

## 4.2 Hypotheses and Prior Knowledge in Qualitative Research

Werner Meinefeld

1	Hypotheses in quantitative and qualitative methodology: an opposition	153
2	Doing without hypotheses as a result of the profiling of methodological position	154
3	Recent discussion	155
4	Starting points for a re-orientation of methodological positions	156
5	Conclusion	157

Methodological justifications of the particular perspective of qualitative research often insist that it is developed in strict separation from the rules of a methodology that aims at standardization and quantification. In view of the dominance and the fully developed state of research in quantitative methods this is not surprising: if this kind of presentation is not merely a question of didactics, but also concerns positioning as to content, then there will also arise out of this negatively based self-definition problems in the realization of specifically qualitative research goals. Both self-location by means of exclusion and latent negative results can be seen particularly distinctively by the way hypotheses are handled in qualitative methodology.

### 1 HYPOTHESES IN QUANTITATIVE AND QUALITATIVE METHODOLOGY: AN OPPOSITION

For quantitatively oriented methodologists the formulation of hypotheses at the *beginning* of an investigation is an indispensable means of subjecting to systematic control the inevitable theoretical loading of every kind of observation

and the unavoidable selectivity of every kind of research. In the first place it is considered obligatory to reveal the researcher's prior knowledge and thereby to control it. Secondly, an explicit link is made to the state of available knowledge and a contribution is made to the integration and cumulation of this knowledge. And thirdly, the time-sequencing, and the separation of data collection and data analysis, require a *prior* elaboration of the theoretical framework, since this defines and restricts the stages in the research and also means that no correction of operational procedures is possible during the data collection, because of the strict phasing of the research process.

Although in qualitative methodology the fact of theory-driven observation is also unquestioned, there is a predominant rejection of hypotheses formulated in advance: precisely because there is an awareness that knowledge influences observation and action, researchers wish to avoid being 'fixed' by the hypotheses on particular aspects that they can only obtain 'in advance' from their own area of (scientific and everyday) relevance, but whose 'fit' with the meaning patterns of the individuals being investigated cannot be guaranteed in advance. In place of the requirement to reveal prior knowledge in the form of hypotheses, therefore,

in qualitative methodology there arises a requirement for a suspension of this prior knowledge in favour of the greatest possible openness to the particular meanings and relevances of actors – an openness that is seen as being endangered by the prior formulation of hypotheses.

The basic problem – the influence of prior knowledge on observation – is therefore seen from both sides, but the chosen strategy for solving it is aiming in a different direction, since the implications associated with it are constantly being given a different weighting. If, in quantitative methodology, the need for control of the researcher and the conscious structuring of research activity are in the foreground (while the agreement of the theoretical categories with the meaning patterns of actors is seen as relatively unproblematic), qualitative methodologists require primarily a guarantee of the appropriateness of the categories used by the researcher and an openness to the potential 'other' of the research field (and see control of the investigator by means of methodological rules as a false 'solution').

## 2 DOING WITHOUT HYPOTHESES AS A RESULT OF THE PROFILING OF METHODOLOGICAL POSITION

The programmatic opposition that we have sketched of these two responses to the fundamental epistemological problem appears, from a historical viewpoint, to be less 'naturally given' than a result of the growing competition between two methodological approaches.

In the classic studies of empirical social research we find no explicit treatment of the problem of checking prior knowledge nor even of the problem of prior formulation of hypotheses (cf., for example, the studies of William I. Thomas and Florian Znaniecki, William F. Whyte, Howard S. Becker or Paul F. Lazarsfeld).<sup>1</sup> As far as practice in dealing with 'prior knowledge' and 'hypotheses' is concerned, it is clear to what a great extent the research activity was oriented particularly to the theoretical but also to the everyday knowledge of the researchers, and how greatly these ideas determined the results of their work, by first making possible the collection and structuring of the data material. Conversely, it may

be shown that a formulation including specified hypotheses at the beginning of such studies would often have been completely impossible; Whyte and Blanche Geer, for instance, point out explicitly how completely their research question changed after the 'first days in the field', and how they had to adapt it to the peculiarities and possibilities of their object of investigation (Whyte 1955: 317ff., 320ff.; Geer 1964: 340).

