

# **DOING QUALITATIVE RESEARCH**

**A Practical Handbook**

**SECOND  
EDITION**

**DAVID SILVERMAN**

 **SAGE Publications**  
London • Thousand Oaks • New Delhi

# 6

## Selecting a Topic

### CHAPTER OBJECTIVES

By the end of this chapter, you will be able to:

- Understand why you need a clear research topic.
- Recognize the main problems which stop you narrowing down your topic.
- Find solutions to these problems.

### 6.1 INTRODUCTION

In this chapter, I discuss the problems that you may find in defining your research topic. I then suggest some strategies you can use to overcome these problems.

People are often impressed when they find out that you are 'doing research'. They may even want to know more. If you have ever been in this situation, you will know how embarrassing it can be if you are unable to explain clearly exactly what you intend to study. Such embarrassment can be multiplied a thousandfold if your interrogator is, say, a smart professor you have never met before. How are you to respond?

The answer to this question becomes easier if you recognize that there are practical as well as social reasons for having a clear research topic. Above all, such clarity can give your research focus as shown in Table 6.1 below.

**TABLE 6.1 THE ROLE OF RESEARCH QUESTIONS**

- |   |   |
|---|---|
| 1 | They organize the project and give it direction and coherence |
| 2 | They delimit the project, showing its boundaries              |
| 3 | They keep the researcher focused                              |
| 4 | They provide a framework when you write up your research      |
| 5 | They point to the methods and data that will be needed        |

Source: adapted from Punch (1998: 38)

Unfortunately, many undergraduate social science programmes reward passive knowledge rather than the ability to use ideas for yourself. They often leave students better able to leap the hurdles to pass their assessments than to use their knowledge to formulate a workable research topic.

In qualitative methodology courses, this phenomenon is seen when courses encourage rote learning of critiques of quantitative research and offer minimal practice of alternative methods. By contrast, in quantitative methods courses, one tends to learn by rote recipe knowledge which is of practical use in drafting a research proposal (e.g. defining variables and measures).

In this context, selecting a research topic to be studied through qualitative methods is a very risky activity. This is because it involves committing yourself to a particular course of action rather than reiterating spoonfed 'critiques'.

Faced with this risk, students often try to play safe by opting for one of three apparently 'low-risk' strategies:

- simplistic inductivism
- the 'kitchen sink' gambit
- grand theory.

I briefly discuss each below before offering some more satisfactory solutions.

## 6.2 SIMPLISTIC INDUCTIVISM

In many social sciences, the qualitative tradition was initially characterized by its opposition to the strict research designs demanded in most quantitative work. So anthropologists would select their tribe, take up residence, learn the language and do no more than keep a field diary. Similarly, sociological **ethnographers** would identify an activity, institution or sub-culture and just 'hang out'. In both cases, the idea was to grasp 'reality' in its daily accomplishment.

The hope was that somehow meaning would 'emerge' by itself from such 'in-depth' exposure to the field. It was believed that any prior definitions of topics or concepts would only stand in the way of a sensitive understanding of the slice of the cultural world to which one was being exposed.

In the 1960s, this belief was apparently supported by Glaser and Strauss's (1967) famous idea of theory 'grounded' in data rather than presumed at the outset of a research study. Ironically, but understandably, the idea of qualitative research as unstructured 'exposure' to the world was also supported by quantitative researchers. So we learn, in one quantitative text, that:

Field research is essentially a matter of immersing oneself in a naturally occurring ... set of events in order to gain firsthand knowledge of the situation. (Singleton et al., 1988: 11)

In common with crude inductivists, Singleton et al., refer to 'the situation' as if 'reality' were a single, static object awaiting observation. Like such qualitative researchers, they emphasize 'immersion' which they implicitly contrast with later, more focused, research. This is underlined in their subsequent identification of qualitative or field research with 'exploration' and 'description' (296) and their approval of the use of field research 'when one knows relatively little about the subject under investigation' (298-9).

This apparent unanimity at both ends of the research spectrum is noted by the authors of one qualitative methodology text: 'The conventional image of field research is one that keeps prestructuring and tight designs to a minimum' (Miles and Huberman, 1984: 27).

