds later resonance not only in Sacks' work but in work such as that by Pomerantz (1980) on 'telling my side as a fishing device' (how to elicit information without asking for it), by Jefferson (1983) on 'embedded correction' (how to induce adoption of a correct form without correcting the wrong one), and others.

Or consider the material in lecture 11 concerned with glancing, looking, and seeing. The parts of this discussion which concerned the categories in terms of which one sees, anticipate the later discussion of 'viewer's maxims' in the lectures on "The baby cried" (lectures 1 and 2 for Spring, 1966, eventually published as 'On the analyzability of stories by children,' 1972b). They display as well Sacks' reflections on what such glance exchanges reveal about 'norms' in the more conventional sociological and anthropological sense, about 'social integration,' 'alienation,' and the like. And perhaps there is here as well a point of departure for Sudnow's later (1972a) work on glances, for example in Sacks' observation (p. 86) that "We start out with the fact that glances are actions."

It is worth noting that in some cases, discussions in these early lectures include points that are not found in later elaborations. Some of these seem to me to have been simply wrong – for example, the claim (lecture 5, p. 33) that 'opinion' is something you don't need a defense for. Others encountered problematic evidence within the conversation – analytic tradition of work. For example, Sacks had proposed that a method for doing greetings consisted in the use of one of the class of greeting terms in 'first position.' Schegloff (1967) disputes the generality of the claim by examining telephone conversation in which "Hello" in first turn is ordinarily *not* a greeting, and shows that claims in this domain of work can be addressed with data, investigated empirically and found to be the case or not.

Still other portions of these early lectures, however, appear to be strong points which simply dropped out of later reworkings of the topic. For example, lecture 6 is a version of (or draws on) 'An initial investigation . . . ,' 'On the analyzability . . .' 'Everyone has to lie,' and a paper which Sacks

never published, entitled 'A device basic to social interaction,' concerned with the character of the categories which compose membership categorization devices as organizing devices for commonsense knowledge about members. But Sacks makes a point in this lecture which I do not believe ever appears in any of the other accounts of these domains, concerning the relativity of category collections such as age and class to the categorizer; he notes that the recipient of some utterance which includes some such categories (such as 'young man') has to categorize the categorizer to know how they would categorize the one who had been categorized in the utterance.

These lectures, then, have more than merely historical interest as embryonic versions of later developed work. Some of the themes here, however insightful and innovative, happen not to have been further developed. And others, which were further developed, left behind some points which are still valuable and can be found here.

VII

As with the 1964–5 lectures, those for the Fall 1965 term include first tries at topics (both accounts of specific data episodes and analytical topics raised from them) taken up and elaborated in subsequent terms, as well as discussions which do not get such subsequent development. Among the latter are, for example, 'hotrodding as a test' (lecture 10) or 'non-translatable categories' (lecture 12). Among the former are "The baby cried . . ." and membership categorization devices (lectures 1 and 2) more fully elaborated as lectures 1 and 2 in the following term, Spring 1966; collaborative utterances addressed via "We were in an automobile discussion" in Spring 1966 (lectures 4 and 5); or 'tying rules' taken up in a number of subsequent lecture sets.

Still, there is good reason to read carefully the discussions of Fall 1965, even for topics which are given fuller, and apparently more satisfactory, treatment later. To cite but a single example, in the outline for the initial lecture on "The baby cried . . ." (here appearing as Appendix A for Fall 1965), at 1.a.2 and a.3, Sacks offers observations which do not appear in subsequent treatments of this material (either in Spring 1966, or in the subsequent publication as Sacks, 1972b) but which differentiate Sacks' point here from other, parallel claims – often characterized as being concerned with the order of narrativity. Others (often more or less contemporaneously, e.g., Labov and Waletsky, 1966) have remarked that in narratives the 'default' organization is that order of sentences is isomorphic with the order of the occurrences which they report. And in later versions of this analysis Sacks seems to be making the same argument. As it is put in the published version (1972b: 330), "I take it we hear that as S[entence] 2 follows S[entence] 1, so O[ccurrence] 2 follows O[ccurrence] 1." But here, in the Fall 1965 outline, he notes that "this cannot be accounted for simply by the fact that S1 precedes S2," for "we can find elsewhere two sentences linked as these are, with

nothing between, where we would not hear such an action sequence." And he offers an instance from the same collection of children's stories, ". . . . The piggie got hit by the choo-choo. He got a little hurt. He broke his neck. He broke his chin."

The point is that what is at work here is more than a matter of narrative technique or of discourse organization, although these may well be involved. Rather 'commonsense knowledge' of the world, of the culture, and of normative courses of action enter centrally into discriminating those actions or events whose description in successive sentences is to be understood as temporal succession from those which are not. It is not, then, a merely formal or discursive skill, but can turn on the particulars of what is being reported. This theme drops out of later discussions of these materials.[23]

If this point seems to resonate basic themes of so-called contextualist, or social constructionist or ethnomethodological stances, there are other elements in these early lectures which operate on a different wavelength. For example, early in the development of what he called 'tying rules' (in which he is addressing matters later often discussed under the rubric of 'cohesion,' cf., for example, Halliday and Hasan, 1976) he proposes (Fall 1965, lecture 5, p. 159) to be

> taking small parts of a thing and building out from them, because small parts can be identified and worked on without regard to the larger thing they're part of. And they can work in a variety of larger parts than the one they happen to be working in. I don't do that just as a matter of simplicity . . . the image I have is of this machinery, where you would have some standardized gadget that you can stick in here and there and that can work in a variety of different machines . . . So these smaller components are first to be identified because they are components perhaps for lots of other tasks than the ones they're used in.

Thus, there is room within a larger, contextually sensitive, address to his materials (cf. the earlier-discussed contrast of Sacks' starting point with that of Searle) for the recognition and more formal description of particular practices and sets of practices – here metaphorized as 'gadgets' or 'machinery' – which members can use in constituting coherent talk and specific lines of action and interaction, and for an appreciation that some of these may operate in a way substantially unqualified by the particulars of local context.

Recall again (cf. the discussion above at pp. xx-xxii) the echoes in Sacks' work in this period of some of the themes of work in generative grammar (more accurately, an analytic model whose most lively embodiment at the time was generative grammar, but which is surely not limited to that domain of work).[24] The lectures for Fall 1965 were for a course whose catalogue title

[23] It does not drop out as a theme of the lectures, however; cf. the discussion at pp. xxxvii–xxxviii below, and n. 26.

[24] It is worth making explicit here that Sacks kept himself informed of

was 'Culture and personality.' Whether or not he would otherwise have been inclined to do so, it was perhaps this title which prompted some discussion by Sacks of the notion 'culture.' In setting out the orientation of his examination of the story told by a child, "The baby cried, the mommy picked it up," Sacks subsumed it, and the 'machinery' by which it was produced and heard, under the notion 'culture,' of which he remarked, "A culture is an apparatus for generating *recognizable* actions; if the same procedures are used for generating as for detecting, that is perhaps as simple a solution to the problem of recognizability as is formulatable' (Fall 1965, Appendix A, p. 226, emphasis in original.)[25] His description of what 'the apparatus' should do is strikingly reminiscent of lines from early Chomsky, and seems directly targetted at transformational grammar, but here, surprisingly, not at its principles – but at its product: 'We are going to aim at building an apparatus which involves building constraints on what an adequate grammar will do, such that what an adequate grammar will do, some of the things it will do, we are going to rule out, and provide for the non-occurrence of' (Fall 1965, Appendix A, p. 229). Sacks' undertaking here seems in important ways to be shaped by the transformational grammar enterprise, albeit in a corrective stance toward it. The stance seems to be something like the following. Given an undertaking like the one generative grammar studies had seemed to set in motion, and operating with similar sorts of goals (e.g., to generate all and only the grammatical/acceptable sentences of a language), getting right results requires looking at something other than just the linguistic or, even more narrowly, the grammatical aspects of sentences or utterances. Not language, but culture, is the key object and resource. And while such an enterprise was understood by some 'as ethnomethodology,' by others it was seen as an anthropological/cultural version of cognitive science (albeit along different

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contemporary developments in a wide range of potentially relevant disciplines, and was aware of what seemed to be 'hot' topics and ways of working. His work recurrently speaks to such developments, sometimes explicitly, sometimes tacitly. He is aware of, and responsive to, his intellectual ambience. The present account often underscores such points of convergence and contrast – both with respect to the ambience at the time Sacks' work was being done and with respect to developments at the time the present publication is being prepared. What may be of enduring interest is the larger picture of the intellectual stances and developments at issue, rather than the more transient excitements that pass over areas in ferment, even if these substantially engage a generation of workers in a field.

[25] On some readings, it is telling to compare this stance with Garfinkel's account of ethnomethodology (1967: 1), about whose studies he writes,

> Their central recommendation is that the activities whereby members produce and manage settings of organized everyday affairs are identical with members' procedures for making those settings 'account-able' . . . When I speak of accountable my interests are directed to such matters as the following. I mean observable-and-reportable, i.e., available to members as situated practices of looking-and-telling.

lines than those previously suggested by studies in ethnoscience and componential analysis).

There are various problem-types addressed and observations developed in these lectures which seem to have a (sociological?) bearing on what came to be called 'cognitive science.' Here I can mention only one of each.

