2 Deconstructing the qualitative-quantitative divide

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Introduction

In this chapter I want to challenge the widely held idea that there are two methodological paradigms in social research: the quantitative and the qualitative. This idea seems to have become a matter of consensus over the past few years among many who see themselves on one side of this divide or the other (and even among some who wish to sit astride it). I shall argue, however, that the distinction between qualitative and quantitative is of limited use and, indeed, carries some danger.

It is striking how prone we are to the use of dichotomies, and how these often come to represent distillations of all that is good and bad. Certainly, 'qualitative' and 'quantitative' are sometimes used to represent fundamentally opposed approaches to the study of the social world, one representing the true way, the other the work of the devil. But even where the evaluative overtones are absent and the two approaches are given parity, the distinction is still misleading in my view because it obscures the breadth of issues and arguments involved in the methodology of social research.

In one form or another, the debate about quantitative and qualitative research has been taking place since at least the mid-nineteenth century. At that time there was much argument about the scientific status of history and the social sciences, with quantification often being seen as one of the key features of natural science.[2] Similarly, in US sociology
in the 1920s and 30s there was a dispute between advocates of case study and of statistical method. Many of the claims made about quantitative and qualitative method today have their origins in these earlier debates (see Hammersley, 1989a). By the 1940s and 50s in sociology, psychology and some other fields, quantitative method (in the form of survey and experimental research) had become the dominant approach. But since the 1960s there has been a revival in the fortunes of qualitative types of research in these disciplines, to the point where their legitimacy is widely accepted.[3] In some areas this has led to a détente (Rist, 1977; Smith and Heshusius, 1986) and to increased interest in the combination or even integration of quantitative and qualitative. But such talk still preserves the dichotomy. And it seems to me that in some respects détente is worse than cold war. In learning to live and let live there is the danger that we will all quietly forget the methodological disagreements that we should be tackling.[4]

What I am recommending, then, is not that we should revert from two paradigms to one, in such a way as to deny the variety of ideas, strategies and techniques to be found in social research. Quite the reverse. My aim is to show that this diversity cannot be encapsulated within two (or, for that matter, three, four or more) paradigms. Nor should the variety of approach be regarded as stemming simply from fundamental philosophical or political commitments. Arguments about the latter are, and should be, important in methodology. However, they are not the only considerations that are significant; the particular purposes of the research and the practicality of various strategies given the circumstances in which the inquiry is to be carried out are others. Nor do philosophical and political assumptions have the sort of determinate implications for method that they are sometimes assumed to have.[5]

What I want to do in this chapter is to identify the various component meanings of the qualitative/quantitative distinction, particularly as used by advocates of qualitative research. I shall argue that these issues are not as simple or as closely related as is sometimes believed. I have identified seven such issues here. There may be others, but these are probably the main ones:

1. Qualitative versus quantitative data.
2. The investigation of natural versus artificial settings.
3. A focus on meanings rather than behaviour.
4. Adoption or rejection of natural science as a model.
5. An inductive versus a deductive approach.
6. The identification of cultural patterns as against seeking scientific laws.
7. Idealism versus realism.

**Qualitative versus quantitative data**

In their book on qualitative data analysis, Miles and Huberman characterize the distinction between qualitative and quantitative research in terms of the use of words rather than numbers (Miles and Huberman, 1984, p.15). While it is rare to find such an interpretation spelled out so clearly, it seems to underlie much talk of qualitative methods. And it is true that research reports do differ sharply in the extent to which tables and statistical analysis, on the one hand, and verbal presentations, on the other, predominate. At the same time, a large proportion of research reports (including many that are regarded as qualitative) combine the two, to varying degrees. More importantly, though, the fact that this is not a very satisfactory basis for the distinction between qualitative and quantitative is illustrated by an exchange that took place in US sociology in the 1930s, between Znaniecki (an advocate of case study) and George Lundberg (a positivistic supporter of statistical method). Znaniecki had written a book in which he largely dismissed the use of statistical techniques in sociology. Here is how Lundberg replies to him:

The current idea seems to be that if one uses pencil and paper, especially squared paper, and if one uses numerical symbols, especially Arabic notation, one is using quantitative methods. If, however, one discusses masses of data with concepts of 'more' or 'less' instead of formal numbers, and if one indulges in the most complicated correlations but without algebraic symbols, then one is not using quantitative methods.