Miles and Huberman note two objections to this position and the cosy consensus that supports it: the omnipresence of theory and the need for a research design. I will briefly consider each in turn.

First, 'any researcher, no matter how unstructured or inductive, comes to field-work with *some* orienting ideas, foci and tools' (Miles and Huberman, 1984: 27). As Gubrium and Holstein (1997) note, the apparently atheoretical position of some ethnographers itself derives from a theory:

The directive to "minimize presuppositions" in order to witness subjects' worlds on their own terms is a key to *naturalistic* inquiry. (1997: 34, my emphasis)

So the idea of just 'hanging out' with the aim of 'faithfully representing subjects' worlds' is a convenient myth derived from a theory that Gubrium and Holstein term **naturalism**. Of course, without some conceptual orientation, one would not recognize the **field** one was studying. So the problem is that many closet naturalists fail to come clean about the theory dependence of their research.

A second objection to simply going out into the field and inducing observations is that it can be an excuse for sloppy, unfocused research. Mason (1996: 6) rejects the suggestion that qualitative research can just 'describe' or 'explore' the social world. As Miles and Huberman point out, such unfocused research can be a recipe for disaster:

the looser the initial design, the less selective the collection of data; *everything* looks important at the outset to someone waiting for the key constructs or regularities to emerge from the site, and that wait can be a long one. (1984: 28)

Moreover, such a purely **inductive** approach can be blind to the need to build cumulative bodies of knowledge. If this is not an intentionally anti-scientific ploy, it can be just naive. In the 1920s and 1930s, research students in Chicago, following Robert Park's injunction to get out of their armchairs into the virgin territory of urban streetlife (see Bulmer: 1984), could justify their inductivist aims. By the 1960s, however, even Glaser and Strauss (1967) were requiring that field

## PART TWO ● STARTING OUT

researchers think about the **formal theories** that might be developed out of apparently isolated substantive, inductive studies.

At the turn of the century, qualitative research would indeed have been in a sorry state if it had not developed such theories and related cumulative bodies of knowledge. As I commented recently:

we no longer need to regard qualitative research as provisional or never based on initial hypotheses. This is because qualitative studies have already assembled a usable, cumulative body of knowledge. (Silverman, 1997: 1)

Sometimes, the previous literature or (for experienced researchers) one's own work will suggest a **hypothesis** crying out to be tested or a finding ripe for re-testing. Where this happens, particularly where the earlier study derived from a theoretical approach to which you are sympathetic, an attempt to strike out afresh would be in danger of reinventing the wheel.

Of course, as Chapter 3 shows, this does not mean that you should necessarily be stuck with your original ideas. The beauty of qualitative research is that its rich data can offer the opportunity to change focus as the ongoing analysis suggests. But such changes of direction, like the original research proposal, do not come out of the blue but reflect the subtle interplay between theory, concepts and data.

### 6.3 THE 'KITCHEN SINK' GAMBIT

Like any piece of advice, you can take too far the suggestion that you should avoid simplistic inductivism. In drafting your first research proposal, it is tempting to select a very broad topic. By including every aspect of a problem that you can think of, you hope to show the breadth of your knowledge and to impress potential supervisors.

Unfortunately, this 'kitchen sink' approach is a recipe for disaster. Unless you have the resources for a big team of researchers, depth rather than breadth is what characterizes a good research proposal. If you define your topic very widely, you will usually be unable to say anything at great depth about it.

As I tell my students, your aim should be to say 'a lot about a little (problem)'. This means avoiding the temptation to say 'a little about a lot'. Indeed, the latter path can be something of a 'cop-out'. Precisely, because the topic is so wide ranging, one can flit from one aspect to another without being forced to refine and test each piece of analysis.

The case study below illustrates how one research student worked to refine and narrow down her problem. Over time, Seta Waller moved from quite a broad psychological interest in the 'alcoholic' to a quite narrow but workable concern with the narrative structure of patients' accounts.

