4 Two methodological disputes

It will perhaps come as no surprise, given the substantial differences in philosophical assumption and practical orientation to be found amongst social researchers today, that there have been, and continue to be, major methodological and theoretical disputes. Moreover, these have occurred not just across the qualitative-quantitative divide but also very frequently *among* different versions of qualitative research. At the most fundamental level, these disputes relate to what sorts of knowledge are possible and desirable, and to the nature and goal of the research enterprise. It is not possible to cover all of the issues that divide qualitative researchers, but I will focus on two very different disputes each of which raises important and difficult methodological problems. Discussion of these is designed to give a sense of the variations in underlying commitment that generate much of the diversity, in both rhetoric and practice, now to be found in qualitative research.

The first dispute centres on the legitimacy of employing interviews as a source of data, though as we shall see its implications extend much more widely – to the use of observational data and documents as well. The second dispute focuses on recent ethnographic work concerned with the lives of the urban poor in the United States, raising challenging questions about the role of theory and evidence, and about the public or political responsibility of researchers.

The 'radical critique' of interviews

Interviews have long been used by qualitative researchers as a central source of data, and in fact studies relying primarily or entirely on this type of data have become increasingly common in recent decades; a trend that has attracted criticism (see, for example, Atkinson and Silverman 1997; Silverman 2007: ch.2). In Chapter 3 we saw that interviews are often central to research that is concerned with investigating people's experience and

perspectives, and that they may also play an important role in work aimed at identifying causal relations or penetrating fronts, and even in some research that seeks to document discursive practices.

Qualitative researchers have generally adopted a very different approach to interviewing from survey researchers. They have used informal as well as formal (in other words, pre-arranged) interviews, and their aim, generally speaking, has been to encourage informants to talk in their own terms about matters relevant to the research topic. In other words, they have tended to use a relatively unstructured approach. As part of this, the aim has *usually* been to minimize the impact of the interviewer on what the informant says (this impact sometimes being referred to as 'reactivity'). In addition, stress is placed upon the importance of listening, on the researcher trying to suspend her or his preconceptions and prejudices in order to understand the perspectives, feelings, or accounts of informants.

Despite these distinctive features, much qualitative analysis uses interviews for more or less the same purposes as other kinds of social science:

1   *As a source of witness information about the social world*. Here, interviews are treated as supplying information about informants' biographies, about events they have observed, about relevant stable or variable features of situations they are familiar with, and/or about the frequency of one or more types of event in such situations.

2   *As a source of participant-analysis*. Here, interviewees are asked to reflect upon their own behaviour, attitudes, character, and/or personality, and perhaps also on that of other people they know, so as to supply their own interpretations. These are then used by the researcher – subject to critical assessment.

3   *As an indirect source of evidence about informants' attitudes or perspectives*. Here, the analyst uses how informants respond to questions as a basis for drawing inferences about their characteristic intentions, motives, preoccupations, preferences, perspectives, attitudes, etc. It is frequently assumed that what can be detected here are stable orientations that generate behaviour beyond the interview setting, though perhaps in a contextually variable manner. And the responses of informants may be treated as typical of some general category of person, or of a larger population.

Much analysis of interview data in qualitative research is directed at all three of these purposes, to varying degrees; and it is not always easy to

distinguish between them in practice, even though they are analytically distinct.

There have long been discussions among qualitative researchers about the value and use of interview data, raising questions for example about whether and how we can 'know the informant is telling the truth' (Dean and Whyte 1958), about the 'incompleteness' of interview data as compared with that from participant observation (Becker and Geer 1957), and about the difference between what people say and what they do (Deutscher 1973). However, the more recent 'radical critique' of interviews takes a skeptical, constructionist line that is at odds not only with much current practice but also with these earlier criticisms (Murphy *et al*. 1998). In effect, the radical critique rejects the use of interview data as a window on the world and/or into the minds of informants (Dingwall 1997; Silverman 1997; Atkinson and Coffey 2002).

As an example of this critique, I will focus on an article by Potter and Hepburn (2005) entitled 'Qualitative interviews in psychology: problems and possibilities.' This article was accompanied by three commentaries in the same journal issue, to which Potter and Hepburn then replied. In their initial article the authors outline their aim as 'to challenge the taken-for-granted position of the open-ended interview as the method of choice in modern qualitative psychology' (Potter and Hepburn 2005: 282). They criticize, in particular, the way in which interview data are usually reported and interpreted, but what they say also has implications for when, if ever, interviews should be used.[1]

Potter and Hepburn distinguish between problems that are remediable ('contingent'), and others that they suggest may be inherent in the use of interviews ('necessary'). Under the first heading they list the following:

1   The interviewer is typically viewed by researchers as simply eliciting pre-existing attitudes, perspectives, etc, possessed by the informant, and as a result her or his role is given little attention in the analysis. Equally important, information about questions asked is rarely included in the transcript excerpts published in research reports. The effect of this, Potter and Hepburn suggest, is to treat what informants say as if they were offering abstract pronouncements about matters

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1 While their article is concerned with qualitative methods in psychology, their arguments apply across social science. For a similar critique relating to qualitative research more generally, see Rapley (2001).

rather than giving 'a specific answer to a specific question put by a specific interviewer' (Potter and Hepburn 2005: 286).

2   There is a failure to attend to other aspects of the *interactional* character of interviews. Potter and Hepburn argue that the forms of transcription generally used by qualitative researchers systematically strip out much that is relevant, for example hesitations, overlaps, the timing of pauses, emphasis, etc.[2] Equally important, research reports quote very short extracts from transcripts that give little sense of surrounding context.

3   Because they use transcripts that omit interactional detail, when analyzing their data most qualitative researchers fail to relate their claims to specific features, making 'global' statements whose validity is difficult to assess.

4   There is often little information provided about the set-up of the interview. Yet, how it was arranged, not least what interviewees were told about the research, and about why they were being invited to participate, could have shaped the data in important ways. As a result, readers may have difficulty assessing the validity of the evidence accurately.