A striking illustration from a recent book by a prominent sociologist will make the point clear. After a discussion of the lamentable limitations of statistical methods, the author appends this remarkable footnote: 'Wherever the statistical method definitely gains ascendancy, the number of students of a high intellectual level who are attracted to sociology tends to fall off considerably' (Znaniecki, 1934, p.235). In short, this author finally reverts to a statistical proof of the deplorable effects of statistics. (Lundberg, 1964, pp.59-60).
It has frequently been pointed out that ethnographers regularly make quantitative claims in verbal form, using formulations like 'regularly', 'frequently', 'often', 'sometimes', 'generally', 'in the main', typically, 'not atypically' etc. And it is fairly obvious, I think, that (as Lundberg indicates) the form in which such claims are made makes no difference to their character.

The contrast between words and numbers does not get us very far, then. But there is an important sort of variation in the nature of data that is not unrelated to the word/number contrast. When quantitative researchers criticise ethnographers' use of words rather than numbers what is usually at issue is precision. They are arguing that ethnographers are insufficiently precise in their claims, and that the necessary precision requires quantification.

However, we must ask what precision is, and whether the most precise formulations are always the best; or, indeed, whether they are always necessary. And I think it is clear that precision does not necessarily mean numbers. For example, where we are concerned with the presence or absence of a particular type of phenomenon in a situation, this can be described quite precisely without the use of numbers. It is also important to remember that precision is not the only virtue in description and measurement. Accuracy is usually even more important. And it is widely recognized that we should not express our findings in terms that imply a greater degree of precision than their likely accuracy warrants. For instance, to report findings to six figures of decimals is rarely if ever justified in social research. It follows from this that sometimes it may not be legitimate to use terms that are more precise than 'sometimes', 'often', 'generally' etc.[6] Handlin (1979, pp.11-12) provides an illustration of this from history:

I cannot wholly agree that historical problems that hinge on the question 'how many?' are always better solved by numerical answers. The more precise statement is not always the more accurate one. In 1813 John Adams tried to estimate how many colonists were for independence and hazarded various guesses - nearly equally divided; a third; five to two. It would no doubt be more precise to be able to say 39 per cent were for, 31 per cent against, and 30 per cent neutral, or to plant a good solid decimal point with a long series of digits behind it. But it would be less accurate to do so, for the data does not support that degree of refinement.
Furthermore, while increased precision may often be of value, it is not always so. It may not be of value because the level of precision already achieved is sufficient for our purposes, or because the likely costs of achieving greater precision are greater than the probable benefits. The latter is an especially important point in the context of case study research, where a relatively wide focus is adopted. Given fixed resources, the attempt to make any part of the picture more precise will necessarily tend to reduce the width of focus that is possible. The researcher must judge whether the benefits of this outweigh the costs, and sometimes they will not. Equally, though, on other occasions they will; and more precise, even numerical, descriptions will be appropriate.[7]

We are not faced, then, with a stark choice between words and numbers, or even between precise and imprecise data. Furthermore, our decisions about what level of precision is appropriate in relation to any particular claim should depend on the nature of what we are trying to describe, on the likely accuracy of our descriptions, on our purposes, and on the resources available to us; not on ideological commitment to one methodological paradigm or another.

Investigation of 'natural' versus 'artificial' settings

A second interpretation of the qualitative/quantitative distinction focuses on the nature of the phenomenon investigated: whether it is 'naturally occurring' or has been created by the researcher. The sharpest contrast here is between experiments and ethnographic research. The former involves study of a situation especially established by the researchers, probably using volunteer subjects, and designed to capture different values of some theoretical variables while controlling relevant extraneous variables. Ethnographic research, on the other hand, requires the study of situations that would have occurred without the ethnographer's presence, and the adoption of a role in that situation designed to minimise the researcher's impact on what occurs. In common parlance, experimenters study 'artificial' settings, while ethnographers study 'natural' settings; and the implication is that only the latter is worthwhile because it is 'natural' behaviour we are concerned to understand.

The charge of artificiality may also be directed at formal, structured interviews of the kind used by survey researchers. These may be contrasted with unstructured and/or informal interviews, where the interviewer plays a less dominant role. While the latter do not represent
an entirely 'natural' setting, it is often argued that their closeness to ordinary conversation renders them approximations to the natural.

In my view this distinction between natural and artificial settings is spurious. What happens in a school class or in a court of law, for example, is no more natural (or artificial) than what goes on in a social psychological laboratory. To treat classrooms or courtrooms as natural and experiments as artificial is to forget that social research is itself part of the social world, something that should never be forgotten.

