Exercise 4.1

Pat Cryer has suggested that we can understand originality in research through an analogy with a travel expedition: 'the research student is the explorer and the expedition is the research programme' (1996: 145). Cryer uses the expedition analogy to suggest different senses of 'original research'.

Review each of the following kinds of originality in terms of what you think your research might contribute and decide which kind is most likely to be applicable to your work:

- originality in tools, techniques and procedures
- originality in exploring the unknown
- originality in exploring the unanticipated
- originality in use of data
- originality in outcomes
- originality in byproducts (1996: 146–8).

You might return to this exercise at regular intervals to review any changes in how you view your research.

5

Selecting a Topic

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 for research or practice.

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.

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 subculture 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' (1988: 296) and their approval of the use of field research 'when one knows relatively little about the subject under investigation' (1988: 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 fieldwork with *some* orienting ideas, foci and tools' (1984: 27). As Gubrium and Holstein 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 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 be 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, 1997a: 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 retesting. 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 2 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. However, 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.

THE 'KITCHEN SINK' GAMBIT

Like any piece of advice, you can take too far the advice to 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.

GRAND THEORY

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 C. Wright Mills (1959) said, 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 willpower to stop reading. They would also be wise to consult Borges's short story 'The Library of Babel' which 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 themselves!).

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'.

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
- using other people's generalizations
- introducing a third variable.

Using sensitivities

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 (Silverman, 1993: 6–8), I sought to distinguish three types of sensitivity:

- historical
- cultural
- political
- 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. Cultural sensitivity reveals how the kind of 'experiences' reported in our four critiqued research studies are shaped by particular forms of cultural representation. 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 as follows:

> By 'contextual' sensitivity, I mean two things:
>
> (a) the recognition that apparently uniform institutions like 'the family', 'a tribe' or 'science' take on a variety of meanings in different contexts;
>
> (b) 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. (1993: 8)

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.[1]

One final point. The four 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 6.

Using earlier findings or theories

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.

First, Alvin 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.

Second, Lipset et al. (1962) noted that Robert Michels's 'iron law of Oligarchy' had encouraged a focus on the factors that make organizations un-democratic. 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, 1997b), 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 now changed to considering how both parties acted to prevent open disagreements while giving or receiving potentially irrelevant advice (1997b: 154–81).

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 more 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 5.1 at the end of this chapter.

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 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:

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

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' (1984: 35).

Finding 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). Jennifer Mason has argued that 'all qualitative research should be formulated around an intellectual puzzle' (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) (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? The following are some further questions that are worth asking:

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

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 forever 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' (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 5.2.

A caution: avoiding 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 both in 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 than 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/postmodernity 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 are valiantly seeking a single theory of matter.

Unfortunately, this does not put off those social scientists who claim that one factor or institution ('power', 'gender') is the cause of everything and do 'research' in order to 'prove' this. For instance, in a study of doctor–patient communication, Waitzkin had the laudable aim of relating 'the everyday micro-level interaction of individuals' to 'macro-level structures of domination' (1979: 601). However, as Rayner and Stimson (1979) point out, Waitzkin used a mechanistic version of Marxism based on notions of the material base and the superstructure, which reduces the doctor–patient relationship simply to an ideological state apparatus of the capitalist state.

Knowing what he was going to find, Waitzkin appeared to treat his data largely as illustrative of a preconceived theory. For instance, he asserts that doctors send ideological messages about the 'work ethic' to their patients. Yet he rests his case on a small part of a medical interview in which, hearing that his patient is tired, the doctor asks whether he is 'able . . . to work a regular day'. When the patient confirms this, the doctor says: 'Wonderful' (1979: 604–5).

As I commented in an earlier book:

> At the very least, Waitzkin is making very limited data do a great deal of analytic work. Without any evidence to the contrary, the reader might prefer to read the doctor's question about the patient's employment as simply establishing the status of the latter's comment about feeling tired. (Silverman, 1985: 186)

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 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)

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.