A central complaint here, then, concerns the forms of transcription used by qualitative researchers and how these affect both the analysis carried out and the evidence made available to readers. On the basis of these complaints, Potter and Hepburn lay down four requirements:

i)   Transcript extracts in research reports should include the relevant interview questions.

ii)   These extracts should be 'transcribed to a level that allows interactional features to be appreciated even if interactional features are not the topic of the study' (Potter and Hepburn 2005: 291).

iii) Transcripts should be presented using line numbers, and short lines, to allow specific references to features within them.

iv)   Research reports 'should include information about how participants were approached, under what categories, with what interview tasks' (Potter and Hepburn 2005: 291).

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2 For an example of specific advice to *exclude* such features, see Finnegan (1992: 196–7) (quoted in Plummer 2001: 150).

   The other type of problem that Potter and Hepburn identify in qualita-
tive researchers' use of interview data, which they regard as intrinsic and
unavoidable, includes the following:

1   Interviews tend to be 'flooded' by social science agendas and
     categories. This is not just a matter of research questions being
     explicitly mentioned to informants, or technical social science terms
     included in interview questions, but also of the more inexplicit ways
     in which the assumptions characteristic of a particular discipline may
     infuse the orientation of the researcher and affect the informant.
     These assumptions might include the idea that the social world is
     characterized by the operation of causal variables, or that people
     necessarily know about their own attitudes and behaviour (Potter
     and Hepburn 2005: 291). The suggestion is that through their
     questions, and in other ways, interviewers effectively 'coach [..]
     the participant in the relevant social science agenda' (Potter and
     Hepburn 2005: 292), and thereby subtly shape what he or she says.
     Potter and Hepburn summarise this concern as follows: '[…] these
     issues present us with the possibility that a piece of interview research
     is chasing its own tail, offering up its own agendas and categories,
     and getting those same agendas and categories back in a refined or
     filtered or inverted form' (Potter and Hepburn 2005: 293).

2   Rather than participants in interviews simply speaking for
     themselves, in fact what we find is that they echo other voices,
     speak on behalf of others, and so on. This is captured, in part, by the
     distinctions drawn by Goffman (1981) among various 'footings' that
     can be adopted in talk: for instance between the speaker and the
     composer of the spoken talk – as illustrated by a politician reading
     a speech written by someone else – and between the speaker and
     the 'voice' being represented – as when a spokesperson speaks on
     behalf of a government or other organization. Potter and Hepburn
     argue that analysts must attend to the different 'footings' people
     adopt in interviews, and they raise questions about the possibility of
     abstracting from these in order to make claims about people's general
     'attitudes' or 'perspectives', as if they always or primarily spoke with
     single voices. More generally, while people are usually interviewed by
     researchers precisely because they are members of some category
     – teachers, social workers, politicians, etc – it is clear that not all of

what they say is spoken under the auspices of this particular identity. People always operate with a multiplicity of potentially relevant identities, and these are more or less salient at different times, even within the course of the same piece of social interaction. There is also the reverse question of to whom informants are talking: it cannot be assumed that their primary or only audience is the interviewer, since they will probably anticipate that others are likely to hear what they say, even if only via the research report. Moreover, even if we take the interviewer as the audience, he or she will have multiple identities, and may be oriented to by informants in terms of different ones at different times within the same interview.

3   When people talk, what they say is attuned to the stakes they have in how they might be interpreted, and the potential consequences of this. They are not, therefore, simply expressing their views, but seeking to 'position' themselves in response to anticipations of how they might be 'positioned' by others. Indeed, it is unclear whether, aside from this, they have some single position which underpins what they say. The implication of the argument here is that once we abandon the assumption that what is said is the simple expression of inner, personal thoughts or feelings, we can do no more than document the different 'attitudes' displayed on particular occasions by speakers and seek to understand these displays.

So, what conclusion should be drawn from this critique? On the face of it, the authors hedge their bets, offering two slightly different ones. They write that their 'ultimate aim is to improve the quality of interviews and their targeting at particular research problems. The ideal would be much less interview research, but much better interview research' (Potter and Hepburn 2005: 282). The implication here is that the findings from studying interviews as interactional processes can be fed back to improve the 'design, conduct and analysis of interviews so that [these] can be used more effectively in cases where [they] are the most appropriate data-gathering tools' (Potter and Hepburn 2005: 281). Along these lines, the authors argue that 'whatever the analytic perspective, inferring things appropriately from interviews involves understanding what is going on in them interactionally, and that in turn involves the complex and demanding task of analyzing the development of an implicit research agenda,

identifying footing shifts, explicating orientations to stake and so on'
(Potter and Hepburn 2005: 300).

However, Potter and Hepburn also comment that: 'As researchers with
some expertise in interaction analysis we would like to emphasise that this
is a challenging analytic requirement. Such analysis is rarely done with any
degree of seriousness in current interview research, and, where it is, the
analysis often highlights just how much the interviewee's talk is a product
of specific features of the interview.' (Potter and Hepburn 2005: 300). This
points to a second, rather different, conclusion: that interviews are not a
satisfactory source of data, that it would be better to use 'naturally occur-
ring data' (Potter and Hepburn 2005: 301).[3]

It is striking that, even in the case of this second more radical conclu-
sion, Potter and Hepburn seem to imply that it derives from method-
ological considerations that would apply to all kinds of research, rather
than specifically from 'a conversational analytic and discursive psycho-
logical perspective' (Potter and Hepburn 2005: 291).[4] Indeed, this point
about general relevance is one that they make at various times in their
article. Early on they comment that 'we expect researchers who work with
interviews to recognize [the problems we are going to discuss] without
difficulty' (Potter and Hepburn 2005: 282); and suggest that the points
they are making 'are intended to have a much broader relevance than
specifically to those researchers with a discourse or conversational inter-
est' (Potter and Hepburn 2005: 282).

Yet, even their discussion of the problems they see as remediable
is governed by distinctive assumptions about the nature of the social
world, and how we can gain sound knowledge of it, that derive from an
ethnomethodological or conversation analytic point of view. While other
researchers would not deny that answers are shaped by the questions
asked, even in relatively unstructured interviews, and that what is said
there is influenced by other aspects of the interactional process, many are
likely to regard the dangers as much less serious a potential source of error
than Potter and Hepburn claim. Similarly, while most would agree that

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3 As Potter and Hepburn acknowledge (2005: 301), there has been some debate
about the possibility of 'naturalistic' data, and about whether it is essential for the
kind of work they propose: see Speer (2002), Potter (2002), ten Have (2002), Speer and
Hutchby (2003), Hammersley (2003).

