On Sacks on Weber on Ancient Judaism

Introductory Notes and Interpretive Resources[1]

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THE ARTICLE 'MAX WEBER'S *Ancient Judaism*' was written by Harvey Sacks during the 1962–3 academic year, while he was a graduate student in the Department of Sociology at the University of California, Berkeley, and associated with the campus's Center for the Study of Law and Society. It was produced in the same time period as the article 'Sociological Description' (Sacks, 1963), with which it shares many preoccupations and commitments. In some respects the articles compose a pair; in other respects, the 'Weber article' turned out to be a step on the way to 'Sociological Description', perhaps even to have prompted it. In fact, the Weber article (as I will refer to it) is one of several articles written in the same 12–15-month period, the third of which was 'Notes on Police Assessment of Moral Character' (the so-called 'police article'), written about the same time, though not published until 1972 (Sacks, 1972). What occasioned the writing of the Weber article was quite mundane and academic in the most literal sense.

At that time, graduate students in sociology at Berkeley were required in their first year of graduate studies to take two semester-long theory courses – ordinarily one in 'classical' theory (Sociology 217) in the Fall term and one in 'contemporary' theory (Sociology 218) in the Spring.[2] Although he had been at Berkeley since the Fall term of 1960, as the 1962–3 academic year was getting under way, Sacks had not yet satisfactorily met the '217' requirement, and was being held to it before being

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allowed to proceed with fulfilling further degree requirements. As I recall, the Instructor of record for the year in which Sacks had taken the course without completing it was William Petersen (best known for his work in demography). 'Max Weber's *Ancient Judaism*' was written to satisfy that requirement; its reception can only be imagined.[3]

It is worth bearing in mind the broader intellectual/academic context in which this ostensibly narrow course 'exercise' was undertaken,[4] as well as the more immediate working context at the Center for the Study of Law and Society within which the production of the article took place.

First of all, of course, the body of work, disciplinary commitment and the company of workers associated with the rubric 'conversation analysis' did not then exist. 'Ethnomethodology' did, but only as something of an exotic whisper from California. Not that it was all that well known in California! Garfinkel's *Studies in Ethnomethodology* was not to be published until 1967.[5] A few things of his had been published by 1962 (Garfinkel, 1959, 1960), and several others were to appear in print that year and the next in various outlets, but they were not widely known.[6] For the most part, the work circulated (both in Berkeley and elsewhere) in mimeographed form under the title *Some Sociological Methods for Making Everyday Activities Observable*, and the work was known from that circulation.[7]

Some sense of the relationship of Sacks's undertaking in the Weber article to the 'ethnomethodology' of the time, may be conveyed by two exhibits, each composed of two parts – one from Sacks's article, the other from Garfinkel.

On the one hand, Sacks:

> We may epitomize Weber's transformational technique by noting that his crucial postulate is – *whatever the Old Testament says is nothing other than how the Old Testament says what it must be talking about*. And, what the Old Testament must be talking about is nothing other than the aspects in terms of which sociologist members of Western societies write and recognize as correctly written, accounts of their own societies. (this volume: 38; emphasis added)

On the other hand, Garfinkel (1967: 28–9):

> If these notions [composing a correspondence theory of signs] are dropped, then *what the parties talked about could not be distinguished from how the parties were speaking*.... Then the recognized sense of what a person said consists only and entirely in recognizing the method of his speaking, of seeing how he spoke. (1967: 28–9; emphasis added)

Then again, on the one hand, Sacks:

> In considering whether any 'proposed description' should stand as adequate, a criterion is available. The society under consideration might be observed. *A comparison of what is being observed with what the description proposes might*

> *be made. Having determined whether the features reported in the proposed description are actually the features that may be observed, a decision as to adequacy might be formulated.* (this volume: 33; emphasis added)

On the other hand, Garfinkel:

> Although it may at first appear strange to do so, suppose we drop the assumption that in order to describe a usage as a feature of a community of understandings we must at the outset know what the substantive common understandings consist of. With it, *drop the assumption's accompanying theory of signs, according to which a 'sign' and 'referent' are respectively properties of something said and something talked about, and which in this fashion proposes sign and referent to be related as corresponding contents*. By dropping such a theory of signs we drop as well, thereby, the possibility that an invoked shared agreement on substantive matters explains a usage. (1967: 28; emphasis added; this is directly followed by the preceding quotation from Garfinkel)

If the first pair of citations convey a convergence of views, the second pair conveys a divergence. What there was most seriously was engagement. Perhaps this can best be conveyed by the acknowledgement footnote to Sacks's article 'Sociological Description' (1963: 1), whose title makes clear the continuity with the themes of the Weber article. As these articles were written the same year, perhaps we would find a similar acknowledgement to Garfinkel in its footnotes, if we had them. It would almost certainly in that case also include the final sentence.

> Almost all of the point [sic] of the following paper have been developed in preparation for, during, or as a consequence of the numerous meetings I have had in the last several years with Professor Harold Garfinkel of U.C.L.A.
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> Professor Garfinkel has not only been, through these meetings and through his (largely unpublished) writings, the stimulus for these thoughts but he has also on occasion provided me with funds for pursuing this work. My debts to him are barely noted by the references in the body of the paper. It might be added that he is far from agreeing with all that I have to say. (Sacks, 1963: 1)

In Berkeley, Garfinkel was not widely known at that time. In the Department of Sociology, Erving Goffman's work was the closest resonance. But at the Center for the Study of Law and Society Garfinkel was better known. Its Director, Philip Selznick, had come to Berkeley from UCLA's Sociology Department; its Associate Director, Sheldon Messinger, had been a graduate student at UCLA's Sociology Department. Both of them had been in that sense associated with Garfinkel and knew the character and commitments of his undertaking. But it was Sacks who was in the closest continuing contact with him, and was the most directly engaged with his

work. (On the circumstances of Sacks's encounter with Garfinkel and his work, see Schegloff, 1992a: xii–xiv).

The most sympathetic ear on the Sociology faculty was Goffman's, and even his openness was limited – both by his own reservations and differences where Garfinkel's work was concerned, and by an inclination of others who knew little of the work of either to lump them together (an inclination which persists to a degree even now). But those graduate students who were attracted by Garfinkel's work were also drawn to Goffman.[8] It is to the point, then, to recall where Goffman's *oeuvre* stood in 1962. He had published *The Presentation of Self in Everyday Life* (1959) and several of the articles (1955, 1956, 1957) later reprinted in *Interaction Ritual* (1967), *Asylums* (1961a) and *Encounters* (1961b). And his teaching in that year and the preceding one focused on material later to be published as *Stigma* (1963a) and on what he was calling 'performances', including in some measure material which was to appear in *Behavior in Public Places* (1963b).[9] Other work such as *Relations in Public* (1971) was still quite far off, not to mention *Frame Analysis* (1974) and *Forms of Talk* (1981) which represented later major reorientations.

During the spring semester of 1962, Sacks, David Sudnow and I all attended Philip Selznick's course in Sociological Theory, and over the course of the term got to know one another – first Sacks and I, then Sudnow and I, and near the end of the term I introduced the two of them. By the end of the term, Selznick had invited the three of us to affiliate as 'Post-Graduate Research Sociologists' with the new Center for the Study of Law and Society which he was launching. During the course of the year we were to become a sort of 'three musketeers' – a close grouping, deeply engaged with one another.

In effect, Selznick gave us our heads; we were invited to join the Center and develop interesting projects subsumable under its mandate, with relatively few constraints, and those largely informal. Sacks had already been working with him at the Institute for Industrial Relations on collective bargaining, and Selznick very likely anticipated further work along those lines. I had spent some time working with Selznick on the place of the study of 'morality' in social science, and in fact went on in the course of that year to develop a project on 'responsibility', asking how societies determined whether their members were to be held responsible for their own conduct by studying the treatment of defendants who plead insanity as a defense in criminal felony proceedings.[10] Sudnow had had less prior exposure to Selznick, but eventually launched a project examining the working of 'the defense' in criminal cases provided by the Office of the Public Defender, a project which is issued in the now-classic paper, 'Normal Crimes' (Sudnow, 1965).

