

Document name:	3 CASE STUDIES AND EXPERIMENTS
Document date:	23/09/2013
Copyright information:	Copyright © 2013 The Open University
OpenLearn study unit:	Open Educational Research Assets
OpenLearn url:	http://www.open.edu/openlearnworks/course/view.php?id=1592



The Open
University

3 CASE STUDIES AND EXPERIMENTS

ACTIVITY 2



What do you think the respective advantages and disadvantages of case studies and experiments are? Jot down your answer before reading on.

If we turn now to the distinction between case study and experiment, we will see highlighted a complementary dimension of strength and weakness on the part of case studies. This is one that is primarily relevant to theory development and testing. Here the trade-off is between more and less researcher control of variables on the one hand, and the level of likely reactivity on the other. What is meant by the term 'reactivity' here is the effects on the phenomena studied of the research process itself. One of the most common criticisms of experiments is that their results are not generalizable to situations outside the laboratory because the behaviour they study is an artefact of the experimental situation. In particular, when people know that they are taking part in an experiment, what they do may be affected by that knowledge, and this could shape the results. This would reduce the ecological validity of the study, the extent to which its findings can be generalized to non-experimental cases.

It is precisely in this respect that the case study has an advantage over the experiment. Because it involves the investigation of naturally occurring cases (rather than cases created by the researcher in the laboratory), the case study provides us with information that is less likely to be affected by reactivity and is therefore more likely to be ecologically valid. Of course, case-study research may involve some reactivity, for instance where the researcher plays an influential role within the setting, either intentionally (as in action research) or inadvertently (as in the, probably apocryphal, story of the ethnographer who investigated a delinquent gang and ended up as its leader! Ball, 1972, pp.163-4). Furthermore, reactivity is not the only source of ecological invalidity: natural cases can be unrepresentative in relevant respects of other cases falling under the same theoretical category, simply because there is variability within that set of cases. In general, however, reactivity is likely to be lower and ecological validity higher in case study as compared with experimental research.

This potentially higher ecological validity in the case study is bought, though, at the cost of making it more difficult to come to convincing conclusions about the existence of causal relationships. By constructing cases for investigation, experimenters can vary theoretical and extraneous variables fairly easily. This enables them to maximize the chances of coming to sound conclusions about whether the causal relationship they are investigating does or does not hold, other things being

equal, though we must remember that experiments do not guarantee the control of all relevant extraneous variables.

Once again, then, we are not faced with a contrast between a superior and an inferior case-selection strategy, but rather with strategies that have related advantages and disadvantages. Furthermore, while it might seem that the difference between case studies and experiments is one of kind rather than one of degree — we either create cases or we study existing ones — this is not so. The difference here is quantitative too. Quasi- and field experiments involve less control over variables than do true experiments, but more than case studies, and therefore constitute mid-points on a scale. What is involved here is variation in the degree of control exercised by the researcher over theoretical and extraneous variables. Once again, I can illustrate the relationship by means of a diagram (see Figure 3).