4 Coulter (1999) has questioned the link between ethnomethodology and discursive
psychology, see also Potter and Edwards (2003), and Coulter (2004).

analytic interpretations should be tied to evidence in research reports, there are differences in view about exactly what this entails.

Much the same can be said of the problems that the authors view as unavoidable. In fact, there was a fourth such problem they identified, which I have not yet mentioned, and this demonstrates the point particularly clearly. They call this fourth issue 'the reproduction of cognitivism'. There is some ambiguity in what they say here. They reject 'cognitivism', while at the same time recognising that: 'For many interview researchers some kind of cognitive perspective will be entirely appropriate.' (Potter and Hepburn 2005: 297). In fact, their arguments are at odds with the orientation of most qualitative researchers, and indeed even with that of many other discourse analysts.

Potter and Hepburn identify two 'facets' of cognitivism that are to be rejected: 'the privileging of rumination over action and the treatment of cognitive language as descriptive'. What becomes clear here is that Potter and Hepburn are, in effect, denying that interview data can be used for any of the three standard purposes I outlined earlier. As regards the first, they complain that in much qualitative work the interviewee is treated 'as a reporter on events, actions, social processes and structures, and cognitions' (Potter and Hepburn 2005: 298), whereas, in fact, informants are engaged in a quite different mode of activity, which might be described as constructing and reconstructing themselves and their world through talk.

In relation to the second function of interviews, Potter and Hepburn explicitly abandon the idea of using people's own 'rumination[s]' (Potter and Hepburn 2005: 297) as a source of data. In part this stems from their denial that people have privileged access to an inner domain of experience that they express through talk, since they must use *common* discursive resources to present *situationally appropriate* accounts of themselves. Indeed, Potter and Hepburn denounce the idea, taken for granted in much research concerned with investigating experience, that 'people are the best experts' (Potter and Hepburn 2005: 299) on their own perceptions, opinions, beliefs, attitudes, etc. Equally, it is clear that they object to researchers accepting people's explanations for what happens in the world, where they are, in effect, treated as 'proto-social scientists' (Potter and Hepburn 2005: 297). Potter and Hepburn 'highlight', by which (it seems to me) they mean 'challenge', the assumption that interviewees can provide information about social processes (Potter and Hepburn 2005: 298).

Finally, they reject the use of interviews as a means by which attitudes, perspectives, etc. can be identified because they deny that actions are causal products of such attitudes or perspectives. As we have seen, they question the existence of all such 'cognitive objects'. Thus, they write:

> Our point is that to fully understand the qualitative interview as an interaction we will need to pay attention to the practical and interactional role of cognitive terms and be very cautious about treating such terms as if they referred to psychological objects of some kind within individuals. (Potter and Hepburn 2005: 299)

Again, read in the context of the whole article, it is clear that 'caution' is a euphemism here, what they mean is that we should *not* treat these terms as referring to psychological objects.

So, in this section on cognitivism, Potter and Hepburn continue to present their arguments as if they derived straightforwardly from commonly recognisable methodological problems with interviews, but in effect they are challenging fundamental assumptions underlying much qualitative research. While they claim that their conclusions derive simply from recognising the interactional nature of interviews, in fact the character and seriousness of these problems have been and continue to be subject to conflicting implications and debate.[5] In other words, there is disagreement among social researchers about how much of an obstacle these problems represent. Moreover, presumably they are problems that face all of us in our everyday lives when we are interpreting evidence and constructing explanations about other people's behaviour; so does this mean that here too we must avoid reliance upon testimony from others? The answer is, presumably, no. At issue, then, is the difference between research and other activities, in terms of the evidential requirements they entail. Interestingly,

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5 Most qualitative researchers are concerned with reactivity and how this might distort their findings, but they do not usually believe that because interviews involve interactional processes it is impossible to draw conclusions from them about the experiences and perspectives of informants, and about the world in which they live. For example, they do not assume that what an informant says in answer to a question is entirely determined by the nature of the question. Nor do they believe that because informants must use a particular natural language, and available discursive resources, that what they say is entirely determined by these resources. See the thoughtful discussion in Murphy and Dingwall (2003: ch.5) of 'what kind of information can we get from interview data?', illustrated by examples from the health field.

this is a contested matter even within the ethnomethodological tradition on which Potter and Hepburn draw (see Lynch 1993).

Finally we should note that the radical implications of the kind of constructionism that Potter and Hepburn adopt extend well beyond the use of interviews. In practice, these apply to all forms of data, whether audio-recordings of naturally occurring social interaction, textual documents, or visual images. For instance, when studying naturally occurring talk, discourse analysts (of the kind represented by Potter and Hepburn) do not use the data to draw conclusions about the experiences or perspectives of the people involved in the interaction, to show what is actually going on as against what is said to happen, or to identify causal relationships. Rather, they focus upon what they see as the constitutive practices revealed in interactional processes. There is a very sharp contrast here not only with most qualitative interview studies but also with most qualitative inquiry of other kinds.[6]

So, despite initial appearances, Potter and Hepburn are not arguing just that researchers using interviews must pay more attention to the interview process, nor simply that it is preferable to base analysis on 'naturalistic materials'. Rather, they are insisting that the only viable or legitimate focus for research is on the interactional practices through which the social world is constituted. Most of the topics typically studied within psychology, and social science more generally, are ruled out by this stance, because these presuppose that there are cognitive and other objects that exist independently of how they are formulated in and through discursive practices, that these causally or intentionally shape human behaviour, and that informants can provide information about them and/or researchers observe them.

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6 In the case of ethnography, the contrast was neatly demonstrated in Wieder's (1974) book *Language and Social Reality*, in which the author first provides an ethnographic account of how an informal set of rules seems to operate among inmates of a 'halfway house' for ex-prisoners, shaping their behaviour, for example in ruling out their giving information about other inmates to the staff. Then, in the second half of the book, Wieder adopts a more constructionist approach (like Potter and Hepburn influenced by ethnomethodology), in which the focus is on how inmates formulate their actions through appeal to this code, and in the process continually formulate and reformulate the code itself. Here the code cannot serve as an explanatory factor accounting for inmates' behaviour since it is recognised to be part of how the meaning of that behaviour is ongoingly constituted.

