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Research Methods Reader

Research Techniques (1)

Action Research: Principles and Practice

Second Edition

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1 What do we know?

The principles of action research

Action research is a name given to a particular way of researching your own learning. It is a practical way of looking at your practice in order to check whether it is as you feel it should be. If you feel that your practice is satisfactory you will be able to explain how and why you believe this is the case; you will be able to produce evidence to support your claims. If you feel that your practice needs attention in some way you will be able to take action to improve it, and then produce evidence to show in what way the practice has improved.

Because action research is done by you, the practitioner, it is often referred to as practitioner research, or a similar name such as practitioner-led or practitioner-based research. It is a form of research which can be undertaken by people in any context, regardless of their status or position. It involves you thinking carefully about what you are doing, so it can also be called a kind of self-reflective practice.

The idea of self-reflection is central. In traditional (empirical) forms of research researchers do research on other people. In action research researchers do research on themselves in company with other people, and those others are doing the same. No distinction is made between who is a researcher and who is a practitioner. Practitioners are potential researchers, and researchers are practitioners (some people who like to maintain their status as 'pure' researchers do not always see it this way, though). Traditional researchers enquire into other people's lives and speak about other people as data. Action researchers enquire into their own lives and speak with other people as colleagues. Action research is an enquiry by the self into the self, undertaken in company with others acting as research participants and critical learning partners.

Action research involves learning in and through action and reflection, and it is conducted in a variety of contexts, including the social and caring sciences, education, organisation and administration studies, and management. Because action research is always to do with learning, and learning is to do with education and growth, many people regard it as a form of educational research.

In one sense, there is no such 'thing' as action research. It is important always to remember this. Sometimes people write about action research as if it were a self-contained object of enquiry, existing separate from themselves. I am doing so now. On this view, action research can become an abstract discipline, a set of procedures which can be applied to practice. It can then turn from being a living process to a linguistic abstraction, and this tends to distort the values of justice and individual autonomy which animate action research. It is important always to locate

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discussions about action research within the real-life experience of real-life people. The 'meaning' of action research is in the way people live together.

While there might be no such thing as action research, there are people who are action researchers. They might not call themselves by that name, but if they wished to give their work a theoretical framework, they could well call the framework action research. When people first encounter the idea of action research they often say, 'This is what I do in any case, only now there is an organising framework for it.' The idea of action research refers to the theoretical framework which guides practice. Action research is not a thing in itself; the term always implies a process of people interacting with one another.

Action researchers share certain sets of beliefs, commitments and hopes. What they do (action research) is a set of practices which demonstrates those beliefs, commitments and hopes in practice. They undertake research to help them learn how to create social hope (Rorty, 1999) and to take action to try to realise the hope in terms of social evolution.

Questions arise, therefore, about what action researchers do, and how and why they do it, questions to do with how we view ourselves (ontology), how we come to know (epistemology), how we do things (methodology), and what we hope to achieve (socio-political intent). These aspects are always interrelated.

Aspects of research

Action research (for that matter all kinds of research) is more than just doing activities. It is a form of practice which involves data gathering, reflection on the action as it is presented through the data, generating evidence from the data, and making claims to knowledge based on conclusions drawn from validated evidence. When we come to producing reports, it is not enough only to offer descriptions and activities lists. Explanations need to be given for the activities, in terms of the researcher's values, intentions and purposes for doing the research. For example, if a researcher makes a claim that they have helped others become more confident, the values that inform their work include the idea that people should feel respected. So when people do demonstrate their confidence, such as asking a question in public, the researcher could claim that they had fulfilled their values, and that they had influenced the quality of someone's life for good.

It is helpful to be familiar with some key ideas and terms used in educational research.

Research is generally held to involve the following:

- *ontology* – the way we view ourselves, a theory of being
- *epistemology* – how we understand knowledge, including how knowledge is acquired
- *methodology* – how we do things.