### Case study: Seta's process of 'narrowing down' her topic

My interest in developing a research study in the field of alcoholism came about naturally as I had been working in this field for many years. My research experience and training, however, had been exclusively in quantitative socio-medical studies on alcoholism.

Following training in social policy and administration, I joined the Alcohol Treatment Unit (ATU) of a psychiatric hospital. At the ATU I was involved in a set of quantitative studies designed to measure treatment outcome in alcoholic patients.

When I decided to develop a PhD study, my initial interest was to find out what patients thought of their drink problem – how they conceptualized it. This would have been a quantitative study but quite different from the usual measurement of outcome studies. I therefore began designing a quantitative study enquiring into alcoholic patients' concepts of alcoholism. The sample was to be drawn from groups of alcoholics, admitted to a four-week in-patient treatment programme in the alcohol treatment unit where I was employed.

Having developed some rating scales on concepts of alcoholism, following interviews with patients, I carried out a pilot study on a small sample. Patients were asked to complete five-point rating scales consisting of statements, by indicating whether they agreed or not with each statement, responses ranging from 'agree strongly' to 'disagree strongly'.

This whole process took about eight months. However, I was feeling uncomfortable with the results of my pilot study as I tried to make sense of the data. I felt very uncertain about the attitudes and beliefs expressed in the scales; I began to question how one could consider that all patients who, for instance, stated 'agree strongly' on the rating scales, meant the same thing.

I started thinking of a new study with a qualitative methodology which I was introduced to on my MA course in Sociology at Goldsmiths College. On this course, I became aware of the relevance of the status of interview data, how **naturally occurring data** or unstructured interview data can be treated as analysable texts which do not need to be considered as being true or false.

As my main interest in my work had been alcoholism for many years, I decided to look at alcoholic patients' accounts of their experiences by means of open-ended interviews rather than the traditional structured methods. I therefore asked the patients to tell me about their drinking, allowing them to talk with a non-directive approach. I tape recorded some interviews in the beginning, and, when I looked at my transcripts, I knew this was what

Cont...

I ought to do. The qualitative data resulting from this kind of approach was so rich and rewarding that I decided to proceed in this way and carried out forty interviews.

At first my approach to the analysis of the data was to look at 'social meanings', concerned mainly with 'why' certain causes of alcoholism were given as explanations, partly inspired by Douglas's work (1975). I then tried a **frame** analysis following Goffman's methodology (Goffman, 1974). After having applied frame analysis to some patients' interview data, I still was not satisfied with the results as I had no means of knowing if the staff used similar frames. Patients' hospital notes did not reveal sufficient data to study this. I realized that my interest lay in how patients were formulating and presenting their drinking problem and then to attempt to look at 'why' they were presenting in these particular ways.

Adopting a qualitative approach, I was able to look at the **narrative** structure of patients' accounts to see how the texts were accomplished and organized. The structure of the accounts seemed to have a common chronologically organized pattern. Examination of the narratives made me realize that patients were showing their skills in presenting themselves as morally adequate individuals, as Baruch (1981) had found in his sample of parents of children with congenital illness. I also found that patients were displaying considerable insights into their problems and were emerging as well-informed individuals.

My current approach is therefore not simply an analytical shift, but another way of looking at interview data to see how it can help our understanding of alcoholic patients' versions and presentations of their problems.

The case study above illustrates how ideas derived from methodology and theory can help in specifying a research topic. However, some people are more comfortable at working solely at a theoretical level and seek to substitute theoretical syntheses or critiques for data analysis. Following Mills (1959), I call this approach **grand theory**.

#### 6.4 THE GRAND THEORIST

While the kitchen-sinker flits about trying this and that, the grand theorist is kept busy building theoretical empires. Stuck firmly in their armchairs, such theorists need never trifle with mere 'facts'. Instead, they may sometimes spin out cobwebs of verbiage which, as Mills (1959) shows, can be reduced to a few sentences.

Nonetheless, a situation in which you can obtain a research degree without ever leaving your familiar university library is not to be despised. Indeed, I should be the last to criticize grand theory since my own PhD was obtained by this very method!