Commentaries on Potter and Hepburn's article and their response

As I noted earlier, Potter and Hepburn's article was accompanied by three commentaries. The first came from a researcher who has championed the use of in-depth interviews for phenomenological analysis in psychology (Smith 2008; Smith *et al.* 2009). He expresses sympathy for their general argument, and for some of their recommendations, agreeing about the need to pay more attention to interview processes. However, he comments that the authors privilege one type of qualitative work (that characteristic of conversation analysis and discursive psychology), and that key aspects of their argument do not apply to other kinds. For example, he rejects the requirements Potter and Hepburn lay down about transcription, and their suggestion that, generally speaking, it is better to rely upon naturalistic data. Indeed, he insists that 'interviews are particularly useful for in-depth idiographic studies exploring how participants are making sense of experiences happening to them' (Smith 2005: 311). He concludes that while such research requires careful attention to the interview process, this is no barrier to drawing conclusions about people's perspectives and lives.

The second commentator also accepts some of Potter and Hepburn's arguments but insists, once again, that they privilege a particular approach. However, where Smith more or less accepts the value of that approach, Hollway (2005) suggests that it is preoccupied with 'the most inconsequential and least important' (Hollway 2005: 313) aspects of social interaction. Drawing on psychoanalysis as part of a psycho-social approach, she argues that the kind of work done by Potter and Hepburn neglects the role of what she calls 'subconscious positioning' (Hollway 2005: 312). As part of this, she claims that 'the meaning of any part of an interview (or conversation) inheres in the whole, so that extracts of text can never function satisfactorily as whole units for analysis, only as selected bits of evidence (and counterevidence, where appropriate) for an argument that is being constructed bearing in mind this larger whole' (Hollway 2005: 312). She also criticizes Potter and Hepburn for assuming that there is some single form of transcription, namely that used by conversation analysts, which fully captures the nature of particular stretches of social interaction. She argues that different sorts of transcript are required for different theoretical purposes, to bring out different aspects of social process. In particular, what she is objecting to is the way in which, through the kind of

transcription that Potter and Hepburn recommend, the focus of inquiry is narrowed to what is 'observable'.

By contrast with the other two commentators, Mishler (2005) dismisses most of Potter and Hepburn's argument as a form of 'naïve realism', 'positivism' or 'behaviourism', claiming that in this respect it is character-istic of recent developments in conversation analysis, which has become transformed into a technical enterprise.[7] Like Hollway, he challenges the assumption that there is 'some atheoretical way of transcribing that will provide a "full", i.e. complete and accurate transcription […]' (Mishler 2005: 317). Indeed, he argues that it may sometimes be necessary to reduce the amount of detail in a transcript to bring into relief the features that are relevant to a particular study.[8] On the basis of this, he questions the requirements they lay down regarding transcription. And, like the other two commentators, he insists on the value of interviews as a source of data for qualitative research, arguing that the problems these involve must be addressed and dealt with in practical terms, not on the basis of abstract methodological argument.

Interestingly, Mishler's use of interviews in his own research, employing what he calls narrative analysis, is close in some important respects to what Potter and Hepburn recommend (Mishler 1991 and 2004). He too has complained, for example, about how in much qualitative analysis using interviews 'the sequence and patterning of successive exchanges is deleted from the analysis' (Mishler 2004: 23). And his work involves 'the use of a systematic transcription procedure that represents paralinguistic features of speech and the interaction between speakers'. Furthermore, he includes transcripts in the text so as to provide readers 'with the evidence on which I base my interpretations' (Mishler 2004: 20). However, there is at least one key respect in which he differs from Potter and Hepburn: he believes that through attention to the narrative structure of interview data it is possible to learn about the lives of the people interviewed and how these have been shaped by social circumstances. In other words, he does not see a constructionist emphasis on the constitutive role of discourse as blocking the social scientific use of interview data to understand people's

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7 This is a common criticism. For a sophisticated example from within ethnomethodology, cited by Mishler, see Lynch (1993: ch.6).

8 In their article Potter and Hepburn acknowledge the 'power' of this idea but counter that they find the reverse argument 'more compelling' (Potter and Hepburn 2005: 288).

lives. Quite the reverse, he believes that the metaphors and other tropes that they use in interviews can tell us a great deal about what has shaped their lives and how they responded and adapted to this.

In replying to these commentaries, Potter and Hepburn (2005) insist that their critics have not provided an effective response to their arguments. They state that they recognize that different kinds of transcription are appropriate for different purposes. And they dismiss accusations of positivism, and seek to show that their arguments have stronger and more significant implications than the commentators have recognized. They say, for example, that they are not against researchers having different goals from those characteristic of their own work, but insist that satisfying any goal 'is likely to require an understanding of *precisely what the interviewee is saying*' (Potter and Hepburn 2005: 321) and that the sort of transcription they recommend is the best means for achieving this, since it allows attention to be given to the interactional accomplishment of interview talk. In effect, then, they continue to maintain that the kind of detailed attention to 'observable' processes that they themselves practice is (at the very least) an essential prerequisite for any rigorous form of analysis using interview data.

Reflections on the dispute

At face value, what we have in this debate are differences in view about what is required if sound inferences are to be made on the basis of interview data. One source of these is a difference between Potter and Hepburn, on the one hand, and their critics, on the other, in what might be called 'methodological severity' (Hammersley and Gomm 2008): in other words, in judgments about how serious a threat the interactive and co-constructed character of interviews poses to the validity of inferences drawn about perspectives and practices outside the interview context. As we noted, Potter and Hepburn suggest that there is a real danger that the researcher-as-interviewer prompts responses from informants that are largely structured by the analytic framework he or she has adopted. In effect, they are claiming that while qualitative researchers adopt unstructured interviewing to minimize reactivity, in fact the responses they get from informants may be just as strongly shaped by the interviewer and the interview context as those of respondents in structured interviews. Most qualitative researchers would disagree, insisting that there is, at the very least, an important difference in degree here.

At the same time, as we have seen, such 'methodological severity', or empiricism, is not the only or the most significant difference between Potter and Hepburn and their critics. There is also a fundamental discrepancy in assumptions about the nature of the social world and how we can understand it. For Potter and Hepburn, drawing on ethnomethodological conversation analysis, there are neither psychological nor social objects existing in the world that shape human behaviour, or that 'lie behind' the accounts that people provide. All objects are constituted in and through specific interactional processes taking place on particular occasions, whether in interviews or in other settings; they have no existence apart from this.

