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Secondary ethnographic analysis: Thinking about things

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Abstract

There is a fruitful tension in ethnomethodological work. On the one hand, real-world data are used to rein in analytical privilege. On the other, conceptual discussions necessarily take place in a more open analytical space. Describing settings in detail and thinking about things in the abstract are both essential components of the ethnomethodological project. What ethnographies might consist in complicates this picture. Garfinkel initially deflated the concept of 'ethnography', using it to refer to how all members of society make sense of their world. Sacks, on the other hand, initially construed his sociological project as a more rigorous form of professional ethnography. Ethnographic methods rightly remain an important tool for ethnomethodological analyses. They provide an empirical grounding for analysis and facilitate 'thinking about things' in a more open manner than some other forms of data. This paper argues that ethnographic analyses more generally can be used as ethnomethodological resources, (re)introducing the idea that others' fieldwork and analyses are legitimate resources for ethnomethodological work. Some materials from Elijah Anderson's classic ethnography *A Place on the Corner* are used to illustrate the possibilities taking this approach might offer.

Keywords

Ethnomethodology, fieldwork, ethnography, secondary analysis, methodology

Introduction

In the mid-1990s, I was doing my PhD on how a multidisciplinary clinical team make decisions about elderly patients with complex health, personal and domestic

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circumstances. I hoped to relate this back to work Harold Garfinkel and his colleagues undertook in the early 1960s, in particular that done under the rubric 'Studies of Decision Making in Common Sense Situations of Choice' (awarded by the Air Force Office of Scientific Research to Harvey Sacks, Lindsey Churchill and Garfinkel). Overoptimistically, I contacted the surviving grant holders to ask if there were any unpublished materials specific to this set of projects I might be able to read. Only one responded, with a charming letter (sent by Air Mail) explaining that no final report was completed – to the best of his knowledge – and that the money was mainly used for 'thinking about things'.[1]

This paper is about thinking about things. It is informed by my participation in a long-standing weekly seminar, in which collective reading of papers acts as a jumping-off point for reflection, criticism and dispute. This contrasts strongly with data sessions, in which, properly, empirical materials act as a constraint on analytical privilege.[2] These two types of discussion do not conflict with one another. Insisting that data constrain analytical privilege does not mean that one's imagination should be stunted, just as using others' ideas as a starting point for discussion does not mean that 'anything goes' in how those ideas are approached. The two support each other, and – over the course of our discussions – 'imagination' and 'constraint' are brought to bear on one another to facilitate ways of working that maintain *both* openness to conceptual possibility *and* faithfulness to the empirical details of settings, activities, relevances and so on.

This paper argues that data are often treated as constraints on what can be said rather than spurs to the analytical imagination, and that – while not all such materials are capable of being used for our analytical purposes – ethnomethodologists do not need to restrict ourselves to AV, transcribed or other forms of 'real-time captured' materials to talk sensibly about how members constitute their settings. Having materials, like transcripts, that prevent analysts from making wild theoretical flights of fancy is important, but so is having materials that allow them to be rather more imaginative and exploratory – and these are sometimes rather neglected. Ethnomethodologists should, in short, be paying more attention to ethnographic work outside ethnomethodology, as this is where some interesting, grounded and suggestive real-world data is readily available. This is congruent with Harvey Sacks's injunction to 'see where things go' when doing data work.

As with the other papers in this special issue, thinking about (particular) things means also (re)thinking others. The relationships between ethnography, ethnomethodology and conversation analysis; the proper uses of empirical materials and what constitutes 'data'; how and where categories are used, and how they are used properly; where the divide between 'lay' and 'professional' analysis can be drawn (if it can be drawn at all) and the relationships between sociological topics and sociological resources are all invoked here. The purpose of the paper is to advocate being more imaginative, to return to the use of found materials as data if they provide for rigorous and interesting analyses. It necessarily, therefore, requires both a conceptual/methodological warrant and a demonstration of what such an analysis might look like.

I will start with a quote from Garfinkel.

Garfinkel on ethnographies

Garfinkel asserted that the 'accomplished sense' of settings 'consists of members doing, recognising, and using ethnographies' (Garfinkel, 1967: 10). This is an attempt on his part to capture what the 'accountability' of activities is: how they are produced and recognised to be themselves. The sense of what someone is doing is displayed as part and parcel of that activity, and is recognised as such by any competent other party. This ability to recognise, to observe, is itself tied to the ability of the person doing the recognising to report on, to describe, the activity being undertaken. Constituted or accomplished sense, therefore, depends on sense being displayed and recognised, and that the observed sense of a setting can be reported – just what is going on can be said in so many words.