However, it is usually wise to assume that every 'solution' contains seeds of further problems. In the case of grand theory, these problems include:

- Can you ever get out of the library in order to write your thesis? One book will surely have a list of further 'crucial' references and so on, *ad infinitum*. Anybody who thinks a library PhD is a 'quick fix' would be well advised to ponder whether they have the will-power to stop reading. They would also be wise to consult a short story called 'The Library of Babel' by the Argentinian writer Borges. This tells a chastening tale of scholars who believe that, if they only keep on looking, all knowledge will finally be revealed by yet another book.
- Theoretical fashions change – nowhere more so than in the social sciences. If you commit yourself to a theoretical topic, you must always be looking over your shoulder at the prospect of some change in direction in the theoretical wind from, say, Paris to an obscure location with a school of thought of which you are totally unfamiliar.

If you do grand theory, you may spend so much time constructing elegant accounts of the world that you never touch base with the ground upon which the world rests. Kafka's (1961) wonderful short story 'Investigations of a Dog' creates a marvellous image of 'Airdogs' (*Lufthunde*) who float on cushions above the ground, surveying the world from on high, yet cut off from any contact with it (so cut off that Kafka's doggy investigator wonders how they manage to reproduce). However, readers of this book will be more interested in solutions than in critiques. In response to this, I set out below some practical strategies that may be of use to potential 'simplistic inductivists', 'kitchen-sinkers' and 'grand theorists'.

## 6.5 STRATEGIES FOR SIMPLISTIC INDUCTIVISTS

If your previous education has equipped you with few research ideas of your own, comfort yourself that your predicament is not unusual and can be resolved.

I outline below three strategies that you can use if you find yourself in this boat. Each seeks to encourage you to use the knowledge you have already gained as a resource in generating a researchable problem. The three strategies I discuss are:

- Using concepts as sensitizing resources.
- Following up findings from other studies.
- Introducing a third variable.

### 6.5.1 Using concepts

Treating the knowledge you have learned as a resource involves thinking about how it can sensitize you to various researchable issues. In an earlier book

## PART TWO ● STARTING OUT

(Silverman, 2001: 9–11), I sought to distinguish three types of sensitivity: historical, political and contextual.

Most of this is self-explanatory. Historical sensitivity means that, wherever possible, one should examine the relevant historical evidence when setting up a topic to research. Political sensitivity shows the vested interests behind current media 'scares' and reveals that this way of determining our research topics is just as fallible as designing research in accordance with administrative or managerial interests.

Contextual sensitivity is the least self-explanatory and most contentious category in the present list. A longer explanation is, therefore, set out below. By 'contextual' sensitivity, I mean two things:

- the recognition that apparently uniform institutions like 'the family', 'a tribe' or 'science' take on a variety of meanings in different contexts
- the understanding that participants in social life actively produce a context for what they do and that social researchers should not simply import their own assumptions about what context is relevant in any situation (Silverman, 2001: 10–11).

Such contextual sensitivity would suggest that matters like 'recovery from depression', 'quality care' and 'urban healing' are not uniform phenomena but take on particular meanings in different local contexts and local cultures (Gubrium, 1988), depending, among other things, on who is the audience for the description.<sup>1</sup>

One final point: the three kinds of sensitivity we have been considering offer different, sometimes contradictory, ways of generating research topics. I am not suggesting that all should be used at the beginning of any research study. However, if we are not sensitive to *any* of these issues, then we run the danger of lapsing into a commonsensical way of defining our research topics. This is a topic to which I shall return, particularly in Chapter 7.

### 6.5.2 Following up other finding

Phillips and Pugh (1994: 49–52) suggest that one aid for the sluggish research imagination is to begin with previously proposed generalizations and then try to find their limits by postulating new conditions.

Since most undergraduate social science teaching places a great deal of emphasis on the 'classic' literature, you can sometimes mobilize your knowledge of 'classical' work in order to generate a research problem. In an earlier book (Silverman, 1985: 10–11), I gave two sociological examples of postulating a new condition for a classical generalization:

- Gouldner (1954) observed that Max Weber's 'ideal type' of bureaucracy was largely based on studies of government bureaucracies. This meant that Weber

stressed the role of democratically defined formal rules in obtaining consent. By studying rule following in the private sector, Gouldner was able to identify varying levels and bases of consent by staff to rules.