With regard to making this prior knowledge explicit in the form of hypotheses, in the course of working out these two methodological positions – which see themselves as alternatives – during subsequent decades a growing process of contrast and reciprocal delimitation may be distinguished in the methodological procedures that are felt to be necessary and sensible. These then resulted in the opposing research strategies which we sketched in the first section. On the side of qualitative methodology the programmatic work of Barney G. Glaser and Anselm L. Strauss (1967), *The Discovery of Grounded Theory*, is of particular significance. Here the authors expressly require that researchers free themselves voluntarily of all prior knowledge and even dispense with prior reading of theoretical and empirical studies in their subject area, in order to embark upon the research field in as unprejudiced a way as possible. According to this approach, the task of empirical research is not (or at least not primarily) to subject to empirical testing the hypotheses that are systematically derived from 'grand' (armchair) theories, since such theories often did not 'fit' the situations that were to be investigated in concrete cases; empirically based general theories are rather only to be expected when researchers personally derive their categories from the data (see 2.1). The formulation of a sociological theory, therefore, should take place not at the beginning of the research process but at the end; the overriding goal of social research is not the testing but the generation of theories (1967: 1–18).

The position developed by Glaser and Strauss in deliberate rejection of the 'mainstream' in empirical social research (see 2.1) showed itself in the reception of qualitative methodology – over and above the grounded theory approach – as extremely influential: a number of authors saw the requirement for doing without hypotheses as a precondition for an interpretative

type of social research and elevated the 'openness' of its methodology to a core belief in qualitative research (e.g. Hoffmann-Riem 1980: 345f., Lamnek 1995: 22f., 139f.). With this shift of attention from *ex-ante* hypotheses to those arising during the research process, the fundamental epistemological problem of checking the prior knowledge which the researcher brings to the job was relegated to a background position. It was believed that this aspect could be overlooked, not least because the very openness of the methods made possible a correction 'by the field': 'unsuitable' prior knowledge would be exposed as such in the course of the study. But even if one concedes the different degree of openness of the various methods, this argument overlooks the fact that even the first setting up of data is already an active undertaking on the part of the researcher and is based on the individual's research interest and prior understanding. The requirement for an 'unconditional as possible' an entry into the field conceals precisely this basic setting up of the field in accordance with the researcher's 'available prior knowledge' at this particular moment. Discoveries about social phenomena do not 'emerge' on their own: they are from the outset constructs of the researcher. The idealization of the 'unprejudiced nature' of the researcher that is sometimes to be found in qualitative methodology, and the idea of a 'direct' record of social reality, are therefore untenable from an epistemological viewpoint (cf. Meinefeld 1995: 287–294).

If we consider this from a distance, it is striking that this methodological idealization is both in contradiction to one of the core theoretical principles of qualitative research ('the interpretation of a situation depends on knowledge') and also not a true reflection of research practice. Glaser and Strauss, in their study *Awareness of Dying*, which appeared in 1965, openly acknowledge their reliance on prior knowledge of this subject area (1965b: 286ff.).<sup>2</sup> One explanation for this discrepancy between theoretical insight, practical research and methodological norm might be sought in the concern to establish as sharply defined an alternative as possible to the prevailing standardizing methodology. Horst Weishaupt, for example, offers the following as a result of his analysis of qualitative research reports: 'The impression emerges that the methodological

debate is determined by concerns about demarcation which are of subordinate interest for the practice of qualitative social research' (1995: 94). And in a case study in the sociology of science, Jean Converse demonstrates the mixing of methodological and research-policy arguments in the conflict about open and standardized interviews in the United States during the Second World War (1984).

## 3 RECENT DISCUSSION

The impetus for a critical methodological discussion, free from the commitment against *ex-ante* hypotheses, was provided by Christel Hopf (1983, 1996). Using two empirical studies as examples, she sought to demonstrate that, on the one hand, the question to be investigated could indeed require a qualitative procedure, but on the other hand, because of the availability of previous studies, there was a focus on content that made the formulation of *ex-ante* hypotheses unavoidable.

If hypotheses are rejected in principle, then on the one hand there is no consideration of the very different aims of the hypotheses, and these differ sharply – in terms of their claim to validity and object – in their suitability for qualitative questions. (For example, do they relate to universal laws or to singular facts; do they make claims about the relationship between variables, or are they interested in social processes and meaning patterns? Hopf 1983: 48–50; 1996: 11f.). On the other hand, experience from research practice would speak against an unconditional openness in data collection: the pressure – resulting from the absence of selection criteria – to *extensive exploration* of all aspects that are *possibly* of interest conflicts with the *intensive meaning-discovery* that is characteristic of interpretative research, and in this situation overburdens the investigator (1983: 50–52). A general rejection of *ex-ante* hypotheses would therefore endanger the realization of genuinely qualitative research goals: it is 'dogmatic and not open to discussion' (1983: 49).