First, observations. Both in Fall 1965 (lecture 7) and in Spring 1966 (lecture 18) Sacks comments on the differential 'owning' or control of certain categories by different social groups, and the not uncommon asymmetry between those to whom a category is applied and those who apply it. One particular focus for this line of analysis is the pair of terms 'adolescent' and 'hotrodder' as applied to teenaged boys. 'Adolescent' is 'owned' by the conventional adult society, and is deployed by its members (together with all the commonsense knowledge or 'conventional wisdom' for which it is the organizational locus in the culture) more or less without regard to the views of those whom it is used to characterize. 'Hotrodder' (or, more recently, 'punker,' etc.), on the other hand, are categories deployed by their incumbents, and in ways often inaccessible to those who are not themselves members. It is this relative independence from the 'official' or conventional culture that led Sacks to term such categories 'revolutionary' (Spring 1966 lecture 18, and Sacks, 1979). There seems to be here a whole area of inquiry which might be termed a sociology of cognition or a cognitive sociology quite distinct from other usages of this term (cf. especially Cicourel, 1974). Insofar as it involves the differential relevance of different category sets for the cognitive operations of persons dealing with categories of persons, its relevance to cognitive science seems transparent.

Second, problem-types. There is a form of problem which Sacks takes up a number of times in the early lectures, each time on a distinct target, which can be best characterized as an 'analysis of the ordering of cognitive operations' (or the ordering of interpretive procedures). Two especially brilliant instances of solutions to this problem-type occur in the Spring 1966 lectures. In lecture 11 (pp. 350–1) and again in lecture 21 (pp. 417–20), in dealing with an instance of 'intentional misaddress,' Sacks wonders how the co-participants in an interactional episode could have found who was being addressed, since the address term employed by the speaker ("mommy") did not 'actually' apply to anyone present. He argues that, if they were finding 'who is being addressed' by finding to whom the address term referred, then they would find no solution. Rather, he argues, they first use sequencing rules to find whom the current speaker would properly be addressing, and they use the product of that analysis in deciding how the address term is properly to be interpreted. He is thus able to sort out the order in which these analyses are conducted – first addressee, then address term – and it turns out to be just the opposite from what one might have thought.

Another instance of the same problem is addressed in lecture 16 for Spring 1966. Here the object of interest is what is conventionally known as 'the possessive pronoun.' Rather than taking a word like 'my' as indicating a relationship of 'possessing' toward whatever it is affiliated to (which yields

results in usages such as 'my brother' or 'my teacher' which are either obviously faulty or in need of subsequent, and questionable, interpretation), Sacks argues that a hearer/receiver must first determine that what 'my' is attached to is a 'possessable' – the sort of thing which in that culture can be possessed (rather than a category from a membership categorization device, for example), in order to decide that 'my' is being used to claim possession. Once again, an ordering of analyses – of cognitive operations – seems clearly involved.

In both of these cases, the upshot of Sacks' analysis is to reject as inadequate the view that linguistic items determine the meaning or the force of an action, and to insist instead that the cultural, sequential or interactional status of the objects employed in the utterance shape the interpretation of the linguistic item.[26]

But for Sacks there was no in-principle ordering of what sorts of things one consults first (e.g., the syntactic, semantic, sequential, interactional, etc.) and no necessary priority, therefore, among the disciplines which study them. Perhaps the first appearance of this problem-type is in lecture 4 for Fall 1965. Here Sacks is discussing various forms of 'tying rules,' forms of talk (such as indexical or anaphoric reference) which require a hearer to make reference to another utterance to understand a current utterance, and which thus 'tie' the utterances to one another. Encountering such usages of 'that' as "I decided that years ago" or "That's the challenge," Sacks remarks that they present a complication relative to other instances of tying procedures which he had previously discussed, for such usages must be distinguished from the use of 'that' in, for example, "I still say though that if you take . . . " Before analyzing a 'that' for the sequential tying connection it makes to some other (ordinarily prior) utterance, a hearer has to do a syntactic analysis to determine that the 'that' is the sort which can tie back to some earlier component of the talk. Here, once again, the sheer occurrence of an item (whether address term, 'my,' or 'that') does not determine what is to be made of it. But whereas in the analyses previously discussed a linguistic analysis is contingent on prior sequential, interactional or cultural analyses. here the sequential 'tying' analysis is contingent on a prior syntactic one.[27]

[26] Related discussions can be found throughout the lectures. For example, in the Spring 1966 lectures: lecture 11, pp. 350–1 (re how sequential and interactional organization controls the semantic and truth-conditional interpretation of an utterance, rather than the opposite, which is the ordinary understanding); lecture 16, p. 383; lecture 21, pp. 417–20; lecture 27, p. 451 (where sequential context is shown to control the very hearing of a word); and lecture 29, pp. 461–3. See also the earlier discussion at n. 23. Fuller discussion of the theme and the particular analyses on which it rests must await another occasion.

[27] Still, there is little doubt that the main thrust of analyses along these lines is that the understanding of talk is, in the first instance, controlled by the hearer's grasp of the sequence in progress (or the sequential context more generally), rather than being derived from the linguistic tokens. Cf., for example, the discussion in Spring 1966, lecture 27, p. 451, where Sacks discusses the difficulty experienced by one participant

Whatever the particulars, both these observations about control of categorization structures and deployments and the problem-type addressed to the ordering of cognitive or psycholinguistic or interpretive operations are theoretically central to the responsibilities of a sociological, or more generally interactional, sector of what are now called the cognitive sciences. And to the degree that the results of these inquiries inform and constrain our understanding of how linguistic and category terms work, indeed *can* work, their import goes well beyond the interactional domain which is their initial locus.

The quasi-generativist themes in the Fall 1965 lectures, and in the 1964–5 lectures as well, co-exist with analyses of particular action types ('how to do action X') based on empirical materials of talk, and co-exist as well with analyses of sequencing and tying practices – also developed on empirical materials, and addressed to the doing of conversation as an undertaking in its own right. This variety of topics and approaches (and I have not mentioned all the separate strands here) are, then, not a matter of stages in Sacks' intellectual development over time. There are in these early lectures different sorts of undertaking underway, differentially developed by Sacks, differentially appealing to various segments of his professional readership, and perhaps differentially susceptable to development by others, and, therefore, differentially institutionalizable as a discipline. Surely, however, the drift of his own subsequent work favored some of these initiatives over others.

VIII

If the lectures of Fall 1965 tilt in the direction of culture (whether incidentally because of the course title or because it was central to Sacks' preoccupations at the time), the Spring 1966 lectures feature culture quite centrally. This was the most extensively taped and transcribed of the lecture sets, and it is as rich as anything in the materials assembled in these volumes. In its range – from the empirical detail of the interactional materials to discussions of some of the classic texts of social science and western culture – it gives the reader some sense of the power of the mind at work here, of the nuanced sensitivity to detail and of the scope of learning being brought to bear, and the distinctive stance being developed through the conjunction of these resources. Here I can touch only briefly on a few of the central themes of these lectures.

One theme, clearly part of the 'culturalist' motif of these lectures, and surely not unrelated to the abiding preoccupation with 'reflexivity' and the 'incarnate character of accounts' central to the continuing development of ethnomethodology in Garfinkel's oeuvre, concerns the relationship between

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in hearing something addressed to him which is acoustically accessible to everyone else. He remarks that the party in question hears that turn by reference to the sequence in which it occurs 'so as to hear, indeed, a puzzle, when he could hear something perfectly clear.'

'commonsense knowledge' and real world conduct or praxis on the one hand, and between commonsense knowledge and 'professional' inquiry on the other hand. This theme provides an opportunity as well to touch on the elements of continuity and discontinuity in the orientation of Sacks' work going back to 'Sociological description' (1963).

Although there is no direct connection between the positions explored in 'Sociological description' and these lectures, there are echoes here, formal similarities to aspects of the earlier paper. By 'no direct connection,' I intend two observations. First, there is a substantial difference between what Sacks is doing in the lectures and the hypothesized program of studies which Sacks entertained in 'Sociological description' as a contrast with his depiction of how contemporary sociological inquiries are conceived and carried through. Second, there was no direct step-by-step theoretical development that led from the position taken up in the 1963 paper to the directions pursued in the lectures of 1964–6. On the other hand, I can only roughly suggest one sort of observation I have in mind in suggesting 'echoes' and 'formal similarities.'

The central metaphor of 'Sociological description' was the so-called 'commentator machine,' a 'device' describable (from one point of view) as composed of two parts – one which engages in some physical activity and another which produces a form of language, understandable as a description of what the first part is doing. Sacks entertains a variety of possible formulations of this device, and the 'proper' understanding of the relationship of its parts. The 'doing part' can be understood as a resource for coming to understand what the 'speaking part' is saying. The 'speaking part' can be understood as a description of what the 'doing part' is doing. The contraption may be understood as two independent devices. And so on. For those views in which the two parts do relate to one another, 'discrepancies' between the parts can be variously understood: for example, as the 'speaking part' offering *inadequate descriptions* of the 'doing part;' alternatively, as the 'doing part' malfunctioning and *badly enacting* the program set forth by the 'speaking part.'

With such a theme in the background consider just a few elements of the first two lectures of Spring 1966 and some elements from the lectures of the intervening year, 1964–5.