- Lipset et al. (1962) noted that Robert Michels' 'Iron Law of Oligarchy' had encouraged a focus on the factors that make organizations undemocratic. By studying a highly democratic organization, Lipset et al. identified both anti-democratic and democratic pressures in how organizations operate. By doing so, they were able to question the inevitability of this iron law.

More recently, I became interested in the conditions under which clients were likely to demonstrate uptake of the advice that they were given in interviews with health professionals. In a study of interviews between British health visitors and first-time mothers, Heritage and Sefi (1992) had found that mothers were more likely to acknowledge the relevance of advice which was related to their expressed concerns.

In my own study of HIV-test counselling (Silverman, 1997), I began with Heritage and Sefi's findings as my initial research focus. However, I observed that time constraints in many counselling centres meant that it was very difficult for counsellors to adopt such an apparently 'client-centred' approach. My research question then changed to considering how both parties acted to prevent open disagreements while giving or receiving potentially irrelevant advice (Silverman, 1997: 154–81).

### 6.5.3 *Introducing a third variable*

As described by Rudestam and Newton (1992: 12–16), introducing a third variable involves adding a focusing factor to your area of research interest. These authors give the example of a student interested in how young people view the elderly. You can make this topic less general, more researchable and interesting by introducing a third variable. For instance, you can ask: does living with a grandparent influence this? Alternatively, you can focus on the effect on young people of media representations of the elderly. Further, using 'contextual sensitivity', as described above, you can limit your focus even more by asking how, when and where young people generate descriptions of elderly people.

If you have a tendency to be a 'simplistic inductivist', you should now attempt Exercise 6.1 at the end of this chapter.

## 6.6 STRATEGIES FOR KITCHEN-SINKERS

Do less, more thoroughly. (Wolcott, 1990: 62)

Wolcott's advice is sound. Narrowing down is often the most crucial task when drafting a research proposal. Kitchen-sinkers have so many ideas buzzing around

## PART TWO ● STARTING OUT

in their heads that getting down to a focused piece of research is entirely beyond them.

Every issue seems so fascinating. Each aspect seems interconnected and each piece of reading that you do only adds further ideas (and suggests further readings). So, while you can grasp the value of making a lot out of a little, it is easier said than done. The question remains: how do you go about narrowing your ideas down?

I set out below three practical techniques which help to answer this question:

- Draw a flow chart.
- Find a puzzle.
- Look through a zoom lens.

### 6.6.1 *The flow chart*

Dealing with data means moving from passive reading to active analysis. If you have failed to use the early stages of your research to narrow down your topic, data analysis is going to be very difficult:

having a large number of research questions makes it harder to see emergent links across different parts of the data base and to achieve successful integration of findings. (Miles and Huberman, 1984: 36)

To help you narrow down, it can make sense to do an early flow chart setting out your key concepts and how they might relate. Following Miles and Huberman:

Conceptual frameworks are best done graphically, rather than in text. Having to get the entire framework on a single page is salutary. (1984: 33)

The single-page flow chart is a useful technique in writing books as well as in doing research. For instance, as I write these words, I regularly move to a second document which houses the outline of this book. This outline was continually revised as I did my preliminary reading. It is still being revised as I write each chapter.

Several attempts will usually be needed to get your flow chart into a state that will be useful to you. Miles and Huberman recommend experimenting with different ways of specifying your research focus. But their basic advice is to 'begin with a foggy research question and then try to defog it' (35).

### 6.6.2 *Find a puzzle*

One way to break out of the vicious circle of unending facts and theories is to put your books on one side and to ask yourself: What am I really trying to find out? More specifically, what *puzzle* am I trying to solve?

