The influence of 'topic and resource' on some aspects of social theorising

Abstract

Developments in sociological theory since the 1960s have been responses to disciplinary problems rather than changes in fashion. The problem of topic and resource—where sociology has to use everyday understandings and practices as study resources even though they are legitimate topics of enquiry—has been an important and sometimes neglected spur to many of these developments. The turn to discourse, conversation analysis and the rise of Bourdieu's reflexivity are all attempts to address the problem, but each is shown to be unsatisfactory in different ways. In summary, they seek to address the issue as requiring either a principled methodological or a principled theoretical solution, and neither approach is capable of comprehensively addressing the matter. It is argued that these 'solutions' depend, in turn, on one of two particular construals of what the 'problem' consists in, neither of which is necessary or coherent. Each, it is argued, depends on a philosophical trick: making language out to need formal improvement (the Bertrand Russell trick) or introducing inappropriate scepticism to everyday life (the René Descartes trick). It is suggested that treating topic and resource not as a problem but as something which opens up new areas of investigation successfully deflates the issue and avoids unnecessary theoretical and methodological contortions.

Introduction

It is news to no one that a sociologist brought forward in time from the 1950s would find it difficult to recognise the contemporary discipline as the same one he or she worked in. Anglophone sociology has undergone a series of 'turns' since the heyday of structural functionalism, and once-dominant positivist and quantitative study approaches now compete with a host of other positions. Some have claimed this as a triumph for previously marginal, critical perspectives (Fine, 1993), while others have suggested that the state of the contemporary discipline is the outcome of a series of turf-wars between academic 'gangs' (Scheff, 1995). Such arguments, however, are unconvincing. There were no grand debates between, for instance, symbolic interactionists and structural functionalists: the two groups were, and remained, relatively insulated from one another. And there is little evidence that interpretative and qualitative approaches have been more agile or vicious in the academic marketplace than their staid predecessors.

Sociology is a more dispersed and heterogeneous discipline than it used to be because it has had to deal with some fundamental theoretical problems, initially raised in the 1960s and 1970s. Its 'turns' (resolving the 'problem' of structure and agency, the linguistic turn, the postmodern turn, and so on) did not *cause* disciplinary change but were rather attempts to solve these problems, many of which were initially integral to the structural functionalist project. A central, and often neglected, dilemma is the problem of topic and resource.[1]

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[1] In some respects, this is the 'problem' to which the 'reflexive turn' was the solution. The approaches addressed here would all claim to be 'reflexive', but this is a poor starting point

This paper aims to show how three sociological approaches appeared to solve this problem, and to suggest that some of the differences between contemporary sociological perspectives rest on the different ways these 'solutions' worked. The topic–resource issue, however, will be shown to be trickier than it appears: none of these approaches adequately solved it, and the arguments that it is an existential problem for sociology are incoherent. Instead it will be argued that the 'problem' of topic and resource reflects a misunderstanding of the relationships between sociology, science and 'mundane reason' (Pollner, 1987) and, if this misunderstanding is cleared up, the 'problem' emerges as an invitation to expand sociology's scope rather than to question its disciplinary foundations.

The argument will start with a specification of how topic and resource was initially framed as a problem for sociology. Three putative solutions—the turn to discourse, conversation analysis, and the work of Bourdieu—will be outlined, and their study policies described. The problems these solutions, in turn, raise will then be considered in terms of the presuppositions underpinning the 'problem': that it is something that requires either (1) a principled methodological or (2) a principled theoretical solution. These presuppositions will be shown to have shaped what might look like a solution. Finally, an alternative construal that requires no such 'solution' will be introduced. It will be argued that this construal is both more faithful to the way the issue was originally framed, and also one that offers more expansive and open study possibilities.

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as what 'reflexivity' might consist in depends on how it addresses the problem (Lynch, 2000).

The problem of topic and resource

Many of the resources sociologists use to account for social activities are, themselves, social phenomena, and so can be analysed as sociological topics in their own right. Mathematical practices are an example: these are used, amongst other things, to measure the statistical significance of survey results, to count how many people are incumbents of different social categories, and to measure the distribution of social phenomena (attitudes, illnesses, tastes, and so on) across populations. The practice of doing mathematics is itself, however, a social phenomenon, and therefore has been subject to sociological investigation (Bloor, 1987; Greiffenhagen & Sharrock, 2011). This is not a problem: one could conduct sociological investigations into interviewing, the organisation of focus groups, the design of survey instruments, etc., etc., without those studies rendering any of those phenomena problematic as devices sociologists can use for doing studies.

