**Summary**

This chapter has provided guidelines for the descriptive and explanatory analysis of data generated by cross-sectional designs. It has not considered the specific techniques, but many of the statistical guidelines provided in Chapter 6 apply to cross-sectional analysis.

The chapter considered the particular problems of using cross-sectional data for causal analysis – problems created by the absence of a time dimension to the design and the lack of randomized control groups. The general way of dealing with these problems is at the analysis stage rather than at the research design stage. The chapter outlined the logic of using statistical controls and multivariate analysis so that cross-sectional data can be useful for causal analysis. It then described one particular form of multivariate analysis – elaboration analysis – to provide an understanding of the ways in which the problems of causal analysis can be tackled. Finally, the chapter discussed a method of analysing data from repeated cross-sectional studies. In particular it focused on cohort analysis – how to construct and interpret cohort tables and to be aware of the limits of this type of analysis.

# PART V

# CASE STUDY DESIGNS

## 13

## CASE STUDY DESIGN

For many years the case study has been the ugly duckling of research design. Most research methods texts either ignore case studies or confuse them with other types of social research. When case study designs have been discussed they have generally been seen – from a methodological point of view – as 'soft' options. Some commentators believe case studies should be used only for exploratory research: to generate hypotheses for future testing with more rigorous research designs.

The methodology of case study design has not been neglected because case studies have been unimportant in social sciences. Indeed they have been fundamental to the substantive and methodological development of the social sciences. In social anthropology studies of tribes have been case studies *par excellence*. Community studies such as the Lynds' *Middletown* (1929) and small group studies like Whyte's *Street Corner Society* (1943) have made major contributions to the development of sociology as a discipline. Psychoanalysis has thrived on a case study approach (Freud, 1955). Educational research, evaluation research and organizational research have all made extensive use of case studies to foster their development.

The lack of a systematic discussion of case study designs has begun to be addressed in recent times. Yin (1989; 1993) has provided a particularly useful treatment of case study designs and a powerful defence of their value.

This chapter examines a range of ways of developing case study designs. This is a different task to that undertaken for experimental, longitudinal and cross-sectional designs because, as Yin points out, 'unlike other research strategies, the potential "catalog" of research designs has not yet been developed' (1989: 27).

## What is a case?

### *Units of analysis*

A case is the 'object' of study. It is the unit of analysis (see Chapter 2) about which we collect information. In case study designs it is the unit that we seek to understand as a whole. The unit of analysis may be a person about whom we try to build up an understanding that is informed by the *context* in which the whole case exists.

But case studies are not restricted to *individuals*. We can select many other types of cases – units of analysis. A marriage, a family or a household may serve as a case. *Places* such as a block of houses, a residential community, a region or a country can all serve as cases; as can *organizations* such as a business, a school, a government department or a union. A case might be an *event* such as a divorce rather than the person who divorces. Alternatively, a *decision* (e.g. decisions about downsizing an organization) might be the unit of analysis for the case study. The case study could involve understanding the decision as a whole, examining the process by which it was made, the participants, the consequences etc. *Time periods* might be the unit of analysis. We might study the 1960s or the 1990s: each of these can be the 'thing' about which we collect information.

### *Holistic and embedded units of analysis*

It is helpful to distinguish between cases as a whole and cases that consist of various levels or components. Yin (1989) uses the terms 'holistic' and 'embedded' designs to refer to this distinction.

Some cases consist of multiple levels or components. For example, a school as a case includes teaching staff, administrative staff, staff at different levels of seniority and experience, students, students at different year levels, parents, government and community members and so forth. A school can be conceived of at the 'holistic' level where we focus on characteristics of the school that apply to that level. The school exists as an entity and has school level characteristics (e.g. size, type of school, location, culture of school, a set of rules, a structure, a system of management, a school philosophy, strengths and weaknesses). But there are also many sublevels of elements to the school. A full picture of the school in all its complexity would only be obtained if we collected information from a wide range of the constituent elements (embedded units) of the larger unit.

A marriage provides another example. At the holistic level we would treat the marriage as a whole in terms of things such as its length, stage, type, level of conflict and intimacy, equality, modes of interaction etc. We could also treat the husband and the wife as embedded units and build up a much fuller and different picture of the marriage by incorporating the experiences and perspectives of the husband and the wife (subunits).



A well-designed case study will avoid examining just some of the constituent elements. It will build up a picture of the case by taking into account information gained from many levels. The final case study will tell us more than, and something qualitatively different from, that which any constituent element of the case could tell us. In the case of a school, the insights gained from students, teachers, parents, administrators and community members will probably differ and, when taken together, provide a much fuller, more complex understanding of the whole than would the perspective provided by any particular element of the case. The whole is greater than the sum of its parts.

Since many cases will consist of different elements, different methods of data collection may be required for the different elements. A survey of students might be appropriate; observation of classrooms and staff meetings might also be worthwhile; while interviews might be a good way of gaining information from teachers. An analysis of school records and archives could provide useful information about the historical context within which the school operates.

## Case studies and theory

Although some case study researchers conduct case studies as though they only have to collect the facts about the case and write about these in an engaging way, the task of the case study researcher is fundamentally theoretical. Collecting and analysing information from case studies must be guided by theory.

### *Explanatory case studies*

Case study designs differ from the designs discussed earlier in this book in that they seek to achieve both more complex and fuller explanations of phenomena. They seek to achieve idiographic as well as nomothetic explanations (see Chapter 14). They also differ in the way in which they go about trying to achieve causal explanations (see Chapters 14 and 15).

Case study research in social sciences must have a theoretical dimension. Without a theoretical dimension a case study will be of little value for wider generalization – one of the goals of social science research. This section considers three ways in which theory is used in designing case studies.

#### Theory testing

This approach, which is seen by Yin (1989) as being at the heart of case studies, begins with a theory, or a set of rival theories, regarding a particular phenomenon. On the basis of a theory we predict that a case with a particular set of characteristics will have a particular outcome. The theory may be simple or complex.

For example, the case study might begin with the following question: 'What is the effect of devolved, school based control of staffing on the quality of education in a school?' Devolved or school based staffing control involves each school appointing, dismissing and promoting staff as well as setting pay levels and teaching conditions. Devolved staffing systems can be contrasted with centralized systems where public service bureaucrats manage these staffing matters at head office level. We might begin with the proposition that devolved control will yield much better educational outcomes than centralized control. The reasoning would be that local control makes people far more accountable, enables a school to build a staff profile appropriate to its needs, and makes it much easier to reward staff who are achieving the outcomes desired by the school. The implicit theory here is that educational quality is fundamentally a result of teacher quality (as opposed to the nature of students, school resources, organizational structure, parental involvement) and that teacher quality is a function of teacher accountability and teacher 'fit' with the school.

The selection of a case to test this theory would lead us to find a school that has introduced a devolved staffing system. By thoroughly investigating the school (including the 'embedded elements' such as teachers, management, parents, past and present students) we would build up a picture of the quality of education in the school (however defined) and map out any links that exist between the educational quality and the staffing system. If possible we would collect historical data relating to the time before the devolved system was introduced to see if there was evidence of improvement after its introduction. We would also seek to identify what else was happening in the school that might have led to improvements in the quality of education or factors that might have prevented improvements despite the introduction of the devolved system.

The point of the case study would be to see if the theory actually worked in a real life situation. If it did work then the theory is supported (not proven). If it did not work then we would seek to understand, from a careful analysis of the case, why the predicted outcome (better education) did not eventuate. Is it because the theory is completely wrong? Does the theory require some refinement? Is the theory applicable only under specific circumstances?

Instead of this simple proposition we might develop a more complex model that specified what would happen with the introduction of a devolved staffing system. The model might anticipate positive effects in particular aspects of educational outcomes and negative effects in others. It might specify positive effects only after a given period of time, and only when the devolved system is implemented in a particular way (e.g. with full teacher and community consultation, with appropriate checks and balances). In other words, the theory we specify might anticipate that when conditions $A + B + C + D$ are met then the devolved system will achieve improvements in aspects $X$ and $Y$ after a period of $N$ years.



Regardless of the complexity of the theory, this approach to case studies begins with a set of expectations derived from previous research and/or theories. We approach the case study with the purpose of testing our theory in a real life situation. Clearly the selection of the real life situation (the case) would need to match the conditions under which the theory proposes particular outcomes (Chapter 14).

### Theory building case studies

Using a theory building approach to case studies we select cases to help develop and refine the propositions and develop a theory that fits the cases we study. For example, we might start with the simple proposition that the introduction of devolved systems will improve educational outcomes. We might select a school where such a system has been introduced and find that educational outcomes did, in fact, improve. We might then find a school in which the introduction of the devolved staffing system did *not* produce better educational outcomes. Why did it produce good outcomes in some cases and not others?

Our analysis of each case would aim to highlight differences between cases where it did and did not work. Our analysis would also identify commonalities among cases where devolved staffing was successful and commonalities among cases where it was unsuccessful (e.g. the way in which the system was implemented). These case studies could then be used to develop a set of propositions about the conditions and context under which devolved staffing systems lead to improvements (Chapter 15).

The difference between the theory testing and theory building approaches is that in the former we *begin* with a set of quite specific propositions and then see if these work in real world situations. In the theory building model we begin with only a question and perhaps a basic proposition, look at real cases and *end* up with a more specific theory or set of propositions *as a result* of examining actual cases.

### Clinical case studies

While the two above case study approaches are *theory centred* in that the goal is to use the case to test, refine and develop theoretical generalizations, the clinical case study uses theories very differently. Clinical case studies are *case centred*. They use theories to understand a case.

The way in which a clinician deals with a client illustrates this style of case study. A child may visit a psychologist because he is performing poorly at school and has become disruptive. The task of the clinical psychologist is to work out what is going on in this case and why it is happening. A good clinician will start with symptoms: what is happening at school? In what areas of school is the child performing poorly? The clinician will undoubtedly have a range of possible explanations for poor school performance and disruptive behaviour at school. She will gather

information to build up a picture of what is going on. She will develop hunches and collect further information to test these hunches. The psychologist may give the child various tests to establish his intelligence quotient, and any specific learning disabilities. The psychologist does this to see if a cognitive explanation of the learning difficulties fits. She may have the child's eyesight or hearing tested to see if sight or auditory problems are contributing to his learning disabilities. The psychologist may evaluate the child's relationship skills to establish if problems in this sphere contribute to the learning difficulties. Alternatively she might probe into the child's family relationships to see if something that is going on at home is creating problems at school. Perhaps the child and the teacher do not get on and the problems spring from their relationship difficulties. It is also possible that the child is suffering from anxiety, depression or some other clinical syndrome.