Garfinkel's use of 'ethnographies' is not metaphorical here. Being able to see what is being done in a setting *is* fieldwork, and being able to describe that to others *is* ethnography. Determining that your children's shouts from the garden are expressions of happy play rather than an angry row may (sometimes) require you to double-check what is being shouted, in what tone of voice, whether it is jokey or serious, and so on. But in order for this check to be useful, one must *already* know what kinds of shouts your children produce, and what the characters of different ones are – as well as what 'shout', 'jokey', 'serious', 'happy play' and 'angry row' mean in the context of children's interactions. These are the 'background understandings' that accurate recognition of intent and motivation rest on. One's immersion in the field (having both broken up fights and allowed noisy play lots of times in the past) allows one to recognise what is going on 'from the point of view of the actor' in this particular case (to adequately recognise the nature of the event in the members' own terms), and to describe it to others – in this case telling your partner that there's nothing to be worried about, for example.

While Garfinkel's use of 'ethnographies' is not metaphorical, then, it is deflationary: nothing special is going on when we recognise (and sometimes describe to others) what is happening in a setting. At the time he was writing, Garfinkel was still troubled by the blurring of sociological topics and sociological resources: in order to do studies one must already know something (often quite a lot) about the setting being studied, simply in order to make sense of what is going on there. This, for Garfinkel, posed a problem: how could sociological investigations have firm foundations if its descriptions relied on the very things they were meant to be descriptions of? By treating everyday sense as 'ethnographic', then, Garfinkel is locating fieldwork and ethnography as mundane methods, matters that are perhaps worth investigating and describing rather than things that can be used unproblematically as resources for sociological description.

This, however, is a somewhat ahistorical and simplistic take on Garfinkel's ideas. Although Garfinkel clearly took topic and resource seriously early in the ethnomethodological project (Garfinkel, 1964), his later work was characterised by the recognition that it was impossible – and undesirable – to attempt to 'overcome' the problem. Ordinary language use inevitably inflects and shapes our investigations (Garfinkel and Sacks, 1970) but that is because it is foundational to any human activity, not because it is a 'problem' – indeed, many of the rather misguided sociological methodological recommendations are attempts to 'overcome' (disguise) this fact. Language could, of course, be

seen as a problem – something that distorts the studies that rely on it – but equally it could be seen as an asset, a precision tool used to make other precision tools. The difference between the two perspectives is one of scepticism versus realism. Garfinkel's mature take on methodology was that one must be familiar with, and be able to describe, the ways sense is produced and recognised in situ as a prerequisite of adequate description (Garfinkel and Wieder, 1992). This implies not just a recognition that *some form of* ethnography is necessary, but further that it is unavoidable.

In this way, the topic and resource 'problem' can be refigured. It is not possible to describe the topics of sociological inquiry without using the mundane resources that members use. To describe the workings of a clinic, one must know – and be able to describe – how that clinic works. To describe it in ways that appear to reduce the description's reliance on such mundane knowledge is to *redescribe* it: in behavioural terms, in terms of the demographic features of the clinic's personnel and users, as a site of oppression, and so on – in fact, describing it as anything other than what it is in itself. Even doing this, the mundane resource of a shared natural language has to be used to make any sense of what is going on, so even descriptions such as these cannot 'escape' the topic–resource 'problem'. Understood from the perspective of the later Garfinkel, then, 'topic and resource' is better framed as a figure–ground issue. In order to produce an ethnographic description of how a setting is ordered, one must necessarily use features of that setting as analytical resources. The point is to *show* how they are used, how they are understood and used *by parties to the setting* in the production of that order. Sociologists use everyday language as a key resource and in their work show how parties to a setting use it (and other things) to produce orderly, rational and mundane activities (see also Dennis, 2003, 2019).

Sacks on ethnographies

Garfinkel's initial ambivalence about professional ethnography was reflected by Sacks's approach. In an early lecture, Sacks said '[i]nstead of pushing aside the older ethnographic work in sociology, I would treat it as the only work worth criticising in sociology, where criticising is giving some dignity to something' (Sacks, 1992: 27). Sacks's early comments are illuminating in relation to what his project morphed into. He contrasted his approach to that of more conventional ethnographers not by virtue of his analysis, but rather because he had a more rigorous approach to what would count as *materials*. Sacks explicitly wanted the reader to have as much access to 'information' as the writer, by providing the data being analysed in full. This, naturally, contrasts with the 'information' generated through fieldwork: this can only be partially captured (via fieldnotes or photographs, and, latterly, AV materials, film, etc.), and would never be reproduced in full but rather as part of a structured narrative. Ethnography is the generation of a written account of a culture built on such materials, not the presentation of that culture in full.