Think of research as one of many kinds of puzzle solving among a set of activities like doing jigsaws, completing crosswords or solving crimes. Each activity will be associated with its own set of more or less unique activities (but see Alasuutari (1995) on the parallel between the qualitative researcher and Sherlock Holmes). Mason has argued that 'all qualitative research should be formulated around an intellectual puzzle' (Mason, 1996: 6). She distinguishes three kinds of question that may generate the type of intellectual puzzle which qualitative researchers would recognize, namely:

- How or why did X develop? (a developmental puzzle)
- How does X work? (a mechanical puzzle)
- What causes X or what influence does X have on Y? (a causal puzzle). (Mason, 1996: 14)

Let us consider how, following Mason, you might find a puzzle. Say you have a general interest in 'child abuse'. You might narrow down your topic by choosing among the following questions:

- How or why was 'child abuse' first recognized? (a developmental puzzle)
- How (and by whom) is 'child abuse' identified? (a mechanical puzzle)
- What are the characteristics of child-abusers and abused children? What effect does child abuse have on each group? (a causal puzzle)

Once you make a list of this kind, you should see that it is impossible to solve satisfactorily all these puzzles. So which puzzle do you choose? Below are some further questions that are worth asking:

- Which puzzle most interests me?
- Which puzzle might most interest my supervisor/funding body?
- Which puzzle most relates to issues on which I already have some theoretical, substantive or practical background?
- Which puzzle would generate questions that could be answered using my own resources and with readily available data?

### 6.6.3 The zoom lens

Wolcott (1990) gives the example of one PhD student who never finished his study of classroom behaviour. The true 'kitchen-sinker', this poor student was always reading more or gathering yet more data.

Wolcott uses the analogy of a zoom lens to suggest a practical solution. Say you want to take some photographs of a holiday resort. You could find some suitably high place, say a nearby hill, and try to take a picture of the whole resort. Then, as Wolcott points out: 'if you want to take in more of the picture, you must sacrifice closeness of detail' (1990: 63).

Alternatively, you can zoom in on one small image. What you lose in breadth, you may well gain in telling detail – say a particular dish that you enjoyed or the interaction between two local people.

Now apply the zoom lens analogy to defining your own research task. Wolcott suggests ‘taking some manageable “unit of one” as a focus’ (1990: 69). So if, like his student, you are interested in classroom behaviour, focus on one student, one day, one lesson or one critical event.

The beauty of this narrowing of focus is that it will produce a manageable and achievable research task. Moreover, you are not locked for ever in this close-up picture. Just like the photographer you can:

zoom in progressively closer and closer until your descriptive task is manageable, then zoom back out again to regain perspective. (Wolcott, 1990: 69)

Following Wolcott, later on you can always attempt to broaden your generalizations through more data at different levels of ‘reality’. But your initial ‘zooming in’ will have got you going – out of the library and into dealing with data.

If you have a tendency to be a ‘kitchen-sinker’, you should now attempt Exercise 6.2 below.

#### **6.6.4 A caution: avoid reductionism**

One of the advantages of introducing a third variable is that it guards against the tendency to try to explain complex social processes in terms of a single cause. Such reductionism is regularly demanded in both legal cross-examinations (‘answer yes or no!’) and in media interviews (where the demand for simplification sometimes makes research scientists seem like incoherent babblers).

So my diagnosis of ‘kitchen sinking’ and my recommendations for specifying a research problem should not be confused with attempts to reduce the complexities of the social world to a single **variable**. Just as doctors talk about meeting patients who make their hearts sink, there is nothing worse when a detailed seminar on one’s research is greeted by some bright spark with a version of: ‘that’s all very interesting. But surely what you’ve described is all to do with power/gender/post-modernity etc.’

What a nice, simple world it would be if everything reduced to one factor! For the moment, however, we should leave the pursuit of this kind of simplicity to bigots and to those theoretical physicists who valiantly are seeking a single theory of matter.

So narrowing down a research problem should not be confused with this kind of reductionism. I can only echo the arguments of the authors of a fairly recent qualitative methodology textbook:

Such reductive arguments are always distressing, given the variety and complex organization of social worlds. They reflect mentalities that cannot cope with the uncertainties and ambiguities of social research. (Coffey and Atkinson, 1996: 15)

## 6.7 STRATEGIES FOR GRAND THEORISTS

Reducing 'reality' to ungrounded sets of categories is an obvious potential failing of grand theorists. However, the minority of readers who feel they have the flair and temperament for theorizing will not, I suspect, be dissuaded by anything I might write. Indeed, sometimes, as I have already remarked, library-based work can be a quick way to write an acceptable thesis.