The point is that the clinician will have a battery of potential explanations for the child's problems. Her task is to build up a full picture of the case so that she can evaluate which explanation best fits the facts of the case. Having correctly diagnosed the nature or cause of the child's problems the clinician can begin treatment.

In this example the goal is to understand the case and solve a problem for this case. The purpose is not to test or *develop* theories but to *use* existing theories. The clinician works with plausible rival hypotheses and progressively collects information to help sort out which fits best.

The same logic can apply in consultancy case studies. An organization might be suffering a great deal of conflict and poor morale. A consultant may have a range of possible explanations for this and should go about the task of collecting information about the organization that can help establish which of the explanations fits this particular organization. The goal is not to develop a theory of organizational conflict and morale loss but to identify which of several alternative theories makes most sense in this particular case.

### *Descriptive case studies*

Although this chapter and the following two chapters focus on explanatory case studies it is important to discuss descriptive case study designs. It is important to consider descriptive case studies for two reasons. The first is that unless we can do good descriptions of cases we will be in no position to achieve good explanations using those cases. The second reason is that a discussion of descriptive case studies further highlights the importance of theory in case study research.

## Description and theory

The problem in any description is where to begin and where to end. This is especially so with case studies. A case study deals with the *whole* case



| | | Acceptance of cultural goals: Accepts | Acceptance of cultural goals: Rejects |
|---|---|---|---|
| Accepts means of achieving the goals: Accepts | | Conformists | Ritualists |
| Accepts means of achieving the goals: Rejects | | Innovators | Retreatists |

Figure 13.1 *Theoretically derived typology of deviants*

but this cannot possibly mean that the case study consists of *everything* about the case. To describe everything is impossible: there must be a focus.

Description must not, indeed cannot, be atheoretical. We always select and organize that which we describe. Descriptions will highlight aspects of the case. It will be more like a painting of a landscape than a photograph; it will be an interpretation rather than a mirror image. We might do this using explicit theories or pre-existing conceptual categories, or using implicit theories of what is relevant and what categories are important. For example, when describing a person I might, as a sociologist, highlight their personality, ethnicity, age, gender and the like because I see these as the critical elements that constitute a social person. Another person, a biologist, might focus much more on physical, genetic attributes. Each of the descriptions reflects a different focus of interest and a different assessment of what is relevant in describing a person.

## Typologies and ideal types

Descriptive case studies may consist of single or multiple cases. One way of reporting multiple case studies is to use typologies and ideal types.

Typologies may be theoretically derived or empirically derived. A theoretically derived typology is one that is logically or theoretically possible. For example, Merton (1968) developed a typology of types of deviants based on the notions of cultural *goals* and institutionalized *means* of achieving those goals (Figure 13.1). Thus the *conformist* is a theoretical construct defined as someone who accepts the *goals* of culture and the *means* by which the culture prescribes the achievement of these goals. The *innovator* accepts the goals but not the prescribed means for achieving them. The *ritualist* rejects cultural goals but nevertheless goes along with the prescribed cultural behaviour. The *retreatist* rejects both the goals and the means of achieving those goals.

Typologies constructed in this deductive way represent a set of ideal types (Coser, 1977: 223–4). Ideal types are a theoretical description of sets

of characteristics we expect will go together in a case. They represent a pure or theoretical type that is not necessarily found in that pure form in empirical reality. For example, while Weber identified the characteristics of a 'pure' form of bureaucracy (Gerth and Mills, 1946: 196–204) we may find no examples of actual bureaucracies that function in exactly the way Weber postulates.

Deductive typologies and ideal types can provide a useful way of analysing case studies. The ideal type can serve as a yardstick against which to compare actual cases (Chapter 15). The template provided by the ideal type can guide the way in which we investigate the case; it can guide what we look for.

#### Inductive typologies

An inductively derived typology is one in which we start with a question and then examine cases in the light of the question. A comparison of cases can then highlight *clusters* of similar cases. For example, I commenced a set of case studies with the question, 'How do adults get on with their elderly parents?' I studied a range of cases and found that cases differed along a range of dimensions (e.g. level of conflict, tension, dependence, balance, enjoyment, intimacy and intrusiveness). I then noticed that cases tended to exhibit distinct clusters of characteristics. On the basis of these common elements I characterized particular cases as being of a particular type. Working from a question and actual cases I then developed a fourfold case based typology of adult–parent relationships that I called parent centred, child centred, remote, interdependent (de Vaus, 1994).

The particular characteristics of each type need not concern us here. The point is that typologies can be developed in different ways – either deductively or inductively. Regardless of the way in which they are developed they can provide a helpful template for conducting, analysing and reporting (Chapters 14 and 15) descriptive case studies.

## Other elements of case study designs

When designing case studies there are a number of elements, apart from those discussed above, that can be built into the design.

### *Single or multiple cases?*

A case study design can be based on single or multiple cases. A single case design will normally be less compelling than multiple case designs. Using the logic of replication (Chapter 14) a single case represents only one replication and does not necessarily provide a tough test of a theory. However, we may have little choice. Limited access to cases or the extreme nature of the case may mean that we can only study a single



example. In other situations we may rely on a single *critical* case. This can be appropriate when we have a clear theory with well-formulated propositions and we have a single case that meets all the requirements of the theory. Such a case can provide a moderately convincing test of a complex theory (Yin, 1989).

Multiple cases, strategically selected, can provide a much tougher test of a theory and can help specify the different conditions under which a theory may or may not hold. Furthermore, multiple cases are essential if the case studies are being used for inductive purposes (see Chapter 15 on analytic induction). Given sufficient resources and access to cases, multiple case designs will normally be more powerful and convincing and provide more insights than single case designs.

However, when using a multiple case design we should endeavour to treat each case as a single case so that we are able to establish a full account of that case before engaging in cross-case comparisons. The unity of the single case should be respected (Stake, 1994; Yin, 1989: 56–7).

### *Parallel or sequential?*

A parallel design is one where all the case studies are done at once (e.g. different investigators each doing a case). Only at the completion of the investigation of each case study are comparisons made between the cases. This strategy can be appropriate when the project adopts a simple theory testing approach.

A sequential design is one where case studies follow one another. Using this approach one investigator could, in principle, conduct each case study. An advantage of the sequential approach is that the selection of each case and some of the issues examined can be informed by puzzles identified in earlier cases. One case can throw up ideas that can influence the selection of subsequent cases and that can be followed up in these later cases (Chapters 14 and 15). When adopting a more inductive, theory building approach a sequential design is more appropriate than a parallel approach.

### *Retrospective or prospective?*

Most case studies, and all explanatory case studies, will incorporate a time dimension. Without this dimension any adequate causal explanation is not possible. Case studies can provide a good way of carefully mapping the sequence of events, which is the basis of causal explanations. It has to be decided whether this time dimension is obtained retrospectively or prospectively.

A *retrospective* design involves collecting, on the one occasion, information relating to an extended period. This requires the reconstruction of the history of the case. This might be done through the use of archival records and documents, or interviews with people who participated in or observed past events.

This design has the obvious problems associated with loss of evidence, reconstruction of the past in the light of the present, and mistaking the sequence in which events occurred. However, in many situations there is little choice but to draw on people's ability to recall the past. With case studies the use of multiple sources of evidence can reduce the problems that go with this approach.

A *prospective* design involves tracking changes forward over time. It has the obvious advantage of enabling the investigator to look at events as they occur rather than relying on partial and reconstructed accounts. Depending on the issues under consideration and the types of cases, prospective case studies might last for years. The obvious disadvantage can be the time and resources required. These constraints can severely limit the number and range of cases that can be studied. This, in turn, can result in very limited tests of a theory.

Regardless of whether a retrospective or prospective approach is adopted, the goal is to build up a clear and reasonably detailed picture of the sequence in which events took place and of the context in which they occurred. To build meaningful causal explanations we must get the sequence and the context right. A convincing causal explanation will be able to track the 'story' by which one (or a set of) event(s) ended up producing a particular outcome.

## Types of case study designs

Unlike other types of research design there is, as yet, no inventory of types of case study design (Yin, 1989). However, the discussion of elements of case study designs discussed in this chapter provides a way of thinking about the different ways in which case studies can be structured. These elements are:

- descriptive or explanatory
- theory testing or theory building
- single case or multiple case
- holistic or embedded units of analysis
- parallel or sequential case studies
- retrospective or prospective.

A cross-classification of these elements results in the logical possibility of 64 different variations of case study designs (Figure 13.2). This figure is presented not to overwhelm with the range of possibilities, nor to develop 64 different names for different case study designs, but to provide a way of thinking about different alternatives for shaping a case study design. The decisions made in relation to these six elements will result in different ways of conducting the case study, and of analysing the data collected in the study.

| Explanatory? | | | Descriptive | | | | Explanatory | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| Time | | | Retrospective | | Prospective | | Retrospective | | Prospective | |
| Case order | | | | | | | | | | |
| Cases | Units | Theory | Parallel | Sequential | Parallel | Sequential | Parallel | Sequential | Parallel | Sequential |
| Single case | Embedded units | Testing | | | | | | | | |
| | | Building | | | | | | | | |
| | Holistic | Testing | | | | | | | | |
| | | Building | | | | | | | | |
| Multiple cases | Embedded units | Testing | | | | | | | | |
| | | Building | | | | | | | | |
| | Holistic | Testing | | | | | | | | |
| | | Building | | | | | | | | |

Figure 13.2 *Variations of case study designs*

## What a case study is not

Before ending this chapter it is worth saying what case study designs are *not*.

### *Not the one-shot case study*

The taxonomy of research designs outlined by Campbell and Stanley (1963) and by Cook and Campbell (1979) has led to misunderstandings of what a case study design can be. Using the template of the classic experimental design they identify a design which they refer to as the *one-shot case study*. This is represented diagramatically in Figure 13.3.

As a simple example of this type of design we could consider a group of children whose parents divorced within the last year and for whom we obtained a measure of emotional adjustment. In effect this design consists of members of an 'experimental' group only and relies only on 'post-intervention' information.

The earlier discussions of the logic of experimental, longitudinal and cross-sectional designs indicate how this design tells us nothing of value about the impact of parental divorce on the emotional adjustment of children. We cannot tell from this design whether children with divorced parents are in any way *distinctive* from other children, and whether there is any *change* in emotional adjustment following divorce.

Representing this type of design as an example of case study design is unfortunate and provides a misleading picture of the potential of a well-conceived case study design. Indeed Cook and Campbell (1979) have acknowledged this and have renamed the design as a *one-group post-test only* design, and now stress that it should not be confused with true case study design.