Ignore fashions

Having found the corner of the intellectual garden which suits you, stick with it. Don't worry about those smart alecs 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.

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 currently writing 'theoretical' PhDs. Nick is interested in what he calls 'the refusal of work' which he links to theoretical ideas about 'the ontology of desire'. Despite this highly complex theory, Nick still feels 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 is interested in a critique of existing theories of the community. In this context, he is attempting what he describes as largely a philosophical exercise. Nonetheless, to aid his thinking, he is observing and interviewing homeless people, beggars and the mainstream community. He is attempting what he calls 'a situated phenomenology of the moral encounter', and the data are intended to be only illustrative.

CONCLUDING REMARKS

As with most dispositions, whether you tend to be a simplistic inductivist, a kitchen-sinker or a 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![1]

On the other hand, it may be over-reductionist 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 inquiry. 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.

SUMMARY

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

First, *simplistic inductivism* assumes that we need make no assumptions in studying the world. Instead, hypotheses will somehow 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.

Second, *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. Kitchen-sinking can be countered by:

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

Third, *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.

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NOTE

1 See Chapter 26 for further discussion of studies of these topics in relation to assessing 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, *Making Sense of Qualitative Data* (Sage, 1996), Chapter 1; Jennifer Mason, *Qualitative Researching* (Sage, 1996), Chapters 1–2; and David Silverman, *Interpreting Qualitative Data: Methods for Analysing Talk, Text and Interaction* (Sage, 1993), Chapter 1.

Useful but more specialist texts are: Pertti Alasuutari, *Researching Culture* (Sage, 1995), Chapter 13; Martyn Hammersley and Paul Atkinson, *Ethnography: Principles in Practice* (Tavistock, 1983), Chapter 2; and Anselm Strauss and Juliet Corbin, *Basics of Qualitative Research* (Sage, 1990), Chapters 1–4.

Exercise 5.1: strategies for simplistic inductivists

1 Attempt to relate your research ideas to *one* or *all* of the types of 'sensitivity' discussed in the chapter:

- historical
- cultural
- political
- contextual.

How might this lead you to reformulate your research interest?

2 Review any theoretical or research study with which you are familiar. Try to postulate new conditions which might allow you to develop a new but related research topic.

3 Try adding a few extra variables into your area of research interest. Now work out which of these variables would add most depth to your project and/or be most simply researched (e.g. are the data available, and can they be relatively easily gathered?)

Exercise 5.2: strategies for kitchen-sinkers

1 Draw a flow chart of no more than one page, setting out your key concepts and how they relate.
2 Review your area of research interest in terms of the following questions and formulate your research problem in terms of *one* kind of puzzle:
   - 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)
3 Use the zoom lens technique to focus in on some manageable 'unit of one' which might serve as an initial dataset to resolve your puzzle.

6

Theory in Qualitative Research

Some people become qualitative researchers for rather negative reasons. Perhaps they are not very good at statistics (or think they are not) and so are not tempted by quantitative research. Or perhaps they have not shone at library work and so are not tempted to write a purely theoretical dissertation.

However, the latter disposition begs the question of the relevance of theory to research. In part, this varies between social science disciplines. For, at least until recently, the different social sciences seemed to vary in the importance that they attached to theory. To take just two examples, psychologists and anthropologists, for all their differences, seemed to downplay theory.

In psychology, the benchmark was the laboratory study. For psychologists, the motto seemed to be: 'demonstrate the facts through a controlled experiment and the theories will take care of themselves'. Anthropologists were just as interested in 'the facts'. However, their most important facts were revealed in observational case studies of groups or tribes usually found in faraway lands. Nonetheless, until recently, most English-speaking anthropologists followed psychologists in elevating 'facts' above 'theories'.

By contrast, generations of sociology students have been made very aware of the primary importance attached to theory in their discipline. For instance, although undergraduate sociology courses tend to be split into three main areas (the 'holy trinity' of social theory, social structure and research methods), it is the course in social theory which is usually given the most prestige. Moreover, theory has recently become much more important in psychology and anthropology, as battles have commenced between traditionalists and qualitative 'discourse analysts' (in psychology) and 'post-modern' and gender theorists (in anthropology).