Although Sacks's Weber article was not written as a contribution to the Center's program, it was informed by the work, the concerns and the discussions which were the preoccupations of this little group housed at the Center, preoccupations which perforce became part of the Center's

informal intellectual agenda. A few words may be in order, then, about some of the activities in whose midst the Weber article was written. I can mention here only a few of the concerns which preoccupied us during that year, and only a few of the activities which grew out of those preoccupations. For the most part, I mention such concerns as the reader may find resonances of in 'Max Weber's *Ancient Judaism*'.

But first it may be useful to sketch very broadly the biographical and academic trajectories which had brought us together at that time, because they give some indication of the local context into which Garfinkel's work was being introduced.[11]

The search for what sort of work to do was not prompted by Garfinkel's work alone, nor even in the first instance.

Sudnow had moved from being a serious, outstanding 'pre-professional' undergraduate at the University of Alabama to pursue graduate work in sociology at Indiana University, but left there after the MA (in what was most likely experimentally oriented social psychology) in search of ... what? Goffman? The excitement of the Bay Area? Something less well defined? Whichever, he was in search.

I had left Harvard after pursuing reading courses in the sociology of knowledge with Parsons and then writing an honors thesis in that area under the supervision of Barrington Moore, Jr (itself a sign of a critical stance toward the social science enterprise) to go to Berkeley (on Moore's advice), only to try out, successively, a range of sociological commitments from survey research with Hanan Selvin, to political sociology and stratification studies with Marty Lipset, to the sociology of culture with Leo Lowenthal (and an MA thesis in that area with Lipset and Reinhard Bendix), to law and morality with Selznick, to deviance studies with Goffman – among other, briefer searches for work which would be defensible, honest and getting at fundamentals.

Sacks had gone from undergraduate studies at Columbia College seeking out the best thinking he could find there from various students of Franz Neumann to Lionel Trilling to Meyer Schapiro to C. Wright Mills; going on to law school at Yale and participating in the circle around Harold Lasswell. But he then went in search of the social foundations by which the work of the law was made possible, first (on Lasswell's advice) in Cambridge in Parsons's seminar (where he met Garfinkel) and then to Berkeley, where the search continued with Selznick, but largely under the influence of Garfinkel's writing (Schegloff, 1992a: xii–xvi).

So although the search in which the three of us engaged together was colored that year more by Garfinkel's writing than by any other single external source (except Goffman, under whose supervision we all undertook our dissertation work), the seed of that influence fell on fields already well prepared and cultivated by doubt concerning method, materials and theoretical stance, and by the search for what would be satisfying work.

One of the preoccupations which involved us during that year was trying to think through the implications of Garfinkel's work, as we variously

understood it, and of other work – such as that of the later Wittgenstein – which seemed to us to challenge the viability of the forms of sociology then in the ascendancy, in which we had been trained. Much of this discussion was theoretical in character, and concerned problems nowadays familiar in retrospect but then just being thought through, at least by graduate students; problems such as the inadequacy of common conceptions of 'norms' or 'rules' as possible 'determiners' of conduct, and therefore as possible 'explanations' of it. And, of course, many other 'theoretical' problems as well. (More on this later, in particular trying to figure out what 'members' methods' or 'practices' might be.)

Another domain of issues concerned what kind of analytic work was defensible, on what kind of materials it could be seriously prosecuted, and how its seriousness could be grounded. One specific expression of this issue centered on the relationship between the material being examined and the ostensible subject matter being written about – 'answers to questions' on the one hand and 'attitudes' or 'sequences of jobs held' on the other; 'organizational records' on the one hand and 'bureaucratic control' or 'rationality' on the other, etc.

And within the scope of this issue were other, embedded ones. Given the concern about the gap between what was examined and what was written about, ethnographic fieldwork invited consideration as a method of choice. But there were issues about both observation and about elicitation as components of fieldwork. If interviewing members was to be part of fieldwork, how could one proceed to ensure that 'informants' (as they were still called then) would take one seriously, and answer seriously, so that the responses could be taken seriously. And if observation was a key part of fieldwork, how might it be done to cope with the issues of common-sense knowledge, reasoning and practical theorizing whose unexplicated immanence in one's method was problematic given sociology's topic.

Our activities were closely related to these (and other, related) concerns and preoccupations. David Sudnow was the most traditional ethnographer among us and (as eventually became widely apparent) an outstanding one as well. And ethnography was his method in his work project at the Center, focused on the Office of the Public Defender in Alameda County. But that work aimed to put the ethnography in the service of the ethnomethodological program and ended up examining (in one way of reading it) the operation of commonsense knowledge and practical theorizing in doing the work of the public defender's office.

A second project growing out of our preoccupations at the time, one which went through considerable development but never came to fruition, was to have been a study of judges and their management of courtrooms in session. The earlier-mentioned concern about 'informants' taking the ethnographer seriously was prompted (as the reader may have inferred) by 'high-end' informants like judges, whose patience might be short, and who might well doubt that the (graduate student) investigator knew enough about the domain (the law, courtrooms and their legally prescribed procedures,

etc.) to understand a serious response, were one to be given. Sacks and Sudnow were the ones at the Center most involved in developing this project. They explored ways of conveying to a judge the exact nature of what they were interested in by offering exemplars of it drawn from other, non-judicial domains, thereby displaying that they understood things about what happened in courtrooms, and in particular how a courtroom's happenings might appear to a judge, and that they, therefore, should be taken seriously.

The idea was to provide the judge an anecdote or vignette of some practice or phenomenon taken from some quite different setting or activity, a practice or phenomenon which was the cognate of something that was thought to occur in the courtroom. For example, Sacks had been learning a little bit about chess (partially motivated by some work of Garfinkel's concerning the chessboard configuration known as 'zugzwang', partially prompted by our friendship), and read a bit about it as well. In his reading he came across an account of the great Cuban Grandmaster Jose Capablanca, about whom it was asserted that he had the greatest 'integrity' of any chess player. By that the author meant that, for Capablanca, no move was made as an individual, atomistic one; every move was a move in a 'projected series', in a strategy. Playing a player with 'integrity', one could always try to reconstruct what strategy a move was a part of, and then use that reconstructed strategy as a resource for interpreting the import of the particular move.

Sacks's idea was that this was how good lawyers operated; each move they made in the courtroom (each objection, each motion and, in particular, each question to a witness) was engendered by a larger strategy, and the judge would employ the premise of 'integrity' to infer and project the strategy from particular 'moves', and use the reconstructed strategy to grasp the import of any particular move (for example, a question which had been objected to by opposing counsel) in deciding how to deal with it. Methodologically, then, the idea was to compose questions to the judge which would recount such a vignette or anecdote as the one about Capablanca's integrity, and ask whether this had any recognizable bearing on the judge's own work in the courtroom. Sacks and Sudnow hoped thereby to prompt a flow of description by the judge-informant, prompted by the judge's recognition that the interviewers must know something about the courtroom if they had selected such a prompt, and yet not overstep their entitlement by seeming to claim legal expertise. For a variety of reasons, this project never came to practical application in actual fieldwork. (The reader of Sacks's 'Weber' article will find echoes of this work in section VI at the end of the article, in remarks about the integrity of chess players.)

A third engagement with these methodological concerns was one which I pursued. For an inquiry into how determinations were made that a member of the society was responsible (or not) for her/his own conduct, I planned to track the production and resolution of pleas of insanity as a defense to felony charges. On the psychiatric side, the plan was to be in attendance at the psychiatric interviews conducted with such defendants,

tape record the interviews, collect the psychiatrists' 'informal' notes made during the interview, the official written report to the court, and the psychiatrist's testimony on the witness stand in court (if such testimony was taken) so as to develop an account of the transformation of some exchanges of utterances in the interview through successive formulations, ending with an assessment of the mental state of the defendant which would become a material part of the judgment whether or not the defendant was to be considered responsible for his/her own conduct.