Topic and resource *becomes* a problem when mundane, common-sense reasoning is considered. Such mundane understandings underpin *all* forms of social activity, are necessarily taken for granted, and are seldom treated as topics of enquiry in their own right, perhaps because of their very triviality and ubiquity. This issue was at the heart of Garfinkel's early work:

> In that commonsense activities and environments are simultaneously the topic as well as the feature of sociological inquiries, a concern for describing the actual features of sociology's attitude and methods as possible modifications of the attitude and methods of commonsense, the 'discovery of culture', reconstructs the

problems of the sociology of knowledge and locates them at the heart of the sociological enterprise and with full seriousness (Garfinkel, 1963, p. 236).

The problem is exacerbated when attention shifts away from the topics that sociology has typically neglected and towards the resources it uses to pursue investigations. These 'resources' depend on a range of everyday competences—most importantly the capacity to speak and understand a natural language—which themselves require examination not simply as interesting topics but, crucially, as fundamental disciplinary phenomena in their own right. To treat *this* seriously would require a more radical reformulation of what sociology should be doing:

> The 'rediscovery' of common sense is possible perhaps because professional sociologists, like members, have had too much to do with common sense knowledge of social structures as both a topic and a resource for their inquiries and not enough to do with it only and exclusively as sociology's programmatic topic (Garfinkel, 1964, p. 250).

Sociology, Garfinkel seems to be arguing, must find a way to transcend its dependence on 'common-sense' *both* to secure its own foundations *and* to facilitate the 'discovery of culture'. Until it has done this, sociology is in trouble, as two of Garfinkel's then students pointed out:

> Sociology's acceptance of the lay member's formulation of the formal and substantive features of sociology's topical concerns makes sociology an integral feature of the very order of affairs it seeks to describe. It makes sociology into an

5

eminently *folk discipline* deprived of any prospect or hope of making fundamen-

tal structures of folk activity a phenomenon (Zimmerman & Pollner, 1970, p. 82).

Later still, Garfinkel appeared to treat the problem as incapable of a principled solution that

would not require a wholesale respecification of the sociological project:

> The fact that natural language serves persons doing sociology, laymen or profes-
> sionals, as circumstances, as topics, and as resources of their inquiries, furnishes
> to the technology of their inquiries and to their practical sociological reasoning *its*
> circumstances, *its* topics, and *its* resources. That reflexivity is encountered by so-
> ciologists in the actual occasions of their inquiries as indexical properties of natu-
> ral language. These properties are sometimes characterised by summarily observ-
> ing that a description, for example, in the ways it may be a constituent part of the
> circumstances it describes, in endless ways and unavoidably, 'elaborates' those
> circumstances and is 'elaborated' by them. That reflexivity assures to natural lan-
> guage characteristic indexical properties such as the following: The definiteness
> of expressions resides in their consequences; definitions can be used to assure a
> definite collection of 'considerations' without providing a boundary; the definite-
> ness of a collection is assured by circumstantial possibilities of indefinite elabora-
> tion (Garfinkel & Sacks, 1970, p. 338).

Language use here, *pace* Wittgenstein (2009), is always embedded in the activities it is a part

of. The sense of utterances depends on the circumstances of their production and understand-

ing (they are 'indexical'), and those utterances, in part at least, determine what those circumstances are. This is as true for sociologists as for everyone else, with complex implications for what kinds of things sociology can do and what kinds of claims it can make about what it is doing.

Topic and resource, as a 'problem', has two elements. Firstly, sociology must have a clear understanding of the resources it uses. Secondly, because those resources necessarily include the shared common-sense understandings members of society use to make sense of the world, those common-sense understandings must themselves be described and understood. Topic–resource is a problem because, in order to undertake *that* description, *some* resources must be used—and it is unclear what they might be, how they might be used, and what the status of descriptions made under their auspices might have. Would the epistemological foundations of such descriptions be 'scientific', (merely) 'common-sense', or some hybrid of the two?

Some solutions to the topic and resource problem

1. Topic and resource as a critique of social science: the turn to discourse

As a critique of social science, the topic and resource problem motivated much of the 'turn to discourse', in particular in the sociology of scientific knowledge (SSK). Towards the end of the 1970s there was an increasing interest in conducting descriptive studies of what scientists actually do in their day-to-day work as scientists. Laboratory studies (Knorr-Cetina, 1981; Latour & Woolgar, 1986; Lynch, 1985)—ethnographic investigations of laboratory prac-

tice—described the mundane competences scientists rely upon to produce warrantable scientific findings. A key finding of these studies was that there is a problematic relationship between what scientists do and what scientists say: Latour (1987) went so far as to suggest that science is 'Janus-faced' and that the two categories are mutually contradictory. A more modest claim was that scientific writing ('inscription') does not reflect the messy, contradictory, situated nature of laboratory practice, and that scientists' claims should, therefore, be treated as social productions ('constructions') rather than simply statements about the nature of the world.