In this situation, all I can usefully do is wish you luck and offer you a couple of suggestions to speed you on your way. First, try to ignore fashions. Second, think about how some data may actually help you to theorize better. I set out these suggestions below.

### 6.7.1 *Ignore fashions*

Having found the corner of the intellectual garden which suits you, stick with it. Don't worry about those smart alocs who have always read a 'crucial' book by some new author – nine times out of ten, it will just distract you. Guided by your supervisor, work out the set of readings that will be your central material and stay with them. When you have written most of your thesis, you may then have the luxury of reading more widely and using that reading to reflect on the implications and limitations of your position – perhaps for your final chapter. Till then, don't be distracted.

### 6.7.2 *Find some data*

Even the most active minds can become a little stilted when confined to their armchairs. So think about examining some empirical materials of some kind. Even though these may not be central to your thesis, they may work as an aid to the sluggish imagination.

Take the case of two students in my own department who wrote 'theoretical' PhDs. Nick was interested in what he calls 'the refusal of work' which he linked to theoretical ideas about 'the ontology of desire'. Despite this highly complex theory, Nick still felt it worthwhile to gather material on the history of *Autonomia* – an Italian movement to refuse work – and the organization of unemployment benefit in the UK.

Jake was interested in a critique of existing theories of the community. In this context, he attempted what he described as largely a philosophical exercise.

Nonetheless, to aid his thinking, he observed and interviewed homeless people, beggars and the mainstream community. Attempting what he called 'a situated phenomenology of the moral encounter', his data was intended to be only illustrative.

## 6.8 STRATEGIES FOR ALL RESEARCHERS

Whether you tend to be this kind of a grand theorist or you are a kitchen-sinker or simplistic inductivist, there are certain general issues that apply to everybody who wants to select a research topic. I call these issues:

- finding a workable (not just narrow) research topic
- recognizing 'feedback loops' between topic(s) and data analysis
- understanding that your categories (or variables) are always theoretically saturated.

I deal with each issue below.

### 6.8.1 Find a workable research topic

'Narrowing down' is necessary but not sufficient for a good research project. It is possible to have a narrowly defined, clear and unambiguous research topic (using concepts which clearly connect to data indicators) which is simply not workable. For instance, there may be no way you could obtain appropriate data or the topic may simply not be very interesting or important. Table 6.2 lists three features of workable research questions.

**TABLE 6.2 WORKABLE RESEARCH QUESTIONS**

- 
- 1 *Answerability*: we can see what data is required to answer them and how the data will be obtained
  - 2 *Interconnectedness*: the questions are related to each other in some meaningful way, rather than being unconnected
  - 3 *Substantively relevant*: the questions are interesting and worthwhile so justifying the investment of research effort
- 

*Source*: adapted from Punch (1998: 49)

### 6.8.2 Recognize feedback loops

Good research rarely moves smoothly from A (research topic) to B (findings). As Seta's case (discussed above) shows, alert researchers are always prepared to change their focus as they learn new things from others and from their own data. Wield has called this to and fro between data and topic a 'feedback loop' (2002: 42). This is how he addresses the issue of research focus in the context of such feedback:

Each stage of the research work will result in challenging a project's focus and lead to some re-evaluation. At all times, you will find that you have to maintain a careful balancing act between the desirable and the practical. Too strong a focus early on may lead to you ignoring what actually are more important issues than the ones you have chosen. Too weak a focus results in following up each side issue as it emerges and not getting anywhere! So focus needs to remain an issue as the research progresses in order to avoid the pitfall of these extremes. (ibid.)

### 6.8.3 Recognize the theoretical saturation of categories

Seta's case, above, nicely illustrates that the categories we use to formulate our research problem are not neutral but, inevitably, theoretically saturated. In her case, the issue revolved around the status which she should attach to her interviewees' accounts. To take two extreme formulations: were these the raw experiences of alcoholics or provoked narratives in which a drinking story was constructed?