Educational research also involves issues of politics, because it is always socially embedded; it is done by real people with the intent of illuminating, explaining and

improving human interaction in education settings. Action research has as a main purpose the generation of knowledge which leads to improvement of understanding and experience for social benefit.

DOING ACTION RESEARCH

What do action researchers believe in? Ontological issues

Action researchers believe that people are able to create their own identities and allow other people to create theirs. They try to find ways of accommodating multiple values perspectives. This is surely very difficult when one set of values is radically at odds with another. They try to find ways to live together in spite of their potential differences. Living together successfully requires hard work and considerable effort to understand the other's point of view; this means developing their potentials to care, and recognising and suspending their own prejudices.

Creating the kind of societies they feel are good societies involves their personal commitment to action. This means having the courage to speak and act in ways which are often contested. They hold a vision of a future which is better than the present, characterised by creative, life-affirming ways of living. The future is embodied in the present; they can realise future potentialities by improving what they are doing in relation with others in the present. They know that if they abandon the vision of a better society in the light of the troubles of the present one, they will probably settle into stasis. However, if they try to do something, just one positive life-enhancing action, there is hope. Improvement is still improvement, no matter how small.

Action researchers accept the responsibility of ensuring that their own lives are in order before they make judgements about other people's. This means honestly critiquing their practice, recognising what is good and building on strengths, as well as understanding what needs attention and taking action to improve it. It involves commitment to the idea that learning will transform into purposeful personal action for social benefit.

They often express these ontological assumptions in the language of values. Action research rests on ideas to do with truth, social justice, compassionate ways of living, respect for pluralistic forms. Often action researchers live in social contexts where these values are prized in principle but denied in practice. The realities of their contexts often show preference for privileged elites rather than the underprivileged and marginalised. Action researchers aim to understand these issues in order to change present realities into futures which are more in tune with their values.

How do action researchers come to know? Epistemological issues

Epistemology is the name given to the study of what we know and how we come to know it. Traditional views of scientific enquiry tend to see knowledge as

a free-standing unit, with an existence of its own, residing 'out there' in books and databases. In this view knowledge is divorced from the people who create it.

Action researchers see knowledge as something they do, a living process. People can generate their own knowledge from their experience of living and learning. Knowledge is never static or complete; it is in a constant process of development as new understandings emerge. This view of knowledge regards reality as a process of evolution, surprising and unpredictable. There are no fixed answers, because answers would immediately become obsolete in a constantly changing future. The very idea of answer becomes meaningless; answers transform into new questions. Life is a process of asking questions to reveal new potentialities. Action researchers ask questions of the kind, 'I wonder what would happen if . . .?' They aim to disturb fixed systems of knowing rather than maintain them.

Learning in this view is rooted in experience. It involves reflecting on the experience of practice (a process of critical discernment), deciding whether the practice was in line with your espoused values base, and then deciding on future action as a result of the reflection. If you consider practice good, how can you develop it to deal with an uncertain future? If you consider it less than good, how can you improve it?

Some theorists believe that learning happens only in critical episodes. Certainly it does, but learning also happens all the time, in our moment-to-moment living. We learn how to walk, to catch a ball, to avoid trouble, to respond to our feelings. Learning, says Mary Catherine Bateson (1994), often happens peripherally; we learn a good deal without effort and without conscious intent. Learning can be accelerated and intensified through critical awareness, and reinforced through intellectual study. Learning mainly involves making new connections and reconfiguring present knowledge in terms of its potential use value, and this process is often carried out at a level not accessible to conscious awareness.

How do action researchers act? Methodological issues

Action researchers regard learning and experience as processes which enable individuals to make choices about who they are and how they are together. However, people's choices often conflict, so they have to be negotiated and accommodated. This can be very difficult, but it can be done if people try to see one another's point of view. The methodology of action research is that people ask questions such as 'How do I do this better? How do we understand?' They do not aim for consensus or harmony, but they do try to create spaces of tolerance to negotiate differences.