### *Not a data collection method*

Case studies have frequently been equated with qualitative methods, especially participant observation and unstructured, in-depth interviews. This identification is no doubt partly because some of the early, influential case studies used participant observation methods (e.g. *Street Corner Society* and *Middletown*). Similarly, most ethnographic case studies used in social anthropology employ participant observation, and ethnographies and case studies have sometimes been taken to be the same thing.[1] In other contexts case studies are equated with unstructured in-depth interviews that enable the researcher to build up a much fuller picture of a case than is possible with more quantitative based methods of data collection. In general there is a tendency to equate 'thick description' (Geertz, 1973) with case studies.

If we equate case studies with a particular data collection method we misunderstand case study design. Yin argues that case study *design* is



| Method of allocation to groups | Time 1 ($T_1$) Pre-test | Intervention ($X$) | Time 2 ($T_2$) Post-test |
|---|---|---|---|
| 'Experimental' group | None | Group is uniform in terms of the proposed causal variable | Measure on outcome variable ($Y$) |
| 'Control' group | None | No 'treatment' | None |

Figure 13.3 *One-shot case study design (one group, post-test only design)*

> the logic whereby initial hypotheses or research questions can be subjected to empirical testing. Deciding between single and multiple case studies [see later], selecting specific cases to be studied, developing a case study protocol and defining relevant data collection strategies [e.g. the period to be covered – retrospective, prospective] are all part of case study design. (1993: 33)

Any method of data collection can be used within a case study design so long as it is practical and ethical. Indeed, one of the distinguishing features of the case study method is that multiple methods of data collection will often be employed (see Chapter 14). Case studies seek to build up a full picture of a case, its subunits (see earlier discussion on units of analysis and embedded designs in this chapter) and its context.

## Summary

Case study designs constitute a major design for social research. They offer a flexible approach, which can result in an extensive variety of particular designs. They make use of a wide variety of data collection methods and they are particularly suited to using a diverse range of units of analysis. Case studies in social science research should be fundamentally theoretical.

Case study designs are particularly suited to situations involving a small number of cases with a large number of variables. The approach is appropriate for the investigation of cases when it is necessary to understand parts of a case within the context of the whole. Case studies are designed to study wholes rather than parts. They are also particularly

appropriate when we need to investigate phenomena where it is not possible to introduce interventions. While experimentally based designs allow us to exclude the influence of many variables, case study designs are particularly useful when we do not wish or are unable to screen out the influence of 'external' variables but when we wish to examine their effect on the phenomenon we are investigating.

### Note

1 Yin (1993: 60ff) carefully distinguishes ethnographies from case studies. One reason Yin provides for this distinction is that ethnographies are not suited to theory testing whereas case studies are. A further basis for the distinction is that ethnographies do not assume a single objective reality that can be investigated following the traditional rules of scientific inquiry.

# 14

# ISSUES IN CASE STUDY DESIGN

All research designs should be internally valid and externally valid, should produce reliable results and should be amenable to replication. Case study designs are often seen to be deficient in all these areas. However, careful attention to these matters at the design stage can deal with many of these criticisms.

## Methodological issues

### *Internal validity*

The internal validity of many of the research designs examined in Parts II to IV of this book depends on having groups that are comparable except in relation to specific key variables. Internal validity in these earlier designs relies on screening out the influence of variables other than the key causal variables. They involve focusing on a small number of variables and removing the influence of other variables by controls of one sort or another. Threats to internal validity stem from the danger that factors other than our key variable are producing any changes we observe.

#### Idiographic and nomothetic explanations

By focusing on a very restricted range of variables the earlier designs achieve a narrow or *nomothetic* explanation. That is, they achieve partial explanations of a class of cases rather than a 'full' explanation of a particular case. They involve an examination of fewer causal factors and a larger number of cases. For example, a nomothetic explanation might examine divorces as a class of cases and identify key factors that contribute to divorce overall (e.g. age of marriage, external family stressors etc.). The nomothetic approach contrasts with *idiographic* explanation. Idiographic explanation focuses on particular events, or cases, and seeks to develop a complete explanation of each case. For example, an idiographic explanation of a divorce would examine a particular divorce and develop a *full* picture of why *that* divorce occurred.

Case studies can be utilized for both types of explanation. A case study adopts an idiographic approach when a full and contextualized understanding of a case is achieved. But case study designs can adopt a nomothetic approach. This occurs when particular cases are used to achieve a more generalized understanding of broader theoretical propositions (see discussion of case studies and theory in Chapter 13).

By developing a full, well-rounded causal account, case studies can achieve high internal validity. By seeing particular causal factors in combination with other causal factors we can assess both the relative importance of particular causes and the way in which various causes interrelate. As such, case studies can achieve a sophisticated and balanced account of causal processes and, in so doing, avoid mistaking cause with correlation.

### Wholes, not just parts

The designs discussed in earlier parts of this book focus on *variables* rather than *cases*. They examine how different traits of cases are associated with one another (Mitchell, 1983: 192). Typically, however, these traits are not considered within the context of the case of which they are part. By wrenching traits out of the context in which they occur we strip them of much of their meaning and consequently risk misreading their meaning and significance and thus misunderstanding their causes.

Blumer (1956) uses the term 'variable analysis' to describe analysis that focuses on variables rather than cases. He argues that although such analysis can establish statistical relationships between variables they do not provide much insight into causal processes. He argues that:

> The independent variable is put at the beginning part of the process of interpretation and the dependent variable is put at the terminal part of the process. The intervening process is ignored . . . as something that need not be considered. (1956: 97)

Although this is a somewhat simplistic version of the statistical analysis of relationships between variables, it contains some truth. While many of the designs covered earlier in this book can isolate variables that produce particular outcomes, they are not so good at telling us *why* they produce these outcomes.

Case studies, on the other hand, emphasize an understanding of the whole case and seeing the case within its wider context. Goode and Hatt (1952) describe case studies as

> a way of organizing social data . . . to preserve the *unitary character of the social object being studied* . . . it is an approach which views any social unit as a whole. (1952: 331, their italics)



They stress the importance of looking at parts within the context of the whole.[1] The case also must be seen within the context in which it exists. By examining this context fully the researcher can gain a fuller and more rounded picture of the causal processes surrounding a particular phenomenon. Yin argues that

> a major rationale for using [case studies] is when your investigation must cover both a particular *phenomenon* and the *context* within which the phenomenon is occurring either because (a) the context is hypothesized to contain important explanatory information about the phenomenon or (b) the boundaries between phenomenon and context are not clearly evident. (1993: 31)

The study of context is important because behaviour takes place within a context and its meaning stems largely from that context. The same behaviour can mean very different things depending on its context. Furthermore, actions have meanings to people performing those actions and this must form part of our understanding of the causes and meaning of any behaviour. To simply look at behaviour and *give* it a meaning rather than *take* the meaning of the actors is to miss out on an important source of understanding of human behaviour.

An example can illustrate this point. The research literature on extended families assumes that exchange of help and care between the generations (e.g. adults and older parents) reflects the quality and strength of intergenerational ties. However, if we examine the meaning of intergenerational help within its context we might interpret its meaning differently. Within a particular family there may be a great deal of intergenerational help: the adults in the family are attentive to the elderly parents, visiting often and helping out where they can. But is this a sign of family solidarity, closeness and caring, or is it a sign of control, dependence, manipulation and blackmail (adults help out of guilt, concern about inheritance, worry that siblings will be favoured unless they help)? Is the help reciprocal? What were the patterns of help in the past? What is the history of parent–child relationships in this family? What particular forms of help are given? What type of help is withheld? Why do people give the help? How happily is it accepted? What happens if help is not offered? What are the norms regarding family loyalty and help within the community, class and ethnic group in which this family is situated? What are the rules of inheritance in this society? What are the legal obligations, if any, of children to care for elderly parents? We would need to address these and other questions before we could build up a picture of the meaning of intergenerational help and the possible causes of help in a particular case. To isolate the behaviour from this broader context and to strip it of the meaning given to it by actors is to invite misunderstanding, and thus threaten the internal validity of the study.

To take a further example: our research goal may be to gain an understanding of drug addiction. To make sense of addiction in a particular

case we would need to understand its social and institutional context. Was addiction the outcome of medical treatment to control pain (addiction from treatment)? Did it originate with emotional distress following relationship breakdown (addiction as escape)? Was drug use a taken for granted part of the immediate social context in which the person lived (addiction as conformity)? Did the person come from a very anti-drug background (addiction as rebellion)? The context is all-important for understanding the phenomenon (addiction) and presumably is crucial in shaping appropriate ways of managing or treating the addiction.

In summary, explanations based on case studies involve much more than explaining variation in one variable in terms of variation in another variable. Case study designs are devised to yield a sensible, plausible account of events and in this way achieve internal validity. They achieve explanations by building a full picture of the sequence of events, the context in which they occur, and the meaning of actions and events as interpreted by participants and their meaning as given by a context. In the end an adequate causal explanation is one that makes sense. It involves telling a plausible, convincing, and logically acceptable story of how events unfold and how they are linked to one another.

### History and maturation

The way in which 'history' and maturation can threaten the validity of causal explanations has been discussed in previous chapters. Experimental designs seek to deal with the problems of history and maturation by using randomized control groups and focusing on *differences* in change between these groups rather than on absolute change (Chapters 4 and 5).

Case study designs try to deal with the 'problem of history' and maturation by looking closely at the wider context and exploring the extent to which these sorts of concurrent events contribute to observed outcomes. That is, rather than *eliminating* historical/contextual and maturational factors from the analysis (by controlling them out) the case study designs *include* them in order to enhance our understanding. In this way we can arrive at a fuller and richer understanding while, at the same time, avoiding the threats that history and maturation present to the internal validity of our case study conclusions. The quality of the case study will largely rest on how well it identifies these historical/contextual and maturational factors and includes them in any explanation.

### Reactive effects

Simply *doing* a case study can produce changes in the case and we can confuse the effects of doing the study with the effects of other variables. Whether or not this will be a problem in a particular case study will depend, in part, on the type of case, the data collection methods, and the nature of the outcome variables.



A life history of a dead person, which relies on documentary evidence, is unlikely to suffer from reactive effects. Retrospective designs will be less prone to reactive effects than prospective studies. But a study of a tightly knit group in which the researcher uses participant observation and interviews will inevitably affect the way in which the group operates.

We can try to reduce the threats of reactivity by using unobtrusive data collection methods (Kellehear, 1993). For some types of cases this can work well. In others it may not be possible to use unobtrusive methods for either practical or ethical reasons. If data can only be obtained by interviews or observation it is very difficult to avoid the reactivity of being a 'foreign object' (Denzin, 1978: 200).