This led to an increasing interest in scientific disputes and the politics of scientific activities—why one account, as a 'version' of reality, is chosen over another. In their *Opening Pandora's Box*, Gilbert and Mulkay stressed that these disagreements are not just problems for scientists, but also problematise the sociological description of their activities. Following Zimmerman and Pollner's understanding of sociology as a 'folk discipline' they argued that 'sociologists' attempts to tell *the* story of a particular social setting or to formulate *the* way in which social life operates are fundamentally unsatisfactory … because they imply unjustifiably that the analyst can reconcile his version of events with all the multiple and divergent versions generated by the actors themselves' (Gilbert & Mulkay, 1984, p. 2). Just as different 'actors' understand the world in different ways, the sociologist's take on what is going on in a setting is deflated to become *just another version* of events. All that can be done is to describe, as best as possible, how differences manifest themselves through 'discourse': to elaborate the versions of reality different participants advocate. This turn to language was paralleled in anthropology:

Thus, in short, an ethnography should not be homological, plagiaristic, positivist, essentialist, or analogical. The name of the game now is aesthetics, pastiche, collage, juxtaposition, framing, heteroglossia, polyphony/polyvocality, or at the very least, dialogue (Caplan, 1988, p. 9).

In short, the problematic relationship between topics of enquiry and resources for investigation became used as a warrant for both criticising what had come before and suggesting that new literary devices and methodological styles are required to embrace the partial—perhaps impossible—nature of sociological claims-making. Such 'new literary techniques' fed back into the sociology of knowledge (Ashmore, 1989; Woolgar, 1988)[2] and became briefly fashionable in more mainstream sociology. The notion that the sociologist's account was privileged over those of his or her 'participants' was abandoned. This perspective found its contemporary representative in Bruno Latour's actor-network theory, which acknowledges the existence of order in activities rather more clearly than its predecessors but which maintains their deflationary tactics:

[I]nstead of taking a reasonable position and imposing some order beforehand, ANT claims to be able to find order much better after having let the actors deploy the full range of controversies in which they are immersed. It is as if we were saying to the actors: 'We won't try to discipline you, to make you fit into our categories; we will let you deploy your own worlds, and only later will we ask you to

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[2] Trevor Pinch's paper on new literary forms in sociology was an analytically appropriate pastiche of this approach, co-authored with himself (Pinch & Pinch, 1988).

explain how you came about settling them.' The task of defining and ordering the social should be left to the actors themselves, not taken up by the analyst. This is why, to regain some sense of order, the best solution is to trace connections between the controversies themselves rather than try to decide how to settle any given controversy. The search for order, rigor, and pattern is by no means abandoned. It is simply relocated one step further into abstraction so that actors are allowed to unfold their own differing cosmos, no matter how counter-intuitive they appear (Latour, 2005, p. 34).

Such approaches as these proved unsatisfactory to many. If sociological texts themselves are 'vulnerable to deconstruction' (Collins & Yearley, 1992, p. 304) a reasonable question might be what point there is to producing them. While the 'discursive turn' was wrestling with the topic–resource problem in its own way, however, other developments were going on.

2. *Topic and resource as foundational to a scientific sociology: conversation analysis*

Firstly, conversation analysis (CA) was becoming increasingly professionalised. Trading on its roots in ethnomethodology, while neglecting much of the work of the late Harvey Sacks, some contemporary conversation analysts claim to have 'overcome' the topic–resource problem by having transcended CA's lay, vernacular roots: CA is now 'scientific' (Arminen, 2008). Its scientific status is underwritten by the proper use of a standardised system of notation, originally devised by Gail Jefferson, allowing CA's analyses to be examined, replicated or challenged by other competent practitioners (Macbeth & Wong, 2016; Macbeth, Wong, & Lynch, 2016):

Jeffersonian transcription provides a shared, standard system for rendering talk-in-interaction in a way that can be textually reproduced. It is compact, transportable and reproducible, and provides for easy random access unlike audio or video records. CA transcription is a fundamental resource for data sessions, presentations and journal articles, and, as such, it is often the medium through which analysts encounter and evaluate each other's work. It is therefore at the center of the epistemic culture of Conversation Analysis. CA transcription has evolved, and will continue to evolve, with the gradual progression of conversation analytic studies of interaction (Hepburn & Bolden, 2013, p. 75).