This can happen because reflection on action is an inherent part of an action research methodology. The idea of reflective practice was originally popularised by Donald Schön (1983). Reflection on action makes sense, however, only when practice is seen as in relation with others, a process of dialogue and encounter (Bryman *et al.*, 1993). For some, myself included, the ideas of encounter, connectedness and relationship can be understood as a form of spirituality. Capra *et al.* (1992) for example, believe that relation should be understood as belonging. We are all connected in deep ways, and, because we are made of the same stuff as stars

(Feynman, 1999), we are also connected to the whole of creation. We belong to one another and ultimately to the universe.

These views have implications for how people understand their practice. In traditional epistemologies, practice tends to be seen as something separate from practitioners. People might imagine work as in a building or an office, for example. I used to think like this; I regarded work as a thing I did. On a relational view, work and practice are how we are in relationship with other people. The focus of the work is how to nurture creative and life-giving encounters. Action researchers regard their work as ensuring that encounters with others are opportunities for learning and growth. When they reflect on practice they are reflecting on their relationships with others, and whether those others have benefited from the encounter. This can be a major test for judging the quality of the practice: has the other person benefited from the encounter? The implications are awesome. If we are always in relation and those relationships have potential influence for changing people's lives, even in small ways, how great is the responsibility to ensure that the influence is life-affirming. If we make ourselves who we are through our capacity for choosing, how important it is that we choose to avoid doing harm.

What are the socio-political implications of our knowledge?

There are serious implications in these views. Here are some of them.

Challenging dominant epistemologies

The purpose of research is generally understood as gathering data and testing it in order to generate new knowledge which can produce new theories of how reality works. In traditional views theories exist as an abstract body of knowledge which informs practice, a theory-into-practice model. In traditional education settings, whether in formal schooling or professional education, there is an expectation that people will attend lessons and take notes, but not raise questions. Some researchers (see Chapter 3) produce conceptual models which work in practice provided people are obedient and comply with how the model says they ought to behave. If people exercise their independence of mind and spirit, however, and disagree with the model itself or the fact that they are supposed to agree with abstract theory, they are often seen as disruptive and anarchic.

The traditional positivist view of research and theory has dominated Western institutional thinking and practice for centuries. New movements such as action research have challenged traditional views. Such challenges are naturally unwelcome to dominant elites, who then gather force to put down the insurgency. They use a range of control strategies including ridicule and marginalisation, what Lyotard (1984) calls intellectual terrorism. The most characteristic response is to pretend that critique does not exist. When a critical mass builds up, however, sufficient to show that it does, other measures must be exercised. The most characteristic of these is to use the language of 'radical', 'unorthodox' and 'alternative'. There is nothing radical or unorthodox about people wanting to have a say in their

own lives. It is important not to let propaganda or fear of being labelled reactionary stand in the way of realising one's vision for what could be a better way of life.

The issue then becomes the legitimacy of forms of theory, who is entitled to generate theory, and how the theory is judged – 'who decides what knowledge is, and who knows what needs to be decided' (Lyotard, 1984: 9). Ball (1990: 17), drawing on the work of Michel Foucault, says that it is not only about 'what can be said and thought but also about who can speak, when, where and with what authority. Discourses embody meaning and social relationship; they constitute both subjectivity and power relations.' The issue then extends to not only what should be judged a worthwhile theory but also who should be judged a worthwhile person.

The topology of epistemological landscapes

Schön (1983, 1995) speaks of the topology of professional landscapes and their characteristic epistemologies. There is a high ground, he says, which favours technical rationality (what I have so far called propositional forms of knowledge), and a swampy lowlands which values intuitive, practical forms. The high ground tends to be found in institutions and is peopled mainly by elitist intelligentsias from the corporate and formal education worlds. Chomsky has often referred to these as a 'high priesthood'. The high priesthood is much occupied with generating abstract theories about issues which, while valuable in themselves, often have little to do with important aspects of everyday living. Because of the prestigious social positioning of the theorists, their abstract form of theory has come to be seen as dominant. Practitioners, on the other hand, deal with issues of everyday significance, but, because practitioners are not viewed as legitimate knowers, either by the high priesthood or by themselves (because 'ordinary' people are systematically taught to devalue their own contributions), their form of theory tends to be regarded as practical problem-solving rather than proper research.