The social sciences' concern with theory is reflected in how PhD dissertations are assessed. As we saw in Chapter 4, 'the discovery of new facts' is rarely an important or even a challenging criterion in the assessment of most qualitative research. Any scientific finding is usually to be assessed in relation to the theoretical perspective from which it derives and to which it may contribute. This means that, while 'facts' are never unimportant, they are always subsidiary to theories. Successful dissertations display 'independent critical thought' (in the words of the University of London PhD regulations) by engaging with theory.

However, this begs an important question. What is 'theory'? In the following section I show why, for qualitative researchers, theory is altogether more interesting than the dry pages of theory textbooks.

WHAT IS THEORY?

Martin O'Brien (1993) has used the example of a kaleidoscope to answer this question. As he explains:

> a kaleidoscope . . . [is] the child's toy consisting of a tube, a number of lenses and fragments of translucent, coloured glass or plastic. When you turn the tube and look down the lens of the kaleidoscope the shapes and colours, visible at the bottom, change. As the tube is turned, different lenses come into play and the combinations of colour and shape shift from one pattern to another. In a similar way, we can see social theory as a sort of kaleidoscope – by shifting theoretical perspective the world under investigation also changes shape. (1993: 10–11)

How theory works as a kaleidoscope can be vividly seen in a concrete example taken from Eric Livingston (1987). Livingston asks us to imagine that we have been told to carry out some social research on city streets. Where should we begin? Some alternatives are set out in Table 6.1.

As Livingston points out, each of these different ways of looking involves basic theoretical as well as methodological decisions. Very crudely, if we are attached to social theories which see the world in terms of correlations between social facts (think of demography or macroeconomics), we are most likely to consider gathering official statistics (option 1 in Table 6.1). By contrast, if we think that social meanings or perceptions are important (as in certain varieties of sociology and psychology), we may be tempted by the interview study (option 2). Or if we are anthropologists or those kinds of sociologists who want to observe and/or record what people actually do *in situ*, we might elect for options 3 or 4. But note the very different views of people's behaviour we get from looking from on high (3), where people look like ants forming geometrical shapes like wedges, and from street level (4), where behaviour seems much more complex.

The point is that none of these data are more real or more true than the others. For instance, people are not really more like ants or complex actors. It all depends on our research question. And research questions are inevitably theoretically informed. So we *do* need social theories to help us to address even quite basic issues in social research.

TABLE 6.1 **Viewing a street: data possibilities**

1 Official statistics (traffic flow, accidents)
2 Interviews (how people cope with rush hours)
3 Observation from a tower (viewing geometric shapes)
4 Observation/video at street level (how people queue/organize their movements)

Source: adapted from Livingston, 1987: 21–7

However, O'Brien's analogy of a kaleidoscope and Livingston's example of viewing a city street only take us so far. What precisely is a 'theory'? And how does it differ from a 'hypothesis'?

Questions like this mean that I can no longer postpone the potentially tiresome business of defining my terms. Once I have completed these definitions, I will, once again, provide a set of concrete examples to clarify what I mean.

THEORIES, MODELS AND HYPOTHESES

In this section, we shall be discussing models, concepts, theories, hypotheses, methodologies and methods. In Table 6.2, I set out how each term will be used.

As we see from the table, *models* provide an overall framework for how we look at reality. In short, they tell us what reality is like and the basic elements it contains ('ontology') and what is the nature and status of knowledge ('epistemology'). In this sense, models roughly correspond to what are more grandly referred to as 'paradigms' (see Guba and Lincoln, 1994).

In social research, examples of such models are functionalism (which looks at the functions of social institutions), behaviourism (which defines all behaviour in terms of 'stimulus' and 'response'), symbolic interactionism (which focuses on how we attach symbolic meanings to interpersonal relations) and ethnomethodology (which encourages us to look at people's everyday ways of producing orderly social interaction).