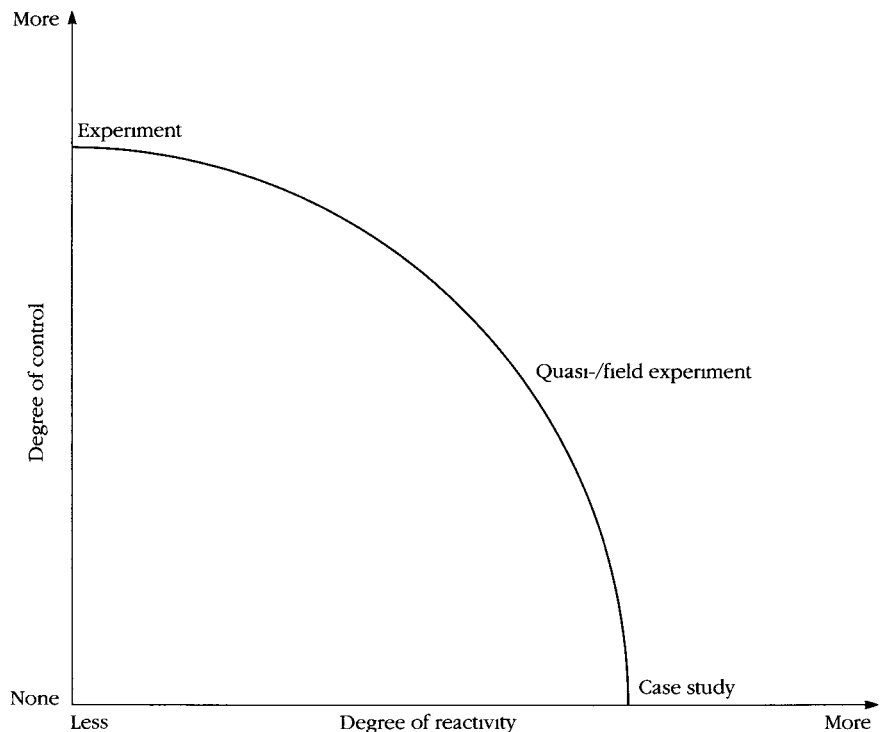


Figure 3 *The relationship between experiment and case study*

The weakness of case-study research as a basis for identifying causal relationships has not always been recognized by advocates of this sort of research. It is sometimes argued that case studies can identify causal relationships in a relatively direct manner. This argument has quite a long history. It occurs in an article from the 1930s by Willard Waller, a Chicago-trained sociologist and advocate of case study. He draws on Gestalt psychology to claim that 'there is in some cases a direct perception of the causal interdependence of events', though later he recognizes that such insight can be mistaken (Waller, 1934, pp.285, 297). More recently, Glaser and Strauss in an influential text on qualitative research claimed that 'in field work ... general relations are often discovered in vivo, that is, the field worker literally sees them occur' (Glaser and Strauss, 1967, p.40). Something of this kind also seems to be implicit in Mitchell's defence of case study (Mitchell, 1983, Hammersley, 1992a, ch 10). Yet, this is not convincing. As the Scottish philosopher David Hume argued long ago, we do not see causal relationships in some immediate fashion; the ascription of causal power always relies on assumptions. Indeed, it is widely accepted by philosophers today that we cannot see anything directly or immediately. All perception and observation involve presuppositions, even though most of the time we are not aware of those presuppositions (Hanson, 1958, Gregory, 1970).

Does this mean that we must simply accept that research employing case study is unable to identify causal relationships? This does not follow at all. For one thing,

the generation and development of causal hypotheses is as important as the testing of them, and that is a task where it is widely accepted that case study can be of great value. The detail and accuracy of information it can provide about particular cases sometimes enables researchers to identify likely causal relationships in a way that is not possible in either survey or experimental research.

An important technique for such theory development is the 'grounded theorizing' of Glaser and Strauss (1967). They criticize the idea that research should simply be concerned with testing hypotheses derived from what they refer to as 'armchair' theorizing. They insist that the generation and development of theory can be done most effectively through the collection and analysis of empirical data. And what they refer to as 'theoretical sampling' is a central element of this. It involves the selection of cases in such a way as to facilitate the development of fruitful theoretical ideas. This may involve both comparison among cases where differences are known to be minimal and among those where the differences are greater, in an attempt to clarify and elaborate the theoretical categories with which the researcher is working. Here is an example from Glaser's and Strauss's own research on 'the awareness of dying' in hospitals and its effects on relationships between staff and patients:

Visits to the various medical services were scheduled as follows. I wished first to look at services that minimized patient awareness (and so first looked at a premature baby service and then a neurosurgical service where patients were frequently comatose). I wished next to look at dying in a situation where expectancy of staff and often of patients was great and dying was quick, so I observed on an Intensive Care Unit. Then I wished to observe on a service where staff expectations of terminality were great but where the patients' might or might not be, and where dying tended to be slow. So I looked next at a cancer service. I wished then to look at conditions where death was unexpected and rapid, and so looked at an emergency service. While we were looking at some different types of services, we also observed the above types of service at other types of hospitals. So our scheduling of types of service was directed by a general conceptual scheme — which included hypotheses about awareness, expectedness and rate of dying — as well as by a developing conceptual structure including matters not at first envisioned. Sometimes we returned to services after the initial two or three or four weeks of continuous observation, in order to check upon items which needed checking or had been missed in the initial period.