### *External validity*

While case studies may achieve excellent internal validity by providing a profound understanding of a case, they have been widely criticized as lacking external validity. A profound understanding of a case, it is argued, provides no basis for generalizing to a wider population beyond that case. A case is just that – a case – and cannot be representative of a larger universe of cases.

### Theoretical and statistical generalization

It is correct to say that case study designs cannot provide a basis for making statistically valid generalizations beyond that particular case (see Chapter 15 on statistical analysis). A case study may consist of just one case (one community, one organization, one person) and we cannot be confident, in any statistical sense, that the case represents a wider class of cases. Even if we conduct multiple case studies, say 10, our $n$ remains too small for credible statistical generalization.

However, case studies do not strive for this type of external validity. There are two types of generalization: statistical and theoretical. *Statistical generalization* is achieved by using representative random samples. On the basis of statistical probability we generalize our findings to a wider population that our sample is designed to represent.

Theoretical generalization involves generalizing from a study to a *theory*. Rather than asking what a study tells us about the wider population (statistical generalization) we ask, 'What does this case tell us about a specific theory (or theoretical proposition)?' In Chapter 13 I argued that case study designs are fundamentally theoretical. They are designed to help develop, refine and test theories. They do this using the logic of replication.

### Replication

To understand the concept of theoretical generalization it is necessary to understand the logic of *replication* in research design (Yin, 1989). This is the logic that lies at the heart of generalizing from experiments. Since experiments typically do not use representative probability samples they provide no basis for statistical generalization to a wider population. An experiment might employ an excellent design that ensures a high degree of internal validity but unless it is based on a probability sample we do not know if those findings will occur in the wider population. There may be something about the particular sample that means that the findings will apply only to the people who participated in the experiment.

Experiments using non-probability samples argue for external validity on the basis of *replication logic*. That is, if the experiment can be repeated again and again under the same conditions and produce the same results we can be confident that the experimental results will hold up more generally. If the experimental results hold up under different conditions and with different types of experimental participants our confidence in the generalizability of the results grows. If the experimental results cannot be replicated under particular conditions then we will need to specify conditions to which the experiment cannot be generalized. We gain confidence in the value of and robustness of experimental findings by repeating the experiment. Where the experiment is repeated with different samples and under different conditions we get more of a sense of the limits of generalizability. Repeated experiments provide some idea of the range of generalization. If an experiment only works once with a particular group of people and the results cannot be reproduced we would have little confidence in its findings or its applicability more generally.

Experimental science is based on the logic of replication. No finding is accepted on the basis of a single experiment. The same logic applies to case studies. Indeed a single case study can be thought of in much the same way as a single experiment. In Chapter 13 I argued that the findings of a case study are tested by repeating case studies (more of this later). We can examine different types of cases under different conditions and maybe even using different methods. The more the cases behave in a way that is consistent with the way we would expect on the basis of our theory, the more confident we are about our theory. If we find that some cases do not behave in the way we would have predicted we need to modify our theory to accommodate that case in the same way that theories will be modified to account for the experiment that does not give the expected results.

### Strategic selection of cases

The external validity of case studies is enhanced by the *strategic* selection of cases rather than by the statistical selection of cases. The strategic selection of cases contributes to *literal* and *theoretical* replication (Yin, 1989).



Case study designs involve selecting cases for theoretical and targeted purposes. We select a case because it tests whether a theory works in particular, real world situations. Alternatively, we select a case because we think it might disprove a proposition, or because we want to see if the theoretical proposition works under particular conditions (like repeating an experiment under different conditions).

The strategic selection of cases means that we know something of the characteristics of a case before the case study proper begins. Since cases are selected because they meet particular requirements (as opposed to being randomly selected) we must first know something about each case. This may require extensive case screening before actual cases are finally selected (see later in this chapter).

How cases are selected strategically can be illustrated by using the earlier example of devolved staffing systems. If we begin with the proposition that 'the introduction of devolved staffing systems in schools improves the quality of education' our first case study will target a school in which such a system had been introduced. We would predict that in this school there would be evidence of educational improvement. Furthermore, we would predict that an intensive study of the school would show the following:

1 The improvement in education could be traced back to the new system.
2 The reasons the new system was effective were those that were anticipated in the theory (better fit of teachers with school, greater feelings of accountability and greater belief that effort was rewarded).

If we found that the case matched what the theory predicted, a *literal replication* of the theory would have been achieved (Yin, 1989: 54). Since this outcome may be due to chance we could seek out some other schools in which the devolved system was introduced and examine its impact on the quality of education in those schools. If the same patterns occur we have further literal replications.

We could then attempt to test the theory in a different way. Since we had concluded that the improvement in education was due to the introduction of the devolved system we would expect, on the basis of our theory, that where a centralized system was retained there would be no improvement in education. We would deliberately seek out instances of schools still functioning with a centralized system to see whether our theoretical predictions were supported.

As a tougher test we might look for cases that run counter to the basic proposition: that is, cases of a failed devolved system. Alternatively, we might look for cases of centralized systems that, over the same period, also delivered improvements in educational quality. However, our

theory may be such that it can account for these 'deviant' cases. It might specify that the basic relationship will only work under particular conditions. If we find that the basic propositions do not hold up and this is predictable on the basis of the theory we have added further support to our theory. This is called *theoretical replication* (Yin 1989: 54).

## Practical issues

### *Sampling*

#### Method of case selection

Since cases are used for theoretical rather than statistical generalization there is little point in selecting cases because they are in some sense representative of some wider population.

Nor do we need to look for 'typical' cases.[2] We have no sure way of knowing whether a case is truly typical and no way of estimating its typicality. The desire by some people to find typical cases reflects confusion about what Mitchell (1983) calls enumerative and analytic modes of induction or what Yin (1989) calls statistical generalization and theoretical generalization. Since the purpose of case studies is *not* to generalize to a wider sample of cases (enumerative induction or statistical generalization) there is little point in trying to find a typical case for a case study. The need is to find cases that will provide valid and challenging tests of a theory.

Glaser and Strauss (1967) have used the term *theoretical sampling* to describe the normal method of selecting cases in case study designs. Hakim (1986) uses the term *focused sampling* to describe the same process. These terms refer to the strategy of selecting cases that will provide illuminating examples of a type of case (as in descriptive case study designs) or that will provide appropriate tests of a theory.

#### Number of cases

There is no correct number of cases to include in a case study design. Case study designs can consist of a single case or multiple cases (Chapter 13). With multiple case studies the number of cases is a matter of judgement (Yin, 1989: 57). A significant factor in determining the number of cases will be the rigour with which the propositions are to be tested. Using the logic of replication a single replication tells us something but repeated replications give us more confidence in findings. If we seek theoretical replications we will need to conduct additional case studies. Where we find cases that do not fit with our expectations this may produce the need to conduct additional case studies to enable us to follow up hypotheses that the 'deviant' case throws up. The search for



cases designed to *disprove* our propositions will also add to the number of case studies we can conduct.

The complexity of our predictions may influence the confidence we have in our replications. The more complex our predictions the more confident we can be in the single case where our predictions are replicated. Where our predictions are simple (e.g. when A exists then Z will occur) a single case study may not represent a tough test and we may want multiple replications before we feel confident in the results. Where our predictions are more complex and demanding (e.g. when *A+B+C+D* exists then *W+X+Y+Z* will occur) it is relatively unlikely that this complex pattern will occur due to chance. Accordingly we may feel quite confident in the toughness of the test that the single case study provides.

How many case studies we conduct will also depend on our knowledge of external factors that might affect results. That is, if we expect that our results will hold up under a wide range of external conditions we may need fewer case studies. Where we are uncertain about the external conditions under which our predictions will hold we may need more case studies under these different conditions to see how consistently our findings hold (Yin, 1989: 58). For example, we may be unsure whether the link between devolved staffing systems and educational performance will hold regardless of region (e.g. inner city, suburban, rural and remote). Or, again, we may not know if the positive impact will hold at different levels (primary, secondary or tertiary), or in the different types of school systems (private, religious or state). In the face of uncertainties we may need to include case studies that accommodate some of these differences to see if the predictions can be replicated in these different conditions.

A further factor that may affect the number of cases is the conceptual framework with which we are working. For example, if social class was an important concept in the study it would be necessary to include cases from each of the different social classes. The number of cases would depend on how social class was conceptualized and on how many class categories this conceptualization included (e.g. just middle class and working class, or a more refined class classification that includes upper class and various classifications within the middle and working classes).

We could go on forever and check things out under any number of different conditions. In the end we must make judgements about likely variations and have good reasons for expecting that these different conditions might affect the patterns. We also must be guided by what is practical within the constraints of time, money and access to relevant cases. The critical thing is to select the most strategic cases to test our propositions rather than aiming for a large number of cases.

#### Case screening

The strategic selection of cases in

governing schools have better educational outcomes we need to know which schools have which system. We might also need to know, for selection purposes, how long a school has had a particular system. We might want to select schools that had good and poor educational outcomes. To do this we would need to have this information before the case selection process took place.

Strategic case selection can require considerable groundwork to identify the characteristics of a large number of cases in order to see which ones satisfy the criteria for selection. This process can be time consuming but is critical and must be built into time lines and budgets. Case screening may be done in any number of ways, depending on the type of case. A survey questionnaire, annual reports of an organization, archival records, databases or a wide range of other sources may yield the relevant screening information.

Even when screening is introduced we may still end up selecting inappropriate cases. Nevertheless, these cases may be useful for other purposes. Alternatively, we may decide to discontinue that particular case study since it does not help answer the original research questions.

### Cost and access

The number of cases selected, and the particular cases selected, will be affected by the amount of time and money available. Well-executed case studies can be very time consuming. Consequently, only a limited number can be conducted. The cost factor highlights even more the importance of the strategic selection of cases: since only a few case studies can be completed, each one has to 'earn its keep'.

Access to cases is the other critical factor that affects both the number of cases studied and the particular cases selected. Certain cases may be identified as being strategically ideal for the design but practical matters may mean that such cases are not available for research purposes. Gaining access to cases that have been identified as strategically appropriate needs to be approached with considerable care and effort.

#### *Number of investigators: getting consistency*

Case studies may involve a number of different investigators. In 'parallel' designs it is almost inevitable that a number of investigators will be involved – perhaps with a different investigator conducting each study. In multiple case designs each case study should be conducted in a consistent way since inconsistency between cases can invalidate case comparisons. Yin (1989) urges the use of case study protocols to help achieve this consistency.