Within the narrower sphere of qualitative research, Gubrium and Holstein (1997) use the term 'idiom' to encompass both the analytical preferences indicated by 'model' and tastes for particular vocabularies, investigatory

TABLE 6.2 **Basic terms in research**

Term	Meaning	Relevance
Model	An overall framework for looking at reality (e.g. behaviouralism, feminism)	Usefulness
Concept	An idea deriving from a given model (e.g. 'stimulus–response', 'oppression')	Usefulness
Theory	A set of concepts used to define and/or explain some phenomenon	Usefulness
Hypothesis	A testable proposition	Validity
Methodology	A general approach to studying research topics	Usefulness
Method	A specific research technique	Good fit with model, theory, hypothesis and methodology

Source: revised version of Silverman, 1993: 1

styles and ways of writing. They distinguish (and criticize) four different 'idioms':

- *Naturalism* A reluctance to impose meaning and a preference to 'get out and observe the field'.
- *Ethnomethodology* Shares naturalism's attention to detail but locates it in talk-in-interaction.
- *Emotionalism* Desires 'intimate' contact with research subjects and favours the personal biography.
- *Postmodernism* Seeks to deconstruct the concepts of the 'subject' and the 'field'.

Concepts are clearly specified ideas deriving from a particular model. Examples of concepts are 'social function' (deriving from functionalism), 'stimulus/response' (behaviouralism), 'definition of the situation' (interactionism) and 'the documentary method of interpretation' (ethnomethodology). Concepts offer ways of looking at the world which are essential in defining a research problem.

Theories arrange sets of concepts to define and explain some phenomenon. As Strauss and Corbin put it: 'Theory consists of plausible relationships produced among concepts and sets of concepts' (1994: 278).

Without a theory, such phenomena as 'death', 'tribes' and families' cannot be understood. In this sense, without a theory there is nothing to research.

So theory provides a footing for considering the world, separate from, yet about, that world. In this way, theory provides both:

- a framework for critically understanding phenomena
- a basis for considering how what is unknown might be organized (Gubrium, personal correspondence).

By provoking ideas about the presently unknown, theories provide the impetus for research. As living entities, they are also developed and modified by good research. However, as used here, models, concepts and theories are self-confirming in the sense that they instruct us to look at phenomena in particular ways. This means that they can never be disproved but only found to be more or less useful.

This last feature distinguishes theories from *hypotheses*. Unlike theories, hypotheses are tested in research. Examples of hypotheses, discussed in Silverman (1993), are:

- How we receive advice is linked to how advice is given.
- Responses to an illegal drug depend upon what one learns from others.
- Voting in union elections is related to non-work links between union members.

In many qualitative research studies, there is no specific hypothesis at the outset. Instead, hypotheses are produced (or induced) during the early stages of research. In any event, unlike theories, hypotheses can, and should be, tested. Therefore, we assess a hypothesis by its validity or truth.

A *methodology* defines how one will go about studying any phenomenon. In social research, methodologies may be defined very broadly (e.g. qualitative or quantitative) or more narrowly (e.g. grounded theory or conversation analysis). Like theories, methodologies cannot be true or false, only more or less useful.

Finally, *methods* are specific research techniques. These include quantitative techniques, like statistical correlations, as well as techniques like observation, interviewing and audio recording. Once again, in themselves, techniques are not true or false. They are more or less useful, depending on their fit with the theories and methodologies being used and the hypothesis being tested and/or the research topic that is selected. So, for instance, behaviouralists may favour quantitative methods and interactionists often prefer to gather their data by observation. But, depending upon the hypothesis being tested, behaviouralists may sometimes use qualitative methods – for instance in the exploratory stage of research. Equally, interactionists may sometimes use simple quantitative methods, particularly when they want to find an overall pattern in their data.