In keeping with the 'problematics' to which we were in general oriented, this was an effort to figure out what kinds of things 'members' methods' were, how to find them in some material and to describe them, and so forth. In effect, I was trying to describe the methods or procedures of talking (i.e. of interrogation) implemented by the psychiatrists and the methods for examining the defendants' talk to make the relevant determination. What I was tracking was the psychiatrists' attending to the defendants' talk by reference to features obliquely related to its transparent topical content, but attentive instead to the manner of speaking – an aspect of speaking taken to be (and this is itself an aspect of the method) not strategically manipulable by the interviewee. It was not what the defendants answered to the questions put to them but *how* they answered, or rather how they talked while doing the answering, that the psychiatrists focused on, and claimed as the grounds for their accounts of their findings. They took it that the questions gave the defendants something overt to respond to, something with which they would be engaged, and that they (the psychiatrists) could then examine the conduct for 'inadvertent' evidence of 'mental states' and other features mandated by the law in making recommendations to the court. I called this the 'method of natural production', but the inquiry came to naught because of obstacles to the data collection.[12]

While waiting for defendants in felony cases to plead 'not guilty by reason of insanity' (which, as it happened, they did not do at all in Alameda County during the first eight months of the academic year), I attended the rounds of the psychiatric ward in the County general hospital conducted by the same psychiatrists participating in civil commitment proceedings, tape recording these very brief interviews in the hope of pursuing a similar project – tracking the same 'members' method' – as a preliminary study. This did not work out either, and all that emerged from that project was the article on psychiatric theorizing (Schegloff, 1963).

The point throughout (the search for 'members' methods' aside), however, was that to do this work one would need to get a re-examinable record of just what was said as some leverage on the otherwise problematic determination of what got noticed in observational studies based on one-time exposure in real time. As far as I know, this was the first undertaking, at least within so-called 'ethnomethodology/conversation analysis',[13] to record episodes of naturally-occurring interaction, not to code them into categories of acts or events, but to analyze them in detail for the practice of ongoing activity which they embodied.

While engaged in trying to figure out *how* to work, and what stance to take up toward elements of our prior training and exposure to sociology which appeared problematic in the light of new ideas with which we were grappling, we were in search of what would be the target and the product of the new direction of work. One of the things we were trying to figure out about Garfinkel and ethnomethodology was what 'members' methods' were, what one looked like or sounded like, how to describe one, etc. And our writing during that period displays this preoccupation among others. For example, Sacks's police article has as its centerpiece (or as one of them) a so-called 'incongruity procedure' by which the police ostensibly spot suspicious characters. My 'psychiatric theory' article described a set of methods constitutive of a 'dialogic relationship' of the patient with the world, with the psychiatrist, etc., and repeatedly proposes to describe 'a procedure for X' or a 'method for X' (e.g. 'for locating those of the patient's behaviors which are seen as peculiarly apt for psychiatric scrutiny', 1963: 79). In Sacks's Weber article this shows up as Weber's use of 'an interrogation procedure'. And Sudnow's 'normal crimes' resonates with Garfinkel's preoccupation with methods for constituting 'perceivedly normal environments of action'.[14]

I have meant with the preceding pages to make available at least a bit of the flavor of the times, the setting and the occasion by reference to which Sacks's article on 'Max Weber's *Ancient Judaism*' might be read. Now a bit on the article itself and its relationship to its kindred articles. (Some readers may wish to read Sacks's article first, before continuing.)

Critical Resources

Examining Sacks's article a reader encounters a number of what may well be taken as puzzles or anomalies. For example:

1. Sacks begins by formulating Weber's project: 'His concern there might be called, the production of a sociological description of Ancient Israel'. 'Might be called?' Sacks's point is that, given the inaccessibility of the society to observation, the project is more aptly described as 'sociological reconstruction' than 'sociological description'. This theme will surface again later in the article, and figures centrally in Sacks's somewhat later article 'Sociological Description'. But he has already set Weber's project in a comparative frame that would have been a bit of a surprise to most sociologists at the time the article was written (let alone when Weber's book was written), and perhaps still is.

But wait. Sacks goes on to claim that 'Weber produced his reconstruction in order to account for certain important features of Judaism ... in particular, its Pariah ethic.' Why then not formulate Weber's project in the first instance as providing an 'account for certain important features of Judaism ... in particular, its Pariah ethic' (this volume: 31)? The 'reconstruction' is, after all, only in the service of that project. Indeed, the first problem to which Sacks turns (in the third paragraph of the article) he formulates *not* as Weber's method for transforming the OT (Old Testament)

into a reconstruction of Ancient Israel, but as Weber's method for transforming the OT 'into that sociological reconstruction of Ancient Israel which accounts for the adduced features [the Pariah ethic – EAS]'. Not then any old reconstruction, but one aimed at accounting for the Pariah ethic.

But by the fourth paragraph this specification has disappeared; now what is being described is Weber's 'method for transforming materials from the OT so as to produce a reconstruction of the features of Ancient Israel'. And by the seventh paragraph, 'Weber's is a method for making transformations from documentary materials.' Accounting for the Pariah ethic has been left behind, even though it was to account for it that Weber was consulting the OT, and the reconstruction of Ancient Israel he was after was that one which would account for the Pariah ethic. In this first section, then, Sacks has moved step-by-step from the problem which *Weber* was addressing to the one *he* wants to address.[15]

2. In setting up his problem (this volume: 33–4) – what is the nature of Weber's reconstruction? – Sacks returns to the contrast between description and reconstruction. In deciding the adequacy of a description, he says, 'a criterion is available', and it turns on observation. It is the unavailability of observation which makes of Weber's work, by contrast, 'reconstruction'.

But with respect to reconstruction, Sacks proposes, 'it is not clear, *a priori*, when any "proposed reconstruction" should stand as adequate'. The problem is so severe, Sacks writes, that 'some might say that AJ [*Ancient Judaism* – EAS] must be treated as no more than a collection of thoughtful remarks ...' (this volume: 33). Indeed, the problem is worse; without a criterion by which to assess reconstruction, 'we don't even know what sort of work that is', and Weber's AJ is to be examined to see just 'what sort of work reconstruction is'.

Yet later on, Sacks seems prepared to articulate 'what the criterion for adequate reconstruction seems to be. The criterion generally employed for assessing reconstructions, and employed as well by Weber, is "recognizability"' (this volume: 37). Leaving aside what is made of this, one wonders how the argument could have gotten to this point without the problematicity of a criterion for reconstruction, a problematicity which seems in retrospect not to have been warranted in the first place.

Already, then, the construction of the argument is not without its problems, its anomalies, its puzzles. Still, something telling is going on. To juxtapose 'reconstruction' with 'description' based on whether the object is accessible to observation or not sets rather more than AJ into the reconstruction column! A considerable range of social research (both classical and contemporary, and in between) invites treatment as 'reconstruction', is properly understood as making transformations from documentary materials, challenges us to describe the character of those transformations in order to understand in what the analysis consists, and confronts the question of adequacy: if observation cannot be used to assess the relationship between description and target, and if 'recognizability' is the invoked criterion, what constitutes 'recognizability'? That is, what – in the description, in the

describer, in the recipient of the description, in the object of the description – makes for satisfying the criterion of recognizability? If some of these issues have a contemporary ring, recall that this was written in 1962–3.

3. As an article about Weber and *Ancient Judaism*, Sacks's article appears on the face of it to be remarkably unspecific and undetailed. It rarely cites specific texts from Weber's book, and then only in an exemplary fashion to illustrate a point, rather than as an object of close, detailed analysis. It turns out that, in many respects, Sacks's article is only incidentally about Weber's *Ancient Judaism*; it is rather about sociology, its mandate and its then current (and perhaps now current) character, executed on a text of one of its founding fathers.[16]

4. But the article is written overtly as 'about Weber', so how is *that* done? As noted, it is unspecific in its textual address. What it focuses on is the *method* of Weber's analysis. But this has little or nothing to do with what Weber says about his method, and this with a figure known today for his contributions to sociological methods (or methods for sociological theorizing) – whether in his writings in the *Methodology of the Social Sciences*, his several accounts of the method of ideal types, the theorizing regarding the relationship between values and objectivity in sociology, etc., or the methodological remarks in *Ancient Judaism*. Sacks does not take Weber's word for what he is doing; he seeks out his own characterization of what Weber is doing and how he is doing it.[17]

In doing so, Sacks introduces a fresh conception of what 'a method for theorizing' might be. Sacks's 'interrogation procedure' is quite unlike other treatments of 'methods' (whether for theorizing or anything else), yet it is meant to capture how in fact Weber comes to propose what is proposed in *Ancient Judaism* about Ancient Israel. One of the article's sorest lacks is a more generous explication of the operation of the interrogation procedure in Weber's text, i.e. how particular analyses in Weber's text are arrived at via this interrogation procedure – for example, the one offered by Sacks in section I to epitomize Weber's 'method for transforming materials from the Old Testament so as to produce a reconstruction of the features of Ancient Israel' (this volume: 31). This we never get; nor do we get an answer of any specificity to what Sacks characterizes at the end of that first section as 'the problem I shall address': 'What set of instructions would be required in order to produce a document similar to AJ from these materials?' No such instructions are ever specified.