One of the central tasks which Sacks sets himself in the lectures on "The baby cried" is providing an account of how recognizable activities are done, and done recognizably. And in particular how the activity of 'describing' is done, and done recognizably. The key starting point here is that descriptions *are* recognizable, *are* recognizable descriptions, and are recognizable descriptions without juxtaposition to their putative objects. Much of Sacks' effort in the early years of this analytic enterprise was given over to building an apparatus that provided recognizable descriptions without reference (by real life co-participants or by professional investigators) to what was putatively being described. The 'membership categorization devices' introduced in lectures 1 and 2 of Spring 1966, and the MIR device introduced in lecture

6 of the 1964–5 lectures (p. 41)[28] are key elements in such an apparatus. And the commonsense knowledge of the social world which is organized in terms of these categories, 'protected' as it is 'against induction' (as Sacks used to remark), provides for just such autonomously recognizable possible descriptions. When some potential discrepancy is suggested between what is provided for by the 'knowledge' organized around some category in a categorization device and what is observably the case about some putative incumbent of such a category, what may well be found (Sacks pointed out, and this is part of what he meant by 'protected against induction') is not the inadequacy of that 'knowledge' but rather the inadequacy of that person as a member of the category involved, an inadequacy which that person may feel and may seek to remedy.

Although vastly transformed (from a 'doing part' and 'speaking part' to 'observable conduct' and 'recognizable description'), the problematics concerning (1) the proper juxtaposition of the practical activities of social conduct, (2) the commonsense knowledge of the mundane world and descriptive practices resident in that world, and (3) the proper formulation of investigators' stances and goals with respect to that world, persist from 'Sociological description' through these lectures.

One component of these problematics is specially important throughout these lectures, surfacing at the end in Spring 1966, lecture 33 but also central at the beginning, and that is the relationship between commonsense knowledge which investigators may share with those whose conduct is the object of inquiry and the proper formulation of research questions, observations and findings. Sacks begins the discussion of "The baby cried" with a number of observations which he makes about the components of this little story, and offers the claim that his audience would have made (perhaps did in fact make) the same observations. But these are not sociological findings, he insists. They are simply the explication of commonsense or vernacular knowledge. Rather than constituting analysis, they serve to pose a research problem, namely, the construction of an apparatus that would generate (or that has generated) such observations, that would (in that sense) have produced them. And such an apparatus would constitute findings.

Both parts of this analytic operation are important: making explicit the understandings which common sense provides of the world which members of the society encounter, including the conduct of others; and the provision of something that can account for those understandings. And it is important to keep them distinct and to insist on both.

Consider, for example, the notion of category-bound activities. It is in order to address the observation that a report of 'crying' makes the category 'baby' (in the sense of a 'stage-of-life' category) relevant that Sacks introduces this notion, and the proposal that the activity 'crying' is 'bound' to the

[28] Sacks (ibid.) explains the term 'MIR device' by saying, "that is an acronym. 'M' stands for membership, 'I' stands for inference-rich, and 'R' stands for representative."

membership category 'baby' as one of the 'stage-of-life' categories in particular. But the observation that "crying is bound to 'baby'" is (like the initial observations in the lecture) not a finding; it is merely the claimed explication of a bit of commonsense knowledge. As such it is just a claim, and cannot be simply asserted on the analyst's authority. It has to be warranted somehow, either by a test of it or by requiring it to yield some further pay-off to analysis.

And this is what Sacks does with "crying being category-bound to baby." He immediately (lecture 1, p. 241) constructs a test of this category-boundedness, even though (as he says) "it's obvious enough to you, you wouldn't argue with the issue." The pay-off, it will be recalled (lecture 1, ibid), is not only the explication of 'praising/denigrating' as a test for the category-boundedness of the action 'crying,' but an account for how to do such recognizable actions as 'praising' or 'deprecating', research goals already familiar from the 1964–5 lectures and from elsewhere in the Spring 1966 opening lectures.

This stance is a basic and persistent one in these lectures. Elsewhere, for example, Sacks insists on testing the claim that the categorization device 'therapist/patient' is 'omni-relevant' in the group therapy sessions which supply the data for most of these lectures (Spring 1966, lecture 6, p. 315; lecture 29, pp. 462–3; then again in Winter 1967, February 16, and Spring 1967, lecture 14), although this claim can be treated as no less 'obvious.' To be sure, when he has recently made the point, Sacks sometimes asserts a claimed category-bound activity without carrying through a test or deriving a further finding (e.g., lecture 4, p. 302), but there can be little doubt that the principle is basic – commonsense knowledge cannot properly be invoked as itself providing an account, rather than providing the elements of something to be accounted for.[29] In my view, Sacks abandoned the use of 'category-bound activities' because of an incipient 'promiscuous' use of them, i.e., an unelaborated invocation of some vernacularly based assertion (i.e., that some activity was bound to some category) as an element of an account on the investigator's authority, without deriving from it any analytic pay-off other than the claimed account for the data which motivated its introduction in the first place.

The editorial effort to combine and blend largely overlapping treatments of the same material, which has prompted the inclusion of lectures delivered during the following term in Fall 1966, here in the Spring 1966 set (e.g., lecture 04.a), brings into relief certain shifts in analytic focus which accompanied a return by Sacks to the same empirical materials. Only two of

[29] See, for example, lecture 04.b, p. 295, here included with the Spring 1966 lectures, though actually delivered later, in Fall 1966: '. . . it is our business to *analyze* how it is that something gets done, or how something is 'a something,' and not to *employ* it.'

This theme – as represented, for example, in the phrase introduced by Garfinkel, 'commonsense knowledge as topic and resource' – is, of course, central to ethnomethodology.

these shifts can be taken up here, and only for a brief mention.

As remarked earlier, the analytic task set front and center in the initial lectures for Spring 1966 was "how recognizable actions get done and get done recognizably." The first two lectures address those questions to the actions 'doing describing' and 'doing storytelling.' (The third lecture, omitted here because of its availability in a published version as 'Everyone has to lie' took up the issue of 'doing a recognizably true statement.') Lectures 04.a and 04.b, interpolated here from Fall 1966, have a different analytic focus – observing and establishing orderliness – but lectures 4, 5 and 6 (delivered in the Spring term) continue the 'recognizable actions' theme (doing recognizable invitation and rejection) and reproducible methods for accomplishing recognizable actions.

At the same time there is an apparent shift toward the invocation of a kind of evidence that was to assume an increasingly central place in Sacks' conception of how to ground an argument or an observation. In lecture 4 (from the Spring) he proposes that, in order to establish that "we were in an automobile discussion" is doing a recognizable invitation, it is necessary not only to agree that it "looks like an invitation" but to show "how that's so" (p. 301) with the description of a method for doing invitations that works for the instance at hand. This echoes the stance of lectures 1 and 2.

In lecture 04.a (pp. 286–7, 288–9) from the Fall 1966 term, Sacks offers as evidence that some earlier talk was attended by others than its overt interlocutors, and as evidence that it constituted a recognizable introduction, the *prima facie* evidence afforded by a subsequent speaker's talk. Specifically, he notes, that when Ken responds to the utterance of his name by the therapist Dan not with "What" (as in an answer to a summons), indeed not with an utterance to the therapist at all, but with a greeting to the newly arrived Jim, he shows himself (to the others there assembled as well as to us, analytic overhearers) to have attended and analyzed the earlier talk, to have understood that an introduction sequence was being launched, and to be prepared to participate by initiating a greeting exchange in the slot in which it is he who is being introduced.

There is a shift here in analytic stance and procedure, from the analyst's understanding as initial point of departure on the one hand to the co-participant's understanding as initial point of departure on the other.

In the former mode, the analysis begins with an asserted convergence of interpretations and recognitions by the analyst and the analyst's audience (for example, that something is a story, that 'the mommy' is 'the mommy of the baby,' that an utterance is doing an invitation, and so on). It proceeds by the provision of a methodical basis for both that convergence of understandings and the convergence between the 'understanders' and the producers of the to-be-understood 'in the data.' In the latter mode, analysis begins with an asserted observation (that not-overtly-engaged participants are attending, and, indeed, are obligated to attend to the talk), and then immediately grounds that observation in subsequent conduct by the co-participants in the episode being examined. That conduct is taken as displaying the product of

their orientation to, and understanding of, the setting and what has been transpiring in it. The site of analysis is located in the setting of the data at the outset. And further: the analysts' so treating the conduct of the participants is itself grounded in the claim that the co-participants so treat it.

This contrast in stance and procedure is visible in this publication of the lectures only briefly, by virtue of the juxtaposition of the material from Spring and Fall 1966. What is seen only in lecture 0.4a–b here is seen increasingly thereafter, starting with the Winter 1967 lectures in the present volumes. Of course, this shift does not entail any abandonment of the commitment to provide an account for how the recognizable outcome – whatever sort of object it may be – is produced, although the form such an account might take does change over time. The subsequently developed description of the turn-taking organization, for example, is offered as a procedural account for how a substantial collection of observable achievements of ordinary talk are methodically produced by the co-participants.

What I have referred to as the 'culturalist' tenor of the Spring 1966 lectures is set in the first of its lectures, when Sacks sums up his initial gloss of the understanding of "The baby cried . . ." as indicative of "the operation of the *culture*" as "something real and something finely powerful" (Lecture 1(R), pp. 245–6, emphasis supplied). The analysis of the membership categorization device and of the commonsense knowledge organized by reference to its categories is, in its fashion, an analysis of culture – "an analysis of *some* culture," as Sacks puts it (lecture 30, p. 469, emphasis supplied). Throughout these 34 lectures (cf. especially lectures 13, 16–21, 24–25 and 31 and the appended manuscript 'On some formal properties of children's games') may be found treatments of various forms and artifacts of 'culture' in at least that anthropological sense in which it refers to the categories through which 'reality' is grasped. Among these forms and artifacts are the categories of persons making up a society and its world and who is entitled authoritatively to 'administer' those categories (lecture 13), notions of possession and possessables, the constitution of observations and descriptions, measurements systems (lecture 24), games (lectures 13 and 31 and 'On some formal properties . . .'), conceptions of danger and their bearing on differentially accomplishing such actions as warning and challenge (lecture 10, 12) etc. A kind of socio-cultural semantics is involved, and a largely anthropological literature is invoked, reflecting Sacks' engagement with then-contemporary work in so-called 'ethnoscience.'[30]

[30] Cf. Sacks' contrast of his own way of working on such matters with the then-mainstream approaches to ethnoscience, for example, with regard to 'measurement systems,' the discussion at lecture 24, p. 436, where the contrast is almost certainly with the work of Berlin and Kay (1969, but circulated in mimeo earlier) on color terms.