Such an approach facilitates the use of CA as a standardised and 'expert' approach to human activities, allowing the analyst's (re)description of what is 'really' going on to be substituted for participants' 'lay' understandings of what they are up to. CA can thus be 'applied' to compare different settings (Drew & Heritage, 1993), provide insight into communicative problems (Wilkinson, 2014), gender relations (Kitzinger, 2000), education (Gardner, 2013), and so on. In short, CA as the study of a topic, ordinary talk, has been superseded by CA as a *legitimate* resource, a way of professionally studying the social world.

Doing CA is not to most sociologists' tastes, however. It is time-consuming and difficult to produce transcripts as detailed as those undertaken by Gail Jefferson (see, for instance, the complex transcriptions of laughter in Jefferson, 2004, in which the possibility of 'applying' CA is systematically rejected), and claims about matters extrinsic to the organisation of talk are discouraged.

11

3. Topic and resource as a warrant for renewed theory: Pierre Bourdieu

While 'literary' DA and CA were of only marginal interest, Pierre Bourdieu's influence on sociology, in particular in the UK, cannot be overstated. During the 1990s and 2000s he became a key theoretical resource for doing studies, and his influence has not diminished significantly in mainstream sociology. His work offered a more palatable solution to the topic–resource problem, via his version of reflexivity:

> I know that I am caught up and comprehended in the world that I take as my object. I cannot take up a position, as a scientist, on the struggles over the truth of the social world without knowing I am doing so, that the only truth is that truth is a stake in struggles as much within the scientific world (the sociological field) as in the social world that this scientific world takes as its object (every agent has his 'idiotic' vision of the world, which he aims to impose—insult, for example, being a form of wild exercise of symbolic power) and with respect to which its struggles over truth are engaged. In saying that, and in recommending the practice of reflexivity, I am also aware of handing over to others instruments which they can turn against me to subject me to objectification—but in so doing, they show that I am right (Bourdieu, 2004, p. 115).

Bourdieu recognised that sociology's legitimate purview included sociology itself, and that this 'reflexive' relationship could not be overcome. By turning analytical attention to the practices of sociologists—by stepping back from sociology to see how it relates to its objects of study—however, the foundations of the discipline could be shored up:

I wanted less to observe the observer as an individual, which is in itself not particularly interesting, than to observe the effects produced on the observation, on the description of the thing observed, by the situation of the observer—to uncover all the presuppositions inherent in the *theoretical* posture as an external, remote, distant, or, quite simply, non-practical, non-committed, non-involved vision. And it struck me that there was an entire, basically false social philosophy which stemmed from the fact that the ethnologist has 'nothing to do' with the people he studies, with their practices and their representations, except to study them: there is an enormous difference between trying to understand the nature of matrimonial relations between two families so as to get your son or daughter married off, investing the same interest in this as people in our own world invest in their choice of the best school for their son or daughter, and trying to understand these relations so as to construct a theoretical model of them (Bourdieu, 1990, p. 60).

By treating practices of theorising as *systematically different* to the 'practical' concerns of members of society, and—for Bourdieu—the reflexive examination of those practices as themselves socially situated phenomena, an apparent *theoretical* solution to the problem of topic and resource appeared. This was (to a degree) convincing, and certainly more amenable to conventional sociological work than its more 'radical' alternatives. A theory founded on a 'reflexive' study of what sociologists do, while perhaps a little odd, provided many sociologists with a warrant to continue more-or-less business as usual.

Bourdieu attempted to distinguish his project from Garfinkel's by insisting on the reality of human agents and the objectivity of social structures as givens. '[A]lthough they point out

that the social world is constructed, they forget that the constructors are themselves socially constructed and that their construction depends on their position in the objective social space that science has to construct' (Bourdieu, 2004, p. 93). The examination of sociology as a sociological phenomenon only goes so far, therefore, as—just like 'science', 'the truth of the social world' and so on—there is no clear reason why *these* concepts cannot themselves be made the topics of description or criticism. Topic–resource, therefore, is used to justify a *modification* of sociological theory rather than a root-and-branch re-examination.

Problems with the solutions

The problems with these 'solutions' is that each construes the problem of topic–resource differently. DA and actor-network theory make the problem out to be one of providing a context-sensitive account of a chaotic and disputed world. To avoid being a 'folk discipline' sociology must produce texts that reflect the 'versions' people construct and use, neither buying into the reality of any of those versions nor constructing a competing, scholarly alternative. Sociology is deflated by topic–resource, and its role becomes to reflect diversity and disagreement rather than to strive to produce definitive accounts.