Once again, though, there is an important issue implicit in this distinction. What is involved is variation in the degree to which the researcher shapes the data. There is a trade-off between, on the one hand, trying to make sure that one gets the relevant data (in the most efficient manner possible) and, on the other hand, the danger of reactivity, of influencing the people studied in such a way that error is introduced into the data. It has long been a criticism of experiments that their findings do not generalize to the 'real world' (that is to non-experimental situations) because people's behaviour is shaped by their awareness of the experimental situation, and by the personal characteristics of the experimenter (or her/his assistants). Similarly, structured interviews have been criticized because we cannot generalize from what is said in them to what is said and done elsewhere.[8]

However, while there is some truth in these arguments, they by no means render the results of research using 'artificial' methods of no value. Much depends on whether the reactivity affects the results in ways that are relevant to the research topic and in ways that cannot be allowed for. All research is subject to potential error of one kind or another. Indeed, even ethnographic research in 'natural' settings is not immune to reactivity. While the ethnographer may strive to minimize her or his effects on the situation studied, no one can guarantee this; and sometimes the effects can be significant despite the researcher's best efforts. Also, we must remember what the significance of reactivity is: it makes the setting investigated unrepresentative of those about which the researcher wishes to generalize, an issue sometimes referred to as ecological invalidity. But reactivity is not the only source of ecological invalidity. Even without reactivity, a natural setting can be unrepresentative because it differs in important ways from most other cases in the same category. Simply choosing to investigate natural settings, and seeking to adopt a low profile in them, does not ensure ecological validity.[9]
Considerations using multi-methods

For these reasons the distinction between natural and artificial settings does not provide a sound basis for the qualitative/quantitative distinction. The terms 'natural' and 'artificial' have misleading connotations. And while the issue of ecological validity is important, it is not the only important methodological issue. Nor does research in 'natural' settings guarantee ecological validity, any more than research in 'artificial' settings automatically debars us from it.

A focus on meanings versus a focus on behaviour

This component of the qualitative-quantitative distinction emphasizes the interpretive or hermeneutic character of qualitative research. Of all the issues discussed in this chapter, this one links most obviously back to nineteenth century debates about the difference between natural science and history, as well as to twentieth century disputes such as that surrounding behaviourism.

It is sometimes suggested that the central goal of qualitative research is to document the world from the point of view of the people studied (from the native point of view, in Malinowski's terms), rather than presenting it from the perspective of the researcher. And it is true that qualitative research does seek to understand the perspectives of the people studied, on the grounds that this is essential if we are to describe and explain their behaviour effectively. However, it is very rare for qualitative research to restrict itself to documenting the native point of view. And there are good reasons for not doing this; not the least of which is that the people studied can often do this for themselves! Even those approaches that restrict the research focus to participants' perspectives do not simply reproduce these, but seek to analyse their structure and/or production in ways that are likely to be alien to the people studied. This is true, for example, of both ethnosemantics and ethnomethodology. But, as I have said, most qualitative research does not restrict its focus this narrowly. It seeks to describe and explain both perspectives and behaviour, recognizing that the latter does not merely flow from the former, and may even be discrepant with it. Indeed, such ironic discrepancies have been a major focus for qualitative research (see, for example, Keddie, 1971; Sharp and Green, 1975; and Willis, 1977).

Conversely, much quantitative research is concerned with attitudes rather than simply with behaviour. Advocates of an interpretive approach may argue that attitude research effectively studies attitudes as
behaviour displayed in response to interview questions. Yet, critiques of behaviourism emphasise that it is not possible to study human behaviour without attributing meanings, and that behaviourists routinely do this despite themselves. Given this fact, it seems that attitude researchers cannot but be studying 'meanings'. At the very least, this shifts the criticism elsewhere. Moreover, most attitude researchers do not operate on the basis of a strict behaviourism.

It is still true, of course, that there are differences between attitude researchers and qualitative sociologists, both in how they conceptualize the meanings held to underlie behaviour, and in how they seek to identify those meanings. Even here, though, the differences are not as great as is sometimes suggested. It is common for ethnographers to ascribe perspectives or definitions of the situation to the people they study, and it is not clear how these differ in character from attitudes. Ethnographers may stress that they do not assume a mechanical relationship between attitude and behaviour. However, the more contingent is the relationship between perspective and behaviour, the less value perspectives have as explanatory factors. So this is not an argument that ethnographers can pursue very far without undercutting the basis of their own hermeneutic approach.