Although ethnoscience is in point for this particular reference, Sacks' reading in, and use of, the anthropological literature was very broad indeed – both in 'areal' terms and in 'approaches.' What he most appreciated was some combination of dense and acutely observed ethnography, tempered by a sharp theoretical intelli-

All of these lectures provide rich materials for analysis and discussion, but in the present context, a brief consideration of Sacks' treatment of games may serve to recall some of the relevant intellectual context for this sort of cultural analysis, as well as to permit a brief consideration of a direction for the study of culture and acculturation, including language acquisition, which deserves fuller exploration than it has been accorded.

The most immediately relevant context for writing about games within American social science in the mid-1960's traces back to the invention of 'game theory' in 1944 by von Neumann and Morgenstern as a branch of mathematics with overtly 'social' applications (the title of their book was *Theory of Games and Economic Behavior*), with its subsequent elaboration by economists and others concerned with strategic thinking, most visibly in the late 1950's and early 1960's, in authors such as Kenneth Boulding (1963), and Thomas Schelling (1961). The analytic force of the metaphor propelled it into the arena of discourse and interaction as well, the language of constitutive rules playing a central role in Searle's development of speech act theory, for example, and strategic considerations entering psychology and sociology through varieties of 'exchange theory' (e.g., Thibaut and Kelley, 1959, or Blau, 1964). More proximately to Sacks' thinking, both Goffman and Garfinkel had explored the game model or metaphor in their own work – Goffman in his essay 'Fun in games' (in Goffman, 1961) and later in *Strategic Interaction* (1969, but written in 1966–7), and Garfinkel in the so-called 'trust' paper (Garfinkel, 1963), a paper from which he subsequently distanced himself, refusing to include it in the collection of his papers in 1967, *Studies in Ethnomethodology.*

One problem with the assimilation of game theory into social science was in establishing the limits of its usefulness as a model of social reality, a concern surely central to both Goffman's and Garfinkel's treatment of it. One central objection is that 'games' fail as a basic model of social order much as 'contract' failed as a basic model in Durkheim's discussion of 'utilitarian' social theory, an element of Durkheim (and Parsons' (1937) treatment of Durkheim) especially emphasized by Garfinkel. In both cases, the 'model' – whether 'contract' or 'game' – is itself 'an institution,' a normatively constrained organization of understandings and conduct, with its own constitutive infrastructure. 'Contract' could not undergird social order because, as a legal institution, it was itself undergirded by the social order it was invoked to explain. So also would 'games' fail as models of social interaction, for the

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gence, and informed by broad learning. I recall especially his appreciation of Hocart and Elizabeth Colson, of Fortune and Edmund Leach, of Evans-Pritchard and Max Gluckman. But less reknowned ethnographers were no less appreciated. His fondness for ethnography crossed disciplinary boundaries, and he collected original issues of the volumes produced by the founding 'Chicago school' of sociological field workers – Nels Anderson, Paul Cressey, Franklin Frazier, Clifford Shaw, Frederic Thrasher, Harvey Zorbaugh – and later sociological ethnographies such as Dollard (1937), Drake and Cayton (1945), and, in a different vein, studies like Cressey (1953), discussed earlier.

conduct of games and their constitution presumed an infrastructure of interactional conduct, and an epistemic/ontological definition as a discrete order of 'reality,' within which games constituted a separate domain of activities. Such misgivings would surely have informed Sacks' approach to games from the outset.

It is noteworthy, in this regard, that Sacks focussed on *childrens'* games,[31] and that one of his central preoccupations was to get at that very infrastructure by reference to which games, as a special class of events, also are undergirded. Thus, both in lecture 13 (on the game 'Button-button who's got the button') and in the draft manuscript on children's games appended to the Spring 1966 lectures, games are treated not as models *of* or *about* social life *for* the social scientist, but as training grounds for formal aspects of social life *in* social life, i.e., as arenas within social life for kids' learning of central features of (the) culture, features such as the operation of membership categorization devices, the management of appearance and emotional display, etc. His treatment of children's games aims to provide analytic particulars for his claim ('On some formal properties . . . , Spring 1966, Appendix A, p. 502) that "Play then becomes an environment for learning and demonstrating criterial matters in real world action." Games provide models *of* social life *in* social life for its initiates, and in that capacity can be looked to for methodically central components of culture. In that regard, for example, such a game-relevant contrast as 'counting' versus 'not counting' can provide materials on which can be built such 'real-world' contrasts as 'legal versus illegal.'

Considerations of enculturation and 'language acquisition' provide an especially provocative focus for a matter which Sacks raises, in the first instance, rather more as a methodological point. Taking up the methodological relevance of sampling, Sacks points out that it depends on the sort of order one takes it that the social world exhibits. An alternative to the possibility that order manifests itself at an aggregate level and is statistical in character is what he terms the 'order at all points' view (lecture 33, p. 484). This view, rather like the 'holographic' model of information distribution, understands order not to be present only at aggregate levels and therefore subject to an overall differential distribution, but to be present in detail on a case by case, environment by environment basis. A culture is not then to be found only by aggregating all of its venues; it is substantially present in each of its venues.

Leaving aside the consequences for the methodology of professional inquiry, consider the implication that ' . . . any Member encountering from his infancy a very small portion of it, and a random portion in a way (the parents he happens to have, the experiences he happens to have, the vocabulary that happens to be thrown at him in whatever sentences he

[31] Recall that this antedates by several years organized attention to play and games in the social science community, as represented, for example, in the wide ranging collection edited by Bruner, Jolly and Sylva (1976).

happens to get) comes out in many ways pretty much like everybody else, and able to deal with pretty much anyone else' (ibid., p. 485).

In such a view, one might conjecture, we have one, and perhaps the major, theoretically available alternative to Chomsky's argument that, given the highly limited and 'degenerate' sample of a language to which first language learners are exposed, most of language – the crucial part – must certainly be innate; they surely could not be induced from the available 'inputs.'

The alternative is to consider a culture – and language as one component of culture – to be organized on the basis of 'order at all points.' If culture were built that way, then socialization and language acquisition might well be designed accordingly, and require induction from just the 'limited' environments to which the 'inductee' is exposed. As Sacks writes (ibid., p. 485), ". . . given that for a Member encountering a very limited environment, he has to be able to do that [i.e., grasp the order] . . . things are so arranged as to permit him to." 'Things' here presumably includes the organization of culture, the organization of language, the organization of learning, and the organization of interaction through which the learning is largely done. What such a view projects is the need for an account of culture and interaction – and the acquisition of culture and language *in* interaction – which would complement a 'cognitive' language acquisition device and innate grammar much reduced from contemporary understanding. Studies relevant to such a view have been pursued for the last two decades or so, but not necessarily under the auspices of the theoretical stance toward culture which Sacks projects here. The evidence for an 'order at all points' view has accrued throughout Sacks' subsequent work and the work of others working in this area.

IX

The sessions from Winter 1967 appear in various respects transitional. There are returns to, and revisions of, themes initially discussed in earlier sets, including 1964–5 lectures, and initial explorations of topics taken up in much greater detail in subsequent terms. The discussion here can only touch on a few of these themes.

It is in the session of March 2, 1967 that we find the first substantial consideration of turn-taking in multi-party settings. Here, as elsewhere in the lectures, a set of materials is treated lightly near the end of one term, and then is taken up in much greater detail in the next. The single session devoted to turn-taking in Winter 1967 is followed by seven lectures in Spring 1967 (the lectures on turn-taking from that term are not printed here), and an extensive run in Fall 1967.[32]

[32] Another 'take,' embodying a different stance toward the work, is presented in the Fall 1968 lectures.

A good deal of this treatment seems to have been prompted by reflections on the difference between the two-party talk discussed in the 1964–5 and Fall 1965 lectures on the one hand (for which the materials were drawn from telephone calls to the Suicide Prevention Center), and, on the other, the group therapy sessions (GTS) on which many subsequent lecture sets are based.

But the relevance of working with multi-person talk was not limited to the issue of turn-taking alone. To cite but one other product of the juxtaposition, the discussion at pp. 529–33 of the March 2 session is concerned with 'derivative actions,' i.e., what a speaker may be doing to a third party by virtue of addressing a recipient in a certain way. This seemed to Sacks but one indication of the need to take up multi-person materials apart from two-party ones (p. 533).[33]

There is a theme taken up in the February 16 session, and touched on again on March 9 (pp. 543–6), whose relevance to contemporary concerns (both then and now) may be worth brief development here. One way of characterizing those concerns is the generic relevance of context to talk in interaction.

The general question taken up is whether there is some way of formulating or invoking the sheer fact of the 'settinged'-ness of some activity, without formulating or specifying the setting. The 'solution' which Sacks points to is the use of indicator terms (e.g., 'here and now' or stable uses of 'this') to do this, a usage which affords us evidence that it can, in fact, be done. Indicator terms can be seen as a machinery for invoking an unformulated setting, for referring to (categorially-) unidentifed persons, or taking note of unformulated activities.