CA makes the problem out to be one of overcoming the lay, common-sense nature of analysis. By describing language use in fine detail, and by producing standardised inscriptions of ordinary talk, it is possible to replace vernacular understandings with technical, professional ones. CA analyses stand as technical accounts of ordinary talk, using only analytical resources that have shed their mundane, reflexive roots.

Bourdieu, finally, understands the problem to be one of the relationship between observer and observed, and claims to have transcended it by taking a step back from sociology to see how it operates in the world. By taking this step back, by undertaking a sociology of sociology, he claims to have seen how the topic–resource issue can be overcome—by recognising and acknowledging the duality of the sociologist's position as both 'scientist' and 'participant'.

Each 'solution', therefore, is unacceptable to the others: because they start from different places they end up in different places, as one might expect. DA, CA and Bourdieu are not, therefore, 'competing solutions', but rather the result of competing *construals* of what topic–resource entails. The question, then, shifts to how one *should* construe the topic–resource problem. To answer this requires a return to its original presentation, and some of the claims made about it at the time. Just as Garfinkel's presentation of the 'problem' shifted in its emphases over the course of the 1960s, his collaborators and students, similarly, construed the problem in rather different ways. To illustrate these differences, two influential 'solutions' to the problem—contemporaneous with Garfinkel's work—will be described.

1. 'Sociological Description': the Bertrand Russell trick

The first construal of the problem was that offered by Harvey Sacks in an early position piece on how he saw his project (which ultimately became CA) relating to more conventional sociology. Sacks (1963, p. 4) sought 'to make current sociology strange', emhpasising the descriptive nature of his approach, in contrast to the then-popular rationales for sociology that construed it as either explanatory or tied to the evaluation or generation of theory:

[E]ven 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 (Sacks, 1963, p. 7).

Description—how people describe the world—is the central concern of Sacks's paper. A problem, perhaps the problem, for sociologists is that they use such 'lay' descriptions as resources for doing studies as, for example, in interviews, surveys, questionnaires, and so on. What 'respondents' *say* is treated as having something to do with what they *do*, and the former is treated as a *description* of the latter for sociological purposes. The problem for sociologists undertaking studies, therefore, becomes one of reconciling what is said—how features of the social world are described—with what those features of the world actually are. This is a problem because, Sacks argues, while the latter are subjected to sociological description the former are incorporated 'as is':

The emergence of sociology will take a different course (when it emerges) from that of other sciences because sociology, to emerge, must free itself not from philosophy but from the common-sense perspective… The 'discovery' of the common-sense world is important as the discovery of a problem only, and not as the discovery of a sociological resource (Sacks, 1963, p. 10–11).

A trick is being played here. Sociology is not simply an odd way of doing things, but is in need of *improvement*. Its use of common-sense resources (natural language use) poses a problem not because their 'discovery' opens up for investigation new and interesting phenomena, but because sociology's failure to examine them represents a *lack of rigour*. The discipline, according to Sacks, cannot be scientific until this is overcome:

> [A]t least some sociologists seek to make a science of the discipline; this is a concern I share, and it is only from the perspective of such a concern that the ensuing discussion seems appropriate. As scientists we seek to produce a literal description of our subject matter. In order to describe we construct (or adapt for our uses) a language. While to begin with our language may be crude, one rule must constantly be attended to: whatever we take as subject music be described; nothing we take as subject can appear as part of our descriptive apparatus unless it itself has been described (Sacks, 1963, p. 2).

Sacks is not, therefore, just pointing towards an area for potential investigation. He is taking the 'discovery' of the common-sense world to be a warrant for founding a truly 'scientific' sociology based on 'literal descriptions' of its phenomena. This means devising a technical language which will allow for further description prior to other activities being undertaken. This technical language was what emerged over the course of Sacks's (and his colleagues') study of ordinary conversation over the decade following the publication of this paper. The formal properties of language use identified by CA, initially using lay understandings, come to supersede lay understandings and place the programme on a more solid analytical footing.