5. Section IV of the article confronts one directly with some similar gaps one would have liked to see filled. Sacks explicates his assertion that 'Weber employs an interrogation procedure' by saying that 'he addresses questions to the Old Testament, and treats what he finds in the Old Testament as "answers to his questions" ' (this volume: 35). But he never offers a characterization of the questions which Weber addresses to the Old Testament or where they come from. Curiously, he never brings forward a particular actual instance from Weber's text which exemplifies Weber addressing a question to the Old Testament. But Sacks has himself

suggested (in section II) that 'In considering whether any "proposed description" should stand as adequate, a criterion is available. The society under consideration [or presumably any other object of proposed description – EAS] might be observed. A comparison of what is observed with what the description proposes might be made' (this volume: 33).

But surely this applies to Sacks's undertaking as well, yet such a comparison is not made here between Sacks's proposed description of Weber's procedure and anything in Weber's text. Rather, Sacks takes up a number of putative objections to his claim that Weber's method is an interrogation procedure, objections that would insist on an actual interview for interrogation to be claimed, would insist on co-presence, on actual sequencing of question and answer, on form of question and on demonstrated seriousness of response. Having disposed of those self-induced objections, but without having made any affirmative showing of the claim that Weber uses an interrogation procedure, Sacks nonetheless uses this stipulation as secured: 'Once we see that Weber uses an interrogation procedure ... (this volume: 35).' But have we seen it? Maybe it was in the footnotes.

This is not to deny the interest of what follows, but it threatens to render much of it equivocal. However relevant the following text may be to 'interrogation procedures', it has not been shown to be relevant to Weber's *Ancient Judaism*, and we are not directed elsewhere to assess its relevance and its correctness. Nonetheless, the rest of Sacks's article rests on the claim that Weber uses an interrogation procedure, and we must accept that in order to proceed. If the article is only incidentally about Weber's *Ancient Judaism*, then perhaps we proceed by assessing the article against the larger domain which Weber 'stands proxy for' here.[18]

6. Section III of the Weber article is devastatingly ironic. In eschewing criticism of Weber's work by reference to ordinary constraints of 'scientific logic', Sacks embodies the position that Weber's text cannot meet them. So Sacks exempts Weber on the grounds that Weber's activity is not one to which those rules apply. The inquiry is accordingly concerned precisely with the questions, what *is* the activity in which Weber is engaged? What *are* its methods? What rules *do* properly underlie and constrain these methods? Surely Sacks was aware that this rejection of critique could not but be taken as itself the most severe of intended critiques.

There are other anomalies and puzzles in the text as well. It should be recalled that this was a graduate student paper, for a required course, some 35 years ago; that it is written in an idiom quite strange to sociology, invoking considerations equally strange at the time. On the other hand, Sacks was no beginning graduate student fresh out of college. He had a law degree from Yale Law School and a year of free-wheeling study in Cambridge around Harvard and MIT. It is most likely that filling in what is missing in the article (to take up only those puzzles) was too substantial an undertaking for the practical purpose at hand, and Sacks had already extracted from the exercise a satisfying result, whose further pursuit would

be better served in ways other than documenting Weber's use of an interrogation procedure.

What that satisfying result might have been may best be understood by setting the Weber article in the context of two other articles Sacks wrote during the same year and a half and in the context of some of the previously mentioned work in progress at the Center. The other relevant articles of Sacks with which the Weber article suggests a trajectory are the 'police article' preceding, and the 'Sociological Description' article following. Relating the Weber article to these others is difficult here for lack of the other articles – both of which are virtually inaccessible, although published. I will offer brief characterizations of the articles and suggest the linkages by juxtaposing passages which embody the continuities and sketch their developmental trajectory. A fuller account must be reserved to another occasion – or the reader's own further inquiry.

A Developing Line of Thought?

'Max Weber's *Ancient Judaism*' is usefully understood in the context of Sacks's 'police article' which precedes it and 'Sociological Description' which follows it. Here I can give only brief accounts of these two articles and several juxtapositions of text which are indicative of the thematic continuities which link the articles, and the developmental trajectory which can be traced through them. A fuller development of this theme would require access to the full text of all three articles and space to trace the several threads of development, neither of which is available here.

Briefly, then. Sacks's article 'Notes on Police Assessment of Moral Character' undertakes to give an analytic description of a putative procedure used by police[19] to recognize 'suspicious persons' in 'public places'.[20] For Sacks, this is simply one locus of a more general issue[21] – the linkage between the appearances of persons, inferences (by others) as to moral character engendered by those appearances and treatments of those persons warranted by the inferences about their moral character grounded in their appearance.[22] The police are taken to be specialists of a sort in this matter, in particular in 'inferring from appearances such a probability of criminality as warrants the treatment of search and arrest' (1972: 281).

Sacks's point is this. The default setting for conduct in public places is that persons 'naively present and naively employ presented appearances as the grounds of treatment of persons they encounter in public places', for example, as to whether they pose a threat or not. The police are trained to avoid taking appearances at face value (i.e. 'naively'), to entertain the possibility that appearances are improper (i.e. evidence of suspect moral character), using as evidence, for example, the 'ease with which an appearance is presented' (i.e. its naiveté or lack thereof), and basing determinations of how to treat the person (whether to investigate, follow, search, arrest, etc. them or not) on this alternative way of inferring moral character from appearances. Sacks goes on to describe an 'incongruity procedure' by which police can be brought to see in 'normal appearances' the improper/illegal

activities which are 'in fact' going on, by building up an understanding of the 'normal appearance' of a location on the beat in its range of variation (e.g. by social time of day, etc.), against which background they are able to detect – via their incongruity with these normal appearances – the appearances of those persons and those activities which warrant further professional police scrutiny.

Sacks elaborates considerable further detail, analysis and sociologically compelling interpretation about this 'incongruity procedure' and its application which it is impossible (and unnecessary) to render in an adequately nuanced way here. But one key line of the analysis rests on the following components: (1) there are *source materials* on which the method/procedure operates – in the case of the police, they are appearances of persons in public places; (2) there is a *procedure or method* for addressing or 'processing' that material – here, the 'incongruity procedure'; (3) there is an *outcome* of the application of this procedure to the source materials – here, inferences as to moral character; (4) there is a *consequence of the outcome* of the application of the procedure – here, a move to more formally investigate (stop, search, arrest, etc.) the presenter of the appearances; (5) there are *criteria* applicable to each of these links – the propriety or adequacy of the use of the procedure, the correctness of the inference, the warrantedness of the consequence engendered by the inference. One theme of Sacks's account may be conveyed by the following excerpts from the article; they set a point of comparison with the Weber article to follow:

> Instead of the proper use of the [incongruity] procedure being decided by reference to the correctness of the inference of probable criminality, the propriety of the inference constitutes the condition for determining whether the persons selected are possibly criminal. And whether the inference was proper is decided in the courts by having the policeman state what it was that aroused his suspicions; the judge (or jury) then considers whether an ordinary person would have been roused to suspicion on such grounds. Only if so is the person selected by the policeman convictable. (1972: section 3.c, 284)

> We have noted above (3.c) that it is not the case that the proper use of the method is determined by the demonstrable correctness of the inferences produced.
>
> The general warrant of the method is not based on the professional status of the police; its general warrant is that anyone can see its plausibility. Its warrant in particular cases is that the inference made is one which ordinary persons would make. This means that the policeman is not simply concerned to develop his sensitivity. He must balance his sensitivity against his ability to verbalize, i.e., to present descriptions of how he became aroused. And what is more, though he is a specialist on the normal appearances of his beat, his inferences are judged by those who lack both his special knowledge and his developed sense of the unusual.
>
> While the police would like their special skills in observation to constitute grounds of a recognition of their professional status, and their

> professional status to then operate as a preliminary warrant of their observations, the fact that the warrant of their observations is decided by a test of reasonableness for an ordinary man is not only irking but also places them in a severe bind. (1972: section 4.g, 288)

What does this have to do with 'Max Weber's *Ancient Judaism*'?