As regards differences in the approach that attitude researchers and ethnographers employ in identifying attitudes/perspectives, the contrast is between the use of attitude scales and more unstructured approaches. As such, it reduces to the previous two distinctions I have already discussed, and to the distinction between inductive and deductive approaches that I shall deal with below. Here again, though, we do not have a clear-cut distinction between two contrasting approaches.

Natural science as a model

It is common for quantitative method to be criticised for taking natural science as its model. It is worth noting, however, that advocates of qualitative method have sometimes themselves regarded the natural sciences as exemplary. Thomas and Znaniecki, two of the most influential advocates of case study and life history methods in the 1920s and 30s, make the following comment at the beginning of their study of *The Polish Peasant in Poland and America*: 
The marvellous results attained by rational technique (that is, by science) in the sphere of material reality invite us to apply some analogous procedure to social reality. Our success in controlling nature gives us confidence that we shall eventually be able to control the social world in the same measure (Thomas and Znaniecki, 1927, p.1).

Nor were Thomas and Znaniecki unusual in holding this view. While he was uncertain about the chances of its achievement, Herbert Blumer was also committed, at least in the 1920s and early 30s, to the pursuit of a science of society modelled on the natural sciences. Much the same was true in social anthropology. Boas, Malinowski and Radcliffe-Brown all took the natural sciences as a paradigm for their approach to the study of primitive society; though, as in the case of Blumer and the Chicago sociologists, this was tempered with ideas about the distinctiveness of social phenomena (Hammersley, 1989a).

From a historical point of view, then, differences in attitude to natural science do not seem to map on to the distinction between quantitative and qualitative research in a straightforward way. And, even today, there are advocates of qualitative method who justify their approach precisely on the basis of its similarity to that of natural scientists.[10]

What this points to is that the issue of whether natural science is an appropriate model for social research is not a simple one. There are at least three complications.

First, we must consider which natural science we are taking as the model, and during which period of its development? There are significant differences, for example, between physics and biology; and, indeed, within each natural science discipline over time.

Second, which interpretation of the methods of natural science is to be adopted? Keat and Urry (1975) identify positivist, conventionalist, or realistic interpretations of physics; and even these distinctions do not exhaust the variety of views to be found among philosophers of science.

Third, what aspects of natural science method are to be treated as generic? Not even the most extreme positivist would argue that the methods of physics should be applied lock, stock and barrel to the study of the social world. And there are few supporters of qualitative research who would insist that there is no aspect of natural science method that is relevant to social research. What is involved here is a matter of degree. Once again, we have a complex set of considerations that resist reduction to a simple contrast.
Inductive versus deductive approaches

It is common for qualitative researchers to contrast their own inductive approach with the deductive, or hypothetico-deductive, method of quantitative research. Here too, though, we have an over-simplification. Not all quantitative research is concerned with hypothesis-testing. Many surveys are straightforwardly descriptive, and some quantitative research is explicitly concerned with theory generation. Equally, by no means all ethnographers reject the hypothetico-deductive method.[11] Indeed, it seems to me that all research involves both deduction and induction in the broad sense of those terms; in all research we move from ideas to data as well as from data to ideas. What is true is that one can distinguish between studies that are primarily exploratory, being concerned with generating theoretical ideas, and those which are more concerned with testing hypotheses. But these types of research are not alternatives; we need both. Nor need the former be quantitative and the latter qualitative in other senses of those terms.

A common version of the inductive versus deductive contrast is built into advocacy Verstehen or understanding, as opposed to forms of explanation that rely on observation of the external features of human behaviour.[12] In its most extreme formulation, Verstehen involves some kind of direct contact with the experience of others, or a reliving of it on the basis of one’s own experience. Some versions place great importance on the nature of the relationship between researcher and researched, perhaps regarding equality as essential if understanding is to occur. But while there is no doubt that it is important in research to take account of one’s own cultural assumptions and to open them up to possible disconfirmation, the idea that Verstehen involves direct contact with social reality or with other people must be rejected. We can never entirely escape our own assumptions about the world.[13] And even in face-to-face contact with people with whom we share a lot, we are not given knowledge that is necessarily beyond reasonable doubt. As has often been stressed in the ethnographic literature, there are advantages and disadvantages to closeness. On the one hand, it may provide us with inside information that we would otherwise not gain, both about what happens and about people’s experiences of events. On the other hand, through a process of over-rapport we may come to take over false assumptions held by the people we are studying, and become unable to see the world in any other way than that in which it appears to them.
Only if we assume that the perspectives of those we are studying necessarily embody genuine knowledge about the world is over-rapport not a danger. And in my view no individual or group has such a monopoly on truth.