Sacks's project narrowed to the study of ordinary talk, as this could be captured perfectly in this way. Especially in recordings of telephone conversations, everything that is interactionally available to each party (the other's speech) is captured and can be written down for the reader to see. What Sacks offered was a *principled* approach to data: given it

is the basis of the claims being advanced it should be available to the reader so those claims can be checked against it. Sacks's use of audio recordings provided him with a technology that allowed him to do revolutionary things with data: they could be played again and again to 'find' things that were not available on first hearing in ways that other forms of sociological data cannot. Sacks, however, did not have the means of providing readers with those data: although audio recordings could be made they could not be distributed easily. Gail Jefferson's transcription system was a work-around for this: provided the reader could 'hear' from the transcript what the audio sounded like it was possible for him/her to check the claims being made. (While much is made of Sacks's use of audio technology, the limitations of what could be published at the time are seldom remarked upon: given the development of multimedia documents since Sacks's death, would Sacks and Jefferson *really* have gone to all the trouble of generating a complex transcription system or is it more likely they would have just embedded the relevant audio into PDFs?)

It is worth emphasising that Sacks explicitly articulated the idea that he did not want to 'push aside' ethnography, and that his project differed from it in as much as he set himself the task of providing the reader with 'as much' information as the author has. This raises two questions. Firstly, what is the relationship between field data as presented in ethnographies and the claims ethnographers make on the basis of those data? Of course, data are selected for publication on the basis of their capacity to show what is being claimed even in 'hard' sciences (Lynch, 1985), and those data are frequently illustrative of the workings of a culture rather than being the raw materials for an analytical transformation of mundane understandings into the 'real social structures' (Garfinkel, 2019) that sociologists are interested in. Both points are, however, moot. Sacks's own materials were chosen to illustrate the point he was making: he thought through ideas, most strikingly in his lectures, by repeatedly listening to tape recordings of ordinary talk, but in his publications – particularly the paper on turn-taking (Sacks et al., 1974) – transcripts were selected *to make the point being argued*. Furthermore, Sacks himself, in the same lecture, pointed towards the utility of field materials for understanding how social activities are structured: 'the relevance of the works of the Chicago sociologists is that they do contain a lot of information about this and that. And this-and-that is what the world is made up of' (Sacks, 1992: 27). The information itself is unproblematic: it is 'about' just the things sociology should be addressing. Indeed, Sacks's criticism of modern anthropology was that it relied too much on informants – on asking people questions – and not examining the categories members use in the course of their activities.

The second question Sacks's comments raise concerns the extent to which his own project diverged from ethnography in a systematic manner. Of course, Sacks provided readers with 'as much' information as he had for the purposes of them being able to check that his claims were warranted by the data he drew on – and, indeed, much of his concern with transcription systems was directed towards ensuring that this was done 'as much' as possible (Lerner, 2004). But what counts as 'as much'? And what counts as 'drawing on data'? Transcription systems remain controversial, and there has always been a recognition that even the 'gold standard' system constructed by Gail Jefferson is 'good enough' rather than 'perfect'. Non-verbal components of interaction may be important, but cannot

be captured without modifying this system (Heath, 1986), and it is unhelpful in documenting events that are not organised around speech (Goodwin, 1984). The point here is that Sacks's work was *about the organisation of talk*, not about other social phenomena. As Schegloff has repeatedly pointed out, this does not reduce its relevance: once the elements of social interaction that are done through language are described and analysed very little is left over (usually). Talk is capable of being presented, more or less, in full, unlike most other social phenomena – which have to be described. This description is not unproblematic: as Sacks argued elsewhere '[f]rom simply reading two descriptions of variant length, style, etc., one could conclude that while one is more elaborate the other is more terse, while one is more extensive the other is more intensive, etc.' (Sacks, 1963: 12). Sacks's work, in short, came to approximate his ambitions about data being available to the reader as much as to the author, but this was a function of his interests shifting to the sequential organisation of talk as a way of framing his earlier interests (in particular around categorisation). In terms of 'drawing on data', again, Sacks's work became more rigorous as Jefferson's transcription system developed and his team's research agenda became more focused, but the grounds on which this work rested remained the everyday meanings utterances have. The reader having as much information as the author is pointless unless the reader can still examine the claims being made by the author – and those claims ultimately rest on their shared mastery of the same language. As Sacks (1992: 664) said elsewhere:

> When you start out with a piece of data that goes like some of these pieces of data went, e.g., 'They make miserable coffee' '—across the street?,' the question of what kinds of findings it will give you should not be any consideration whatsoever with respect to what you do with it. That is to say, it's a nothing. There's nothing exciting about it, nothing obviously of direct theoretical interest about it. Furthermore, starting a consideration and developing points on it does not require a hypothesis. It just involves sitting down at some point and making a bunch of observations, and seeing where they'll go.