(Glaser and Strauss, 1967, p 59)

While Glaser and Strauss present grounded theorizing primarily as a strategy for developing rather than testing theory, they sometimes suggest that it also serves to *test* theoretical hypotheses. Indeed, in a later publication, Strauss claims that in grounded theorizing 'the theory is not just discovered but *verified*' (Strauss, 1987, p.17). And he argues that built into grounded theorizing is the same logic as governs experimental research: the hypothetico-deductive method. However, while grounded theorizing conforms to this method in some respects, it deviates in others: notably in that while the selection of cases it recommends is systematic, it is not designed in such a way as to test theoretical hypotheses rigorously.

Another strategy sometimes used to guide case-study research with a view to developing and testing theory is analytic induction. Cressey provides a classic account of this approach.

First, a rough definition of the phenomenon to be explained is formulated. Second, an hypothetical explanation of that phenomenon is formulated. Third, one case is studied in light of the hypothesis with the object of determining whether the hypothesis fits the facts in that case. Fourth, if the hypothesis does not fit the facts, either the hypothesis is

re-formulated or the phenomenon to be explained is re-defined, so that the case is excluded. This definition must be more precise than the first one. Fifth, practical certainty may be attained after a small number of cases [have] been examined, but the discovery by the investigator or any other investigator of a single negative case disproves the explanation and requires a re-formulation. Sixth, this procedure of examining cases, re-defining the phenomenon and re-formulating the hypothesis is continued until a universal relationship is established, each negative case calling for a re-definition or a re-formulation.

(Cressey, 1953, p.16)

The process of analytic induction is summarized in Figure 4.



Figure 4 *A formalization of the process of analytic induction*
(Source: Hammersley, 1989)

In the case of Cressey's own research into embezzlement, he reports how he began from a legal definition of embezzlement but found that the term was not used consistently. He therefore formulated his own definition of what he came to refer to as 'financial trust violation'. This was held to have occurred where a person had accepted a position of financial trust in good faith and later exploited that position. Cressey notes that, 'These criteria permit the inclusion of almost all persons convicted for embezzlement and, in addition, a proportion of those convicted for larceny by bailee, forgery, and confidence game'. For data, he relied on interviews with 'all the prisoners whose behavior met the criteria and who were confined at the Illinois State Penitentiaries at Joliet' (Cressey, 1950, p.740).

In setting out to explain financial trust violation, Cressey began with a first hypothesis that,

... positions of financial trust are violated when the incumbent has learned, in connection with the business or profession in which he is employed, that some forms of trust violation are merely 'technical violations' and are not really 'illegal' or 'wrong'.

(Cressey, 1950, p.741)

However, this explanation was soon abandoned because it was found that many financial trust violators stated 'that they knew the behavior to be illegal and wrong at all times and that they merely "kidded themselves" into thinking that it was not illegal' (Cressey, 1950, p.741).

An alternative hypothesis was developed that 'positions of trust are violated when the incumbent structures a real or supposed need for extra funds or extended use of property as an "emergency" that cannot be met by legal means' (Cressey, 1950, p.741) This hypothesis was also soon rejected in the face of counter evidence, for example 'persons were found who claimed that while an emergency had been present at the time they violated the trust, other, perhaps even more extreme, emergencies had been present in earlier periods and they did not violate it' (Cressey, 1950, p 741).

The fourth hypothesis was that people violate financial trust when they incur 'financial obligations that are considered as nonsocially sanctionable and which, consequently, must be satisfied by private or secret means' (Cressey, 1950 p 741). However, Cressey reports that,

.. when the cases were re-examined in light of this hypothesis it was found that in a few of them there was nothing which could be considered as financial *obligation*, that is, as a debt which had been incurred in the past and for which the person at present felt responsible. Also, in some cases there had been nonsanctionable obligations at a prior time, and these obligations had not been alleviated by means of trust violation. It became increasingly apparent at this point that trust violation could not be attributed to a single event, but that its explanation could be made only in terms of a sequence of events, a process.