Planning to ensure consistency across case studies and across investigators is important for two reasons. First, it enhances the reliability of our



information. Gaining reliable information is a prerequisite for good research. This means that the information must not simply be an artifact of the particular circumstances of its collection. We need to be confident that the same information would emerge if it were sought again in comparable circumstances. We certainly do not want comparisons between cases to be invalidated by sloppy collection or different approaches being applied in each case study because that makes cases incomparable. For example, it would be completely inappropriate to compare marriages where in one case marital satisfaction was ascertained by using a structured battery of questionnaire items while in another satisfaction was measured by investigator observation, or in a third satisfaction was determined on the basis of reports of other people (e.g. friends, children, parents). Although each case study would provide information about 'marital satisfaction', the way this concept is measured using the different techniques means that, in effect, it is being conceptualized very differently in each case.

Second, it is imperative that any study is capable of replication. Meeting this condition requires that if other investigators conducted a similar study they would find the same patterns. Successful replication is a safeguard against fraud. It also enables others to see if the pattern only occurs when a particular person conducts the observations or interviews. If findings are only capable of being found by one person they provide little basis for generalizing to theoretical propositions.

#### *When to go into the field*

The temptation is to 'get your hands dirty' early on – to get out there and learn from the cases in full confidence that the truth of the case will somehow emerge. Too often people commence case studies without knowing what their research question is or what propositions they are evaluating.

This is a recipe for disaster. It is essential that we have a clear research question before beginning a case study.[3] Indeed we cannot even begin to select cases until we have a clear statement of the research question. We need to go beyond having a question. We should be able to formulate some initial propositions – some initial answers to the question that the case studies will help us test.

There will be those who urge that fieldwork should be free of theories since these impose our preconceptions and pre-existing categories on the data. Some will urge that we should go into the field free from theoretical encumbrances as these simply blind us to what we might otherwise see and make us deaf to what people are trying to tell us. Our job, they will say, is to allow others to tell us their story rather than us imposing a theoretical interpretation on their story, or using their story for theoretical purposes.

In my view this is not the task of the social scientist. It might be the role of a biographer, a novelist or an activist. The role of a social scientist is to develop and evaluate theoretical generalizations that enable us to understand whole classes of cases – not, in the final analysis, individual cases.

We do not have to have a well-formulated theory to test. Our questions may be such that there are no obvious theories to test. But this does not release us from the need to be well read and well prepared theoretically before going into the field. Pasteur observed that, 'Where observation is concerned chance only favours the prepared mind' (quoted in Mitchell, 1983: 204). If we are unaware of relevant theories, concepts, debates and the like, we will probably miss the significance of much of what we might come across. Without having some idea of what we are looking for we will not know what we have found.

### *Presenting case studies*

Case studies can involve the collection of a vast amount of information. This information must be carefully processed and distilled before it can be presented. We cannot simply describe the case and somehow let the facts speak for themselves. Any attempt to present all the facts will result in an indigestible mess that is unreadable and unread.

The facts do not speak for themselves and we should not pretend otherwise. Describing a case always involves selection and ordering and at least implied construction of causal sequences and interpretations.

There are many different ways in which case studies might be presented. We might describe each case as a whole so that the reader builds up a clear picture of the case. Alternatively, we might describe good examples of particular types of cases and then compare these different types of cases and draw general conclusions regarding our theories. Hochschild's *The Second Shift* (1989) provides a good example of this type of reporting. She describes a series of marriages and from each of her cases she extracts lessons about the domestic division of labour and the processes by which marital inequality in this sphere is maintained.

Alternatively, we might report the results of a series of case studies by extracting *themes* from the case studies and reporting the findings of the case studies thematically. For example, Vaughn (1986) conducted retrospective case studies of couples whose intimate relationships had ended. From these case studies she identified a set of underlying patterns and stages beneath each disintegrating relationship. Another approach is to focus on a series of *questions* and report the cases in terms of how they shed light on these questions. For example, Dempsey (1990) conducted a single case study of a small country town. The guiding questions in the case study were questions such as, 'How unequal are people in this community?', 'What are the key bases of social differentiation?', 'How, in the face of considerable social inequality, does the community maintain a strong sense of cohesion?' These questions guided the study and provided the framework around which the study was reported.



Regardless of which approach is adopted it is highly desirable that the logic of the research design remains clear. The reader should be clear about the proposition(s), and why a particular case was selected to test the proposition. The reader should be able to see that there is a structure and a logic to the selection of cases – that they have been selected for a purpose rather than simply because they happen to be available.

Any report that merely provides a number of stories is incomplete. In reporting case studies it is critical that we relate the cases to propositions and that we seek to draw comparisons between cases and arrive at generalizations or more refined propositions as a result of the case studies. We need to tell the story of the set of cases – the generalizations, the propositions and the questions they answer and the propositions we end up supporting.

## Ethical issues

The same ethical principles apply to case study designs as to the designs discussed in previous chapters. The particular ethical issues confronted will vary, depending on the type of case study design adopted and the particular form of data collection used for that design.

For example, the ethical issues will be different if the case study involves an active intervention or a passive intervention, or requires that we obtain a retrospective account of an event, intervention or change. The ethical issues will also differ according to whether data are collected by interviewing individuals, using informants, analysing official records, passive observation, conducting a survey of case elements, or using participant observation. Since case studies frequently employ a range of different data collection techniques for the one study it is likely that a greater range of ethical issues will arise when using a case study design than with other designs

There is no point in repeating the ethical matters identified earlier in this book. You should read these earlier sections carefully and you should see how they apply to the specifics of the particular case study design you adopt. However, since case study designs frequently involve obtaining more in-depth information and frequently employ participant observation techniques, some ethical issues arise that are less of an issue with other designs.

I argued earlier that reactivity can be a significant threat to the internal validity of case study designs: the presence of a researcher can alter the dynamics of the cases being observed. This can be particularly so where participant observation techniques are used. One way in which some

researchers might try to reduce the effects of reactivity is to disguise their identity as researchers to avoid their *researcher* identity intruding. For example, an investigator may want to examine sexual harassment in the workplace. If he went into a company revealing his identity and purpose it is likely that people in the organization would behave differently while the investigator is present and thus undermine the study. As an alternative the investigator might undertake the research by obtaining a job with the company and covertly observing what goes on.

The ethical issues of deception, and failure to obtain informed consent, obviously arise in this situation. The dilemma it highlights is the tradeoff that occurs between ethics and internal validity: keeping to ethical guidelines can threaten the internal validity of the study.

There can be little doubt that revealing one's identity and gaining informed consent can undermine a great deal of what Punch (1994) calls 'street style' ethnography. In some cases revealing one's identity can effectively kill the research project. In these situations careful consideration must be given to the merits of some form of deception in the light of the likely benefits of the research and the potential for harm to the participants. While it is easy for a researcher to rationalize that the benefits will outweigh any potential for harm, there is a lot of sense in obtaining advice or even in having decisions made by third parties such as institutional ethics committees.

Investigators will need to resolve competing ethical considerations. To what extent should the ethics of informed consent and voluntary participation take priority over the value of research exposing highly unethical behaviour?

Even if a researcher revealed his/her identity and gained permission to undertake the study, the question arises of how to get informed consent, and from whom to obtain that consent. If we were conducting a study of sexual harassment in a workplace, would we obtain permission from the gatekeepers in the organization, or from all people whom we might observe in the course of the study?

Do people need to be informed that their *public* behaviour is being observed for the purpose of research? Clearly in many cases this is simply impractical (e.g. observing the behaviour of people at a party, in a demonstration, shopping at sales). In my view observing public behaviour raises few ethical problems. So long as individuals are not identifiable there should be no danger of harm to participants. After all, if the behaviour is public there is hardly any invasion of privacy.

Another ethical problem that is more likely to arise when using observation in a case study is the problem of what to do when we observe illegal or harmful behaviour. Should the ethical issues of informed consent and voluntary participation take priority over exposing highly unethical behaviour? For example, what would be your ethical responsibility if your case studies of families revealed cases of domestic violence or child abuse? What should you do when conducting a case study of a school and you learn of the identity of a person selling dangerous drugs to students? What do you do if you discover, during your research, that a group participant is about to commit a crime? Is the betrayal of trust of those around you more unethical than one's responsibility to those in danger?



What is the responsibility of the researcher to other researchers? If one researcher betrays the trust of a community, even if for ethical reasons, does this then undermine the capacity of other researchers to study in that community? Once a group has learned to distrust social researchers it can be very difficult to conduct any further research with that group, and this may have further ethical consequences. The behaviour of some researchers with indigenous groups has led to profound mistrust by some indigenous communities of outside researchers.

How do you resolve the objective of feminist research to promote identification, trust, empathy and non-exploitative relationships between the investigator and those participating in the research on the one hand, and to expose unethical and harmful behaviour on the other?

We lack clear-cut answers to these dilemmas. They highlight the fact that ethical principles can often conflict with one another, and that ethical issues can certainly compete with methodological and practical principles.

## Summary

This chapter has emphasized that the internal validity of case studies relies on a logic that sets this design apart from the other designs discussed in this book. Case studies focus on a large number of variables and the way in which they interrelate. In contrast, the other designs rely on controlling out the influence of other variables and on focusing on the influence of a very limited number of variables. One of the strengths of case studies is that they attempt to understand the significance of particular factors within the context of the whole case rather than by screening out this context. As such case studies have the potential for good internal validity based on a more thorough understanding of the meaning of particular behaviour and events than the other designs provide.

The external validity of case studies is based on the logic of replication rather than on sampling logic. Case studies are used to generalize to a theory rather than to a population. Cases in case study designs are selected strategically rather than statistically and are selected as critical tests of theoretical propositions. Issues in selecting cases, the selection of research methods to collect information about the case, and conducting case studies were discussed. Finally, particular ethical questions that can arise in case studies, particularly those employing participant observation and ethnographic methods, were raised.

## Notes

1 The concept of a whole is, as Goode and Hatt (1952) point out, a construct itself. It is us who defines what the 'whole is'. The delineation between the case (the whole) and its context is indeed arbitrary.

2 'Typical' cases should not be confused with selecting 'ideal typical' cases – cases that may represent an ideal type (Chapter 13).

3 It may be appropriate to conduct case studies without clear research questions or propositions when we are doing exploratory case studies. If we are using the case study to clarify concepts, then to help identify questions we might enter the field before a research question has been finalized. But this type of case study, designed to help produce a research question, is very different from a case study design. A case study *design* is the structure of a study that will enable us to answer research questions. It refers to the logic and structure of the study.

# 15

# CASE STUDY ANALYSIS

Methods for analysing case studies are less systematically developed than are the techniques for analysing data collected with other types of research designs. Analysis with these other designs typically uses statistical techniques that rely on having many cases and revolve around comparing groups of cases. Furthermore, attempts to generalize from these designs are based on probability theory which, in turn, relies on having sufficient cases from which to generalize with a given level of precision and certainty.