From this point of view, then, we cannot but rely on constructing hypotheses, assessing them against experience and modifying them where necessary. This is true whether we engage in hypothesis testing in a formal, explicit and narrow way that involves subjecting hypotheses to crucial tests; or whether we adopt a more informal approach in which we sacrifice some of the sharpness of the test for a more wide-ranging approach in which we allow more of our assumptions to be thrown open to challenge. Furthermore, which of these approaches is most appropriate depends on our purposes, and the stage that our research has reached, not on paradigmatic commitments.

**Identifying cultural patterns versus pursuing scientific laws**

Following on from the contrast between qualitative and quantitative approaches in terms of a commitment to the model of natural science is the idea that these approaches differ in their goals. Quantitative research is often believed to be committed to the discovery of scientific laws; whereas qualitative research is concerned with identifying cultural patterns. However, as I pointed out in the previous section, much qualitative research is concerned with description rather than with theory development and testing. And, indeed, rather more survey research may appear to be concerned with discovering theoretical laws than is actually the case because survey researchers sometimes fail to distinguish between this goal and that of explaining particular events or relationships.[14]

Similarly, while it is common these days for qualitative researchers to deny the possibility of scientific laws, this was not always so. In the early decades of this century case-study researchers often justified their approach on the grounds that it could produce laws, whereas statistical method could only produce probabilistic generalizations (Blumer, 1928; Znaniecki, 1934). Even today qualitative researchers often claim that their goal is theory rather than the mere description of cultural patterns. And sometimes the concept of theory involved seems to be not far removed from that characteristic of survey research; though it should be said that there is considerable uncertainty about the precise nature of
ethnographic theory (Hammersley, 1992, Chapters 1 and 2). Furthermore, both analytic induction and grounded theorising seem to depend on the assumption of laws. For instance, analytic induction involves reconstructing theories when counter examples are discovered. However, this is only sensible if we assume that theories consist of deterministic laws that apply to all cases. Thus the distinction between identifying patterns and pursuing laws seems to provide little clear basis for the division between quantitative and qualitative methods.

**Idealism versus realism**

At the most abstract philosophical level it has been claimed that qualitative and quantitative researchers are committed to different epistemological positions. A clear example of this argument is to be found in the writings of John K. Smith. He argues that quantitative research is wedded to a realist epistemology in the sense of assuming that true accounts correspond to how things really are and that competing accounts must be judged in terms of whether the procedures adopted ensure accurate representation of reality. By contrast, qualitative method is idealist, he claims, in that it rejects any possibility of representing reality. It recognizes that there may be 'as many realities as there are persons' (Smith, 1984, p.386).

I think it can be shown with little difficulty that this characterisation is inaccurate empirically. First, not all quantitative researchers are realists. Take the following quotation:

In any valid epistemological or scientific sense we must say that the substitution of a Copernican for the Ptolemaic theory of the universe represented a major change in the universe. To say that it was not the universe but our conception of it which changed is merely a verbal trick designed to lead the unwary into the philosophical quagmires of Platonic realism, for obviously the only universe with which science can deal is our 'conception' of it.

What we have here is an idealist account of natural science knowledge in which there is a denial that it can represent some independent reality. But it does not come from a qualitative researcher. It comes from George Lundberg again, positivist advocate of quantitative method in the 1930s, 40s and 50s (Lundberg, 1933, p.309). There was a strong element
of phenomenalism in late nineteenth and twentieth century positivism, and Lundberg’s anti-realism reflects this. By contrast, Herbert Blumer’s influential concept of naturalistic method is quite clearly realist in character: he talks of research being concerned with discovering the nature of social reality, of tearing away the veil of our preconceptions so that we may see it (Hammersley, 1989a). In more recent times, Harré has based his advocacy and practice of qualitative research in social psychology on an explicit realism.[15] And, indeed, the reliance of ethnography on realism has come under increasing criticism, for example from those who stress the creative character of ethnographic writing (see Tyler, 1985 and Clifford, 1988).