The relation between models, concepts, theories, hypotheses, methodology and methods can be set out schematically as in Figure 6.1. Reading the

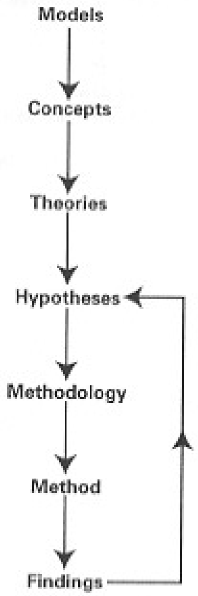

FIGURE 6.1 *Levels of analysis*

figure downwards, each concept reflects a lower level of generality and abstraction. The arrow from 'findings' to 'hypotheses' indicates a feedback mechanism through which hypotheses are modified in the light of findings.

Let me now try to put flesh on the skeleton set out in Figure 6.1 through the use of some concrete examples. Imagine that we have a general interest in the gloomy topic of 'death' in society. How are we to research this topic?

Before we can even define a research problem, let alone develop a hypothesis, we need to think through some very basic issues. Assume that we are the kind of social scientist that prefers to see the world in terms of how social structures determine behaviour, following the sociologist Emile Durkheim's (1951) injunction to treat social facts as real 'things'.

Such a model of social life will suggest concepts that we can use in our research on death. Using such a model, we will tend to see death in terms of statistics relating to rates of death (or 'mortality'). And we will want to explain such statistics in terms of other social facts such as age or social class.

Armed with our concepts, we might then construct a theory about one or other aspect of our topic. For instance, working with our assumption that death is a social fact, determined by other social facts, we might develop a theory that the rate of early death among children, or 'infant mortality', is related to some social fact about their parents, say their social class. From this theory, it is a quick step to the hypothesis that the higher the social class of its parents, the lower the likelihood of a child dying within the first year of its life. This hypothesis is sometimes expressed as saying that there is an 'inverse' relationship between social class and infant mortality.

As already implied, a model concerned with social facts will tend to favour a quantitative methodology, using methods such as the analysis of official statistics or the use of large-scale social surveys based on apparently reliable fixed-choice questionnaires. In interpreting the findings of such research, one will need to ensure that due account is taken of factors that may be concealed in simple correlations. For instance, social class may be associated with quality of housing and the latter factor (here called an 'intervening' variable) may be the real cause of variations in the rates of infant mortality. This overall approach to death is set out schematically in Figure 6.2.

Figure 6.3 sets out a very different way of conceiving death. For certain sociologists, social institutions are created and/or stabilized by the actions of participants. A central idea of this model is that how we label phenomena defines their character. This, in turn, is associated with the concept of 'definitions of the situation' which tells us to look for social phenomena in how meaning gets defined by people in different contexts. The overall message of this approach is that 'death' should be put in inverted commas and hence leads to a theory in which 'death' is treated as a social construct.

Of course, this is very different from the 'social fact' model and, therefore, nicely illustrates the importance of theories in defining research problems. Its immediate drawback, however, may be that it appears to be counter-intuitive. After all, you may feel, death is surely an obvious fact. Either we are dead or not dead and, if so, where does this leave social constructionism?

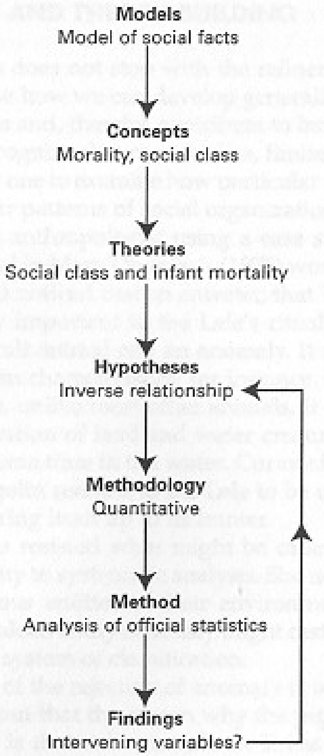

FIGURE 6.2 *Death as a social fact*

Let me cite two cases which put the counter-argument. First, in 1963, after President Kennedy was shot, he was taken to a Dallas hospital with, according to contemporary accounts, half of his head shot away. My hunch is that if you or I were to arrive in a casualty department in this state, we would be given a cursory examination and then recorded as 'dead on arrival' (DOA). Precisely because they were dealing with a President, the staff had to do more than this. So they worked on Kennedy for almost an hour, demonstrating thereby that they had done their best for such an important patient (cf. Sudnow, 1968a).