Well, Sacks casts Weber and his enterprise in the same mold as the police and theirs. Consider: Weber starts with source materials as well – the text of the Old Testament. He also seeks and achieves an outcome from this source material – a reconstruction of Ancient Israel. He does so with the use of a method – an 'interrogation procedure'.[23] The use of this method and an assessment of its outcome are subject (as with the police) to a constraint – recognizability. In brief then:

	Source	*Outcome*	*Method*	*Criterion*
Police	appearances	suspicion	incongruity	reasonableness
Weber	document	reconstruction	interrogation	recognizability

To the extracts from the police article above, juxtapose the following from the Weber article:

> My analysis will first be devoted to the following problem: what method did Weber employ in transforming what may be found in the Old Testament into that sociological reconstruction of Ancient Israel which accounts for the adduced features [the Pariah ethic]? (this volume: 31)

Then, following a sample text from the Old Testament and its reconstruction:

> I am proposing then, that Weber's is a method for making transformations from documentary materials. The problem I shall address is, *how* does Weber produce his transformations? Imagine that one had the Old Testament and its critical exegeses on the one hand, and blank paper on the other. What set of instructions would be required in order to produce a document similar to *Ancient Judaism* from these materials? (this volume: 33)

The cognate text for the police article would be something like this: 'I am proposing then that the police employ a method for making inferences from ordinary appearances. The problem I shall address is, *how* do the police produce these inferences ... etc.' And this is indeed quite directly the project of the police article.

Both articles then are addressed to the problem of the methods used to do practical theorizing: to bring to bear formulatable procedures on a set of observables so as to arrive at assertions grounded in them which are not otherwise accessible. And Sacks is concerned with the ways in which the deployment of these methods is constrained and warranted, and how the outcomes of their application are assessed.

Although formulated as part of his descriptive task, Sacks's proposal about the police procedure – that it is not that the adequacy of procedure is determined by the adequacy of the outcome but that adequacy of procedure *determines* adequacy of outcome; and furthermore what determines the adequacy of procedure is its mundane plausibility or reasonableness 'for an ordinary man' – may also suggest something which is possibly problematic. Of course, in the case of the police, the problematicity may concern justice, civil liberties, the problems of false accusation and unjustified police intrusion, etc.

But much the same issue comes up in the examination of Weber's *Ancient Judaism*. Asking what constraint there is on Weber's reconstruction of Ancient Israel from the Old Testament, Sacks writes that 'the criterion generally employed for assessing reconstructions, and employed by Weber as well, is "recognizability" '. Weber, then, is accountable to his readers for the 'recognizability' of his reconstructions.

> Since it is by no means apparent that members of modern Western societies will be able to decide verisimilitude for actions which took place within Ancient Near Eastern societies, Weber's problem in providing for the competence of his readers is far from trivial. He could easily have written a book which provided an account that his readers simply would not feel able to judge.
>
> Weber proceeds to assure the competence of his readers through the use of the following techniques.
>
> 1. He doesn't include within his report the materials that he is analyzing. If one is to understand his analysis, a familiarity with the Old Testament and the 'higher criticism' are prerequisite. . . .
> 2. The book is further restricted to *sociologists* familiar with the above-mentioned materials. Weber adopts a method of analysis which involves employing a sociological idiom as a familiarization technique. The scenes of life in Ancient Israel as they are portrayed in the biblical idiom . . . might cause the reader to feel without a grasp of how the social world was then constituted. Weber transforms reported conversations between fig trees and bramble bushes so that they may be read as ways of talking about class conflict, international politics and the like. He thereby permits the sociologist reader to see Ancient Israelite society as 'just the same old story' of social life as we know it. (this volume: 37–8)

The problem of recognizability, then, is solved by restricting the competent audience to those for whom what Weber will describe, and the terms in which he will describe it, are by professional preparation and commitment, recognizable matters. This is the equivalent for this professional audience to the criterion of reasonableness to the ordinary man in the context of police activity. But the implicit question (or perhaps not so implicit) is what the status is of analytic products of such procedures, constrained by such criteria. If the question for police procedure is justice and civil society, the

issue in Weber's case is analytic adequacy and the status of a corpus of knowledge.

My point in juxtaposing the treatment of the police and of Weber is to make manifest the treatment of Weber as a practical theorist, as object of inquiry rather than as scholarly precedent, colleague and resource, as engaged with common-sense knowledge and inference, even if – with the police – in a specialized corner of the mundane world of everyday life. As will be seen, this theme is extended – both later in the Weber article and in the 'Sociological Description' article – to pair Durkheim with Weber in this regard, although by a different route, and to capture the whole issue in the metaphor of the 'commentator machine'. And, it turns out, it extends – more by implication than in explicit statement – past that article into conversation-analytic work itself.

Before turning to the further development of these themes in the article 'Sociological Description', another juxtaposition of extracts from the police and Weber articles may serve to display another thematic thread which Sacks elects to focus on, one which also continues through 'Sociological Description'. This theme concerns the 'seriousness' of objects of inquiry which may otherwise be taken as ephemeral and not orderly, their seriousness being a prerequisite to their serious investigability.

First, then, the ordinary appearances of people on streets as a matter to which the police are oriented.

> 5.a While the police might treat the streets as merely incidental locales of the persons they encounter, in fact they treat the streets with great seriousness. The police take it that what takes place in the streets stands in a determinable relation to that organization of concerted courses of action which involves people in using the streets. If they discover whom to investigate, then by tracking him they can at least determine the strategic problem that exposing the course to which he is oriented poses. (1972: 290)

And then the seriousness of materials to which 'inquirers' address themselves.

> For inquirers the determination that the subject is serious in producing his activity (answer) means that the activity is analyzable. We may explicate this central point in the following way.
>
> Chess players talk of the response to a move as 'an answer'. In order for a move to be answered, as compared with merely being followed by the opponent's move, it is necessary for the opponent to feel confident in assuming that the move to which he is to respond was produced by a strategy. The opponent must feel confident that the move was produced as part of a course of action which can be located by analyzing the move. The opponent doesn't seek to answer the move, but the strategy by which the move is assumed to have been generated. Indeed, when chess players talk of the integrity of a player they mean that he will not make a move that is not justifiable as part of a strategy. Such a player has integrity because whatever

> he does can be responded to on the basis of an analysis of what he is trying to do by that and related future moves.
>
> It seems to be the case that *inquirers take it that if they can treat responses or documents as answers, they will then be able to produce the reconstruction of a course of action as their analysis of the material under consideration*. In reading *AJ* we see that Weber constantly transforms the phrases, parables, sagas, etc. of the Old Testament into reconstructed courses of action.
>
> If an analyst knows that a set of materials has been produced by an interrogation procedure, then he takes it that however obscure it may appear at first glance, the orderly character of the social world within which it was produced can be assumed, and the analyst can take recovering that order as his problem.
>
> If alternatively one knows that an analysis one is reading is an analysis of materials collected by interrogation, then one can know that the courses of action therein described are reconstructions produced by transformations performed on whatsoever diverse and strangely appearing materials the analyst may have started with. (this volume: 38–9; emphasis in original)

Sacks follows with Durkheim's *Suicide* as a case in point, proposing that Durkheim makes the suicide statistics with which he begins into raw materials for sociological analysis by treating suicide as 'the most serious of answers one can make to the problems social life poses'. But consider that virtually all social science answers to this description if it deals with data other than direct observation of what is then written about in the analysis, and formulates its objects by reference to the categories of vernacular knowledge and practical theorizing.

Although in the Weber article Sacks explicitly disavows critical intent, this turns out to be (of course) a bit ironic. This becomes explicit in 'Sociological Description', which returns to Durkheim's *Suicide* and to Weber, now in an explicitly critical vein (in the second footnote Sacks proposes that 'In terms of the history of sociology, nothing is more tragic than that Durkheim's *Suicide* should be conceived as a model investigation').

I will limit myself to registering two points of thematic continuity between 'Max Weber's *Ancient Judaism*' and 'Sociological Description' in conveying a sense of the continuing enterprise in which each marks a step.

First, recall from the Weber article the treatment of criteria for the adequacy of analytic operations.