More important than the empirical question of whether it is true that quantitative researchers are realists and qualitative researchers idealists, though, is the philosophical issue of whether there is any necessary connection between qualitative method and a particular epistemological position. As I have shown, history suggests that there is little reason to believe that there is such a connection. And we must remember that there are more than two epistemological positions available within philosophy, nor can these be reduced to a single dichotomy without great distortion.[16]

**Conclusion**

In this chapter I have looked at some of the components of the conventional distinction between qualitative and quantitative method. In each case I have argued that what is involved is not a simple contrast between two opposed standpoints, but a range of positions sometimes located on more than one dimension. It should also be clear, I think, that there is no necessary relationship between adopting a particular position on one issue and specific positions on the others. Many combinations are quite reasonable. Furthermore, I emphasized that selection among these positions ought often to depend on the purposes and circumstances of the research, rather than being derived from methodological or philosophical commitments. This is because there are trade-offs involved. For instance, if we seek greater precision we are likely to sacrifice some breadth of description; and vice versa. And the costs and benefits of various trade-off positions will vary according to the particular goals and circumstances of the research being pursued.
What all this implies is that the distinction between quantitative and qualitative approaches does not capture the full range of options that we face; and that it misrepresents the basis on which decisions should be made. What is involved is not a crossroads where we have to go left or right. A better analogy is a complex maze where we are repeatedly faced with decisions, and where paths wind back on one another. The prevalence of the distinction between qualitative and quantitative method tends to obscure the complexity of the problems that face us and threatens to render our decisions less effective than they might otherwise be.

Notes

1. My use of the term 'deconstructing' in the title of this chapter is no more than a rhetorical flourish: my philosophical assumptions are very different from those of deconstructionists. However, given their views about meaning, they can have no justifiable complaint against my theft of this term! For an excellent critique of deconstructionism, see Ellis 1989. See also Dewey, 1987.

2. This debate has persisted within history, indeed it has intensified in recent years as a result of the growth of 'climetrics'. For contrasting perspectives on this development see Fogel and Elton, 1983.

3. For a useful discussion of the current state of this debate, see Bryman, 1988.

4. To this extent I am in agreement with Smith and Heshusius, but I disagree with much of the rest of what they say. In my view the commitment to paradigms, in whatever form, tends to close down the debate rather than keep it open (Hammersley, 1989b).

5. This is illustrated by the debates about methodology among Marxists and feminists. See, for example, the debate about Marxism and method in the Berkeley Journal of Sociology, 35, 1989, especially the articles by Wright and Burawoy. On feminism and method, see the very different views expressed by Mies, Jayaratne, Reinhartz, and Stanley and Wise in Bowles and Klein (1983).
6. My own use of imprecise formulations will not be lost on the reader!

7. There is also the practical question of how much precision is possible. While I would not want to suggest any insuperable barriers to increased precision of measurement of social phenomena, there is no doubt that as things presently stand there are severe practical limits to the level of combined precision and accuracy that can be achieved.

8. In some respects this is a misleading argument since it fails to draw the necessary distinction between, on the one hand, inferring from what people do in interviews to what they do elsewhere, and, on the other, the question of the truth of what people say in interviews about what they and others do elsewhere.

9. Equally, it is worth noting that some quantitative researchers carry out their research in natural settings, notably in the form of systematic observational research.

10. See, for example, the work of Harré: Harré, 1970; Harré and Secord, 1972.

11. On exploratory quantitative analysis, see Baldamus, 1979 for an example; Erickson and Nosanchuk, 1979 for techniques. Some practitioners of analytic induction (such as Lindesmith, 1937) and of grounded theorizing (Strauss, 1987) explicitly equate their approach with the hypothetico-deductive method.

12. Platt (1985) points out that Weber's discussion of Verstehen seems to have had little influence on early qualitative researchers. However, Weber drew the concept from earlier nineteenth century discussions, notably those of Dilthey, and these did have an influence on Chicago sociologists; both directly, and indirectly, through Windelband, Rickert and Simmel, for example. It also seems likely that Cooley, who was quite influential on the Chicagoans, drew his concept of sympathetic introspection from the German Romantics.
13. This is the conclusion of more sophisticated versions of hermeneutics, notably that of Gadamer.

14. On this distinction, see Hammersley, 1992, Chapter 2.

15. See Note 7.

16. For a development of the argument in this section dealing specifically with Smith’s position, see Hammersley, 1989b.

References


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