Sacks here is adopting the same approach as that found in Bertrand Russell's (e.g., 1905) early philosophy. Something is being done—for Sacks people producing descriptions, for Russell people using the word 'the'—that is clearly ubiquitous, that is used naively as a resource by analysts, and that should legitimately be (one of) the topics they are investigating. Its presence renders the scientific (Sacks) or logical (Russell) bases of the rest of their disciplines (sociology and logical philosophy respectively) questionable. By treating it as a topic, describing it, and making those practices of topicalising and description the bases of further work, it will be possible to overcome that 'questionableness'. For Sacks, as for Russell, the 'shape' of what this 'new' form of the discipline will look like is already known: it will be an empirical, scientific, body of work, based on clear and distinct terms of reference. The assumption for Sacks, as for Russell, is that the common-sense basis of natural language use should be *overcome* to produce a properly scientific method of analysis as, unless this happens, such analyses must necessarily rest on unsecured and possibly erroneous foundations.

There are two problems with this approach. Firstly, it requires 'descriptions' to be separated from 'the things being described'. Effectively what people say—and what sociologists claim—is being treated as a commentary on, or description of, separate phenomena. This is the nominalism rejected by, amongst others, Garfinkel (& Sacks, 1970) and Wittgenstein (2009), who emphasised the situated and contextual nature of language use. Language is split from the circumstances of its use, and the ways those circumstances are given meaning by language is neglected, in order to achieve a one-to-one relationship between a separated description and the thing it describes.

Secondly, Sacks's account presupposes that the preconceptions of scientific method are the actual ways it gets done. The complex, situated practices of scientists undertaking research are reduced to a notion of science as 'formal description', and sociology must 'free itself' from both philosophy and common-sense to produce such accounts itself. As Winch (1990) has it, Sacks is buying into a picture of philosophy that makes it an 'underlabourer' to science, and neglecting the strong relationships between ordinary language philosophy and the aims and objectives of sociology.

Construing topic–resource as something amenable to a methodological solution cannot overcome these problems. Language must be treated as standing in an ostensive definitional relationship to the things it is 'about', and a particular construal of 'science' must be accepted for the project to get off the ground. What people say is treated as a description of what they do, and the sociologist's account as a potentially competing description. The idea that language is primarily about description, and that this somehow stands in an ironic relationship to sociological description, is *itself* an assumption that need not be made.

2. 'The Everyday World as a Phenomenon': the René Descartes trick

Zimmerman and Pollner, in contrast, construed the topic–resource problem as being about sociology's terms of reference:

> Sociology shares with 'lay' understandings of the social world its terms of reference ('delinquency' for instance), and so can only ever be a 'folk discipline' until it liberates itself from these. It is *part* of society, an alternative way of looking at things, rather than a means of *studying* society. Sometimes sociologists will agree

19

with members, sometimes they will disagree—but either way '[o]nce lay members' accounts are the object of evaluation, the professional investigator has implicitly raised the lay member to the status of a professional colleague (however incompetent)' (Zimmerman & Pollner, 1970, p. 87).

They recommend treating such features as a setting's 'historical continuity, its structure of rules and the relationship of activities within it to those rules, and the ascribed (or achieved) statuses of its participants' (p. 94) as *achievements*, 'occasioned' by parties to the setting. The job of sociology is to describe those achievements and examine their organisation. *How* this might be undertaken, however, is a problem. Zimmerman and Pollner construe this achievement as 'the occasioned corpus of setting features': how members assemble features that are deemed to be relevant to what the setting 'is'. The trick to make this a *theoretical* matter is played later:

> The occasioned corpus is thus conceived to consist in members' methods of exhibiting the connectedness, objectivity, orderliness, and relevance of the features of *any* particular setting as features in, of, and linked with a more encompassing, ongoing setting, typically referred to as 'the society'. The work of the occasioned corpus is the work of displaying the society 'in back of' the various situated appearances constituent of everyday, located scenes (Zimmerman & Pollner, 1970, p. 99).

The trick is the sudden reappearance of 'the society'. Where does this come from? Given the 'reduction' that Zimmerman and Pollner conducted on the lay understanding of what constitutes context—it is something external and constraining—the reintroduction of 'society' here is jarring. After all, the argument being pursued is one that is premised on the idea that one should not buy into the terms of reference used by members of society as if they were unproblematic analytical resources. That there is a 'more encompassing, ongoing setting', however, is *itself* one such term of reference. And the reintroduction of *this* term as a theoretical convenience has serious theoretical consequences. This is the *Rene Descartes trick*, in which—after systematically ruling out of analytical bounds everything except a single phenomenon (for Descartes, using systematic doubt, the *cogito*, for Zimmerman and Pollner, using Husserlian bracketing, the accomplished situation)—something previously discarded (for Descartes, God, for Zimmerman and Pollner, society) is brought back under, now, radically different conditions.