Now think of contemporary debates about whether or when severely injured people should have life-support systems turned off. Once again, acts of definition constitute whether somebody is alive or dead. And note that such definitions have real effects.

Of course, such a way of looking at how death is socially constructed (sometimes called 'social constructionism') is just one way of theorizing this phenomenon, not intrinsically better or worse than the 'social fact' approach.

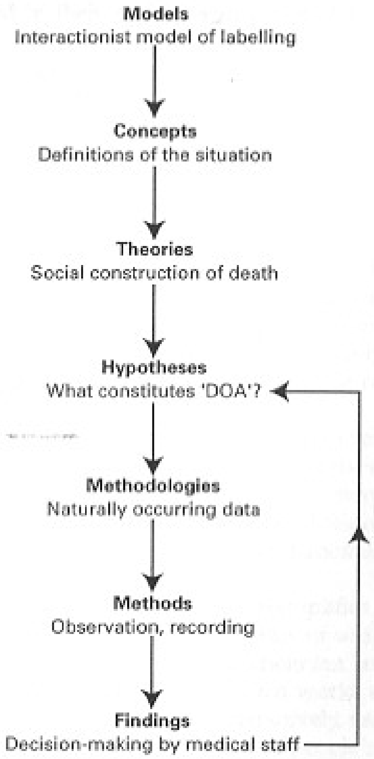

FIGURE 6.3 *Death as a social construction*

But, once we adopt one or another model, it starts to have a big influence upon how our research proceeds. For instance, as we have seen, if 'dead on arrival' can be a label applied in different ways to different people, we might develop a hypothesis about how the label 'dead on arrival' is applied to different hospital patients.

Because of our model, we would then probably try to collect research data that arose in such 'naturally occurring' (or non-research-generated) contexts as actual hospitals, using methods like observation and/or audio or video recording. Note, however, that this would not rule out the collection of quantitative data (say from hospital records). Rather, it would mean that our main body of data would probably be qualitative. Following earlier research (e.g. Jeffery, 1979; Dingwall and Murray, 1983), our findings might show how age and presumed moral status are relevant to such medical decision-making as well as social class. In turn, as shown in Figure 6.3, these findings would help us to refine our initial hypothesis.

GENERALIZATIONS AND THEORY BUILDING

Theorizing about data does not stop with the refinement of hypotheses. In this section, I will show how we can develop generalizations out of successfully tested hypotheses and, thereby, contribute to *building* theories.

First, we need to recognize that case studies, limited to a particular set of interactions, still allow one to examine how particular sayings and doings are embedded in particular patterns of social organization.

A classic case of an anthropologist using a case study to make broader generalizations is found in Mary Douglas's (1975) work on a Central African tribe, the Lele. Douglas noticed that an anteater, that Western zoologists call a 'pangolin', was very important to the Lele's ritual life. For the Lele, the pangolin was both a cult animal and an anomaly. It was perceived to have both animal and human characteristics: for instance, it tended only to have one offspring at a time, unlike most other animals. It also did not readily fit into the Lele's classification of land and water creatures, spending some of its time on land and some time in the water. Curiously, among animals that were hunted, the pangolin seemed to the Lele to be unique in not trying to escape but almost offering itself up to its hunter.

Fortunately, Douglas resisted what might be called a 'tourist' response, moving beyond curiosity to systematic analysis. She noted that many groups who perceive anomalous entities in their environment reject them out of hand. To take an anomalous entity seriously might cast doubt on the 'natural' status of your group's system of classification.