Sacks's (1972) analysis of the story 'The baby cried. The mommy picked it up' would be a case in point, but his later work on poetics, dirty jokes, puns, spouse talk and so on remained informed by this spirit. While Sacks's key focus was on the organisation of ordinary talk, the technical analysis of this did not prevent him from undertaking parallel analyses of what members *do* with that talk.

'Professional' ethnography and ethnomethodology

It might be concluded from these remarks that Garfinkel identified ethnography as a topic for investigation, and Sacks as a resource (albeit modified) for inquiry. These two positions are not, however, as distant from one another as they might appear. They remained distinct because of two binaries in the early ethnomethodological project: lay versus professional and topic versus resource.

It is important to state here that questions about the utility of fieldwork and ethnography for ethnomethodological studies are *not* the same as questions about

ethnomethodology's *disciplinary* relationships to constructive analysis. As Watson (2021: 16) succinctly points out:

> Garfinkel sets up his ethnomethodology as an irreconcilable alternative to orthodox, 'formal analytic' sociologies, but he defined ethnomethodology by contrasting it with orthodox ones. In this sense, he treated orthodox sociologies as, sort of, 'having a place' in ethnomethodological study; indeed, he alludes to an ethnomethodology–formal analysis 'pair', related if only through a gestalt switch. By contrast, Sacks's position is that orthodox sociologies should be, by and large, ignored, and that we should start again in the discipline. Of course, this distinction is not an absolute one. For instance, Sacks valorised the Chicago ethnographies as just about the only thing in (orthodox) sociologies that are even worthy of criticism. For Sacks, almost all sociology was to be ignored. Sacks, for good reason, would not compromise on this—rather like his early hero, the sociological outlander C. Wright Mills. In Sacks's lectures, after his graduate work (Sacks, 1992), one certainly does not find any dialogue with orthodox sociologies, and, as time goes on, we find him subjecting his sociology more and more to interdisciplinary concerns. By contrast, Garfinkel's work can be read as an uncompromising dialogue with orthodox sociology.

Garfinkel's construal of 'lay' and 'professional' sociological work as being on a level was one of the more shocking elements of the early ethnomethodological project. Sense-making by ordinary members of society and the kinds of sense sociologists make of them share more features than sociologists sometimes feel comfortable with. Both rely on the reflexive relationship between events and their circumstances to make sense of the world: what something 'is' depends on its circumstances, and, to an extent, it shapes those circumstances by changing them. In professional sociology, this extends to things like coding organisational documents: these are seldom good enough to be mapped directly onto a coding scheme and so must be made sense of with respect to what they must mean given the way the organisation works (Garfinkel, 1967, chapter 6). The problem here (for sociology's claims-making rather than for its practice) is that what counts as 'good enough', what practices constitute 'made sense of', how 'what they must mean' is determined and whether 'the way the organisation works' can be spelled out in detail are *all* matters that are worked out in situ and using the same kinds of lay forms of understanding ordinary parties to the organisation use.

This places ethnomethodology in a strange position. Is it itself a 'lay' form of description or, by studying this reflexivity, can it somehow move onto a more 'professional' footing than other forms of sociology? Here the distinction between Sacks and Garfinkel becomes more marked. Sacks did seem to think that his project would 'become' professional as its technical and analytical frameworks became more refined (Lynch and Bogen, 1994), although this is perhaps a rather uncharitable view given its relative unimportance to the *body* of Sacks's work (Mair and Sharrock, 2021). It is a view that has, however, informed some of the subsequent developments in conversation analysis. Garfinkel, on the other hand, came to reject the idea that 'professional' analysis was possible insofar as it might 'free itself' from ordinary language and mundane understandings. Indeed, the later Garfinkel's unique adequacy requirement of method

(Garfinkel and Wieder, 1992) explicitly states that a native familiarity with how activities are undertaken, and the ability to describe them as self-organising, necessarily has to be at the heart of ethnomethodological analyses. Inasmuch as ethnographic reports describe scenes 'from the actor's point of view', they weakly satisfy some of this requirement. Inasmuch as they describe actions-in-context and language-use-in-action, they fulfil a 'stronger' reading of the requirement.

What does this mean for the ethnomethodological use of ethnographic materials, then? For one thing it means that 'doing ethnomethodological ethnography' is simply a different rendering of 'doing ethnomethodology': as ethnographies are *the ways in which we understand and describe the world* anyway. For another thing, it means that – although ethnomethodologists might, and probably should, be more 'rigorous', 'careful', 'scrupulous' and so on, than constructive analysts – describing the 'this and that' of the social world is *essentially* the basis of what ethnomethodological studies are 'about'. And, for a third, it means that ethnomethodologists have much to gain from repurposing *others'* ethnographic materials for themselves.