(Cressey, 1950, pp.741-2)

After further revisions and developments, Cressey arrived at his final formulation

Trusted persons become trust violators when they conceive of themselves as having a *financial problem* which is non-shareable, have the knowledge or awareness that this problem can be secretly resolved by violation of the position of financial trust, and are able to apply to their own conduct in that situation verbalizations that enable them to adjust their conceptions of themselves as trusted persons with their conceptions of themselves as users of the entrusted funds or property.

(Cressey, 1950, p.742)

Cressey reports that this explanation fitted all the cases that he had investigated and that no new cases emerged that challenged it

Like grounded theorizing, analytic induction is often presented as being concerned with testing as well as developing theoretical hypotheses. But once again it deviates from the hypothetico-deductive method, this time in being concerned only with the identification of necessary rather than sufficient conditions for the causal production of the type of phenomena being studied. It does this because the cases selected for investigation are only those in which the *phenomenon* to be explained occurs. In order to identify sufficient conditions, the researcher must also investigate cases where the *conditions* specified by the theory are known to hold in order to find out whether or not the phenomenon occurs there (Robinson,

1951; Hammersley, 1989, chs 7–8). Cressey does this to some degree by looking at relevant experiences in the lives of the people he interviewed before they became trust violators. But to study sufficient conditions systematically, he would have had to study some cases where people not yet identified as trust violators saw themselves as having financial problems which were non-shareable, realized that these could be secretly resolved by violation of financial trust and had rationalizations available to them that would preserve their trustworthiness in their own eyes. This involves modifying the process of analytic induction as indicated by Figure 5.



Figure 5 *A modified formalization of the analytic induction process*
(Source: Hammersley, 1989)

In the case of Cressey's work, it would have been very difficult to find the sort of cases demanded by this modified version of analytic induction. However, there are examples of case-study research that come close to the hypothetico-deductive ideal. One is a sequence of investigations in the sociology of education by David Hargreaves, Colin Lacey, and Stephen Ball, each involving a lengthy study of a single secondary school. This research was concerned with the development and testing of differentiation-polarization theory: the claim that differentiation of

pupils on academic-behavioural grounds (in the form of streaming or banding in secondary schools) produces polarization in their attitudes towards school, with high-status (i.e. top stream/band) pupils becoming pro-school, and low-status (i.e. bottom stream/band) pupils becoming anti-school

The first of these studies to be published was an investigation of a streamed, secondary modern school, where the researcher can have had little doubt that polarization of attitudes would be found since it was a well-known feature of such schools (Hargreaves, 1967) What Hargreaves' study amounted to was the description of a case where differentiation was high and so too was polarization.

ACTIVITY 3

On the basis of the very limited information I have supplied, can you see any problems in concluding that differentiation causes polarization on the basis of Hargreaves' study? Answer this question before you read on.

Useful though Hargreaves' evidence is, it is rather weak support for the validity of the theory. After all, many other factors were likely to have been operative in the situation, some of which may well have produced the high level of polarization. For instance, perhaps the streaming system in the school Hargreaves studied sorted pupils on the basis of their attitudes to school, these being largely a product of extra-school factors. This could have occurred both directly, because pupils' attitudes were judged by teachers to be an important consideration, and indirectly, because these attitudes affected the pupils' academic performance. Differences in pupil attitude produced by factors outside the school could therefore explain the correlation between stream and orientation to school that Hargreaves found

If we turn now to Lacey's study of Hightown Grammar School, we can see how it contributes to the process of testing the theory (Lacey, 1970). Not only does he document much the same correlation between stream and attitude as Hargreaves, but.