But where does this 'question' come from? Why is its solution of any interest? The beginning of the discussion, of course, is given not by a question, but by some observations which end up as the 'solution.' This was a common, and recommended, analytic procedure for Sacks: begin with some *observations*, then find the problem for which those observations could serve as (elements of) the solution.

The observations in point here concerned the use of such 'indicator terms,' terms whose special relevance for ethnomethodology had (under the name 'indexical expressions') already been developed and underscored by Garfinkel (1967, passim). And the central observations here had come up in a train of considerations with a quite different focus, along the following lines.

The discussion begins with the problems of the 'professional' analyst (i.e., the 'conversation analyst,' not the 'therapist' in the data) establishing the categorization device 'patient/therapist' as omni-relevant for the participants (which would cast it as always-invocable – 'on tap,' so to speak – both by participants and by analyst). One way of doing that analytic task is to establish a formulation of the setting as 'group therapy session' as omni-relevant. Sacks then observes that this is but one form of 'formulating as a

[33] Subsequently it turned out that derivative actions can be found in two-party conversation as well (Schegloff, 1984 [1976]).

such-and-such,' and that this is something that *Members* do. When they do it, it is consequential, that is, they are doing some possible *action* in doing the formulation. (Recall the discussion in Spring, 1966 of "we were in an automobile discussion" as a formulation of the topic as a such-and-such which is consequential – which does a possible invitation.)

The question then is: is there some way of referring to the context, or components of the context, without *formulating* the context (or persons or actions in it) as such-and-such – without, therefore, potentially doing the actions which such a formulation might do. (Note that this can be a consideration both for members/participants-in-the-interaction and for professional analysts: for members so as to avoid doing the potential actions and the responses they would engender in the interactional setting; for analysts because it is precisely the escape from control by that interactional consequentiality, from what otherwise constrains or 'disciplines' formulations, that makes professional use of the lay device problematic).

It is in this context that the observation about the indicator terms finds its resonance: terms like 'here and now' can invoke any present context and any conception of scope-of-context ('in this room,' 'in 20th-century America,' etc.) without formulating it. And by requiring a recipient to provide its sense, they recruit the recipient into the speaker's project; they make the recipient complicit in forming up its sense.

Several further brief comments will have to suffice here:

1 The observation that formulating does more than simply naming what is formulated is focussed especially on 'formulating what someone is doing' in the March 9 lecture (pp. 544–6), and it sounds a theme central to the paper 'Formal properties of practical action' (Garfinkel and Sacks, 1972), though that paper was (according to Sacks) Garfinkel's work. In this discussion in Winter 1967 we see something of Sacks' 'take' on similar issues, possibly one source of discussions of this theme between them.

2 The considerations raised here (and when this theme is addressed elsewhere) impose a constraint on discussions of 'context' and its bearing on talk and action which has not been fully absorbed in the literature. The same problems raised about the categorization of persons/members pertain:

the set of available characterizations is indefinitely extendable;
the selection of some one or more is potentially a way of doing something, (i.e., is open to such understanding by others);
in actual interaction, such possible interpretation by interlocutors and the responses they may offer in turn, can serve as a constraint on actually selecting such a formulation;
the absence of such a constraint in the activities of professional analysts leaves the grounds of such choices undisciplined, and therefore problematic.

The positivist solution to this problem (i.e., constraining the choice of formulation by explanatory adequacy as attested by 'evidence,' leaves the

actual orientations of participants out of the picture. Where 'context' is made a central notion, these concerns will have continuing relevance.

3 The effort to cast 'therapist/patient' as an omni-relevant categorization device has as one continuing relevance the concern with the bearing of gender (and perhaps other of what Hughes (1971) used to call 'master statuses') on talk-in-interaction. Were those who bring considerations of gender to bear on all phenomena of interaction to take seriously the considerations touched on just above, they might undertake to show that the pervasive relevance of gender can be grounded in the demonstrably equally pervasive orientations to it by participants to interaction. In effect, this would amount to showing that the categories and terms of gender identification are omni-relevant for interaction.

Sacks' exploration of this issue in Winter 1967 is left unresolved. By the time he comes to cast the indicator terms as ways of invoking the settinged-ness of the interaction without formulating it, the problem of establishing omni-relevance of either member-formulation or context-formulation has been abandoned. In its place is the possibility of non-formulation, of a kind of specific abstractness in treating the contexted character of activity. But the exploration of omni-relevance is taken up again in lecture 14 for Spring 1967 (cf. discussion below at pp. liii–livff.).

One theme from the 1964–5 lectures which reappears in the Winter 1967 lectures, reapplied to a related topic, is that of 'direct' versus 'in-various-ways-non-direct' speaking; the topic to which it is now applied is 'euphemism' (or what may be, once the data are examined, better termed 'irony'). Although the 1964–5 lectures asked over and over again 'how to do X without doing it overtly,' the message here is that to ask why a euphemism or ironic trope was used instead of a direct or 'literal' saying is to get the question wrong. What Sacks is urging here (March 9, 1967, pp. 545–6) is that the first-order consideration is not directness/indirectness or literalness/figurativeness. Rather it is (for the speaker) a saying which displays its *relevance* at that point in the talk, and (for the hearers) a saying such that their understanding (their *capacity* to understand) 'proves' the utterance's relevance. The 'norm' is not, in the first instance, direct or literal reference, but rather ways of talking that are *locally adapted* and can show *local relevance.*

The first-order considerations are thus tying rules and other local connections between elements of the talk, rather than 'saying it directly.' The issue of 'directness/indirectness' comes to the fore only with academic analysts determined to understand the talk 'in general,' stripped of its local context. For them what comes to identify a bit of talk, to constitute its re-referable core, is its semantico-lexical content and perhaps its pragmatic upshot. With that as the core, then various ways of realizing that central identity can come to be formulated as more-or-less straightforward, direct, literal, or 'tropic' in some respect. What was *in situ* a production tailored to the details of local context is reinterpreted as a design for indirectness when local context is

stripped away and no longer accessible as the source of the utterance's design.

How is this line to be reconciled with the analysis in the 1964–5 lectures, where just this question is asked – e.g., why seek out the other's name without asking for it directly? Perhaps this is one locus of development and change in Sacks' thought during this period. But it is also possible that when the embodiment-of-indirection cannot be understood (by recipient, or by professional analyst) as an adaptation to the local context, then the question of why the indirect rather than the direct may in fact be warranted and useful, and in just those terms.[34]

X

There are three predominants 'casts' to the lectures of Spring 1967.

As noted earlier, the first seven lectures (not published in this edition) constituted the first sustained set on turn-taking, expanding the treatment in the lecture of March 2 in the Winter 1967 set. This is a 'sequential organization' cast.

Lectures 8–9, earlier treated in lecture 3 for Spring 1966 and subsequently published as 'Everyone has to lie' (1975), have what might be termed more of a 'socio-logic' cast – juxtaposing to what might appear 'logical' ways of analyzing the conversational materials properly socio-logical ones.

From lecture 11 on, the materials take on the same flavor of anthropological/cultural analysis that so heavily informs the Spring 1966 set. This is largely the result of a focus on membership categories underlying talk and relationships between those categories (their relative positionedness for instance), notions of activities 'bound to' those categories, and the sorts of commonsense 'knowledge' organized by reference to those categories (in the manner of 'Y do X,' where Y is a category name, such as 'women,' 'freshmen,' 'politicians,' etc.) Some of this material was organized into a draft manuscript under the title, 'On a device basic to social interaction,' around the time of writing of 'An initial investigation . . .' As introduced into these lectures, much of the earlier statement seems to have been substantially refined.

The discussion here will be limited to some reflections on the "Everyone has to lie" analysis and on the reconsideration by Sacks of the matter of the

[34] And Sacks does sometimes work on an utterance by addressing, what it *prima facie* would be *out* of context, in a more-or-less 'literal' hearing, and with good results; cf. Spring 1966, lecture 29 pp. 461–2, where he shows how various components of the utterance "Usually there's a broad in here" are neither produced nor grasped in their 'bare' literal sense: e.g., 'here', means not 'this place' but 'when we are in [therapy] session;' 'a broad,' means not 'some woman' but 'the same woman,' indeed 'a particular same woman,' and one who is a member of the group, etc.

omni-relevance of formulations of setting and participants, earlier taken up in Spring 1966 and in Winter 1967.

At least one underlying source and rationale for the animating question being addressed in the 'exercise' concerned with the assertion "everyone has to lie" is formulated by Sacks (Spring 1967, lecture 8, p. 549) as "How could we as social scientists go about saying about something that a Member said, that it's true." It may be useful to 'unpack' the background for this question at least partially.[35]

As rhetoric as a core method and discipline for the analysis of what can be said gradually became demoted in the intellectual hierarchy of western culture, and logic developed an increasing hegemony, it brought with it an increasingly exclusive preoccupation with 'truth' as the paramount feature of assertions requiring definition and assessment. In part this concern was in the service of 'science,' and its aims of establishing stable propositions about the world whose truth could be established once and for all.

When attention began in the 20th century to turn to statements in so-called ordinary language, the analytic apparatus available for use was that of formal logic, and it was in part by virtue of the results of applying a formal logic developed in the service of science and mathematics to ordinary language that natural languages were found defective and the need for 'formal languages' made compelling. But the goals of logic/science and ordinary discourse are by no means the same, and the use of language in them may be quite different. What is relevant to establishing the truth of a proposition in science – and what might be 'meant' by 'truth' – may be quite different from assessing the truth of a 'commonsense assertion' in ordinary circumstances. It is this gap which, in part, Sacks is addressing.