This third implication is important, and it gets to the heart of the argument being pursued here. Sacks used others' data ('The baby cried' is taken from a collection of stories by children) routinely to illustrate his points. Many of Garfinkel's own studies relied on autoethnographic data (interviews with Agnes in Garfinkel, 1967, chapter 5), straight fieldwork (the study of the Los Angeles Suicide Prevention Centre in Garfinkel, 1967, chapter 1), found materials (an audio recording of the discovery of a pulsar in Garfinkel et al., 1981), and so on. Finally, one of the key resources for ethnomethodology's conceptual core stems from anthropological fieldwork (Evans-Pritchard's, 1937 study of Zande magic) and how it problematises concepts of rationality, science and the everyday (Winch, 1964).

Broadly speaking, there are three sets of reasons ethnomethodologists have tended not to use others' fieldwork data. The first is about the status of the data themselves: the analyst did not collect the materials her/himself, and other forms of data would allow for different kinds of analysis. Who collects the materials is irrelevant, however: Gail Jefferson's transcriptions of talk have been used by generations of conversation analysts as examples (and exemplars), and besides an argument of this sort would preclude critical examination of others' claims. One would be restricted to advancing arguments only in cases where one has a unique dataset. It is true, but irrelevant, that different forms of data would allow for different forms of analysis: this is hardly something unique to ethnomethodology, and its assertion only makes sense if there are a priori claims that one form of data and one form of analysis are 'superior' to others. What criteria could be employed to make this determination remain unclear.

The second set of objections relate to the nature of fieldwork: it is inherently loose and impressionistic, and its findings are largely subjective. These chime somewhat with Sacks's objections to ethnographic studies, but are subtly different. Sacks was not claiming that fieldnotes and other records of participant observation were not useful, but rather that for *the kinds of analysis he wanted to undertake* they were insufficient. These objections, rather, are about the accuracy and detail of fieldwork materials. Again, they are not unique to ethnomethodology. As discussed above, however, such materials have been

used as the basis of ethnomethodological studies in the past, and continue to inform conceptual discussions in discussions of ethnomethodology. Furthermore, Garfinkel's studies of how sociology gets done reveal that the same objections could be made to *any* study, including ethnomethodological ones. One cannot escape from 'looseness' because there is no adequate criterion for determining how 'tight' or 'loose' a description should be. Furthermore, the *point* of data is to constrain the analyst's capacity to make claims. If 'subjectivity' is an issue it would apply to any kinds of data collection. It seems these objections may be being used as proxies for an argument that fieldwork data are *inaccurate*, but it is not clear whether that can be argued and – if it can – whether that does not, therefore, mean that any study in any discipline is therefore flawed.

Finally, the professional ethnographer's purposes are different to those of parties to the settings he or she is studying. This means that little of the mundane 'this and that' of a setting makes it into professional published work: 'what is going on' is described and a selection of 'thises and that's' are used to warrant the adequacy of that description. Answers to questions are treated as propositions about the world (see, in a rather different idiom, Widmer, 2002), the ways data are the products of a local organisation of interaction are neglected (Hester and Francis, 1994) and the standardised techniques by which a 'finished' analysis is achieved are underinvestigated (Anderson and Sharrock, 1982). For these reasons care is required in the use of others' data: these data are always the outcomes of processes that may be more or less explicitly stated, and so, when repurposed for ethnomethodological purposes, best used to suggest lines of enquiry. At their best, ethnographies provide good professional constructive analyses of social settings – which can be used for non-constructive purposes by others.

An example from Elijah Anderson

This has all been very abstract so far, so a concrete example of what reading field materials in an ethnomethodological mode might look like would be useful. For the purposes of this paper I will use an extract from Elijah Anderson's (2003) monograph *A Place on the Corner*. This is not to criticise or find fault with Anderson's analysis, but rather to show how his *materials* might be of use to ethnomethodologists. Anderson's work has been chosen because it is exemplary in the way Sacks found the Chicago sociologists' work to be worthy of criticism: it is internally coherent, politically savvy and generated novel findings that were empirically sensitive and (in his later works) impactful at a policy level.

Anderson's book describes the lives and practices of a group of men living in Chicago's South Side. They frequent a liquor store, Jelly's, and, for many in the peer group, hanging around there is the most important part of their social lives. Anderson shows how the peer group is constituted, how it relates to other elements of the men's lives and how respectability is produced and maintained in and around a rather 'rough' venue in a rather 'rough' part of the city. Central to this is the men's own classification of themselves and one another as 'regulars', 'wineheads' or 'hoodlums'.

'Regulars', in one sense, just means people who regularly come to Jelly's. The term is also used, however, by a distinct group who use the term to refer to themselves and are so recognised by others. They are respectable, and want to be treated as such by others. They are typically

older than other patrons, they work regularly and maintain steady employment (often doing as much overtime as possible or holding more than one job at the same time), they save for things like cars, houses and furniture, and many are in stable nuclear family relationships. They are 'decent', and expect to treat others – and be treated by others – with consideration and respect.