Zimmerman and Pollner make a series of assumptions to warrant their construal of topic–resource and its 'solution'. Firstly, although they do not make language use out to be purely descriptive, they do construe everyday understandings as 'theories' (insofar as they are sociologically interesting): sociologists and policemen [sic], for instance, 'may entertain very different theories of how a person comes to be a juvenile delinquent' (Zimmerman & Pollner, 1970, p. 81). Although Zimmerman and Pollner go on to question whether it is appropriate for lay concepts such as 'juvenile delinquency' to make their way unexamined into sociology, the idea that everyday understandings are something like theories runs through their account. What is occasioned as part of a setting is treated as a theory of what that setting is,

21

and—particularly where competing or conflicting definitions and construals appear—arbitrating between the alternatives is seen as a political and conceptual task rather than a practical one (Cf. Pollner, 1975).

Secondly, members' understandings of the world are subtly ironicised: the existence of the world, for instance, is 'taken for granted', and the idea that different people see the world in broadly the same ways is 'merely assumed' (Zimmerman & Pollner, 1970, p. 84). The meaning of 'assumption' here is not the same as that intended by Schütz, who Zimmerman and Pollner are referencing. For Schütz, 'I apprehend the contemporary only mediately, by means of typifications' (Schutz, 1964, p. 42).[3] What for Zimmerman and Pollner is a *mere* assumption is, for Schütz, part of the 'attitude of daily life', the way members of society *necessarily* experience one another and their surroundings. Schütz here is explicitly attacking Cartesian doubt by pointing out that, amongst other things, we assume things are what they seem unless there are particular reasons not to. Zimmerman and Pollner, on the other hand, by making this aspect of everyday understandings one of *defining situations* rather than taking things for granted open up the possibility of treating the taken-for-granted as something arrived at by choice.

Finally, Zimmerman and Pollner neglect the orderliness of everyday life. The fact that things get done in routine ways, without much thought, repetitively and without trouble does not ap-

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[3] Schütz's surname was given the variant spellings 'Schuetz' or 'Schutz' in different publications. The spelling as per publication is given for citation purposes.

pear as a sociological phenomenon, but is rather the outcome of a series of otherwise dis-jointed occasioned settings. The facts that parties to those settings overwhelmingly organise them to be orderly, that a proper orientation to the orderly features of everyday life is insisted upon by members (as shown, *inter alia*, by Garfinkel's 1963 breaching procedures), and that—without such order—there could be no stable 'background' to activities does not ap-pear. Order, for Zimmerman and Pollner, is an emergent property of occasioned settings ra-ther than a prerequisite of their sensible constitution. Again, Zimmerman and Pollner cherry-pick some ideas from Schütz's work but neglect others: ' such processes—typical experi-ences of 'someone'—*exhibit the idealization: 'again and again'*, i.e., of typical anonymous repeatability' (Schutz, 1964, p. 44).

Zimmerman and Pollner, and Sacks, in their different ways, treat topic–resource as a discipli-nary problem. A principled solution must be found, whether it rests on describing ordinary language use to provide a methodologically sound foundation for further studies (Sacks), or suspending judgement on the 'reality' of the everyday world to allow for more coherent the-ory-building (Zimmernan and Pollner). The routes to CA, via Sacks, and to both discourse analysis (DA) *and* Bourdieu's reflexivity, via Zimmerman and Pollner, can be mapped out from these initial interpretations of how topic–resource can be 'solved'.

Dissolving the problems

The assumptions made in these accounts of what topic–resource is and how it might be 'solved', although pervasive, are not essential. Both Sacks and Zimmerman and Pollner make

a set of assumptions about the 'problem' that are, perhaps, unnecessary. These rest on the notion that everyday understandings should be *solely* the topic and never the resource of sociological descriptions and investigations—apparently in accordance with Garfinkel's (1964) injunction. The fundamental problems here are that 'everyday understandings' are treated as a coherent and fixed body of phenomena, that these phenomena have a problematic relationship to sociological descriptions, and that a principled reconciliation of the two is required to ensure 'scientific' (or at least conceptual) integrity.

These rather deeper assumptions need not be made. The very early Garfinkel was heavily reliant on Schütz in setting out his project, and his comments on topic–resource are best understood in that way. Schütz (e.g., Schuetz, 1945), it should be remembered, argued that scientific rationality and scientific theorising were not separate to everyday life but *modifications* of it—variations of the natural attitude. The natural attitude is the 'paramount reality', and other ways of experiencing the world (e.g., dreams, experiencing a dramatic performance, and so on) are temporary ways of seeing things differently experienced *within* it. Scientific rationality, in this understanding, is simply the addition of four extra criteria for evaluating something as rational added to the mundane everyday understandings of rationality used all the time (Garfinkel, 1960).