The research designs dealt with earlier all rely on comparisons *between* groups using a limited number of variables. Case studies, however, achieve explanations by intensive analysis of many variables *within* a case. Multiple cases are used to replicate results – not as a means of locating causal explanations. In addition multiple case studies are useful for making comparisons between cases and thus extrapolating to broader theories.

## Statistical analysis

Although the logic of the earlier designs does not require statistical analysis, such analysis is widely used. Methods such as correlations, cross-tabulations, regression and comparison of means rely on *sets* of cases and on building a typical picture or describing trends across *multiple* cases rather than reflecting the characteristics of any particular case.

Because case study designs focus on individual cases they employ different methods of analysis. A number of such methods have been developed or are in the process of being developed. Case study analysis revolves around assessing the 'fit' between an individual case and the theory or theories being tested. The goal of case study analysis is theoretical generalization rather than statistical generalization (Chapter 14). This goal affects the way in which we go about our analysis.

Because the focus is on individual cases it is inappropriate, when analysing case data, to *count* the number of cases that had particular characteristics or behaved in a particular way. The small number of cases

and the atypical nature of the cases selected make it meaningless to analyse case studies by counting.

This is not to say that case study analysis cannot have a quantitative or statistical element. A case can be *described* statistically. A particular region might be a case in a research project. It can be described in terms of its size, wealth, average age, unemployment rate, crime rate and the like. However, the focus of the analysis is to describe the region – to build up a picture of that case.

Furthermore, where cases include embedded units of analysis (see Chapter 13) the analysis of some levels may well include the sort of analysis typically associated with other designs. For example, a case study of a region might include a survey of residents in the region (the residents being an embedded unit within the case). The survey of residents would include the type of analysis typically used in a cross-sectional design. However, the focus of the overall analysis would be on the case as a whole – the region. The survey results would simply be one piece in the jigsaw to build up a picture of the whole case.

In Chapter 13 I emphasized that case studies are fundamentally theoretical. Their analysis is also fundamentally theoretical. The analysis needs to be built around the theoretical propositions being tested or developed.

## Meaning and context

In Chapter 14 I stressed that case studies are particularly suited to research problems when the phenomenon in which we are interested either cannot be distinguished from its context or must be seen within its context. Many social scientists believe that adequate causal explanations need to take account of the meaning that participants in a social situation attribute to their behaviour. They argue that the meaning of behaviour helps us 'make sense' of why one event produces particular outcomes. Therefore when conducting case studies it is very useful to collect information about the subjective meaning of behaviour for participants and build this subjective data into our analysis of the case.

## Analysis in descriptive case studies

### *Theoretical dimension of descriptive analysis*

Case descriptions should be structured around theoretical ideas. There are scholars, however, who argue that case descriptions should simply describe the case and not seek to interpret it. This view argues that we should let the 'facts' speak for themselves and avoid imposing our own interpretation on 'the facts'. Such an approach is both undesirable and



impossible. Any description of any case always involves a *selection* of facts. This selection will be based on what we see as relevant and important. The very act of selecting means that we are making decisions about what is relevant. This selection process will be heavily influenced by our implicit theories. Furthermore, any reporting of a case will involve *ordering* the selected facts. The inevitable selectivity and ordering will mean that all descriptions are *our* descriptions, rather than *the* description of the case.

The inevitable subjective element of case study analysis does not mean that such analysis is therefore not worthwhile. What it does mean is that we should be explicit about the basis on which we are constructing the description.

### Ideal type analysis

One way of constructing description is to use ideal types (see Chapter 13). This involves constructing theoretically derived descriptions of types of cases. On the basis of theoretical considerations and our knowledge of the existing research literature we can list the elements of a type (e.g. an authoritarian person, a perfectly bureaucratic organization, a democratic electoral system, an egalitarian marriage).

The analytic strategy is to use this ideal type as a template to guide the analysis of an actual case. We can use the template to see how closely our actual case fits the template. We might have good reasons for expecting that particular cases will represent the characteristics of a particular type. For example, we might expect, for various reasons, that a government department in a Westminster form of government might be close to an ideal type of a bureaucracy as defined by Weber (Gerth and Mills, 1946). Using our template of an ideal type bureaucracy we can examine a selected department (case) to see how closely it approximates the ideal type. As well as identifying where the case fits the ideal we would also use the template to help highlight where the case diverges from the ideal. Repeated case studies (e.g. a range of government departments) might show us that empirical examples consistently diverge from the ideal in so far as ministerial interventions might override the ideal of rule-governed decision making which is a characteristic of the ideal type bureaucracy. It should be remembered, however, that the ideal type still stands as an ideal type regardless of the empirical findings from a set of cases. The ideal type remains a 'pure' idealized abstraction.

The use of the ideal type provides a way of looking at and organizing the analysis for the descriptions of actual cases. Using this approach we avoid description that simply describes whatever we happen to find out about the case or simply reports the features that catch our attention. Instead, the description is structured, planned and purposeful.

### Typologies

A typology consists of a set of types. Thus we might develop typologies of different sorts of personality, forms of government, types of organizational structure or types of marriage. This set of types may be based on a set of ideal types or be derived empirically (see Chapter 13). Typologies and ideal types provide excellent methods of analysis in which the 'wholeness' of a case is preserved. Rather than just concentrating on traits, a typology attempts to build up an overall picture of a case taking a wide range of characteristics into account.

Where we employ a theoretically derived typology the task of analysis is simply an extension of the ideal type analysis described above.

Where we use our case studies to derive empirically grounded typologies (i.e. those based on our actual cases) we need to adopt a different approach to analysis. These approaches are only possible if we have multiple cases.

*Cluster analytic approaches* These approaches involve identifying a set of variables we want to use as the basis for our typology. For example, in a study of an organization we might have collected from our case studies information about the way decisions are made, the way in which rules and regulations are used, the degree of hierarchy, degree of rationality etc. In a set of case studies we could then group cases that had similar constellations of characteristics.

If we have a small number of cases that we know well we can do this manually. If we have a larger number of cases we can use a statistical technique called cluster analysis to achieve the same end. Essentially, this technique involves identifying the characteristics on which we want the typology to be based, coding each case in terms of each of those characteristics, and then feeding that information into a cluster analysis computer program. The program will then identify which cases are most similar to one another. It will cluster cases into groups that have relatively similar sets of characteristics.

### Time ordered descriptions

Rather than presenting cases in terms of types we can represent a case as a history of some sort or another. If we are dealing with individuals the case description might be presented as a biography – an account of a life. The biography will necessarily be selective and will represent the person in a particular light. It is likely that the biography will go beyond simple description and will include either implicit or explicit theories of why the person's life went in particular directions.

Histories of events, organizations, policies, or whatever the unit of analysis might be, represent a way of describing a case where the emphasis is on the sequence of events. The sequential account may seek to explain the course of events and may be designed to support a particular explanation. Alternatively, it may simply be designed to provide a record of events without an explicit attempt to explain or interpret those events.



## Explanatory case studies

As in descriptive analysis, in explanatory analysis theory plays a key role. Theoretical propositions will either direct the analysis or be the goal of analysis.

When multiple case studies are being conducted the aim of the first step in the analysis is to understand each case *as a whole*. Only after a picture of each case has been constructed is it appropriate to *compare* the cases (Yin, 1989; Stake, 1994). The analysis of each case must be organized around questions and theoretical concepts. Each case should be used either to test or to build theoretical propositions. When each case is analysed around a common framework it is then possible to compare the cases to arrive at higher level generalizations, or to provide tougher tests of theories. If each case is examined in an idiosyncratic way then it will be very difficult to compare cases meaningfully.

The way in which we approach the analysis of explanatory case studies will vary somewhat depending on whether we are using them for theory testing or theory building.

### *Theory testing analysis*

There are a number of ways of using case studies when testing theoretical propositions. Yin (1989) outlines two approaches: pattern matching and time series analysis. In my view time series analysis is simply a type of pattern matching but in the discussion below I will treat them separately.

### Pattern matching

Pattern matching is a form of theory testing analysis that establishes a detailed set of predictions before the case study is conducted. These predictions stem from a theoretical model and therefore represent a clear theory testing approach. The analysis could proceed by establishing a set of alternative patterns we would predict on the basis of competing theories.

The form and complexity of pattern matching can vary considerably. The basic principle is that the more elaborate the predicted pattern (so long as it still follows logically from the theory) the tougher the test of a theory. Having predicted a particular pattern we would then conduct the case study to see if the case does, in fact, match the predicted pattern. If the case matches the predicted pattern then the case supports the theory

| | | Independent variable $X_a$ | Independent variable $X_b$ |
|---|---|---|---|
| Dependent variable | $Y_a$ | Pattern 1 | Pattern 2 |
| | $Y_b$ | Pattern 3 | Pattern 4 |

Figure 15.1 *Pattern matching for two variables each with two categories*

in the same way that a successful experiment supports a theory. If, however, the case does not match the predicted pattern the theory requires modification.

Yin (1989) describes pattern matching that varies in complexity according to the number of independent and dependent variables included in the predicted patterns.

*Simple patterns* At its simplest level pattern matching involves one independent variable with two values (e.g. male and female) and one dependent variable with two possible values (behaves in one of two ways in a particular situation). In this case there are four possible different patterns (Figure 15.1). For a given case with a given characteristic we could predict one of two patterns. Which pattern we predicted would depend on theoretical considerations.

The pattern we might predict for a particular case that we know has value $X_a$ on the independent variable is that the case will have value $Y_b$ on the dependent (outcome) variable. The prediction takes the form: if $X_a$ then $Y_b$.

For example, our theory of work performance in schools might lead us to expect that when staffing appointments, dismissals and promotions are managed at the local school level rather than by a highly bureaucratic, centralized and remote system ($X$ variable), staff teaching commitment ($Y$) will be high. If devolved staffing systems are symbolized as $X_a$ and high level of teacher commitment is symbolized as $Y_b$, our prediction is:

> When $X_a$ (local based staffing system) exists then $Y_b$ (high levels of teacher commitment) will follow.

We would also expect that when $X_b$ (centralized system) exists then $Y_a$ (low levels of teacher commitment) will follow.

Our test, even in this very simple example of pattern matching, would be more interesting and more powerful if we were testing alternative theories that predicted different patterns. One theory might predict



| | | Independent variable $X_a$ | Independent variable $X_b$ | Independent variable $X_c$ |
|---|---|---|---|---|
| Dependent variable | $Y_a$ | Pattern 1 | Pattern 2 | Pattern 3 |
| | $Y_b$ | Pattern 4 | Pattern 5 | Pattern 6 |
| | $Y_c$ | Pattern 7 | Pattern 8 | Pattern 9 |

Figure 15.2 *Patterns for two variables each with three categories*

pattern 1 while a competing theory might predict pattern 3. The case study would enable us to see which pattern emerged and therefore which of the rival theories was supported in this case.