At root, then, what we have in this particular debate is a clash between very different assumptions about the nature of the social world, what kind of knowledge it is possible for psychological (and social) research to produce, and about the means that would be required to achieve this. However, with the partial exception of Mishler, the fundamental character of the conflict tends to be obscured by both sides.[9]

The critique of urban ethnography

The second dispute we will look at is very different in focus from the first, and in the issues it raises. It was prompted by a lengthy, and highly critical, review (Wacquant 2002) of three ethnographic studies of impoverished African-Americans living in inner-city contexts in the United States, those of Duneier (1999), Anderson (1999) and Newman (1999). The three authors then responded to this critique (Duneier 2002, Anderson 2002 and Newman 2002).[10]

Background to the dispute

To start with, we should note that there is a back-story to this debate. Anderson (2002: 1577) reports that 'long before its publication in the *American Journal of Sociology* in 2002', Wacquant had 'been busy

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9 For one attempt to sketch a mid-position between radical critics of interviewing like Potter and Hepburn and the widespread complacency about its capacities that they rightly criticise, see Hammersley and Gomm (2008).

10 See also the debate over Wacquant's own book *Body and Soul* (2004), in the pages of the journal *Symbolic Interaction*: Adler and Adler (2005), May (2005), Sanders (2005), Wacquant (2005).

distributing his attack around the globe and across the profession'. Even more importantly, this dispute should be seen against the background of previous work (and disputes) in the field concerned. There is a long history of qualitative, especially ethnographic, studies of working class and poor urban communities. For instance, one of the aims of Chicago sociologists in the 1920s and 1930s was to map the character of various parts of their city, inhabited by people in very different material circumstances and with very different backgrounds, characteristics, preoccupations, and attitudes (Bulmer 1984). As part of this, the Chicagoans investigated 'slum' and 'skid row' areas, and since that time there have been many other studies focusing on inner-city, poor, urban communities in other parts of the United States, and elsewhere.

One long-running theme in this body of research has, of course, been about the factors that generate and sustain poverty. Wilson (2009) formulates this in terms of differential emphasis upon structural or cultural factors: in other words, on social factors that affect the situations faced by people (such as restriction to low paid jobs, unemployment, poor quality housing, etc) or on factors to do with differences in how people respond to situations (notably, their local cultures, attitudes, beliefs, knowledge and skills). And discussions about the role of these two types of factor have been preoccupied not just with their relative causal power but also with what are taken to be their political and moral implications. A stress on social structural factors (such as de-industrialisation, punitive government policies, etc) tends to undercut any blaming of poor communities for their circumstances but at the same time may seem to portray them as passive victims. By contrast, emphasizing cultural factors is often viewed as recognizing that people can respond in creative ways to their situations, that they are *not* passive, but also as opening up the possibility of 'blaming the victim'. Such debates have a long history (see Matza 1967).[11]

Much of the debate immediately prior to Wacquant's critique had focused on the concept of an underclass. Wacquant and Wilson (1993) put forward an argument that pointed to the importance of structural factors producing inner-city, black communities that are largely separated

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11 There is no automatic connection between emphasis on one or other of these factors and particular political and social implications: people can, for instance, be portrayed as passive victims of the culture into which they have been socialised. Gomm (2001) has documented the discursive ploys generated by oscillation between these two emphases.

from mainstream society in material terms. However, the notion of an underclass had also been taken up by some social scientists and political commentators who formulated it in cultural terms, sometimes in ways that denied or downplayed the role of structural factors; thereby, it was claimed, blaming these communities for their own impoverishment. Here is Wilson's (1993a: 2) summary of these developments in the United States:

> During the decade of the 1970s, significant changes occurred in ghetto neighbourhoods of large central cities; however they were not carefully monitored or researched by social scientists during the 1970s and early 1980s. [This was because in] the aftermath of the controversy over Daniel Patrick Moynihan's report on the black family [in the late 1960s] scholars, particularly liberal scholars, tended to shy away from researching any behavior that could be construed as stigmatizing or unflattering to inner-city minority residents. [...] Accordingly, [...] the problems of social dislocation in the inner-city ghetto did not attract serious research attention. This left [the field] open to conservative analysts who, without benefit of actual field research in the inner city, put their own peculiar stamp on the problem, so much so that the dominant image of the underclass became one of people with serious character flaws entrenched by a welfare subculture and who have only themselves to blame for their social position in society.

Wacquant's critique

Wacquant (2002: 1469) summarises the three books he is criticising as follows:

> Mitchell Duneier's *Sidewalk* tracks the trials and tribulations of black homeless book vendors and magazine scavengers who ply their trade in a touristy section of Lower Manhattan; Elijah Anderson's *Code of the Street* chronicles the raging battle between 'street' and 'decent' families in the ghetto of Philadelphia; and Katherine Newman's *No Shame in My Game* depicts the gallant struggles of the 'working poor' of Harlem to uphold the hallowed values of thrift, family, and community in the bowels of the deregulated service economy.

From this Wacquant (2002: 1469) moves very quickly into evaluation:

These books assemble a mass of rich and nuanced empirical data variously drawn from firsthand observation, in-depth interviews, life stories, and institutional reports gathered over years of fieldwork conducted individually or in a team. They would have greatly advanced our knowledge and understanding of the ground-level social dynamics and lived experience of urban marginality and racial division in the United States at century's end, were it not for their eager embrace of the clichés of public debate (albeit in inverted form), the pronounced discordance between interpretation and the evidence they offer, and the thick coat of moralism in which their analyses are wrapped, which together severely limit the questions they raise and the answers they give.

Wacquant (2002: 1469–70) fills out these criticisms as follows:

Thus *Sidewalk* proffers a sprawling stockpile of data without any theory to organize it and strives, by default, to bring these data to bear on a crime-and-policing issue that they are ill-suited to address; *Code of the Street* is animated by a thesis, that proximate mentoring makes a difference in the fate of ghetto residents, that is glaringly disconnected from, even invalidated by, its own findings; and *No Shame in My Game* subordinates both observations and theorization to public policy considerations, such as the ideological dispute over 'family values', that are so constricting that it ends up slighting its own discoveries and reading like a business tract in praise of low-wage work.