'Wineheads', on the other hand, are usually out of work or only work sporadically to earn enough to fulfil their immediate needs. They value 'gettin' a taste' and 'havin' some fun' (Anderson, 2003: 93), and spend much of their time at Jelly's either drinking wine or trying to obtain money (often by begging) for food and wine. The term 'winehead' is denigrating, and is used by others to describe members of this category; although they seldom admit belonging to the category, wineheads tacitly or explicitly accept the treatment others give them. This may mean moving away from the main group to be with other wineheads, or deferring to others' judgements about group values. Their activities are relatively stable but are looked down on by others.

'Hoodlums' are typically younger men who have emerged from ghetto street gangs. 'The mere attributes of youth, including particular styles of self-presentation, are usually enough to qualify one for the identity of hoodlum until one proves otherwise by one's actions, associates, or both' (Anderson, 2003: 129). While they value 'big money' and 'being tough', the hoodlums at Jelly's are largely petty criminals, engaged in petty theft, stickups, burglary, handling stolen goods and so on. Apart from the small number who have regular jobs, hoodlums are generally regarded as threatening and 'to be watched out for' by Jelly's other patrons. 'If regulars mainly value a "visible means of support" and "decency", and wineheads care about "getting some wine" and "having some fun", then hoodlums appear to care mainly about presenting themselves as "tough" and able to "get big money"' (Anderson, 2003: 130).

Anderson's book examines the lives of different members of these categories, and how they interact with one another within and between categories. His analysis of the achieved orderliness of Jelly's as a setting parallels – and is, at least in part, informed by – ethnomethodologically influenced ethnography. What is perhaps most interesting for ethnomethodological purposes, however, is a feature of the ways the men who frequent Jelly's classify themselves and one another:

> Despite such questions [about why Anderson played the role of the 'cultural dope' or from time to time 'played dumb'], I persisted, asking person after person. 'So what are you?' 'I'm a regular.' And 'What are you?' 'I'm a regular.' 'What are you?' 'I'm a regular.' Finally, I got to Red Mack, one of the most well-known wineheads around, and even he declared, 'I'm a regular!' Virtually everyone was a regular! But then I began to ask how they viewed one another, or what they thought of others in relation to themselves. So I asked, 'What is he?' 'Oh, he's a hoodlum' or 'Who is he?' and the answer would be, 'He's a winehead.' It turned out that while group members would declare themselves 'regulars', absolutely no one would identify themselves as 'hoodlum' or 'winehead' (Anderson, 2003: 258).

Anderson used this odd phenomenon as a jumping-off point to examine group hierarchies and the movement of people within and between groups. His observation here, however, can be put to alternative purposes. It is important in doing this to retain the identifying details of the text, leaving them where they are and not treating them as if they require improvement or amendment (they do not: they are wonderful pieces of

sociological description). Those details can, however, be *repurposed* to see how they might inform ethnomethodological concerns. To use the term suggested by Anderson and Sharrock (2017: 9), we can problematise 'what these data are data of'.

Repurposing ethnography

The first thing to notice about the term 'regular' is that it has two possible meanings: someone who regularly goes to Jelly's and someone who is 'respectable' in their life. Anderson is explicit in both the main body of the book and in the appendix that these meanings are distinct, and that 'regular' in terms of how the men describe themselves and one another is used in the latter sense. This use of the term 'regular' is thus capable of contrast with the terms 'winehead' and 'hoodlum': the three terms form a collection in the sense intended by Sacks:

> An instance of a categorisation device is the one called 'sex': Its collection is the two categories (male, female). It is important to observe that a collection consists of categories that 'go together'. For now that may merely be seen as a constraint of the following sort: I could say that some set of categories was a collection, and be wrong (Sacks, 1972: 332).

'Regular', 'winehead' and 'hoodlum' are therefore the three categories in the collection 'types of men who frequent Jelly's'. The fact that all members of the collection use one to describe *themselves* but all three to describe *one another* becomes ethnomethodologically interesting in a rather different way to that which Anderson explores.

Sacks does address categories that are applied differentially in a similar way. In his lectures he repeatedly made reference to the expression 'Abraham the Hebrew' from chapter 14 of Genesis:

> The phrase 'Abraham the Hebrew' is apparently unique in the Bible, in that it's pretty much the only place that an Israelite is referred to by the term 'the Hebrew'. That term, 'the Hebrew', is only used by an Israelite for self-identification to a foreigner, or by foreigners about Israelites. Its importance lies in the fact that, given the usage, 'Abraham the Hebrew', biblical critics can feel that that section of Genesis was a segment taken from some document which had not been written by the Jews, and thus is some independent information about the historicity of Abraham (Sacks, 1992: 397).