Other authors, in their plea for an unprejudiced approach to both the need and the possibility to reflect prior knowledge in qualitative social research, draw attention to the identical effects (from an epistemological viewpoint) of

hypotheses and prior knowledge in relation to the structuring of subsequent research activity, and therefore demand that this 'gap' in qualitative methodology be closed. Here we see, in the first place, the simple necessity of accepting the general state of epistemological discussion and not laying oneself open to the accusation of requiring an epistemological special status for qualitative methods, with this demand for 'unprejudiced' observation; and secondly this question, which every form of social research must confront, opens up the possibility of reconsidering the relationship between qualitative and quantitative methodology and redefining both the differences and the common ground (Böttger 1998; Meinefeld 1997; Strobl 1998).

#### 4 STARTING POINTS FOR A RE-ORIENTATION OF METHODOLOGICAL POSITIONS

How could these apparently contradictory expectations be resolved? On the one hand we have to meet the epistemological requirement to include prior knowledge in methodological control, and on the other we should not abandon the sociological *a priori* of allowing the sociological analysis to proceed from the genuine meaning attributions of actors and should not, in the act of interpretation, impose the categories of the investigator on the actions.

One precondition for the solution of this dilemma is, first and foremost, a recognition of the fact that the latter requirement can only be met in an approximate way. It cannot simply be a question of opposing a 'pure' reconstruction of the view of the actors to a recording of social reality in the categories of the investigator: it is only possible, in all cases, to understand the categories of others on the basis of one's own categories (on this point see also Schütz's thoughts on the observation of one's fellows, 1932: 287ff.). Here is precisely the misunderstanding of a *sociological* idea of understanding, for example on the part of Theodore Abel (1948) or Hans Albert (1985), who saw (and therefore rejected) 'understanding' as a direct recording of subjective meaning on the basis of individual sensibility, whereas it can only mean identifying

the actions of others as belonging to a particular meaning pattern available in the knowledge of the social group in question and subsuming them in this meaning pattern *in the way in which, and to the extent that, it is familiar to the person understanding* (on this cf. Meinefeld 1995, ch. 1). We have to accept the fundamental restriction that every observation only takes on meaning in respect of one's own meaning schemata, and so prior knowledge inevitably gives structure to our observations and must therefore be seen as the foundation of all research. In this way, however, the opposition of categories is transformed into a difference of degree, and the fundamental problem exists for all researchers in the same way.

A second step towards the resolution of this opposition might be found in distinguishing research questions according to the nature and extent of the knowledge already available of the area under investigation. If we consider the situation of the classic studies mentioned above, it becomes clear that in these cases a pre-formulation of content-based hypotheses is out of the question. On the other hand, if anyone wished to investigate interaction with the dying today they would scarcely be able to avoid taking note of the prior work of Glaser and Strauss and setting up their own research under consideration of the events reported there.

This does not necessarily mean, however, that one should no longer be open to new observations. If we can learn to distinguish between the principled *methodological* openness and the explicitness with which prior knowledge is reflected and expressed, it will be possible to reconcile the formulation of hypotheses with the reconstruction of object-specific meaning contents. The openness to new matters does not depend on our not taking account, at the level of content, of the old and the familiar, but on the how, in methodological terms, we set up the search for the new. Logically, these two levels are independent of one another – the question of putting prior knowledge into concrete terms and selecting the methods to be used to obtain new knowledge are only related (at the concrete practical level) when, for example, a standardized questionnaire is unable to provide information from beyond the dimensions the researcher

considers important because there was no room for it in the chosen instrument. In the first place, however, this does not mean that the result has been predetermined, as critics sometimes claim: it is only the framework of the dimensions involved in the investigation that has been fixed, but not their concrete manifestations of content. We could indeed find surprising results as to content using this route (Opp 1984: 65f.). In the second place, it does not mean that the choice of (more) open methods (such as participant observation, see 5.5, or interviews, see 5.2) will *per se* guarantee an openness of content: prior knowledge that remains implicit, even using these methods, will lead to selective observation and interpretation, because the recognition of whether something is new or not lies with the researcher and not with the individuals under investigation. The openness required in qualitative methodology to the potentially special nature of the field of investigation is therefore not helped by failing to make prior knowledge explicit, but by a conscious use of methods that permit the recognition and recording of a 'deviation' in the field of study from what was expected. This does not mean, however, that there is a conscious awareness of such an expectation.

As far as the possibility of reflecting prior knowledge is concerned, it should be noted that this can take a number of different forms.