More significantly, all three authors put forth truncated and distorted accounts of their object due to their abiding wish to articulate and even celebrate the fundamental goodness – honesty, decency, frugality – of America's urban poor. To do this Duneier *sanitizes* the actions and neighborhood impact of sidewalk bookselling by systematically downplaying or suppressing information that would taint the saintly image of the vendors he wishes to project; Anderson *dichotomizes* ghetto residents into good and bad, 'decent' and 'street', and makes himself the spokesman and advocate of the former; and Newman *glamorizes* the skills and deeds of her low-wage workers, extolling their submission to servile labor as evidence of their inner devotion to the country's ordained 'work ethic'. All three authors make the urban poor, and to be more

exact the *black* subproletariat of the city, into *paragons of morality* because they remain locked within the *prefabricated problematic* of public stereotypes and policy punditry, for which it is the only guise under which this subproletariat is deemed 'presentable'.

Identifying the grounds of disagreement

At face value, Wacquant is challenging these studies in terms of the quality of their academic scholarship, in other words according to principles that would be shared by most social scientists. He complains that they did not deploy an adequate theory to organize their data; that their conclusions are not supported by the evidence they provide, and may even be contradicted by it; and that these faults derive, in large part, from bias caused by the fact that the authors set out to challenge prevailing ideas about an underclass – seeking to undercut public assumptions about the immorality of poor, black inhabitants of inner-city areas and to reveal their commitment to 'respectable' ways of life. Most qualitative researchers, and social scientists more generally, would surely want studies to display adequate theorization, conclusions well-supported by evidence, and the avoidance of bias.

The rejoinders by Duneier, Anderson, and Newman also operate within these terms: they are largely concerned with showing that Wacquant has misrepresented their work.[12] Thus, Duneier argues that Wacquant uses selective quotation to produce a misleading impression of what he was claiming in *Sidewalk*. For example, at one point Wacquant refers to 'Duneier's uncritical acceptance of his informants' self-portraits', and provides the following quotation from Duneier in support: 'I have *never* doubted *any* of the things Hakim told me about his life.' (Wacquant 2002: 1478). However, the footnote to Duneier's book that this comes from reads as follows: 'Although I have never doubted any of the things Hakim told me about his life, in conducting this study I have looked upon it as my responsibility to check salient things people tell me about themselves before reporting them.' (Duneier 1999: 360). Thus, Wacquant has added misleading emphasis and excised material that counts against his interpretation. And, Duneier goes on to explain how he asked Hakim to apply for a copy of his college records and to obtain official employment

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12 For reasons of space, I will focus primarily on Wacquant's critique of Duneier and the latter's response to this.

information about the last firm for which he worked, so as to validate what he had claimed; reporting that 'everything checked out' (Duneier 1999: 360; Duneier 2002: 1553). In this and other cases, it appears that Wacquant's critique involves serious misrepresentation.

However, this is not the most significant aspect of this dispute for my purposes here. I suggest that while it may seem as if the protagonists are all engaged in the same enterprise, and are therefore simply disagreeing about how well it has been pursued in particular studies, this is not in fact the case. For example, if we take Wacquant's first criticism, what he means by 'theory', and how he views its role in 'organising the data', are almost certainly at odds with the assumptions about theory and evidence on which Duneier, and perhaps also Anderson and Newman, operate.

'Theory' is a notoriously problematic term. While most, though not all, social scientists are concerned with producing and drawing on theory, and emphasize its importance, in practice they often use the word to refer to different things: from broad methodological philosophies, through comprehensive social theories, to specific explanatory ideas (Hammersley 2012b). Wacquant seems to assume that any ethnographic study should be located within a macro-level theory that identifies the dominant social forces and structural patterns that characterize the wider society. And he also seems to believe that such a theory must precede and structure any empirical research: that the phenomena to be investigated must be theoretically constituted (Wacquant 2005; Bourdieu and Wacquant 1992). A sign of this is his reference to grounded theorizing as an 'epistemological fairy tale' (Wacquant 2005: 1481), the implication being that it is a form of empiricism.

Indeed, Wacquant had already put forward a theoretical framework for the study of inner-city black communities (see, for example, Wacquant 1997 and 2008). It is largely on the basis of this that he accuses Duneier, and his fellow ethnographers, of neglecting wider structural forces in favour of focusing on local cultural factors. He charges them with 'blindness to issues of class power' and a 'stubborn disregard for the deep and multisided involvements [...] of the state in producing the dereliction and human wretchedness they sensibly portray [...]' (Wacquant 2002: 1470). Specifically in the case of Duneier, Wacquant claims that he 'does not discuss the structural forces – the desocialization of labor, the erosion of the patriarchal household, the retrenchment of the welfare state, the criminalization of the urban poor, the conflation of blackness and dangerousness in public space – that directly shape and bound the material and

symbolic space within which the vendors [studied by Duneier] operate'
(Wacquant 2002: 1480).

As we saw in Chapter 2, this emphasis on the structuring role of macro
theory is a central feature of a 'critical' orientation, and this is more or
less the stance that Wacquant adopts. Another closely related feature
is the assumption that the commonsense ideas prevalent within a soci-
ety are likely to be ideological – in other words, both false and serving
regressive social interests. For this reason, these ideas should not be used
as a framework for social analysis, they themselves must be challenged
and explained. And it is a key part of the task of theory to do this. Thus,
Wacquant criticizes Duneier and the others for their 'naïve acceptance
of ordinary categories of perception as categories of analysis' (Wacquant
2002: 1470). He asks: 'Why does Duneier swallow whole the sing-song
claim of his subjects that they "made a conscious decision to 'respect'
society by scavenging trash or panhandling (instead of breaking into
parked cars or selling drugs)" […]?' (Wacquant 2002: 1481).[13] As this makes
clear, Wacquant, like Potter and Hepburn, does not believe that people are
'experts on their lives', although for very different reasons from them.

By contrast, Duneier seems committed to a rather different meth-
odological approach, where the starting point is a particular research
topic and the selection of a specific location or group of people that
allows its investigation. Here, theoretical ideas play the role of tools that
can be deployed and developed *in so far as they make sense of what is
going on and enable us to answer the evolving research questions* (Duneier
2002: 1566–7). Furthermore, for him, the perspectives and experience
of participants are an essential starting point for understanding what is
happening in a setting.