- 1 The case he studied involved some comparative control over the factor of differences in pupils' attitudes on entering the school. This is because Hightown was a selective school and most of the pupils who were recruited to it had been successful in their primary schools, and were highly motivated to continue their academic success.
- 2 He shows that the polarization in attitude increased over time from the point of initial differentiation in the first year through to the fourth year of secondary schooling.

In my judgment, these features add considerably to the plausibility of the theory, even if they do not render it absolutely convincing.

ACTIVITY 4

On the basis of the information I have provided, can you see any ways in which differentiation-polarization theory may still be false despite the evidence Lacey provides?

There are always potential doubts. For instance, in relation to point 1 above, we must remember that differences in attitude are a matter of degree. Despite relative homogeneity by comparison with differences in attitude across the whole age group, there will still have been differences in attitude among the new recruits to Hightown Grammar, and the streaming system may simply have allocated pupils to streams on that basis, hence reflecting, rather than generating, attitude polarization. Similarly, we could explain the growing polarization over time, not as the product of differentiation, but as the result of external factors operating on existing differences in attitude. After all, a lot of things happen to children outside of

school during their secondary school careers — e.g. changes in relationships at home and in peer groups — and some of these are likely to be systematically related to attitude towards school.⁴

In the third of this sequence of studies, Stephen Ball (1981) investigated a comprehensive school. As a result of this choice of school type, he was not able to control for pupils' attitudes on entry in the way that Lacey had. However, he did look at change in pupils' attitudes over time, and confirmed Lacey's findings. Furthermore, he documented a change in the level of differentiation in the institution (the abandonment of banding in favour of mixed ability grouping), looking at the effects of this on the level of polarization. Given that this involved a change within a single institution over a relatively short period of time, we can assume that much (though not everything) remained the same between the two situations, before and after. Ball shows that the level of polarization was lower after mixed ability grouping had been introduced.

This is further important evidence in support of differentiation-polarization theory, increasing the confidence we can reasonably have in it. Of course, it does not place it beyond all doubt. The selection of cases studied does not rule out all other relevant possibilities. For example, the data produced by these studies could be explained by a kind of imitation theory whereby the attitudes of members of a school class are affected by the initial balance of pro- and anti-school pupils to be found within it, a theory that some teachers hold. This would explain the reduction of polarization following the abolition of banding, independently of the level of differentiation. But, as we saw earlier, even experimental research cannot establish causal relationships beyond all possible doubt. And what this series of investigations illustrates is that while case studies do not involve manipulation of variables, it is sometimes possible to use the comparison of existing cases to make reasonable judgments about causal relationships.⁵

Case studies may of course be combined with experiments in order to test hypotheses about causal relationships. A classic example is *When Prophecy Fails* (Festinger *et al.*, 1956), a social-psychological study that was designed to test cognitive-dissonance theory — a theory about the conditions of attitude change. It involved an ethnographic investigation of a small religious sect whose leader had prophesied the end of the world on a particular date. Festinger and his colleagues had tested cognitive-dissonance theory under experimental conditions but recognized the opportunity offered by the sect: 'we were understandably eager to undertake a study that could test our theoretical ideas under natural conditions' (Festinger *et al.*, 1956, p.1). They set out to investigate what would happen when the prophecy failed to be fulfilled. The theory predicted that rather than leading the sect's members to abandon their religious ideas, prophetic failure would result in their engaging in increased proselytising; and that is indeed what happened!

4 CONCLUSION

In this unit I have tried to clarify the concept of case study, treating it as one case-selection strategy amongst others. I started out from the position that it is not fruitful to think of social research methods in terms of contrasting paradigms. The methodological decisions faced by social researchers are more complex than this, allowing for more variation than such dichotomous models assume.

I defined case study as one means of tackling the problem of case selection. I contrasted it with two other case-selection strategies: surveys and experiments;

⁴ Lacey was aware of these factors and incorporated them into his account, but as subordinate factors to differentiation. His analysis of differentiation *within* school classes, which I have not discussed here, adds further support to his general argument.