- 1 In every case we have at our disposal an *everyday prior knowledge* on which, however vague and uncertain it may be, we are forced to rely in the absence of better information in order to be able to carry out any kind of initial orientation in the research field. This prior knowledge can only partially be made explicit, because ultimately an infinite regress is possible here. But it is at precisely this level that the basic but not otherwise reflected nature of the research object is decided, and what may be taken for granted from a cultural viewpoint remains fixed, so that its reflection becomes a (frequently irredeemable) desideratum (for an example cf. Bourdieu et al. 1991: 44ff.).
- 2 Furthermore, every researcher, in his or her approach to the research field, has recourse

to a corpus of *general theoretical concepts* which similarly contribute to the researcher's basic definition of the object. Although these are to a large extent conscious, they too cannot be made fully explicit, but the requirement for a conscious reflection may be made with a greater prospect of success.

- 3 Finally, there are a range of *object-related concepts* which permit the researcher to focus on particular aspects of content in the research area under investigation, and which, even in the context of qualitative research, can therefore facilitate and perhaps require the formulation of *ex-ante* hypotheses.

With regard to measuring the effects of this prior knowledge on the research process, it should be remembered that this does not begin only when hypotheses are formulated or when one 'enters the field' without hypotheses. If the *total* research process is to be reflected methodologically, then a fixation on the formulation of *ex-ante* hypotheses (positive in quantitative and negative in qualitative methodology) is not tenable: the development of the researcher's attention begins earlier and in a more fundamental way. In any case, in thinking about the control of this pre-structuring of limitation, one should be aware that this reflection – at least at the present time – can scarcely be standardized. How it is to be dealt with in the future should be tested in empirical research practice, before any methodological pronouncement is made.

#### 5 CONCLUSION

In the process of self-assurance of having an independent methodology, the decision against *ex-ante* hypotheses has indeed led to a consolidation of the qualitative position as distinct from quantitative methodology, but it has also led to a claim that is epistemologically untenable, and has restricted the applicability of qualitative research. Experience in research practice, however, has shown, on the one hand, that the majority of quantitative research studies also fail to follow the norm of *testing* hypotheses (cf. Meinefeld 1997: 23f.), and on the other hand the examples from Hopf



cited above support the view that in a qualitative research programme the testing of hypotheses may also occupy a legitimate place. The deciding line about how and to what extent prior knowledge should be made concrete does not follow the 'quantitative-qualitative' boundary, but is clearly dependent on other factors. It would be highly desirable if this fact could be ratified methodologically and if an uninhibited way of dealing with the problem of structuring research activity could be achieved in both qualitative and quantitative social research.

## NOTES

- 1 The actual methodological procedure at this phase of justifying modern empirical research may be captured very aptly in an observation, where Marie Jahoda relocates the retrospective over-emphasis of methodological reflectiveness that characterized the preface to the new edition of Lazarsfeld's *The Unemployed of Marienthal*, published 27 years after the first edition – a study which even today is seen as a model of exemplary empirical research: 'If [this explanation] should give the impression that these principles were available to us during the study, this would be misleading. We had no clear plan, in terms either of content or method. ... The methods grew out of concentration on the problem, not for their own sake' (Jahoda 1980/81: 139). Furthermore – to complete the picture – it seemed legitimate to use, as a research strategy and method, whatever procedure promised to make it possible to obtain interesting data for the research question (see 2.8).

- 2 It is of course true that in later publications (1987: 10f. and passim; Strauss and Corbin 1990: 48–56) Strauss recognizes prior knowledge as an important source of theoretical sensitivity; but since Strauss (and Corbin) insist on 'discovery' as a primary goal of qualitative research, they hedge this direction with a renewed warning of the risk of 'constraint' that affects the openness to new matters (1990: 32f.) because of categories known in advance – and in this way they essentially adhere to the normative demand of the position formulated earlier. Even more explicitly, Glaser insists upon dispensing with all prior knowledge (cf. Kelle 1994: 334f., and also the excellent presentations of the positions of Glaser and Strauss in Kelle 1994: 283ff.).

## FURTHER READING

- 
- Bourdieu, P., Chamboredon, J. C. and Passeron, J. C. (1991) *Craft of Sociology: Epistemological Preliminaries*. Berlin: de Gruyter. Pt III and ch. 2.
- Chalmers, A. F. (1982) *What is This Thing Called Science?* Queensland: University of Queensland Press. ch. 3.
- Glaser, B. G. and Strauss, A. L. (1967) *The Discovery of Grounded Theory. Strategies for Qualitative Research*. Chicago: Aldine. esp. pp. 1–43.