Here, as elsewhere, Sacks' exploration of this theme (the contrast between 'common sense' and 'scientific' procedures) is focussed on a class of terms which is especially symbolic of logic – quantifiers. In 'An initial investigation . . .' as well such a term had become a focus of analysis. There it was the term 'no one,' in the claim by a suicidal person that they have "no one to turn to," and Sacks undertook to explicate how 'no one' is used, and used 'correctly,' given the 'paradox' that the assertion is made precisely in the conversation in which its speaker has turned to 'someone.'

'Initial investigation . . .' showed how "no one to turn to" was not belied by having turned to someone for the conversation in which it was said because 'no one' had as its scope only certain categories of person; 'no one' was not being used in some formal logical sense, as 'no person.' It was therefore misguided to begin with a 'logical' understanding of the term, when that was not the use being made of it in the production of the utterance.

In lectures 8 and 9, the quantifier under examination is 'everyone.' Again, Sacks proposes not to begin with some sense of the term derived from logic (some 'strict usage' as he puts it), and find how trivially to disprove the assertion by showing that there is at least one person who does *not* have to lie.

[35] A similar question is taken up in Spring 1966, lecture 26.

Rather, he proposes that we must investigate anew, and for its usage in ordinary conversation, how a term like 'everyone' is constituted and used.

And more generally, assessing the truth of the assertion involves not just a manipulation of truth conditions, but rather an explication of those practices of talk-in-interaction which the assertion could reflect an orientation to, and whose actual operation could be what is being invoked in the asserted claim. In the context of this lecture, this refers to the contingencies of the 'How are you' question, its privileges of occurrence, its types of relevant answer, and how the further courses of action which its answers make contingently relevant affect the choice of answers in the first instance (pp. 556ff.). By the end of the discussion, this structure is generalized well beyond 'How are you,' and is used to specify where lying may be generically suspected, where confessions of it will be readily believed, etc.

In any case, what emerges as criterial to the inquiry is not a *logical* analysis of the component terms of the assertion and an assesment of their combination, but a *social* analysis of those contingencies of interaction which could give rise to the condition which the assertion claims. The upshot here is to blunt the *prima facie* application of 'logical' analysis as the first-order consideration in much the same fashion as several of the Spring 1966 lectures had the import of blunting the *prima facie* linguistic analysis of an utterance (cf. above at pp. xxxvii–xxxix, the discussion of 'the ordering of analyses'). In both cases, the tools of linguistic and logical analysis are shown to have their relevance and applicability constrained by, and contingent on, prior sequential, interactional and cultural specifications of the practices of talking underlying production of the utterance.

What emerges is, then, a wholly different conception of what the analysis of ordinary discourse should consist in. It is this result which is adumbrated by asking at the outset how social scientists might go about assessing the truth of what a Member says, and this which animates that question.[36]

In lecture 14 (from p. 594 to the end of the lecture) Sacks again takes up the question of the 'omni-relevance' of a category collection. In the discussion of Winter 1967, the issue became redefined as invoking a context (and potentially associated membership categories) without actually formulating them – invoking the sheer fact of 'settinged-ness' (cf. above, pp. xlviii–l). Here, the discussion remains focused on the possibility of omni-relevance.

What he means by 'omni-relevance,' Sacks says, is two-fold: 'on the one hand, there are some actions which, for their effectiveness [i.e., to be recognized as that type of action], involve categorial membership in that collection, and, on the other hand, until the course of action is ended, one can't rule out the further use of that collection.' The elegant solution to the problem of showing 'therapist/patient' to be omni-relevant in the empirical materials under examination lies in noting that the effective doing of an ending to the occasion requires reference to the status of one of the parties as

[36] This sort of inquiry may be seen to inform the first paragraphs of lecture 11 as well.

'therapist.' The point is made even more exquisite by 'the therapist' actually only hinting at the 'session's' closure, and one of the more experienced patients interpreting that hint for a new patient.

What is key to the solution is its focus on the efficacy of the utterance in implementing the action of initiating the ending of the session and the non-contingency of that action. Other actions could be understood to activate the relevance of the categories germane to their efficacy, but those categories might not on that account alone necessarily be claimable as omni-relevant. But accomplishing an ending is, first, a non-contingent occurrance for the occasion (the issue is not whether it will be done, but when), and therefore prospective, i.e., relevant even before an action might invoke it. It is this non-contingent prospective relevance of an action – an action which itself makes a membership category relevant – which grounds the argument for omni-relevance here.[37]

This lecture affords an especially clear example (as Sacks' own lead-in makes clear) of one form which his kind of theorizing took. It regularly began with an observation about the particular materials being examined (an observation, of course, commonly informed by his prior work and wide reading). That observation might then be 'developed:' its terms being given an 'anterior' development, i.e., he would find and explicate what his own initiating observation could be seen, on reflection, to have presupposed; those presuppositions might well be more 'observations,' and more consequential ones. That package of observations might be followed up through discussion of matters in the literature which they touched off, through exploring purely formal kinds of logics they suggested, purely 'theoretical' possibilities they seem to entail, etc. But, recurrently, these 'theoretical' developments would be brought back to empirical materials – either what had initiated the whole line, or other materials which the line of theorizing brought to mind. It was in this sense that the effort was prosecuted to put theorizing at every point under the control of empirical materials.

The actual presentations sometimes obscured this way of working. In lecture 14, for example, Sacks begins with what appear to be very abstract considerations about applying categories to partition a population, and the relationship between the partitionings yielded by different category collections. This then is putatively 'applied' to the material at hand, in the analysis of 'teenager/adult' as a 'cover' collection preserving partitioning constancy with 'patient/therapist;' and in the covering of 'patient/observer' with 'performer/audience.' It was initially an observation about the latter – re the utterance "Testing" (p. 593) in particular – which motivated much of this line. Of course, the most extensive such reversal of order of discovery and order

[37] How Sacks' line of argument might bear on a claimed omni-relevance of gender (to re-pose an issue earlier discussed) is unclear. At the least, the constraint of "until the course of action is ended one can't rule out the further use of that collection" requires working out in any occasion being examined, specifically what 'the course of action' can be taken to be.

or presentation is the paper 'An initial investigation . . . ,' in which the originating observation was about "no one to turn to," the serious exploration of which led to formulating it as the result of a search procedure, which required formulating the terms of the search and the categories by reference to which it is conducted, etc. It was with the last of these that the paper itself began.

XI

The Fall 1967 lectures turned out to be the last at UCLA. Sacks' teaching during the Spring 1968 term was in seminar format, although he did offer sustained presentations on occasion, and these are included in the present volumes. And by Fall 1968 Sacks had moved to the University of California, Irvine (although there is no reason to think the prospect was already known at the time of the Fall 1967 lectures, or informed their delivery).

These lectures include the first extended treatment of turn-taking presented in these volumes, although the first seven lectures for Spring 1967 (not printed here) represented Sacks' actual first effort on this scale. The Fall 1967 lecture set is the only one in which Sacks offered extended treatments of both turn-taking organization and tying structures. Tying structures are discussed in several earlier lecture sets, but not again after Fall 1967. And the discussions of identification and categorization to which Sacks returned several times in the lectures preceding Fall 1967 are not taken up here, and henceforth reappear only sporadically and for much briefer treatment. Sequential organization increasingly dominates the agenda of Sacks' lectures, including expanding discussions of turn-taking, of sequence structure and adjacency pairs, of overall structural organization, of story-telling organization, etc.

If the Spring 1966 lectures were especially 'anthropological' in orientation, then the Fall 1967 lectures are especially oriented to linguistics.

This note is sounded early, when in the initial lecture, a general introduction, Sacks (pp. 622–3) projects the preoccupations of the course with 'sequential analysis' (though not under that name), which he introduces by remarking that ' . . . the discoverable aspects of single utterances turn out to be handleable – perhaps handleable only – by reference to sequencing considerations . . . ,' and declaring his interest in ". . . how it is that sequencing considerations turn out to be implicative of what happens in a given utterance."

"Linguistics," by contrast (he argues), "is that study of the utterance which involves detecting those features of it which are handleable without reference to such considerations as sequencing; i.e., without reference to that it has occurred in conversation" (ibid.).

One question, then, is whether "there is the *possibility* of . . . a fully comprehensive, coherent linguistics *without* such matters." Another is how such study of single utterances can be "brought into alignment with what we

know about sociology and anthropology. And if not, what then?"[38]

Recurrently throughout these lectures Sacks brings the results of a line of analysis or argument into juxtaposition with the main thrust of contemporary linguistic theory and analysis (i.e., of the early to mid-1960s). One result is the sketching of whole orders of observable regularity and apparent normative organization which have largely, in some cases entirely, escaped the notice of the main thrust of the contemporary study of 'language.' In some respects, this is undoubtedly related to the ambition of modern linguistics (tracable at least to de Saussure) to transcend particular contexts and media of language use – not only social and cultural settings, but also oral and written embodiments – so as to describe an underlying, presumably invariant, linguistic code. The attention to sequential organization – an order of organization seemingly inescapable in the effort to understand and describe actual, naturally occurring talk in interaction – forcefully belies the premise of the currently dominant commitments of linguistics. Running through both the Fall 1967 lectures and the presentations of Spring 1968 are several recurrent themes, whose central upshot is:

How sequential considerations necessarily inform or bear on the construction and understanding of single utterances;
How understanding of some talk is regularly displayed by its recipients; and
What that has required of recipients, and how those requirements are formative of *their* talk in turn.