⁵ For a more detailed discussion of the work of Hargreaves, Lacey and Ball from this point of view, see Hammersley, 1985. See also the recent development of this work by Abraham 1989.

and I emphasized the trade-offs involved in selecting one rather than another of these strategies. Compared with the survey, case study involves a potential trade-off between generalizability of findings to finite populations on the one hand, and the likely detail and accuracy of information on the other. Compared with the experiment, case study involves a trade-off between control of variables and level of reactivity. I noted that the significance of these trade-offs varies according to the goals and circumstances of the research. In both respects, case studies have considerable strengths. I also argued that there are various supplementary strategies which can be used to at least partially overcome their weaknesses.

When we broaden our focus from single studies to looking at the development of research in particular fields, we can see that it may be possible to maximize the advantages of two or more case-selection strategies by combining them. Succeeding units will provide more detail about other strategies and will also explore the possibilities of combining case-selection strategies in order to capitalize on their respective strengths and overcome their relative weaknesses.

REFERENCES

- Abraham, J. (1989) 'Testing Hargreaves' and Lacey's differentiation-polarization theory in a setted comprehensive', *British Journal of Sociology*, vol. 40, no. 1, pp.46–81.
- Ball, D.W. (1972) 'Self and identity in the context of deviance: the case of criminal abortion', in Scott, R.A. and Douglas, T. D. (eds) *Theoretical Perspectives on Deviance*, New York, Basic Books.
- Ball, S.J. (1981) *Beachside Comprehensive*, Cambridge, Cambridge University Press.
- Becker, H.S. (1971) *Sociological Work. Method and Substance*, London, Allen Lane.
- Bertaux, D. and Bertaux-Wiame, I. (1981) 'Life stories in the bakers' trade', in Bertaux, D. (ed.) *Biography and Society. The Life History Approach in the Social Sciences*, London, Sage.
- Booth, C. (ed.) (1889–1902) *Life and Labour of the People of London*, 17 vols, London, Macmillan.
- Bromley, D. (1986) *The Case Study Method in Psychology and Related Disciplines*, Chichester, Wiley.
- Bryman, A. (1988) *Quality and Quantity in Social Research*, London, Unwin Hyman.
- Bullock, A. (1962) *Hitler. A Study in Tyranny*, Harmondsworth, Penguin.
- Bullock, A. (1991) *Hitler and Stalin Parallel Lives*, London, Harper Collins.
- Cicourel, A. V. and Kitsuse, J.I. (1963) *The Educational Decision-Makers*, New York, Bobbs-Merrill.
- Cressey, D. (1950) 'The criminal violation of financial trust', *American Sociological Review*, vol. 15, pp.738–43.
- Cressey, D. (1953) *Other People's Money*, Glencoe Ill., Free Press.
- Festinger, L., Riecken, H., and Schachter, S. (1956) *When Prophecy Fails. A Social and Psychological Study of a Modern Group that Predicts the Destruction of the World*, Minnesota, University of Minnesota Press.
- Fielding, N. (1981) *The National Front*, London, Routledge and Kegan Paul.