These kinds of issues have already been discussed in Chapter 4 when I examined several interview studies. They are considered at greater length in Silverman (2001: 83–118). The interdependence between research design and such analytical issues is examined in the next chapter of this book.

## 6.9 CONCLUDING REMARKS

Like most dispositions, whether you tend to be a simplistic inductivist, kitchen-sinker or grand theorist is likely to arise from a combination of temperament and experience. As such, you are unlikely to be deflected by anything I write. So this chapter will have succeeded not by converting you, but if it helps you to speed along your ordained path.

On the other hand, it may be overreductionist to view these three tendencies as personal dispositions. Jay Gubrium (personal correspondence) has suggested to me that simplistic inductivism, kitchen-sinkism and grand theory are occupational hazards of all social science enquiry. In this sense, they are tendencies present in all of us and we need to be constantly wary of them if our enterprise is going to be theoretically informed *and* empirically grounded.

### KEY POINTS

Selecting a research topic can be made easier if you resist three temptations:

- 1 *Simplistic inductivism* assumes that we need make no assumptions in studying the world. Instead, hypotheses will somehow just emerge if we just

'hang out' with the aim of 'faithfully representing subjects' worlds'. Simplistic inductivism is at best a convenient myth which ignores the theory-saturated nature of any observation and can be an excuse for sloppy, unfocused research. It is best countered by:

- using concepts as sensitizing resources
- using other people's generalizations
- introducing a third variable.

2 *The kitchen sink gambit* seeks to include every aspect of a problem that you can think of in order to show the breadth of your knowledge and to impress potential supervisors. However, if you define your topic very widely, you will usually be unable to say anything at great depth about it. Depth rather than breadth is what characterizes a good research proposal. It can be countered by:

- drawing a flow chart
- finding a puzzle
- looking through a zoom lens.

3 *Grand theorists* build theoretical empires. Stuck firmly in their armchairs, such theorists need never trifle with mere 'facts'. The consequence may not be enlightenment but merely cobwebs of verbiage. This tendency can be countered by:

- ignoring the latest fashions
- finding some data.

#### NOTE

- 1 See Chapter 15 for further discussion of studies of these topics in relation to evaluating the 'quality' of qualitative research.

#### Further reading

To help you think some more about defining your research, I recommend three basic texts: Amanda Coffey and Paul Atkinson's *Making Sense of Qualitative Data* (Sage, 1996), Chapter 1; Jennifer Mason's *Qualitative Researching* (Sage, 2nd edn, 2002), Chapters 1–2, and David Silverman's *Interpreting Qualitative Data: Methods for Analysing Talk, Text and Interaction* (Sage, 2001), Chapter 1. Useful but more specialist texts are: Pertti Alasuutari's *Researching Culture* (Sage, 1995), Chapter 13; Martyn Hammersley and Paul Atkinson's *Ethnography: Principles in Practice* (Tavistock, 1983), Chapter 2; and Anselm Strauss and Juliet Corbin's *Basics of Qualitative Research* (Sage, 1990), Chapters 1–4.

**Exercise 6.1 Strategies for 'simplistic inductivists'**

1. Identify the main research question or problem that the study is intended to address.

2. Identify the

3. Identify the

4. Identify the

5. How will the research be conducted (i.e. with whom, where, how)?

6. Review how the researcher has identified the study's theoretical framework and how this framework informs the research design and data analysis.

7. The researcher should provide a clear and concise summary of the research objectives, the research design, the data analysis, and the conclusions drawn from the research. The researcher should also provide a clear and concise summary of the research's contribution to the field.

**Exercise 6.2 Strategies for 'kitchen-sinkers'**

1. Identify the main research question or problem that the study is intended to address.

2. Identify the main research question or problem that the study is intended to address.

3. Identify the main research question or problem that the study is intended to address.

4. Identify the main research question or problem that the study is intended to address.

5. The researcher should provide a clear and concise summary of the research objectives, the research design, the data analysis, and the conclusions drawn from the research. The researcher should also provide a clear and concise summary of the research's contribution to the field.