For example we might propose the following two rival theories:

- *Theory A* Local control will lead to higher commitment to work because effort and 'fit' is seen and rewarded (and lack of effort and not fitting in with school needs is punished).
- *Theory B* Local control makes people feel more demoralized, and vulnerable to local politics and prejudices, and does not recognize wider professional development etc. This leads to a lack of commitment and a lack of professionalism and to playing politics to win favour rather than fostering performance.

A slightly more complex version of essentially the same situation would arise if either of the variables had more categories (e.g. medium levels of performance and a hybrid of local and centralized staffing models). The more values either variable or construct possessed, the greater the number of possible patterns we could predict. For example, if both variables had three possible values there are nine possible patterns we could find in any particular case (Figure 15.2). In this situation we would still predict a particular pattern for a given case: if they were $X_a$ we might predict $Y_c$.

For example, we might have a theory about the impact of parenting styles on child behaviour. Each of these two factors might have three categories as illustrated in Figure 15.3. Using this example we might predict that authoritarian families will produce children who are rebellious (pattern 1) A case study of an authoritarian family will help address whether this proposition receives support. Repeated case studies of authoritarian families will provide a firmer test. A further proposition may be that permissive families will produce children who constantly engage in limit testing behaviour (pattern 9).

| | | Parental style of authority | | |
|---|---|---|---|---|
| | | Authoritarian | Authoritative | Permissive |
| Child behaviour | Rebellious | Pattern 1 | Pattern 2 | Pattern 3 |
| | Compliant | Pattern 4 | Pattern 5 | Pattern 6 |
| | Limit testing | Pattern 7 | Pattern 8 | Pattern 9 |

Figure 15.3 *Pattern matching for parental style and child behaviour*

The best test of the theory would be a series of case studies predicting particular types of child behaviour for each parenting style and for *each* of the mutually exclusive propositions to be confirmed by the relevant case studies.

*More complex patterns: multiple independent variables* A more complex set of patterns can be predicted if two independent variables are used: a different outcome is predicted for each *combination* of independent variables (Figure 15.4).

Here we might make different predictions, depending on the combination of parental authority style combined with the child's level of anxiety. We might predict a different outcome pattern of permissive parenting depending on whether we are looking at a family where the child is anxious or secure/confident. We might predict that the anxious child with permissive parents will exhibit the 'tests parental limits' response (pattern 24) while the confident/secure child with permissive parents may set her own limits (pattern 17).

We could extend this example to include more independent variables and thus anticipate even more possible patterns.

Figure 15.4 illustrates the *logical* possibilities – the different patterns one could conceivably find. Of course, our theoretical reasoning will lead us to predict *particular* patterns in particular circumstances, that is, for particular types of cases as illustrated in the shaded cells in Figure 15.4.

Having established the possible patterns and the predicted patterns we would then seek out cases that enable us to test the propositions. For example, we would conduct a case study on a family in which authoritarian parenting and child anxiety occur. Our prediction would be that the child would be very compliant. If this proved to be true we would have achieved a *literal replication*. We might then look for a case of authoritarian parenting with a child who is not anxious. Our prediction here might be that the child will *not* be compliant. This would be a *theoretical replication*. That is, where the particular combination of factors



| | | Dependent variables | | | | | |
|---|---|---|---|---|---|---|---|
| | Parental authority style | Authoritarian | | Authoritative | | Permissive | |
| | *Child anxiety level* | *Low* | *High* | *Low* | *High* | *Low* | *High* |
| Independent variables: *Child behaviour* | *Compliant* | Pattern 1 | Pattern 2 | Pattern 3 | Pattern 4 | Pattern 5 | Pattern 6 |
| | *Rebellious* | Pattern 7 | Pattern 8 | Pattern 9 | Pattern 10 | Pattern 11 | Pattern 12 |
| | *Child sets own limits* | Pattern 13 | Pattern 14 | Pattern 15 | Pattern 16 | Pattern 17 | Pattern 18 |
| | *Tests parental limits* | Pattern 19 | Pattern 20 | Pattern 21 | Pattern 22 | Pattern 23 | Pattern 24 |

Figure 15.4 *Logical patterns with two independent variables and one dependent variable*

is not present we will not get a specific outcome (see later for fuller discussion).

Another approach to collecting and analysing case study data is to look for cases with particular outcomes (e.g. highly compliant children) and predict that these children will be anxious and have authoritarian parents. In terms of the predictions in Figure 15.4 we have predicted this, but if we find cases of highly compliant children who are neither anxious nor have authoritarian parents then our theory needs refining.

One way of testing a theoretical proposition is to demonstrate, through case studies, that when certain conditions apply (e.g. authoritarian parenting and child anxiety) there will also be a specific outcome (highly compliant child). The problem with this sort of matching is that it only specifies *sufficient conditions* for an outcome. It does not tell us whether these are the *only* conditions under which the outcome occurs. As such it does not represent a demanding test of a proposition. A more demanding test would involve looking for cases where the particular outcome is present and then asking whether this outcome *only* occurs under the theoretically predicted conditions.

Independent variables

| | | Condition 1 | | | Condition 2 | | |
|---|---|---|---|---|---|---|---|
| | | Values of dependent variables | | | Values of dependent variables | | |
| | | Low | Medium | High | Low | Medium | High |
| Dependent variables | A | 1 | 2 | ***3*** | ***1*** | 2 | 3 |
| | B | 1 | 2 | ***3*** | ***1*** | 2 | 3 |
| | C | ***1*** | 2 | 3 | 1 | 2 | ***3*** |
| | D | 1 | ***2*** | 3 | 1 | ***2*** | 3 |
| | E | 1 | 2 | ***3*** | ***1*** | 2 | 3 |

Figure 15.5 *Logical patterns with one independent variable and five dependent variables*

A further test of a proposition is whether we find cases where all the conditions are met but the outcome does *not* occur. For example, do we find cases of authoritarian parenting and anxious children where children are not compliant? If we do then our proposition is not supported and our theory will need to be revised to accommodate this type of case.

*More complex patterns: multiple dependent variables* The previous section focused on situations in which we predict a single outcome variable under a particular *set* of conditions (multiple independent variables). We can envisage situations where the opposite applies – where we have a specific condition (one independent variable) and predict that this condition will produce a particular *set* of outcomes. That is, we will predict a *pattern* of outcomes under a given condition (Figure 15.5). We will then match that predicted pattern with cases that meet the condition.

Here we predict a pattern of outcomes in cases that meet condition 1 and a different pattern for cases that meet condition 2. The pattern is a particular *combination* of outcomes. In this case we predict that for cases meeting condition 1 (on the independent variable) we will find the



Independent variables

| | | High level of worker autonomy | | | High degree of direct control over worker | | |
|---|---|---|---|---|---|---|---|
| Values of DV | | Low | Medium | High | Low | Medium | High |
| Dependent variables | Morale | 1 | 2 | ***3*** | ***1*** | 2 | 3 |
| | Work quality | 1 | 2 | ***3*** | ***1*** | 2 | 3 |
| | Staff turnover | ***1*** | 2 | 3 | 1 | 2 | ***3*** |
| | Efficiency | 1 | ***2*** | 3 | 1 | ***2*** | 3 |
| | Innovation | 1 | 2 | ***3*** | ***1*** | 2 | 3 |

Figure 15.6 *Predicted patterns of outcomes on five dependent variables by level of worker autonomy*

following characteristics on the five outcome variables (A, B, C, D, E): $A_3$, $B_3$, $C_1$, $D_2$, $E_3$. For cases that meet condition 2 we predict a different pattern of outcomes: $A_1$, $B_1$, $C_3$, $D_2$ and $E_1$.

This can be illustrated with an example (Figure 15.6). The theory being used here is one that predicts that autonomy leads to better outcomes than high levels of supervision. While there may be no payoff in terms of efficiency, worker autonomy has positive benefits in terms of morale, work commitment, initiative and improved work quality.

In other words we predict that organizations (cases) that permit high levels of autonomy among members of their workforce will display a particular *pattern* of outcomes: high morale + high quality of work + low staff turnover + moderate levels of efficiency + high levels of innovation.

Organizations with high degrees of supervision and direction are predicted to behave differently. They are predicted to have a different *set* of outcome characteristics: low morale + low quality of work + high staff turnover + moderate levels of efficiency + low levels of innovation.

*Highly complex pattern matching* The most complex version of pattern matching will occur when we have a *set* of independent variables and a *set* of dependent variables. In this situation we may be able to predict that when a case has a particular *set* of causal attributes they will be matched to a particular *set* of outcomes.

For example, we might predict that when an organization is fairly small, and possesses a 'flat' organizational structure, respected consultative mechanisms and worker reward plans, then there will be a valued set of outcomes. These might be high worker commitment, excellent quality output, strong ties between workers outside the workplace, ready acceptance of innovation, low levels of absenteeism and no industrial disputation.

*Refining theories* Strictly speaking theory testing involves specifying a theoretical proposition, testing it and either accepting or rejecting the proposition. In reality, however, if a proposition is not supported by a case study then the next step is to refine the theory so that it can take account of the exception provided by the case. In this way the proposition covers a wider and wider set of cases and becomes more powerful. This process is called *analytic induction* and will be outlined more fully later in the section that deals with theory building.

### Time series analysis

Although Yin (1989) treats this method of analysis as separate from the pattern matching described above, the logic is identical and it should therefore simply be treated as a form of pattern matching. Instead of predicting a particular pattern for a set of variables it predicts a particular trend or sequence of events. The analytic strategy involves predicting a particular pattern of change over time. This type of pattern analysis can take one of two forms: *trend* analysis and *chronological* (event sequence) analysis.

*Trend analysis* Trend analysis is an examination of the direction of change in a particular variable or set of variables. We address the question of whether the trend is upward (steep or gradual), shows no change, variable (up and down) or downward (steep or gradual).

Predicted trends can range from the *simple* to the highly *complex*. The simplest forms predict a trend in one dependent variable. We might then predict a different pattern of change on this variable in different contexts. For example, we may anticipate that enrolments in a particular type of university course (the case) will increase in the short term but decline in the longer term. Another course might be predicted to maintain stable enrolment levels while another type of course is predicted to experience sharp increases in enrolment levels.



Trend analysis could be far more complex. Rather than simply predicting a different trend on the same variable for different cases, a complex version of trend analysis would predict trends for a set of variables in any given case. For example, we might predict that in a given course the enrolments will increase, the ratio of females to males will improve with the increased enrolment levels, there will be an increase in mature age students, and there will be no reduction in the quality of students. In another course we might predict a different combination of trends: a steady enrolment, a decline in standards, a decline in mature age students, and fewer female students.