- Firestone, W.A. and Herriot, R.E. (1984) 'Multisite qualitative policy research: some design and implementation issues', in Fetterman, D.M. (ed) *Ethnography in Educational Evaluation*, Beverly Hills, Sage.
- Glaser, B.G. and Strauss, A.L. (1967) *The Discovery of Grounded Theory*, Chicago, Aldine.
- Gregory, R.L. (1970) *The Intelligent Eye*, London, Weidenfeld and Nicolson
- Hammersley, M. (1985) 'From ethnography to theory', *Sociology*, vol 19, pp.244-59.
- Hammersley, M. (1989) *The Dilemma of Qualitative Method Herbert Blumer and the Chicago Tradition*, London, Routledge
- Hammersley, M. (1991) *Reading Ethnographic Research*, London, Longman.
- Hammersley, M. (1992a) *What's Wrong with Ethnography?*, London, Routledge
- Hammersley, M. (ed) (1992b) *Social Research Philosophy, Politics and Practice*, London, Sage (DEH313 Reader)
- Hanson, N. (1958) *Patterns of Discovery*, Cambridge, Cambridge University Press
- Hargreaves, D.H. (1967) *Social Relations in a Secondary School*, London, Routledge and Kegan Paul
- Klockars, C. (1975) *The Professional Fence*, London, Tavistock.
- Lacey, C. (1970) *Hightown Grammar*, Manchester, Manchester University Press.
- Langlois, C.V. and Seignobos, C. (1966) *Introduction to the Study of History*, London, Duckworth.
- Lewis, O. (1961) *The Children of Sanchez. Autobiography of a Mexican Family*, London, Secker and Warburg.
- Lewis, O. (1964) *Pedro Martinez*, London, Secker and Warburg
- Lewis, O. (1970) *A Death in the Sanchez Family*, London, Secker and Warburg
- Millham, S., Bullock, R., Hosie, K. and Haak, M. (1986) *Lost in Care: The Problem of Maintaining Links between Children in Care and their Families*, Aldershot, Gower.
- Mitchell, J.C. (1983) 'Case study and situational analysis', *Sociological Review*, vol. 31, no. 2, pp.187-211.
- Olesen, V.L. and Whittaker, E.W. (1968) *The Silent Dialogue: A Study in the Social Psychology of Professional Socialization*, San Francisco, Jossey Bass.
- Parker, H., Casburn, M. and Turnbull, D. (1981) *Receiving Juvenile Justice*, Oxford, Blackwell.
- Pilcher, W. (1972) *The Portland Longshoreman: A Dispersed Urban Community*, New York, Holt, Rinehart and Winston
- Platt, J. (1971) *Social Research in Bethnal Green. An Evaluation of the Work of the Institute of Community Studies*, London, Macmillan
- Plummer, K. (1983) *Documents of Life: An Introduction to the Problems and Literature of a Humanistic Method*, London, Allen and Unwin.
- Punch, M. (1979) *Policing the Inner City*, London, Macmillan.
- Robinson, W.S. (1951) 'The logical structure of analytic induction', *American Sociological Review*, vol. 16, no. 6, pp 812-18.
- Rock, P. (1973) *Making People Pay*, London, Routledge & Kegan Paul
- Schofield, J.W. (1990) 'Increasing the generalizability of qualitative research', in Hammersley, M. (1992b) (DEH313 Reader)
- Shaw, C. (1930) *The Jack Roller*, Chicago, University of Chicago Press

- Sieber, S.D. (1973) 'The integration of fieldwork and survey methods', *American Journal of Sociology*, vol 78, pp 1335–59.
- Skolnick, J.P. (1966) *Justice without Trial*, New York, Wiley.
- Snodgrass, J. (1982) *The Jack Roller at Seventy A Fifty Year Follow-up*, Lexington, Mass., Lexington Press
- Strauss, A (1987) *Qualitative Analysis for Social Sciences*, Cambridge, Cambridge University Press.
- Strong, P. (1979) *The Ceremonial Order of the Clinic*, London, Routledge and Kegan Paul.
- Thompson, P (1983) *Living with the Fishing*, London, Routledge and Kegan Paul.
- Waller, W. (1934) 'Insight and scientific method', *American Journal of Sociology*, vol. 40, no. 3, pp 285–97.
- Woods, P (1979) *The Divided School*, London, Routledge and Kegan Paul.
- Yin, R (1984) *Case Study Research*, Beverley Hills, Sage
- Zelditch, M. (1962) 'Some methodological problems of field studies', *American Journal of Sociology*, vol. 67, pp.566–76

ACKNOWLEDGEMENTS

Grateful acknowledgement is made to the following sources for permission to reproduce material in this unit:

FIGURES

Figure 2: Hammersley, M. (1991) *Reading Ethnographic Research*, Longman Group UK Ltd. Adapted from Parker *et al.* (1981) *Receiving Juvenile Justice*, Basil Blackwell Ltd; Figures 4 and 5 Hammersley, M (1989) *The Dilemma of Qualitative Method: Herbert Blumer and the Chicago Tradition*, Routledge, © Martyn Hammersley, 1989