> Before considering the techniques he employs to assure the competence of his readers, we must note what the criterion for adequate reconstruction seems to be. The criterion generally employed for assessing reconstructions, and employed as well by Weber, is 'recognizability'. (this volume: 37)

This is in contrast to 'description':

> In considering whether any 'proposed description' should stand as adequate, a criterion is available. The society under consideration might be observed. A comparison of what is observed with what the description proposes might be made. Having determined whether the features reported in the proposed description are actually the features that may be observed, a decision as to adequacy may be formulated. (this volume: 33)

This stance is silent on what criterion is used to determine 'whether the features reported in the proposed description are actually the features that may be observed'. It seems to be taken as transparent.

Now a very similar hypothetical investigative scenario is sketched in 'Sociological Description'. Sacks formulates what he takes to be 'one of the favorite sociological hypotheses' (1963: 2) 'about the relation of the language persons use to other parts of their behavior' (1963: 2). It is this:

> Persons in everyday life have reasonably accurate theories of social life. The language they employ expresses their theories and constitutes a description of activities. By employing the language to describe a segment of activities which they see they are able to predict a further segment of those activities. The hypothesis concludes with an explanation; by way of the predictions persons are able to adapt to each other's behaviors. (1963: 2)

Sacks contrasts a conventional sociological procedure for inquiry guided by this 'hypothesis' with 'what is actually required first to pose and then to test the hypothesis' (1963: 3).

> Consider its first part: the language persons employ constitutes a description of other behavior. . . .
>
> Our first problem is to describe a segment of their language and a segment of their other behavior.
>
> Secondly, we need a criterion to decide whether some segment of language constitutes a description of some segment of behavior. Suppose we adopt as a first criterion, recognition.

I mean to call attention to several points about the relation between the two articles in this regard:

- first, the recurrence of the project of juxtaposing some candidate description with what it is purportedly a description of;
- second, the proposal of 'recognition' or 'recognizability' as the criterion of adequacy;
- third, the observation that, whereas in the Weber paper 'recognizability' is taken to be the criterion of adequacy for *reconstructions* in the absence of the possibility of direct observation, in 'Sociological Description' 'recognition' is taken to be the criterion of adequacy of *description* in juxtaposing description with its target 'describable'.

Clearly a same theoretical and methodological project is being addressed; clearly there is movement in it.

Second, the core of Sacks's 'Sociological Description' is embodied in its 'representative metaphor':

> At industrial and scientific exhibitions one encounters a machine which the layman might describe in the following terms. It has two parts; one part is engaged in doing some job, and the other part synchronically narrates aloud what the first part does. We shall call this the 'commonsense' perspective on the machine. For the commonsense perspective the machine might be called a 'commentator machine', its parts 'the doing' and 'the saying' parts. (1963: 4)

Sacks goes on to describe other takes on the machine. In the common-sense perspective the encounterer of the machine understands the doing part from the saying part, which is already understood by virtue of the language being known. 'The stranger's' version of the common-sense perspective is represented by the foreign engineer, who does not know the language but to whom what the machine is doing is transparent; this encounterer might be said to understand the saying part from the doing part, which is already understood. A third, 'more-radical', perspective is brought by an encounterer 'who knows both what it is doing and the language' (1963: 5) and can address 'the possible problematic relation of the parts' (1963: 5). This encounterer – whom Sacks characterizes as engaged in 'practical theory' – may use the 'doing' part to ground a critique of the 'saying' part as inadequate description, or may use the 'saying' part to ground critiques of the 'doing' part as defective realization or performance (or 'deviance' – once we explicate this perspective in the metaphor as representing the tack taken by conventional sociology). The final stance introduced by Sacks is that of the 'naive scientist' – 'an E[ncounterer] of the machine who knows neither the language nor what it is doing'. This stance foregrounds, by contrast, the presupposition in the other perspectives that the two parts stand programmatically in the relationship of 'doing' and 'describing the doing'. The 'naive scientist' requires first a descriptive account of the 'doing' part and of the 'saying' part, including a justification of treating them as 'parts', and as 'doing' and 'saying' or 'describing' or 'describing the doing part' respectively.

With this sketchy account as background, consider the resonance between Sacks's characterization of Weber's method as an 'interrogation procedure'[24] and the following observation on the 'machine' (1963: 9):

> 5. Problems in understanding can be resolved ... by posing more or less sophisticated interrogatories, i.e., questions having the feature that between the interrogator and the subject there is a common language such that answers to questions constitute answers-to-questions.[25] The double feature of the common language may be noted; not only is it the case that the interrogator knows the language emitted by the object (so as to be able to address it), but the object knows the language of the interrogator. His answers

> are not just answers to the questions asked, but they are answers to the questions the interrogator has about the object.[26] That is, the answers themselves, or a version of them, can be reported as the interrogator's description.[27] In the metaphor as written the machine did not seem to have an attachment permitting interchanges, but that was for the sake of simplicity. A version of the machine might have been: It is silent until asked a question ... When asked a question, apart from replying with a request for clarification, it emits a narrative description of what it is then doing. A program of questions might then be devised the answers to which constitute the descriptions the version actually encountered emitted freely.

Here, then, the role of the Old Testament in the Weber article, which is to supply answers to the questions which Weber puts to it, answers which constitute sociological descriptions of Ancient Israel, is cast as the 'saying' part of the machine, which responds when queried with a description of what the 'doing' part is then doing.

Upshot and Conclusion

Although there have been recent claims that what used to be called 'ethnomethodology' is not actually ethnomethodology but a precursor to it (Lynch, 1993), the work in these three articles by Sacks is 'ethnomethodology' in the classical sense of the study of commonsense knowledge of social structure and practical theorizing. Whether incongruity procedures or interrogation procedures, whether answerable to criteria of plausibility or of recognizability, these are 'ethno-methods' being addressed. The focal concern to which Sacks returns over and over again is the relationship between some material (appearances, documentary materials) and some candidate analysis of it (description, reconstruction), the procedure by which the second is produced from the first and the criteria by which the relationship of the two can be assessed and the propriety and applicability of the procedure described.

By the time of 'Sociological Description', Sacks characterized the place he had reached this way (1963: 7):

> The problems of this paper ... are: Given a variety of senses of the notion 'description' or given a variety of criteria for deciding adequacy of proposed descriptions, (a) what criteria does sociology currently use, and (b) what criteria ought it use given the postulate that for it social life constitutes a subject matter?
>
> The essential 'message' of this paper is: even if it can be said that persons produce descriptions of the social world, the task of sociology is not to clarify these, or to 'get them on the record,' or to criticize them, but to describe them. That persons describe social life (if they can be conceived as doing so) is a happening of the subject quite as any other happening of any other subject in the sense that it poses the job of sociology, and in contrast with it providing a solution to sociology's problem of describing the activities of its subject matter.

And this preoccupation, this line of thinking, would resurface later on in quite a different context. In introducing his discussion of a story told by a child ('The baby cried. The mommy picked it up') in his lectures for spring 1966, Sacks addressed himself to 'describing as an activity', with resonances to some of the themes sketched above (including the relationship between a description and that which is being described) which may not otherwise have been apparent. Having offered a series of observations about this two-sentence story, Sacks proposes:

> With this fifth observation it may now be noticed that what we've essentially been saying so far is that the pair of sentences seem to satisfy what a Member might require of some pair of sentences for them to be recognizable as 'a possible description.' They 'sound like a description' and some form of words can, apparently, sound like a description. To recognize that some form of words is a possible description does not require that one must first inspect the circumstances it may be characterizing.
>
> That 'possible descriptions' are recognizable as such is quite an important fact, for Members, and for sociologists.
>
> The reader ought to be able to think out some of its import for Members; for example, the economies it affords them. It is the latter clause, 'and for sociologists,' that I wish to now attend.
>
> Were it not so both that Members have an activity they do, 'describing,' and that at least some cases of that activity produced, for them, forms of words recognizable as at least possible descriptions without having to do an inspection of the circumstances they might characterize, then it might well be that sociology would necessarily be the last of the sciences to be doable. For, unless sociologists could study such things as these 'recognizable descriptions,' we might only be able to investigate such activities of Members as in one or another way turned on 'their knowledge of the world,' when sociologists could employ some established, presumptively correct scientific characterization of the phenomena Members were presumably dealing with, knowing about.
>
> If, however, Members have a phenomenon, 'possible descriptions,' which are recognizable per se, then the sociologist need not in the first instance know how it is that babies and mommies do behave to examine the composition of such possible descriptions as Members produce and recognize. Sociology and anthropology need not await developments in botany or genetics or analyses of the light spectra to gain a secure position from which Members' knowledge, and the activities for which it is relevant, might be investigated.
>
> What the sociologist ought to seek to build is an apparatus which will provide for how it is that any activities, which Members do in such a way as to be recognizable as such to Members, are done, and done recognizably. Such an apparatus may be called 'a culture.' (Sacks, 1992, I: 245)