In this sense, Garfinkel's comments on topic and resource relate to the problematic relationship between everyday and scientific rationalities where the 'everyday' is itself a topic of enquiry. Sociology is inevitably a 'second-order' discipline (Schutz, 1954), depending on everyday understandings and mundane categories both as phenomena of interest and—inevitably—as resources for analysis. *This* is the topic–resource problem: how this situation can be

managed, not how it can be overcome. Garfinkel's comment that sociologists 'have had too much to do with common sense knowledge of social structures as both a topic and a resource for their inquiries and not enough to do with it only and exclusively as sociology's programmatic topic' should, therefore, be read not with the emphasis on 'only and exclusively' but rather on 'too much' and 'not enough'. But how can this be achieved?

Garfinkel himself is little help here. He abandoned the topic–resource issue after 1970, when he acknowledged that language use is always embedded in the circumstances of its use—for members of society, sociologists and scientists alike—and pointed out that 'formulations' (descriptions of settings) tend to be attempts to repair ambiguities—which may or may not succeed—rather than descriptions in any formal or scientific way. Garfinkel also abandoned Schütz as an analytical resource, moving instead towards Husserl and Merleau-Ponty for philosophical ammunition. His subsequent arguments that studies of the social world should be hybrids between sociology and the practices being described, and that analysts conducting such studies should themselves be expert practitioners in the fields of enquiry, are not models of clarity and their implications are confusing even to his own followers (Garfinkel & Wieder, 1992; Lynch, 1993).

A topic–resource tradition that *could* have been taken further, however, was similarly abandoned. The ways concepts, formulations and other terms of reference are used provides a fascinating and under-researched area for investigation. Bittner's (1965) careful deconstruction of the sociological terms 'bureaucracy' and 'organisation' through an examination of how

those terms are used in practice—and the practices their use warrants—provides a good example of what such an analysis might look like. Bittner's critique of topics being used as resources is similar to others':

> [I]f the theory of bureaucracy is a theory at all, it is a refined and purified version of the actors' theorizing. To the extent that it is a refinement and purification of it, it is, by the same token, a corrupt and incomplete version of it; for it is certainly not warranted to reduce the terms of common-sense discourse to a lexicon of culturally coded significances to satisfy the requirements of theoretical postulation (Bittner, 1965, p. 246).

But, unlike those outlined above it is a *practical* rather than a *programmatic* matter:

> In general, there is nothing wrong with borrowing a common-sense concept for the purposes of sociological inquiry. Up to a certain point it is, indeed, unavoidable. The warrant for this procedure is the sociologist's interest in exploring the common-sense perspective. The point at which the use of common-sense concepts becomes a transgression is where such concepts are expected to do the analytical work of theoretical concepts. When the actor is treated as a permanent auxiliary to the enterprise of sociological inquiry at the same time that he is the object of its inquiry, there arise ambiguities that defy clarification (Bittner, 1965, p. 241).

Wieder's (1974) examination of how the 'convict code' is used to elaborate the sense of activities in a post-prison halfway house, and in turn is elaborated by those activities, provides

another example. It is impossible not to use everyday concepts to understand the workings of the social world, but to understand any one it may be sufficient to 'bracket' it—to be indifferent to its apparent meaning and examine how it is deployed and used in practice—so as to clarify what *for members of society* that concept does. This radically changes the role of the sociologist in conducting investigations:

> While no longer living naively in the practical intentions of daily life, he attempts to live within the methods of inquiry of that practical life and then to withdraw and reflect on them while 'reducing' them to mere methods of inquiry and the correlative objects of inquiry—both objects inquired into and reported about (Wieder, 1977, p. 8).

Recognising that language and other activities are intrinsically linked, and that that link cannot be broken, does not mean the end of sociology any more than it means the end of ordinary understanding. We *do* make sense of the world, the world is overwhelmingly orderly, and—when required—we can say just what is going on in a setting in ways that others can recognise and agree (or disagree) with. This provides sociology with a technical problem, and one which requires a technical solution. The modesty of Bittner's and Wieder's approaches, and the sensitive content of their descriptions, seems to indicate that 'principled' solutions to such problems are often more trouble than they are worth.

One of the central problems afflicting contemporary social theory—and ethnomethodological practice—is muddled thinking about the relationships between language and other practical

activities. A second is the relationship between sociological descriptions and ordinary understandings. The fact that these remain apparently intractable, and attempts to solve them have frequently ended up as fudges, perhaps indicates that the 'problem' of topic and resource requires revisiting.

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