Trend analyses can be either *interrupted* or *uninterrupted*. Interrupted time series analysis refers to a situation where a specific event takes place somewhere within the sequence of events. This enables us to examine the pattern of events before and after the interruption (or intervention). In this respect an interrupted time series analysis is similar to a before and after design. Although it does not involve a control group, we would conduct studies of cases where the interruption did and did not take place. While we cannot control for other differences between cases we can collect detailed information about all cases to enable us to consider whether these other differences (rather than the interruption) might be responsible for the apparent effect of the interruption.

Interrupted trend analysis would involve predicting a pre- and post-interruption pattern (e.g. decline pre and increase post, followed by longer term stability) and matching this predicted pattern with that for actual cases.

Not all trend studies will have an interruption. Some will simply predict a trend that will be anticipated in a particular context. For example, we might anticipate from what we know about the demographic and economic profile of a region (the case) that it is likely to experience long term population decline. Another region with a different mix of demographic and economic characteristics might be predicted to experience growth while yet another region with a different profile might be predicted to stagnate.

*Chronological analysis* Chronological analysis involves predicting a *sequence* of events (or even a sequence of trends) involving a number of different events or variables. That is, we would predict what would change, or what events would take place, and in what order. The sequence of events might be a *cause and effect* sequence or it might be a descriptive sequence that proposes particular *stages* in a process.

Examples of a staged version of chronological analysis might be models that propose predictable stages in becoming a marijuana user (Becker, 1966), stages in the disintegration of intimate relationships (Vaughn, 1986), stages in the process of adjustment to retirement (Atchley, 1976), or changes in the relationships between adults and their parents as parents age (Marsden and Abrams, 1987).

Regardless of whether the analysis involves a cause and effect sequence of events, or a set of predicted stages, the analytic question is whether the predicted *pattern of change* occurs in the cases where theory would expect it to occur. Or does the pattern occur when we would predict it should *not* occur?

The forms of the predicted patterns can vary. Yin (1989) indicates four types of ways in which events might be predicted to change in relation to each other. We can predict chronologies in which:

- some events must always occur before other events, with the reverse *sequence* being impossible;
- some events must always be followed by other events, on a *contingency* basis;
- some events can only follow other events after a specified *passage of time*; or
- certain *time periods* in a case study may be marked by classes of events that differ substantially from those of other time periods [stages]. (1989: 119)

*Literal and theoretical replication*

In Chapter 13 I argued that generalizing from case studies relies on the replication logic of experiments rather than the statistical logic of surveys. We gain confidence in experimental results not just from the elegance of the experiment but from our capacity to predictably replicate results and to predictably fail to replicate results (i.e. we anticipate that the intervention will have its effect under specific conditions but not under other conditions).

Similarly we gain confidence in case study findings when we can accurately predict which types of cases will display particular patterns and which cases will *not* display specific patterns.

Where we have a predicted *set* of outcomes and a single causal factor a similar logic holds. We would ask the following sorts of questions:

1 Does the full set of outcome characteristics occur when the presumed causal factor is present? If so we have confirmation of our theory.
2 We would then find another case where the presumed causal factor is present and see whether the full set of outcomes is also present in that case. If so we have a literal replication of the previous case and further confirmation of our theory.
3 Do we get cases where the presumed causal factor is present but only *some* of the predicted outcome characteristics are present? If we find such cases then we have failed to replicate the theory and we would either reject or modify the theory. If we could find *no* cases where the cause was present and the full set of outcomes was *not* present then we have a theoretical replication.
4 We would then seek to find a case in which the presumed causal factor is *not* present. We would expect that the full set of outcomes would not occur when the cause was not present. That is, we should not find cases where we have the effects without our presumed cause.[1] If we *fail* to find any such cases we have achieved further theoretical replication.



**Analysis for theory building: analytic induction**

At its heart, analytic induction is 'a strategy of analysis that directs the investigator to formulate generalizations that apply to all instances of the problem' (Denzin, 1978: 191). It is a method that can be used to achieve descriptive generalizations or to arrive at causal explanations. It is a strategy that moves from individual cases and seeks to identify what the cases have in common. The common element provides the basis of theoretical generalization (see Figure 1.2).

Analytic induction is a strategy that seeks to arrive at generalizations that apply to *all* cases. In this respect it differs from the strategies of analysis discussed in previous sections of this book. In Chapter 3 I distinguished between deterministic and probabilistic notions of causation. The modes of analysis discussed in previous sections are based on probabilistic causation: we estimate whether one group is more likely than other groups to behave in a particular way. Analytic induction, however, seeks to achieve universal generalizations.

Denzin (1978: 192) summarizes six key steps in the process of analytic induction:

1 Specify what it is you are seeking to explain (the dependent variable).
2 Formulate an initial and provisional possible explanation of the phenomenon you are seeking to explain (your theory).
3 Conduct a study of a case selected to test your theory.
4 Review (and revise if necessary) your provisional theory in the light of the case or exclude the case as inappropriate.
5 Conduct further case studies to test the (revised) proposition and reformulate the proposition as required.
6 Continue with case studies (including looking for cases that might disprove the proposition) and revise the proposition until you achieve a causal proposition that accounts for all the cases.

The process might be illustrated using the hypothetical example in Chapter 13 about the impact on the quality of education of devolved versus centralized school staffing systems.

In this example the quality of education is the phenomenon we are seeking to explain (step 1). Our provisional (partial) explanation is that devolved staffing systems will produce greater improvements in the quality of education in a school than will centralized systems (step 2). We would therefore anticipate that cases (schools) with devolved systems would have better quality education than those with centralized systems

(other things being equal). Alternatively, we might anticipate that schools that introduced a devolved staffing system would exhibit an improvement in the quality of education and that this improvement would be greater than in schools that did not introduce such a system.

We would then select a case to test our proposition (step 3). We might find a school that has recently introduced a locally based staffing system. Having developed a definition of what constitutes educational quality and worked out how to measure this, we could then conduct a case study to see if the introduction of the new staffing system had led to the predicted improvements. Let us suppose that the predicted improvements had in fact taken place. This lends support to our proposition but the support is hardly overwhelming and probably would not convince a sceptic (step 4). The sceptic could say that one case hardly proves the point, that there may have been a general improvement in educational quality in all schools over the same period, and that schools that had kept the old system might have also exhibited an improvement in educational quality. At the very least they might say that they could find a school with a central system that had improved their quality of education over the same period. They might also argue that they could find examples of schools where the local system was introduced with disastrous consequences for educational quality.

We would need to do more work – further case studies (step 5). Initially we might look for further cases of newly introduced local staffing systems to check that the initial case study was not just good luck.

Let us imagine that we then come across a school that our critics alluded to – one where the introduction of the new system had been a disaster. It had led to teacher, student and parent dissatisfaction, loss of morale and a clear decline in many aspects of educational quality. How do we make sense of this case? What is different about this case that should make it behave differently from that which we predicted provisionally, and so differently from the other cases we have already studied?

Since our study should produce a complete picture of each case and its social and cultural context we will probably pick up some initial clues. How is the 'deviant' school different from the others? We might notice some features about the *process* by which the devolved scheme was introduced in the school in which it was a failure. For example, teachers report that it was imposed on them. They were not consulted and there are no processes of appeal against the principal's decisions. Our hunch then is that the success of this type of scheme depends on the way in which it is implemented.

We then look back at our earlier cases to see if this explains the different outcomes. Do the successful cases stand out as having different implementation processes to the disastrous case? Let us suppose that the successful cases implemented the scheme only after a great deal of consultation and there was widespread consensus about the desirability



of the scheme and the process by which it was to operate. We would then be in a position (step 6) to modify our initial, provisional proposition to read something like: 'When implemented in a climate of consultation and consensus, devolved staffing systems will produce improvements in educational quality.'

However, our next case causes problems. We have located another case where the locally based staffing system also has been a disaster. In this case, however, the system was implemented only after careful consultation and with the full agreement of staff and others. This is not what we expected. Why does this case not fit? Further examination of the case reveals that the school has had a new principal appointed since the system was introduced. This principal is not trusted and is believed to play favourites. He is also believed to victimize particular popular staff. Maybe these are the reasons why the system did not work. Checking back on earlier cases, the success stories all had principals who were widely liked and trusted.

Do the two failures have anything in common? Although the cases are different in certain ways, what they have in common is that teachers do not have any *confidence in the system*. In one case it was forced on them without consultation. In the other, the principal has undermined their confidence in the fairness of the system. We might then modify our revised proposition to something like: 'Where teachers have confidence in the fairness of a devolved staffing system it will lead to improvements in the quality of education.'

We could go on with additional case studies to check out how well this proposition holds. In addition to looking at cases where a local staffing system was introduced we should look at some cases where the centralized system was retained. We would expect that we would not see the same level of improvement in education in these schools. If this proved to be so it would strengthen our confidence in our initial theory about the impact of local staffing systems.

We might look for cases that could *disprove* our theory. For example, if we could find a school that had kept the centralized system and had shown an improvement in educational quality we might be able to further improve our theory. We might study a school where we have identified that there has been an improvement in education but the centralized staffing system had been retained. This would tell against our revised proposition. However, further investigation of the school might show that when we look at the school in its wider context there is a hostile relationship between the school and the local community. We learn that there had been a previous attempt by the school council (on which there were many local community representatives) to introduce a local staffing scheme. Teachers had successfully resisted this and felt that the centralized staffing system was protecting them.

When we learn these things about what at first sight looks like a 'deviant case' that disproves our proposition, in fact it further confirms

our proposition that stresses the notion that teachers must have confidence in the system. Perhaps this case would cause us to further broaden our proposition to emphasize the importance of teachers having confidence in the system and to identify the factors that produce that confidence rather than stress the importance of a local or centralized system in itself.

## Summary

This chapter has stressed the point that case study analysis should be fundamentally a theoretically informed undertaking. This applies equally to descriptive and explanatory case studies. The use of ideal types as a method of analysing descriptive case studies was discussed. Two broad methods of analysing case studies for explanatory purposes were outlined. Where case studies are used to test a theory, pattern matching provides an appropriate way of analysing and comparing cases. Pattern matching can range from matching very simple predicted patterns to highly complex patterns involving multiple independent and dependent variables. Where case studies are used to build a theory, analytic induction provides a useful way of analysing and using each case study.

## Note

1 Of course this logic assumes that our causal variable is the only factor that can produce a given outcome. If we find cases where the effects occur but our favoured cause is absent then we have alternative explanations of the phenomenon. Our research design should be designed to eliminate such alternative explanations.

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