The classic example of the rejection of anomaly is found in the Old Testament. Douglas points out that the reason why the pig is unclean, according to the Old Testament, is that it is anomalous. It has a cloven hoof which, following the Old Testament, makes it clean; but it does not chew the cud, which makes it dirty. So it turns out that the pig is particularly unclean precisely because it is anomalous. Similarly, the Old Testament teachings on inter-marriage work in relation to anomaly. Although you are not expected to marry somebody of another tribe, to marry the offspring of a marriage between a member of your tribe and an outsider is even more frowned upon. In both examples, anomaly is shunned.

However, the Lele are an exception: they celebrate the anomalous pangolin. What this suggests to Douglas is that there may be no *universal* propensity to frown upon anomaly. If there is variability from community to community, then this must say something about their social organization.

Sure enough, there is something special about the Lele's social life. Their experience of relations with other tribes has been very successful. They exchange goods with them and have little experience of war.

What is involved in relating well with other tribes? It means successfully crossing a frontier or boundary. But what do anomalous entities do? They cut across boundaries. Here is the answer to the puzzle about why the Lele are different.

Douglas is suggesting that the Lele's response to anomaly derives from experiences grounded in their social organization. They perceive the pangolin favourably because it cuts across boundaries just as they themselves do. Conversely, the Ancient Israelites regard anomalies unfavourably because their own experience of crossing boundaries was profoundly unfavourable. Indeed, the Old Testament reads as a series of disastrous exchanges between the Israelites and other tribes.

By means of this historical comparison, Douglas has moved from a single-case explanation to a far more general theory of the relation between social exchange and response to anomaly. Glaser and Strauss (1968) have described this movement towards greater generality as a move from *substantive* to *formal* theory. In their own research on hospital wards caring for terminally ill patients, they show how, by using the comparative method, we can develop accounts of people's own awareness of their impending death (i.e. a substantive theory) and move to accounts of a whole range of 'awareness contexts' (formal theory).

Douglas's account of the relation between responses to anomaly and experiences of boundary crossing can also be applied elsewhere. Perhaps bad experiences of exchanges with other groups explains why some Israeli Jews and Palestinian Muslims are so concerned to mark their own identity on the 'holy places' in Jerusalem and reject (as a hateful anomaly) multiple use of the same holy sites.

In any event, Douglas's study of the Lele exemplifies the need to locate how individual elements are embedded in forms of social organization. In her case, this is done in an explicitly Durkheimian manner which sees behaviour as the expression of a 'society' which works as a 'hidden hand' constraining and forming human action. Alternatively, using a constructionist framework, one can look at the fine detail of people's activities without treating social organization as a purely external force (e.g. Moerman, 1974). In the latter case, people cease to be 'cultural dopes' (Garfinkel, 1967) and skilfully reproduce the moral order.

HOW TO THEORIZE ABOUT DATA

Unlike Moerman or Douglas, most readers will not bring to their research any very well-defined set of theoretical ideas. If you are in this position, your problem will be how you can use data to think in theoretical terms. The following list is intended merely as a set of suggestions. Although it cannot be exhaustive, it should serve as an initial guide to theorizing about data. It can also be read in conjunction with my discussion of the three kinds of research sensitivity in Chapter 5.

In carrying out your research, it is suggested that you think about the following five issues:

1. *Chronology* Can you gather data over time in order to look at processes of change? If not, it is worth searching out historical evidence which may at least suggest how your research problem came into being.

2. *Context* How are your data contextualized in particular organizational settings, social processes or sets of experiences? For instance, as Moerman shows, answering an interviewer's question may be different from engaging in the activity which is the topic of the interview. Therefore, think about how there may be many versions of your phenomenon.
3. *Comparison* Like Mary Douglas, who generated her theory by comparing how different groups treated anomalies, always try to compare your data with other relevant data. Even if you cannot find a comparative case, try to find ways of dividing your data into different sets and compare them. Remember that the comparative method is the basic scientific method.
4. *Implications* When you are reporting your research, think about how what you have discovered may relate to broader issues than your original research topic. In this way, a very narrow topic (e.g. how the Lele perceive the pangolin) may be related to much broader social processes (e.g. how societies respond to anomalous entities).
5. *Lateral thinking* Be like the Lele. Don't erect strong boundaries between concepts but explore the relations between apparently diverse models, theories and methodologies. Celebrate anomaly!