These themes are returned to persistently, almost compulsively, and they are considerations of a 'foundationalist' sort – that is, they go to the matter of what foundations a discipline of language must be understood to rest on.

Sacks has seemed to some to have abandoned his commitment to contextually-sensitive analysis in turning to the study of sequential structure, and turn-taking in particular. But in insisting on the decisive relevance of sequential organization as furnishing the most proximate reference points of context, Sacks showed the consequences of disattending the fact that language was being used in a medium which was inexorably temporal and interactional. The results of these explorations of sequential context offer, in their own way, as sharp a contrast to formal linguistic analysis as did Sacks' earlier explorations in the 1964–5 lectures offer a contrast with Searle's efforts at context-free speech act theorizing (cf. above, pp. xxiv–xxix).

[38] Later (for example, in a letter to me in 1974) Sacks seems to have taken a different tack, namely, that a systematic discipline might not be buildable on the analysis of single utterances, or single instances of other units or occurrences, but that large amounts of material might be needed. At the time of his death, we had just begun a large-scale investigation of 'next turn repair initiators' which was going to be an exploration of that sort of undertaking. This subsequent development, of course, in no way blunts the impact which Sacks produced by asking what was to be made of the single utterance or the single sequence or the single exemplar of anything to be analyzed, and the detailed findings which this way of working led him to.

Appreciation of the recurrent linguistic orientation in many of these lectures should not be allowed to obscure the range and variety of matters taken up in them, and the diversity of the intellectual resources being called upon from many different traditions of inquiry. One case in point must suffice.

In lecture 6 for Fall 1967 Sacks returns to a point which had come up in earlier sets of lectures, concerning the inclusion in analysis of things which did not happen, here offering as one special relevance of 'next-speaker selection techniques' and 'paired utterances' (the later 'adjacency pairs') that they provide enhanced analytic leverage for speaking of something being absent – e.g., the utterance of an unresponsive selected next speaker, or the absence of a responsive paired utterance. The problem of warranting claims about 'absences' has resonated to many corners of the conversation-analytic domain of issues. Then Sacks adds (p. 670):

> A way, perhaps, to develop a notion of 'absence' involves looking to places where such a notion is used and attempting to see whether there are various sorts of relevance structures that provide that something *should* occur. Parenthetically, I'll give as a rule for reading academic literature, that whenever you see somebody proposing that something didn't happen – and you'll regularly find, e.g., sociologists, anthropologists, or historians particularly, saying that something didn't happen, something hadn't been developed yet – that they're proposing that it's not just an observation, but an observation which has some basis of relevance for it.

Sacks' interest in the matter of 'absences' antedates his work with conversational materials. He had taken a special interest in an observation of Max Weber's that some aspect of ancient Middle Eastern history was to be understood by reference to the fact that (as Sacks would put it in conversation) "that was before the appearance of the horse as an instrument of warfare."[39] The issue this posed was, how could something be the consequence of something which had not happened yet? Clearly some set of relevancies to which the theorist was oriented informed this way of thinking.

And, earlier yet, I recall a conversation at the Law and Society Center in Berkeley in 1962–3 (involving Sacks, a Marxist graduate student in sociology from Argentina and myself) in which the discussion lingered on 'explanations' for the absence of revolutions founded on the Marxist notion of 'false consciousness.' At issue were both the theoretical status of observations

[39] Weber (1952: 6, emphasis supplied):

> Because the nature of military and administrative technology of the time precluded it, before the seventeenth century BC, a lasting political conquest was impossible for either of the great cultural centers. *The horse, for instance, while not completely absent, at least, not in Mesopotamia, had not as yet been converted into an implement of special military technique.*

concerning the non-occurrence of revolution, and the reliance, in the concept of 'false consciousness,' on a stipulated account by the theorist/analyst of what the 'real' interests of the proletariat were, a correct appreciation of which was 'absent' from their (i.e., workers') understanding of the world. What made those 'understandings' relevant, such that not sharing them amounted to their 'absence,' and rendered other beliefs of the working class to be 'false consciousness,' with sufficient explanatory power to account for the absence of revolution?

So when Sacks refers in lecture 6 to a 'rule for reading academic literature,' there is specific background informing the line he is recommending. Having initially engaged this issue in the social science literature, Sacks came to find it illuminated in his engagement with interactional materials. For the underlying 'logic' was, although encountered in the first instance in academic materials, but an aspect of 'commonsense' or 'practical' theorizing which had been incorporated in professional social science theorizing.

Eventually Sacks pursued this matter with a variety of interactional materials. For example, in one of the 1964–5 lectures he remarks on the special intimacy and power of a line reportedly addressed to a beloved in explanation of some past bit of biography, "That was before I met you, and I was lonely then." Here again a 'state-of-the-world' is explained by something that had not yet happened, in a powerful display of retroactive relevance.

So these lectures of Fall 1967, however oriented to exploring their interface with contemporary linguistics, retain their grounding in social (even 'sociological') and cultural analysis. Indeed, it is at the meeting point of these disciplines that the analytic action of these lectures is situated.

XII

This volume presents roughly the first half of those lectures which Sacks chose to tape record and have transcribed. The introduction to this point has attempted to provide some thematic overview of these lectures, and somewhat more detailed background and exploration of a few selected issues.

This effort at an overview has been truly daunting, indeed, beyond my own capacities at the present time. Part of this may surely be traced to my own shortcomings. But, for the most part, it reflects rather the extraordinary richness and multi-facetedness of Sacks' corpus. In its variety, depth, and freshness of vision it defies domestication into convenient guidelines to a reader. At least part of this derives from the methodological character of Sacks' initiative – the new way of working he introduced. Starting out with a commitment to lay bare the methodicity of ordinary activities, and with his talent for seeing in singular occurrences the structural elements of which they were formed and composed, a world of data which refreshed itself every moment more than a legion of Sackses could ever make a dent in provided a virtual infinity of opportunities for new observations, and new orders of observation.

Not that it was easy! Sacks often complained about how hard the work was, and that it did not seem to get easier. He spoke in the early 70s of giving it up and working on something less demanding. The problem was, he observed, the need to see "around the corner," to penetrate through the blinders of the implacable familiarity of the mundane materials with which we worked, and the commonsense models and expectations derived from a social science which had never addressed itself to the simple observational tasks of a naturalistic discipline in which such models ought to have been grounded in the first instance. If we were to try to build a discipline, we needed to be able to be freshly open to what could be going on in any given piece of interaction, and to how activities and conduct could possibly be organized. And it was hard to say which was more difficult – to see clearly what was going on in some bit of material, or to figure out how to build from such observations and analyses a worthy discipline. And, of course, these were not independent orders of task – for how to address the empirical materials was always being informed by the direction in which it appeared a discipline might be pursued, and one surely wanted the character of the discipline to be shaped centrally by one's sense of how social activities were actually organized.

In any case, the main line of engagement for Sacks was in directly taking up particular occurrences, particular bits of tape and transcript. And in leaving as open as he could what there was to be noticed about that bit of occurrence, what there was to be learned from it, what we might get to see the importance of for the first time. And this insistence on freeing each next engagement with data from the past – not only the past of the social sciences, but also past work of this sort, including (especially) his own – while still allowing it somehow to inform analysis is what allowed each new fragment of data, each next look back at an old fragment of data, to provide a possible occasion of discovery. Although the sorts of things which emerged (however rich and multifaceted) were constrained by the particular metier of his mind, their range was truly astounding. They overflow efforts to contain them and package them for overview.

References

Anderson, Alan Ross and Moore, Omar Khayyam, 1960. Autotelic folk models. *Sociological Quarterly,* 1, 4, 203–16.

Atkinson, J. Maxwell and Heritage, John C. (eds), 1984. *Structures of Social Action* (Cambridge: Cambridge University Press).

Austin, John L., 1962. *How to Do Things With Words* (Oxford: The Clarendon Press).

Barker, Roger G. and Wright, Herbert F., 1951. *One Boy's Day* (New York: Harper and Bros.).

Barker, Roger G. and Wright, Herbert F., 1954. *Midwest and its Children* (Evanston, III.: Row Peterson and Co.).