Although the immediate occasion for these remarks was quite different from what had occasioned the work in the three articles I have been discussing, and would almost certainly not have been reached by persisting

in the modality of work which was embodied in the earlier articles, it is clear enough that these issues had been visited before, and provide fertile ground for the products of Sacks's later engagement with conversational (and related) material.[28]

I have wandered from the proximate object of this introduction – Sacks's paper on 'Max Weber's *Ancient Judaism*'. But if I am correct in taking this article to use Weber's work as a point of departure for a thinking through of what sociology has been and what it could be, then the larger trajectory of that thinking through is indispensable. And if that premise is correct, then Sacks's result – a grounded conjecture about a possible sociology and a problematic about the past sociology – remains as relevant today as it was in the early 1960s. As does its pithy summing up in the last sentence cited above:

> What the sociologist ought to seek to build is an apparatus which will provide for how it is that any activities, which Members do in such a way as to be recognizable as such to Members, are done, and done recognizably. Such an apparatus may be called 'a culture.' (1992, I: 245)

Appendix: Garfinkel's Book – 1962

It may be useful to register here the table of contents of the informally circulated collection of Harold Garfinkel's papers *Some Sociological Methods for Making Everyday Activities Observable* for the sense it conveys of Garfinkel's preoccupations around that time. The collection as a whole is dated 'July, 1962'. I include as well the dates inscribed on each of the papers or chapters, if dates are given. I should note that some chapters are not included in my copy of the 'volume', in spite of being listed in the Contents. Whether these 'missing' manuscripts actually existed but were not included in my set, or whether the listing of them in the Contents represented planned rather than accomplished text, I do not know. Here then is the table of contents; titles in brackets are the titles which actually appear on the chapters, where these differ from the titles given in the Contents. Asterisks mark chapter titles for which no manuscript is included in my copy of the volume. Note especially Chapters 10, 15 and 16 – as titles if not as texts – for their bearing on Sacks's preoccupations in the Weber article, in the police article, and in 'Sociological Description'.

Notes

1. Many years having passed since I last had occasion to read Harvey Sacks's 'Max Weber's *Ancient Judaism*,' I was (I hope understandably) skeptical that this student

article should be published in *Theory, Culture & Society*. But re-reading the article has convinced me that there were themes here worth re-presentation, over and above their display of the very special mind of the writer. Asked to provide an introduction and discussion, I have done so (it might be thought) with a vengeance. I have proceeded more or less as I did with Sacks's *Lectures on Conversation* (1992), although with a single article the task turned out to be quite different. Some may find the detail – whether on setting or on scholarly trajectory – more than they want to know. But if this article is to be 'set in context', this is the only way I know to do it.

2. I specially mark the terms 'classical' and 'contemporary' because their referents were not yet fixed by 'tradition' as they are now understood, and varied with the instructor. For example, in the year in which I took the former course it was taught by Reinhard Bendix and the anthropologist Lloyd Fallers (at the time the courses were offered jointly with the Department of Anthropology), and the syllabus included such authors as Fustel de Coulanges and Jakob Burckhardt, among others who would not nowadays be counted part of the 'classical' canon.

3. Actually, if I remember correctly, it was *not* accepted as satisfying the course requirement, and Sacks wrote another article on the 'discovery of a gap between the "is" and the "ought" ' with which he finally completed the course.

4. See also the accounts offered in my separate introductions to the two volumes of Sacks's *Lectures on Conversation* (1992) and in my introductory note to the early paper on 'The Lawyer's Work' (Sacks, 1997).

5. And then its publication was brought about in part by virtue of the editorial work of David Sudnow, who figures in the narrower context to be described below.

6. By the time of the Purdue Symposium on Ethnomethodology (Hill and Crittenden, 1968) this had changed to the point that the Preface to the publication of the Proceedings of the Symposium could begin (Hill, 1968), 'A body of work which has been labeled "ethnomethodology" is the focus of considerable controversy within contemporary sociology.' By that time Garfinkel's *Studies* had, of course, been published. But in providing the background to the Symposium, Richard Hill cites a presentation by Garfinkel at the 1965 meetings of the American Sociological Association and its ensuing discussion, and a paper presented by Lindsay Churchill at the 1966 meetings of the American Sociological Association with discussion by Melvin DeFleur and Aaron Cicourel as sources of the increasing visibility of ethnomethodology in a broad professional context. The period under discussion here is three years before the first of these events.

7. In the Appendix, I reproduce the Table of Contents of my copy of that collection to provide some indication of Garfinkel's preoccupations at that time, and what he was prepared to put on display as ethnomethodology's work.

8. The opposite was not the case; most of Goffman's students had only a limited interest (at best) in ethnomethodology.

9. Sacks's article 'Notes on Police Assessment of Moral Character' (1972) was first written for the latter course, in the same time frame as both the Weber article and 'Sociological Description'.

10. The only product of this project, which was eventually abandoned when access to the data became problematic, was the article 'Toward a Reading of Psychiatric Theory', published in the same issue of the *Berkeley Journal of Sociology* as Sacks's 'Sociological Description'.

11. I cannot here describe the broader social, political and intellectual context. Recall, however, that Berkeley and the Bay Area were the scene of the Beat movement starting in the late 1950s, the anti-capital punishment movement centered on Caryl Chesman in 1959–60, the House Un-American Activities Committee protest demonstrations ('riots') in the early 1960s and, in the year following the 1962–3 academic year, the start of the Free Speech movement in Berkeley, not to mention the involvement of Berkeley students in the civil rights movement and in the early protests against the Vietnam War. Berkeley was then the site of considerable restiveness in the life of the mind and in politics.

12. Elements of the theme I was pursuing may be found in Scott (1963), in the same issue of the journal as contains Sacks's 'Sociological Description' and my 'Toward a Reading of Psychiatric Theory'. Scott was an occasional participant in a group of graduate students who met irregularly at Sacks's apartment, including (in addition to Sacks, Sudnow and myself) Roy Turner, Marvin Scott and Henry Elliot, as well as others I don't recall.

13. It was not until later that we became aware of work done under the aegis of Roger Barker which involved the collection of such data, specifically Soskin (1963) and the discussion in Soskin and John (1963). And, of course, various of Garfinkel's studies (such as the alternative counseling methods based on randomly selected yes/no answers [Garfinkel, 1967: 79–94] were based on recordings of the participants' talk, but these were 'experimentally' arranged interactions.

14. The term 'normal crimes' itself appears to have been taken from Sacks's police article (1972: section 4.f, 288).

15. Accounting for the Pariah ethic reappears at p. 37 (this volume). And, a propos 'a method for making transformations from documentary materials', Sacks's so-called 'police article', of the same year is his own exercise in 'a method for making transformations from documentary materials'.

16. But we would do well to exercise caution here, and not 'over-incidentalize' the role of Weber's work in the article; in trying to catch the biggest fish we can, we can lose sight of the actual topic at hand, which is *Ancient Judaism*.

17. In this regard, by reference to the metaphor of the commentator machine in 'Sociological Description', Sacks declines to take the 'saying' part as authoritative on the 'doing' part, i.e. he declines to take Weber's statements about what he is doing (his 'methods') as authoritative accounts of those methods, of what he is doing. Nor does he take Weber's practice as a resource for the clarification of Weber's principled statements about method. Rather he develops his own account of what Weber's project amounts to – reconstruction of a society from its documentary traces (in contrast to a description of it), and his own account of the method by which that project is prosecuted. In describing Weber's practice in *Ancient Judaism* as a case study in a kind of practical theorizing, Sacks is engaged in an 'empirical' undertaking of the sort which Parsons claimed (1937: 3) for the undertaking in *The Structure of Social Action*. That was cast as an empirical study in the development of social theory in the direction of a voluntaristic theory of action; Sacks in effect treats Weber as an empirical case study in a method of practical theorizing.