CONCLUSION

The philosopher of science Thomas Kuhn (1970) has described some social sciences as lacking a single, agreed set of concepts. In Kuhn's terms, this makes social research 'pre-paradigmatic' or at least in a state of competing paradigms. As I have already implied, the problem is that this has generated a whole series of social science courses which pose different social science approaches in terms of either/or questions.

Such courses are much appreciated by some students. They learn about the paradigmatic oppositions in question, choose A rather than B, and report back, parrot fashion, all the advantages of A and the drawbacks of B. It is hardly surprising that such courses produce very little evidence that such students have ever thought about anything; even their choice of A is likely to be based on their teacher's implicit or explicit preferences. This may, in part, explain why so many undergraduate social science courses actually provide a learned incapacity to go out and do research.

Learning about rival 'armed camps' in no way allows you to confront research data. In the field, material is much more messy than the different camps would suggest. Perhaps there is something to be learned from both sides, or, more constructively, perhaps we start to ask interesting questions when we reject the polarities that such a course markets?

Even when we decide to use qualitative and/or quantitative methods, we involve ourselves in theoretical as well as methodological decisions. These decisions relate not only to how we conceptualize the world but also to our theory of how our research subjects think about things.

But theory only becomes worthwhile when it is used to explain something.

Howard Becker (1998: 1) reports that the great founder of the Chicago School, Everett Hughes, responded grumpily when students asked what he thought about theory. 'Theory of what?,' he would reply. For Hughes, as for me, theory without some observation to work upon is like a tractor without a field.

Theory, then, should be neither a status symbol nor an optional extra in a research study. Without theory, research is impossibly narrow. Without research, theory is mere armchair contemplation.

SUMMARY

Research questions are inevitably theoretically informed. So we *do* need social theories to help us to address even quite basic issues in social research. But theories need to be distinguished from models and concepts:

- *Models* provide an overall framework for how we look at reality.
- *Concepts* are clearly specified ideas deriving from a particular model.
- *Theories* arrange sets of concepts to define and explain some phenomenon.
- *Hypotheses* are testable propositions.
- *Methodologies* define how one will go about studying any phenomenon.
- *Methods* are specific research techniques.

You can improve your ability to theorize about data by thinking about:

1. *Chronology* Gathering data over time in order to look at processes of change.
2. *Context* Considering how your data are contextualized in particular organizational settings, social processes or sets of experiences.
3. *Comparison* Trying to find ways of dividing your data into different sets and comparing each.
4. *Implications* Thinking about how what you have discovered may relate to broader issues than your original research topic.
5. *Lateral thinking* Exploring the relations between apparently diverse models, theories and methodologies.

Further reading

Becker's book *Tricks of the Trade* (University of Chicago Press, 1998) contains two chapters which are highly relevant to learning how to theorize about your data (Chapter 2 on 'Imagery' and Chapter 4 on 'Concepts'). Jaber Gubrium and James Holstein's book *The New Language of Qualitative Method* (Oxford University Press, 1997) is an invaluable, thought-provoking guide to the vocabularies, investigatory styles and ways of writing of different theoretical 'idioms'.

Exercise 6.1

Howard Becker reports that his colleague Bernard Beck responded to students seeking to theorize about their data by instructing them: 'Tell me what you've found out, but without using any of the identifying characteristics of the actual case' (1998: 126).

Becker gives the example of his own research on Chicago teachers which seemed to show that these teachers sought to improve their situation by moving to different schools rather than trying to get promoted in their present school. Using his data, but forbidden to talk about 'teachers' or 'schools', how might Becker have generated an account of his research that would have satisfied Beck?