Sacks uses this observation as a jumping-off point to discuss self- and other-classifying terms used in anthropology. For the purposes of the materials he is working with (a group therapy session), the category 'hotrodder' has similar features: it is a category generated and policed by its incumbents (young men who have put a lot of work into their cars) and can only be used by others in the way those incumbents intend. It allows, in short, for their autonomy and independence as members of a group.

'Regular', in Anderson's ethnography, has some of these features. Its use is (potentially) contested insofar as it is a *moral* category: everyone wants to be a 'regular' but only some people are (correctly) categorised by others as such. What is interesting for Anderson is that there are relatively stable applications of the categories 'regular', 'winehead' and 'hoodlum'

when applied to others, but these applications do not hold when applying the category to oneself. Self- and other-categorisation only cohere where the person being classified is categorised *by others* as a regular; in all other cases, the self-categorisation 'regular' sits in contrast with the other-categorisation 'winehead' or 'hoodlum'.

It is tempting here to invoke Goffman (1959), talk about impression management and have done with it.[3] There is something deeper going on, however, which Sacks's analysis of 'Abraham the Hebrew' opens up for scrutiny. Categories that can be applied *within* a group can be different to categories that can be applied *to* that group, and furthermore which individuals 'fit' into one category or another can be contested. This means that systematic differences between self- and other-categorisation might be treated as a warrant for considering more formally how those differences are socially produced and used.

A candidate typology of self- and other-category attribution

Categories are moral entities: social obligations, status and power, codes of proper behaviour and so on all depend on their production and recognition. Where categories are applied *to* an individual but that individual applies a different category *to themself* this moral element becomes most clear. The category being applied by others is typically one that denigrates its bearer. Much of the classic symbolic interactionist literature addressed how these features of categorisation play out in concrete situations. Davis (1961), for example, addressed how people who are visibly disabled 'break through' in their interactions with others, reducing the salience of the category 'disabled' compared to other, more personality-based ones. Hughes (1945), on the other hand, described how people with contradictory statuses – black medics, who white patients might see as low status because of their race but high status because of their profession – are moved into positions where such status dilemmas will not cause problems (for instance, working in black neighbourhoods or taking jobs that involve library or laboratory work only).

To return to the workings of the categories themselves, however, it is possible to construct a *possible* typology of their moral statuses. This may not be exhaustive, but it serves as both a candidate for a longer list and as a way of starting to unpack the moral aspects of these kinds of collections. The candidate typology is as follows:

1. There are categories you would rather not apply to yourself but you can apply to others.
2. There are categories you cannot properly apply to yourself but others can apply them to you.
3. There are categories you can be a candidate for but others determine their applicability.
4. There are categories you can claim for yourself but others may contest this claim.

Categories you would rather not apply to yourself are typically those which are morally compromising. In Anderson's work, these would be 'winehead' or 'hoodlum', or in Evans-Pritchard's study of the Azande 'witch'. Characteristically people will try to avoid having these categories applied to themselves, by denying their applicability, making

category incumbency out to be less damaging than is commonly believed, by operating in a social context in which that category will not be salient and so on.

Categories you cannot properly apply to yourself are typically the opposite: they are morally upgrading. Anyone, for example, can claim to be a generous person but such a claim should never be taken at face value. One's generosity is something that only others can judge, as it is a category that requires 'objectivity' to be correctly applied, where 'objectivity' means others' judgements aligning with that being an appropriate category to be an incumbent of.

Such categories are related to those that someone can be a candidate for, but others decide whether or not that candidacy is appropriate. These are still, in general, morally upgrading categories, but they have the additional features of quantifiable difference and/or appropriateness of claim. Donald Trump's famous claim to be the 'least racist person that anybody is going to meet' is an example of quantifiable difference. For that category to be applicable is it not enough for Trump to be viewed by others as 'not a racist'; it is also essential that he is properly classifiable as 'more not-a-racist than anyone else'. Recently in the UK, a long-delayed report was published in which the Metropolitan Police were categorised as 'institutionally corrupt'. On the day of its publication leaders of the organisation argued that this was inaccurate and there was no such corruption. This is an example of appropriateness of claim. If the leaders of the Metropolitan Police are right, the report must be inaccurate; if, however, the report is accurate, then the leaders of the Metropolitan Police *would say that they are not corrupt* as that is what corrupt people do.