Berlin, Brent and Kay, Paul, 1969. *Basic Color Terms: their universality and evolution* (Berkeley and Los Angeles: University of California Press). Previously circulated as a

Working Paper by the Laboratory on Language Behavior Research, University of California, Berkeley).
Blau, Peter, 1964. *Exchange and Power in Social Life* (New York: John Wiley).
Blumer, Herbert, 1939. *An Appraisal of Thomas and Znaniecki's The Polish Peasant in Europe and America* (New York: Social Science Research Council).
Boulding, Kenneth, 1962. *Conflict and Defense: a general theory* (New York: Harper).
Brown, Penny and Levinson, Stephen C., 1978. Universals in language usage: politeness phenomena. In Esther Goody (ed.), *Questions and Politeness: strategies in social interaction* (Cambridge: Cambridge University Press), 56–310.
Bruner, Jerome S., Jolly, Alison, and Sylva, Kathy, (eds), 1976. *Play: its role in development and evolution* (New York: Basic Books).
Button, Graham and Lee, John R. E. (eds), 1987. *Talk and Social Organization* (Clevedon: Multilingual Matters).
Chomsky, Noam, 1957. *Syntactic Structures* (The Hague: Mouton).
Chomsky, Noam, 1965. *Aspects of the Theory of Syntax* (Cambridge: MIT Press).
Cicourel, Aaron, 1974. *Cognitive Sociology: language and meaning in social interaction* (New York: Free Press/Macmillan).
Cressey, Donald, 1953. *Other People's Money* (Glencoe, III.: The Free Press).
Dollard, John, 1937. *Caste and Class in a Southern Town* (New Haven: Yale University Press).
Drake, St. Clair and Cayton, Horace, 1945. *Black Metropolis* (New York: Harcourt Brace & Co.).
Garfinkel, Harold, 1959. Aspects of the problem of commonsense knowledge of social structure. *Transactions of the Fourth World Congress of Sociology*, 4, 51–65.
Garfinkel, Harold, 1960. The rational properties of scientific and commonsense activities. *Behavioral Science*, 5, 72–83.
Garfinkel, Harold, 1963. A conception of, and experiments with, 'trust' as a condition of stable concerted actions. In O. J. Harvey (ed.), *Motivation and Social Interaction* (New York: Ronald Press), 187–238.
Garfinkel, Harold, 1964. Studies of the routine grounds of everyday activities. *Social Problems,* 11, 225–50.
Garfinkel, Harold, 1967. *Studies in Ethnomethodology* (Englewood Cliffs, NJ: Prentice Hall).
Garfinkel, Harold and Sacks, Harvey, 1972. On formal structures of practical actions. In John C. McKinney and Edward A. Tiryakian (eds), *Theoretical Sociology* (New York: Appleton-Century-Crofts).
Goffman, Erving, 1959. *The Presentation of Self in Everyday Life* (New York: Doubleday Anchor).
Goffman, Erving, 1961. *Encounters: two studies in the sociology of interaction* (Indianapolis: Bobbs-Merrill).
Goffman, Erving, 1963. *Behavior in Public Places* (New York: The Free Press).
Goffman, Erving, 1964. The neglected situation. In John J. Gumperz and Dell Hymes (eds), *The Ethnography of Communication,* special issue of the *American Anthropologist,* 66, 6, part 2, 133–6.
Goffman, Erving, 1969. *Strategic Interaction* (Philadelphia: University of Pennsylvania Press).
Goffman, Erving, 1971. *Relations in Public* (New York: Basic Books).
Goffman, Erving, 1981. *Forms of Talk* (Philadelphia: University of Pennsylvania Press).
Halliday, Michael A. K., and Hasan, Ruqaia, 1976. *Cohesion in Discourse* (London: Longman).
Havelock, Eric A., 1963. *Preface to Plato* (Cambridge, Mass.: Harvard University Press).
Hill, Richard J, and Crittenden, Kathleen Stones, (eds), 1968. *Proceedings of the Purdue Symposium on Ethnomethodology,* Institute Monograph Series No. 1 (Institute for the

Study of Social Change, Department of Sociology, Purdue University).
Hughes, Everett C., 1971. Dilemmas and contradictions of status. In *The Sociological Eye: selected papers* (Chicago and New York: Aldine and Atherton), 141–50. Reprinted from *The American Journal of Sociology,* 50 (1945).
Jefferson, Gail, 1983. On embedded and exposed correction in conversation. In *Studium Linguistik,* 14, 56–68. Reprinted in Graham Button and John R. E. Lee (eds), *Talk and Social Organization* (Clevedon: Multilingual Matters), 86–100.
Jefferson, Gail, 1989. Editor's notes. *Human Studies,* 12, 395–404.
Kaufmann, Felix, 1944. *The Methodology of the Social Sciences* (New York: Humanities Press).
Kneale, William and Kneale, Martha, 1962. *The Development of Logic* (Oxford: The Clarendon Press).
Labov, William and Waletsky, Joshua, 1966. Narrative analysis: oral versions of personal experience. In J. Helm (ed.), *Essays on the Verbal and Visual Arts* (Seattle: University of Washington Press), 12–44.
Lakoff, Robin, 1973. The logic of politeness: or minding your p's and q's. *Proceedings of the Ninth Regional Meeting of the Chicago Linguistic Society,* 292–305.
Levinson, Stephen C., 1983. *Pragmatics* (Cambridge: Cambridge University Press).
Opie, Iona and Opie, Peter, 1959. *The Lore and Language of Schoolchildren* (Oxford: The Clarendon Press).
Parry, Milman, 1971. *The Making of Homeric Verse: the collected papers of Milman Parry,* edited by Adam Parry. (Oxford: The Clarendon Press).
Parsons, Talcott, 1937. *The Structure of Social Action* (Glencoe, Illinois: The Free Press).
Pomerantz, Anita, 1980. Telling my side: 'limited access' as a 'fishing' device. *Sociological Inquiry,* 50, 3–4, 186–98.
Psathas, George (ed.), 1979. Everyday Language: studies in ethnomethodology (New York: Irvington Press).
Sacks, Harvey, 1963. 'Sociological description.' *Berkeley Journal of Sociology,* 8, 1–16.
Sacks, Harvey, 1972a. An initial investigation of the usability of conversational data for doing sociology. In David N. Sudnow (ed.), *Studies in Social Interaction* (New York: The Free Press), 31–74.
Sacks, Harvey, 1972b. On the analyzability of stories by children. In John J. Gumperz and Dell Hymes (eds), *Directions in Sociolinguistics* (New York: Holt, Rinehart and Winston), 325–45.
Sacks, Harvey, 1972c. Notes on police assessment of moral character. In David N. Sudnow (ed.), *Studies in Social Interaction* (New York: The Free Press), 280–93.
Sacks, Harvey, 1973. On some puns, with some intimations. In Roger W. Shuy (ed.), *Report of the 23rd Annual Roundtable Meeting on Linguistics and Language Studies* (Washington, DC: Georgetown University Press), 135–44.
Sacks, Harvey, 1974. An analysis of the course of a joke's telling in conversation. In Richard Bauman and Joel Sherzer (eds), *Explorations in the Ethnography of Speaking* (Cambridge: Cambridge University Press), 337–53.
Sacks, Harvey, 1975. Everyone has to lie. In Ben Blount and Mary Sanches (eds), *Sociocultural Dimensions of Language Use* (New York: Academic Press), 57–80.
Sacks, Harvey, 1978. Some technical considerations of a dirty joke. In Jim Schenkein (ed.), *Studies in the Organization of Conversational Interaction* (New York: Academic Press), 249–70.
Sacks, Harvey, 1985. The inference-making machine: notes on observability. In van Dijk, 1985, 13–22.
Sacks, Harvey, 1986. Some considerations of a story told in ordinary conversation. *Poetics,* 15, 127–38.
Sacks, Harvey, 1987 [1973]. On the preferences for agreement and contiguity in sequences in conversation. In Button and Lee, 1987, 54–69.

Sacks, Harvey, 1987. "You want to find out if anybody really does care" In Button and Lee, 1987, 219–25.
Sacks, Harvey, 1988/89. On members' measurement systems. *Research on Language and Social Interaction,* 22, 45–60.
Sacks, Harvey, Schegloff, Emanuel A., and Jefferson, Gail, 1974. A simplest systematics for the organization of turn-taking for conversation. *Language* 50, 696–735.
Schegloff, Emanuel A., 1963. Toward a reading of psychiatric theory. *Berkeley Journal of Sociology,* 8, 61–91.
Schegloff, Emanuel A., 1967. *The First Five Seconds: the order of conversational openings.* PhD Dissertation, Department of Sociology, University of California, Berkeley.
Schegloff, Emanuel A., 1984 [1976]. On Some questions and ambiguities in conversation. In Atkinson and Heritage, 1984, 28–52, [first appeared in *Pragmatics Microfiche*, 1976, 2.2, D8-G1.]
Schegloff, Emanuel A., 1988. Goffman and the analysis of conversation. In Paul Drew and Tony Wooton (eds), *Erving Goffman: perspectives on the interaction order* (Cambridge: Polity Press).
Schelling, Thomas, 1960. *The Strategy of Conflict* (Cambridge, Mass.: Harvard University Press).
Schenkein, Jim (ed.), 1978. *Studies in the Organization of Conversational Interaction* (New York: Academic Press).
Searle, John, 1965. What is a speech act? In Max Black (ed.), *Philosophy in America* (Ithaca: Cornell University Press), 221–39.
Searle, John, 1969. *Speech Acts* (Cambridge: Cambridge University Press).
Searle, John, 1976. The classification of illocutionary acts. *Language in Society,* 5, 1–24.
Sudnow, David N., 1967. *Passing On* (Englewood Cliffs, NJ.: Prentice Hall).
Sudnow, David N., 1972a. Temporal parameters of interpersonal observation. In Sudnow, 1972b, 259–79.
Sudnow, David N. (ed.), 1972b. *Studies in Social Interaction* (New York: Free Press).
Thibaut, John and Kelley, Harold, 1959. *The Social Psychology of Groups* (New York: John Wiley).
van Dijk, Teun (ed.), 1985. *Handbook of Discourse Analysis, volume 3: Discourse and Dialogue* (London: Academic Press).
von Neumann, John and Morgenstern, Oskar, 1944. *Theory of Games and Economic Behavior* (Princeton: Princeton University Press).
Weber, Max, 1952. *Ancient Judaism*, translated and edited by Hans H. Gerth and Don Martindale (Glencoe, III.: The Free Press).
Zimmerman, D. and West, C. (eds), 1980. *Sociological Inquiry,* 50, 3–4 [special issue on language and social interaction].