There seem to be several points of difference with Wacquant here. First,
the theoretical resources that Duneier employs are multiple rather than
forming a single, closely-structured framework. Secondly, Duneier regards
selection from and development of the theoretical resources available
to the researcher as an inductive process, with what is discovered in the

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13 Wacquant's work has been strongly influenced by the work of Bourdieu (see
Bourdieu and Wacquant 1992). However, Cole and Dumas (2010) argue that the
relationship between analytic and commonsense perspectives is treated as more
complex in the book that Bourdieu and Wacquant produced together, and in
Bourdieu's work generally, than it is in Wacquant's critique of Duneier, Anderson and
Newman.

field playing a guiding role. Thirdly, the concepts and understandings of the people studied are treated as potentially providing key information and insights, even though they are not to be adopted uncritically. Thus, for Duneier, any wider picture of how societal forces are affecting the setting investigated must be generated out of detailed study of it and of the perspectives of participants. In short, a prior comprehensive theory is not required, indeed it is not desirable since it could operate as a set of blinkers. Instead, the relevance and validity of any explanatory ideas must emerge from detailed empirical investigation of the particular case.

Thus it is not that Duneier ignores structural conditions in *Sidewalk*, but he does place primary emphasis on local factors, seeing these as playing a significant role in mediating wider social forces, so that the relevance and effects of the latter can be discovered only through local investigation. Moreover, he believes that much is to be learned by the ethnographer from the experience and perspectives of the people being studied. What is at issue here, then, is not differential emphasis on cultural or structural factors, but rather differences in assumptions about the nature and role of theory, about the relative emphasis that should be given to macro and micro factors, about how these are related to one another, and about the proper relationship between analytic and commonsense understandings.

As this should make clear, Duneier's approach shows strong signs of the influence of interpretivism, and *Sidewalk* was, indeed, largely focused on documenting and explaining the experience of a specific group of people. Indeed, he places particular emphasis upon what he calls 'showing the people'. He has criticized much previous qualitative research for neglect-ing this:

> If you are going to get at the humanity of people, you can't just have a bunch of disembodied thoughts that come out of subjects' mouths in interviews without ever developing characters […]. (Duneier and Back 2006: 554).

In line with this idiographic orientation, Duneier includes photographs in his book, and uses people's real names, where they have agreed to this. He writes:

> Being able to show the people in photographs, rather than keeping them anonymous, lends a certain kind of immediacy to the people as people and makes it possible to really conceive of them as full human beings [...]. (Duneier and Back 2006: 554)

Interestingly, one of the ways in which Wacquant (2002: 1525) formulates his criticisms of Duneier is to accuse the latter of recognizing no 'epistemo-logical divide' between sociological research and journalism. In response, Duneier acknowledges that there are some similarities, singling out a concern with checking evidence, but he insists that there are important differences:

> My ethnography […] has other commitments that most journalists do not share: being public about procedures and clear about uncertainties, presenting alternative interpretations and counterevidence, considering rival hypotheses, striving to achieve replicability, seeking to be aware of investigator effects, and using fieldwork to modify and improve theory.

As we have seen, Wacquant also criticises Duneier and the others for 'moralism'. What this amounts to, in large part, is the claim that the account of the lives of street vendors that Duneier provides is biased: that it de-emphasises or reinterprets those aspects of their lives that would generally be regarded as morally unacceptable, and that it gives undue attention to those that are taken to show the vendors in a good light. This is a charge that Duneier vigorously denies.

What is of interest here is that this criticism could be taken to imply that Wacquant is committed to documenting the workings of society from a non-moral or value-neutral perspective. Yet this is far from the case. What he is objecting to is what he sees as the failure of Duneier and the other ethnographers to locate their work within the moral framework provided by the sort of comprehensive theory that he believes is necessary. Thus, Wacquant certainly does not resist engaging in evaluation himself: he criticizes state policies and the structural inequalities built into US society, for example the way in which the inhabitants of black inner-city commu-nities are restricted to 'lousy' jobs (Wacquant 2002: 1472) or deprived of any legitimate income at all. And, elsewhere, he has himself provided a highly value-laden view of the lives of African-Americans in black ghettoes and of the social forces 'incarcerating' them there (Wacquant 2009).

This might seem to imply that what is at stake is a clash of substan-tive value perspectives about US society. Yet it is not clear that the two authors differ very greatly in terms of their substantive values. The real difference between them can be clarified by reference to a very influential article by a researcher whose position is quite similar to that of Duneier. As we saw in Chapter 1, Becker's (1967) 'Whose side are we on?' is frequently

cited as suggesting that social research should be partisan: that it must support some particular social group or political position against others, more or less in the manner of 'critical' research. However, a careful reading of Becker's article makes clear that this is not his view (see Hammersley 2000: ch.3). He argues that the researcher's primary commitment should be to document what is actually happening in the social world, rather than setting out to criticize it. He argues that this requires strenuous effort to avoid taking over the perspectives that are dominant within a society, *and* to take seriously the experience and perspectives of those who are at the bottom of the 'hierarchy of credibility' within that society. At the same time, he insists on the importance of avoiding what he calls 'sentimentality', one aspect of which would be a refusal to investigate issues that the researcher finds politically or ethically uncomfortable, for example whether the behaviour of marginalized groups matches mainstream stereotypes of them. Becker writes:

> Whatever side we are on, we must use our techniques impartially enough that a belief to which we are especially sympathetic could be proved untrue. We must always inspect our work carefully enough to know whether our techniques and theories are open enough to allow that possibility (Becker 1967: 246).