Categories one can claim for oneself but others may contest that claim are typically those that are about whether one category is a subset of another or a separate member of a collection. These then, unlike categories one can be a candidate for, are about the relationships between categories rather than about whether or not an individual can be properly classified as one thing or another. They are usually politically or socially controversial. In recent years in the UK, one such category has been the subject of much dispute: what 'a Jew' is. Judaism is a category that can be understood in a number of different ways: one can be ethnically Jewish and an atheist, one can be a practicing Jew by converting to the religion despite having a Gentile ancestry, one can 'discover' one is Jewish by finding one's ancestors were Jewish and so on. Anti-semitism trades on these different understandings: it is a form of racism (as Judaism is an ethnicity) which can hide behind claims that it is not (because 'you can't tell' whether or not someone is Jewish from their appearance alone, in the way you can for other ethnic groups) (see, for example, Baddiel, 2021). The question this raises is whether saying 'David is Jewish' or 'David is a Jew' is making a claim about David's religious beliefs (David is not a Catholic, a Muslim or a Sikh) or his ethnicity (David is not black, Chinese or Hispanic).

Conclusion

By taking Anderson's observation about the ways men who frequent a liquor store classify one another as a starting point for reflection, it is possible to generate a typology of the

ways in which moral classifications depend on alignment or discongruity between self- and other-classification. This does not 'replace' or 'improve' Anderson's analysis, but rather reframes his observations in an ethnomethodological idiom. It gives us a warrant to 'think about things' in a different way, here reframing some central symbolic interactionist concepts in relation to the internal logics of categorisation.

There are (at least) two potential traps in taking this kind of approach. Firstly, it might stretch the links between empirical materials and concepts too much. As Bittner (1973) pointed out, much ethnographic work is loose, impressionistic and vague. This might mean that descriptions of, or reflections on, such work are therefore *themselves* problematic. Secondly, there is a danger that advocating the use of particular materials is somehow an attack on the use of others: AV, transcripts, etc. It is tempting to construct a straw man argument in which the 'problems' of 'conventional' ethnomethodological data are 'solved' by 'something new'. I hope I have avoided these traps.

Much current work in ethnomethodology is coalescing around rethinking the relationships between data and the claims those data allow one to make. In recent considerations of the status of video data (Watson, 2018), transcriptions of spoken materials (Lynch and Macbeth, 2016) and the relationships between data and analyses more generally (Anderson and Sharrock, 2017), a concern has emerged that a way of doing things when doing ethnomethodology has taken on a normative status. To do ethnomethodology means to do particular things, to look at particular materials, to treat them in particular ways and to find in them particular features. This is not in keeping with the perspective's radicalism, its attempt to configure a 'first sociology' (Anderson and Sharrock, 2018, chapter 11), its rejection of constructive analysis (Garfinkel and Sacks, 1970) and its replacement of professional methodologies by a commitment to use whatever is required to describe the methods and techniques members use and have mastery of (Garfinkel, 2002, chapter 5). To that extent, this paper is part of an ongoing set of discussions and is not 'disinterested' in relation to where those discussions are going.

The core question posed by these discussions is disciplinary: what is the relationship between ethnomethodology and constructive sociology? As Watson argues, this relationship was pursued differently by Garfinkel and Sacks, but it is difficult to say that one of these thinkers was 'better' than the other. They did different things, and came to different conclusions about what they were doing. The issue here is not whether we should use formal analytic sociological findings as ethnomethodological resources, however, but whether we can use some of the materials that are gathered to produce those findings *for our own purposes*. Schwartz's (2002: 16) impeccably clear description of the situation is important here:

> Sociology tends to treat practical activities as types of sociological activities. We do the reverse and treat sociology, sociological work, as just another variety of practical action done in everyday settings.

The point is that we are *not* treating sociological work as being any different to other forms of practical action. We are not privileging its methods or its findings. We are not looking to hybridise ethnomethodology with constructive sociology. We are, however,

saying that, along with any other form of practical activity, sociology – ethnographic sociology – generates materials that we can use, and in the same way, we might use anything that captures the 'this and that' of ordinary life. We would, however, use these differently.

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Notes

1. This was not the case. All the air force grants, including an earlier one with Edward Rose, were reported on by Garfinkel, Sacks and Churchill in 1969. The final report consists of an introduction, three pages of conceptual materials that were worked up into the 'Formal Structures' paper (Garfinkel and Sacks, 1970), and 10 pages of bibliography. I am grateful to Clemens Eisenmann and Anne W. Rawls for letting me see this document, held by the Harold Garfinkel Archive, Newburyport, MA.
2. For an analytical and critical assessment of the use of fieldwork and ethnography as forms of data collection and analysis by ethnomethodologists, see Meier zu Verl and Meyer, this issue).
3. Again, however, it should be pointed out that Anderson's analysis is infinitely more sophisticated than this: what is being suggested here is that differences between self- and other-categorisation could be 'explained away' using Goffman in a conventional sociological conceptual manner.

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