18. Or a more ironic suggestion may be offered. In the next section of the article, Sacks describes how Weber provides for the competence of his readers to assess the claims of his book. 'Weber proceeds to assure the competence of his readers

through the use of the following techniques: 1. He doesn't include within his report the materials that he is analysing. If one is to understand his analysis, a familiarity with the Old Testament and the "higher criticism" are prerequisite' (this volume: 37). Can Sacks be employing the same technique himself? In order to understand *his* analysis a familiarity with AJ is prerequisite. And readers who lack it, and are unwilling to return to the book, make judgments about the analysis at their peril! This is, of course, rather a cheeky stance for a graduate student to take in a course paper, and a risky one – as the rejection of the paper showed. But perhaps this enhances, rather than detracting from, the attractiveness of this account of what Sacks might have been doing here!

19. Sacks did not study the methods actually used by the police. In keeping with a common practice of Goffman's work, he relied on manuals of proper practice, anecdotal accounts, etc. There is a development in the three articles of Sacks being examined here which interacts with the work which I was doing at that time. In the police article, Sacks examined prescriptive and anecdotal writing but couched his account as what 'the police' do. In my article 'Toward a Reading of Psychiatric Theory', I insisted that such prescriptive accounts do *not* describe actual practice: 'In looking at this writing, we seek to discover not how psychiatry is done, but rather how it is written about and what proper accounts of it look like. We assume we will find described in the literature ideal psychiatric procedure and theory, i.e. procedure and theory as it is publicly avowed it ought to be' (1963: 62 *passim*). Sacks's Weber article then took the practice of the text itself as its object of inquiry. 'Sociological Description' is entirely 'theoretical' and does not pose this issue.

20. Throughout even this abbreviated and impoverished summary, the reader should detect the echoes of various themes central to Goffman's work around this time, especially in *Behavior in Public Places* (1963b) and *Stigma* (1963a), the latter of which turns specifically on the accessibility of the stigma to observation in Goffman's distinction between the 'discreditable' and the 'discredited' (1963a: 41ff.). As noted earlier, Sacks's police paper was written for a course of Goffman's, and was originally titled 'Methods in Use for the Production of a Social Order: A Method for Warrantably Inferring Moral Character'.

21. 'Since I am only interested in the police instantially ..' (1972: 282).

22. It is, of course, the theme of this Introduction, that this larger undertaking is itself 'instantial' – of an exploration of what 'common-sense' methods might be and how they might be described, and, even more generally, of the possible relationship between common-sense or vernacular inquiry on the one hand, and professional, disciplined, even 'scientific', inquiry into the social, on the other.

23. 'Weber employs an interrogation procedure in producing his reconstruction. That is, he addresses questions to the Old Testament, and treats what he finds in the Old Testament as "answers to his questions"' (this volume: 35).

24. 'Weber employs an interrogation procedure in producing his reconstruction. That is, he addresses questions to the Old Testament, and treats what he finds in the Old Testament as "answers to his questions"' (this volume: 35).

25. Compare Weber article: 'That is, he [Weber] address[es] questions to the Old Testament, and treats what he finds in the Old Testament as "answers to his questions"' (this volume: 35).

26. Echoes here of 'the move in the strategy' in the Weber article, and 'the action on the street as part of a course of action' in the police article.

27. Sacks refers to this as placing scientist and subject 'in a catechistical relation'.

28. Note again the thematic continuity from the treatment of police procedure (the adequacy of procedure – given by reasonableness for an ordinary man – determines the adequacy of outcome, pp. 15–16) to the treatment of ordinary interaction: 'For viewers, the usability of the viewer's maxims serves to warrant the correctness of their observations. And that is then to say: The usability of the viewer's maxims provides for the recognizability of the correctness of the observations done via those maxims. And that is then to say: "Correct observations" or, at least, "possible correction observations", are "recognizable"' (1992, I: 260).

References

Garfinkel, H. (1959) 'Aspects of the Problem of Commonsense Knowledge of Social Structures', *Transactions of the Fourth World Congress of Sociology* 4: 51–65.

Garfinkel, H. (1960) 'The Rational Properties of Scientific and Commonsense Activities', *Behavioral Science* 5(1): 72–83.

Garfinkel, H. (1967) *Studies in Ethnomethodology*. Englewood Cliffs, NJ: Prentice-Hall.

Goffman, E. (1955) 'On Face-Work: An Analysis of Ritual Elements in Social Interaction', *Psychiatry* 18(3): 213–31.

Goffman, E. (1956) 'Embarrassment and Social Organization', *American Journal of Sociology* 62: 264–74.

Goffman, E. (1957) 'Alienation from Interaction', *Human Relations* 10: 47–59.

Goffman, E. (1959) *The Presentation of Self in Everyday Life.* Garden City, NY: Doubleday.

Goffman, E. (1961a) *Asylums: Essays on the Social Situation of Mental Patients and Other Inmates*. Garden City, NY: Anchor Books, Doubleday.

Goffman, E. (1961b) *Encounters: Two Studies in the Sociology of Interaction*. Indianapolis: Bobbs-Merrill.

Goffman, E. (1963a) *Stigma: Notes on the Management of Spoiled Identity*. New York: Simon & Schuster.

Goffman, E. (1963b) *Behavior in Public Places: Notes on the Social Organization of Gathering*. New York: Free Press.

Goffman, E. (1967) *Interaction Ritual: Essays in Face to Face Behavior*. Garden City, NY: Doubleday.

Goffman, E. (1971) *Relations in Public: Microstudies of the Public Order*. New York: Harper & Row.

Goffman, E. (1974) *Frame Analysis: An Essay on the Organization of Experience*. New York: Harper & Row.

Goffman, E. (1981) *Forms of Talk*. Philadelphia: University of Pennsylvania Press.

Hill, R.J. (1968) 'Preface', in *Proceedings of the Purdue Symposium on Ethnomethodology*. Lafayette, IN: Institute for the Study of Social Change, Purdue University.

Hill, R.J. and Crittenden, K.S. (eds) (1968) *Proceedings of the Purdue Symposium on Ethnomethodology*. Lafayette, IN: Institute for the Study of Social Change, Purdue University.

Lynch, M. (1993) *Scientific Practice and Ordinary Action: Ethnomethodology and Social Studies of Science*. Cambridge: Cambridge University Press.

Parsons, T. (1937) *The Structure of Social Action*. Glencoe, IL: The Free Press.

Sacks, H. (1963) 'Sociological Description', *Berkeley Journal of Sociology* 8: 1–16.

Sacks, H. (1972) 'Notes on Police Assessment of Moral Character', pp. 280–93 in D.N. Sudnow (ed.) *Studies in Social Interaction*. New York: Free Press. Reprinted pp. 45–65 in William P. Sanders (ed.) *The Sociologist as Detective: An Introduction to Research Methods*. New York: Praeger Publishers, 1974 (only this edition; it is omitted in later editions of this volume).

Sacks, H. (1992) *Lectures on Conversation*, 2 vols, edited by G. Jefferson, with Introductions by Emanuel A. Schegloff. Oxford: Blackwell.

Sacks, H. (1997) 'The Lawyer's Work', in *Law in Action: Ethnomethodological and Conversation Analytic Approaches to Law*, edited by M. Travers and J. Manzo. Aldershot: Dartmouth.

Schegloff, E.A. (1963) 'Toward a Reading of Psychiatric Theory', *Berkeley Journal of Sociology* 8: 61–91.

Schegloff, E.A. (1992a) 'Introduction, Volume 1', pp. ix–lxii in *Harvey Sacks: Lectures on Conversation*, edited by G. Jefferson. Oxford: Blackwell.

Schegloff, E.A. (1992b) 'Introduction, Volume 2', pp. ix–lii in *Harvey Sacks: Lectures on Conversation*, edited by G. Jefferson. Oxford: Blackwell.

Scott, M.B. (1963) 'A Note on the Place of Truth', *Berkeley Journal of Sociology* 8: 33–8.

Soskin, W.F. (1963) *Verbal Interaction in a Young Married Couple*. Lawrence: University of Kansas, primary records in psychology, publication no. 3.

Soskin, W.F. and John, V.P. (1963) 'The Study of Spontaneous Talk', in *The Study of Spontaneous Talk*, edited by R.G. Barker. New York: Appleton-Century-Crofts.

Sudnow, D.N. (1965) 'Normal Crimes: Sociological Features of the Penal Code in a Public Defender Office', *Social Problems* 12(3): 255–76.

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