While Becker assumes that research following this pattern will have politically progressive social consequences, he does *not* believe that researchers should set out to support any particular side, or indeed that they should act directly to promote such consequences. This seems to be Duneier's position too. Certainly, he is primarily concerned with documenting rather than evaluating the behaviour of the people he studied. And he is keen to avoid conventional assumptions biasing his account. While he clearly hoped that his research would dispel stereotypes and thereby lead to better policies, it was not directly geared to bringing about these changes, but rather to providing a true account of the vendors' lives and of the factors that shape them.[14]

In other words, Duneier seeks to maintain some detachment for his research from political struggles over racial discrimination and poverty. By

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14 His 'moralism', to the extent that this label applies, amounts to an interest in how people relate to moral values, and a concern to establish a relationship with the people he was studying that was significantly more egalitarian than in some previous ethnographies, see Duneier and Back (2005).

contrast, 'critical' researchers deny that there is any scope for such 'detachment', they insist that, whether researchers are aware of it or not, their work always amounts to some kind of political intervention. Furthermore, Wacquant seems to assume not only that high quality research will have desirable political effects but also that any piece of research having what he takes to be undesirable political implications must be false. Thus, for him there is an intrinsic connection between what he sees as methodological defects in Duneier's work and the political implications he believes it carries (see Loader and Sparks 2010: 406–9). His fundamental complaint is that it serves to reinforce the prevailing ideological framework in terms of which urban poverty is publicly discussed. He writes: 'One could hardly formulate a better brief for continuing the state policies of urban abandonment, social disinvestment, workfare, and "prisonfare" that have spawned the mounting social refuse strewn on the streets of the U.S. metropolis.' (Wacquant 2002: 1485–6). He argues that these ethnographic accounts distract attention from the fundamental causes of poverty, in effect suggesting that if poor people would only take proper responsibility for their lives they could work their way out of poverty.

So, the key difference here concerns the role that Wacquant sees social research as playing and that he believes it should play. This is not just a matter of what role researchers might adopt, in terms of how they define the task of inquiry, but also of the role that is, if you like, thrust upon them: what is involved is not simply the intentions of the ethnographers whose work he is discussing, but rather what objective function their work serves in the wider society. Any idea of detachment seems to be rejected by Wacquant, as both impossible in practice and unethical as an ideal.

This is illustrated at one point where he refers to a previous debate from the 1960s, in which the work of Becker and some other Chicago-School sociologists of the time was criticized. He writes that:

> just as the romantic ethnographies of the cool, the marginal, and the lowly produced during the progressive sixties in the style of the second Chicago School were organically tied to the liberal politics of America's semi-welfare state and its then-expanding "social-problems complex" (Gouldner 1973), the neo-romantic tales spun by Duneier, Anderson, and Newman at the close of the regressive nineties suggest that U.S. sociology is now tied and party to the ongoing construction of the neoliberal state and its

> "carceral-assistential complex" for the punitive management of the
> poor, on and off the street (Wacquant 1999: 83–94) (1470–1).

Here Wacquant is aligning his critique of the work of Duneier, Anderson,
and Newman with Gouldner's criticisms of the work of Becker and others
in an earlier period. For Gouldner, as for Wacquant, social research must
be understood as playing a reflexive role within the wider society: unless
it is specifically directed at resisting wider social forces it will inadvertently
serve those forces, irrespective of whether this was intended by the
researchers concerned.

So, it seems that Wacquant believes that the primary responsibility of
the researcher is to orient her or his work in relation to the current politi-
cal situation in such a way that it will have desirable political implications
and consequences. His main objection to the research he criticizes is that
it does not do this, and therefore reinforces the socio-political status quo.
However, he believes that this is intimately related to the methodological
defects that he sees in this research, in the sense that only if research has
this orientation, operating within an appropriate theoretical framework,
will it be methodologically sound.

What this shows is that the methodological criticisms that Wacquant
makes of these studies, which are the main focus of the three authors'
responses to him, are secondary to more basic disagreements between
them. Like Potter and Hepburn in the first dispute I discussed, Wacquant is
laying down a more severe set of criteria regarding what should be treated
as reliable evidence and inference, and one that relies upon distinctive
assumptions about the proper nature of inquiry; though, of course, his
assumptions are very different from theirs. His critique is based upon a
notion of 'critical' sociology as a discipline that is both scientific and politi-
cally engaged, these two aspects being closely interrelated (see Bourdieu
and Wacquant 1992: 47).[15]

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15 However, it seems to me that his assumptions about the nature of this relationship
are rather unclear. As we saw in Chapter 2, for Marx the connection between science
and politics lay in its capacity to identify the potential for realising genuine human
nature and the barriers to achieving this. It is not clear whether Bourdieu and Waquant
accept this or rely upon some alternative means of deriving 'ought' from 'is'. In my
view none of the proposed strategies for doing this are successful. For discussion
of the sophisticated attempt to solve this problem proposed by critical realists, see
Hammersley (2009).

We can see here, then, that this dispute, like the previous one, is under-pinned by some quite fundamental philosophical differences: concerning the purpose and character of social research, the nature of social scientific knowledge and how it can best be produced, the role of political values in the research enterprise, and (following on from this) the social role and political responsibilities of the researcher.[16]

Conclusion

In this chapter we have looked at two important disputes among qualita-tive researchers that are very different in focus and in the issues they raise, as well as in the sorts of approach championed by the protagonists. Yet, what both these disputes show, I suggest, is that qualitative research is currently riven by a range of fundamental differences in methodological philosophy and practical orientation, albeit ones that are often partly obscured through appeals to common issues. Thus, Potter and Hepburn seek to persuade researchers of all kinds that there are serious problems with using interviews, appealing to what they present as common ground. However, they base their arguments on assumptions about the nature of psychological and social phenomena, and how we can understand them, that are informed by radical constructionism, and at the same time by an empiricism which demands that any analysis is tied down to the observable details of interaction. These assumptions are sharply at odds with those adopted by many other qualitative researchers, including the commentators on their article.

Similarly, Wacquant appeals to various kinds of inaccuracy and meth-odological weakness in the studies he reviews. Yet his critique is motivated in large part by a 'critical' orientation that, it seems to me, the people whose work he is reviewing do not share. He insists on the importance of a general theory about the nature of US society that provides him with a framework within which both to describe and explain the behaviour of African-Americans who live in inner-city communities, *and* to evaluate the various structural forces and agents involved. And he insists that socio-logical work must be directed at challenging existing society so as to bring

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16 By contrast with the approach I have taken here, Cole and Dumas (2010), while recognising important differences between Wacquant and Duneier, emphasise what they see as similarities.

about change, and above all it must not reinforce the dominant ideology. However, those whose work he is criticizing do not fully share this view of the character and role of social research, even if they have similar values.

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