The situation is topsy-turvy to the realities of daily living. Precisely those issues of daily significance which occupy practitioners are trivialised, along with the status of the practitioners as knowledge workers and theory generators, while abstract theorising continues to maintain institutional legitimacy.

Schön calls for a reappraisal of what counts as scholarship. Research which addresses the important issues of daily living needs to be given as much prestige as traditional scholarship. Practical theorising is an important methodology for making holistic cultural, social and intellectual progress. Practical, experiential theorists should have status equal to abstract theorists in corporate and higher education contexts: they are in the front line of social theorising. Practical forms of theory are as legitimate as 'pure' conceptual forms. The most powerful and appropriate form of theory for dealing with contemporary social issues is one which is located in, and generated out of, practice, and which values tacit knowledge as much as cognitive knowledge. This all comes down to action research, a way of researching one's own practice and generating personal theories of practice which show the process of self-monitoring, evaluation of practice, and purposeful action to improve the practice for social benefit.

Levels of adequacy

In 1965, and focusing on linguistic analysis, Noam Chomsky explained that research can operate at three levels of adequacy: observational, descriptive and explanatory. In a sense, all research begins with observation, and most research offers descriptions of events. In 1960s linguistics the dominant research methodology was behaviouristic. The aim was to study a particular language, gather instances of its significant features, and provide descriptions of the language under study (Lyons, 1970). The same tendency is visible today across the social sciences, education research and organisation study. Everywhere there are descriptions of how things work, or ought to work, and what needs to be done to make them work in this way. These are inert theoretical models. They work in principle, but often there is no live evidence to show that they work in practice.

It is not enough, in Schön's view, to stay at the level of hypothetical theorising. It is necessary to move to explanation in Chomsky's sense. Moving from observation and description of action means moving to offering explanations for action. The focus of research then develops from observing and describing what is happening to considering why it is happening – that is, the reasons and intentions of the person which inform the behaviour.

The issue remains, however, whose research is it? Some views of action research say it is acceptable for an external researcher to observe, describe and explain the actions of others who are doing action research. This belief animates an interpretive view of action research (see Chapter 3). In my opinion, this is a distortion of the values of democracy and respect for others who should be regarded as thinking people who have the capacity to judge their own practice, also recognising that the process of self-evaluation is likely to be enhanced within a community of critical friends. For action research to operate successfully as a methodology for social change, the locus of responsibility for conducting the research needs to shift from an 'external' researcher who is observing and describing other people's activities to practitioners themselves who give accounts of their own activities in terms of their values and hopes.

E-theories and I-theories

I again draw on the work of Chomsky to support this view.

In his *Knowledge of Language* (1986) Chomsky developed the idea of E-language and I-language. The emphasis in traditional American linguistics in the 1970s and 1980s was still on the sound and word structure of sentences, and a language could be understood 'as a collection (or system) of actions or behaviours of some sort' (p. 20). Chomsky refers to this as an 'externalized language' (E-language). An 'internalized language' (I-language), on the other hand, is 'some element of the mind of the person who knows the language acquired by the learner, and used by the speaker-hearer' (p. 22). In 2000 Chomsky developed the concept of I-conceptual and I-belief systems, a concept that revolves around the internalised nature of beliefs and ideas. This indicates a shift away from description of language or thinking or theory generation, as an external object of study, towards an explanation of how

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language or thinking or theory generation informs the way a person creates their own version of reality.

This is a most important concept, and I wish to develop the notion of 'E' and 'I' (as I have already done in McNiff 1993) to refer to different forms of theory and ways of coming to know. An E-theory exists as a form of theory external to its creator and which is generated from study of the properties of external objects. This is a propositional form of theory, much admired in social scientific analysis, behaviourist in orientation, and synchronic (in linguistics this is understood as abstracted from time). An I-theory is a dialectical form of theory, a property of an individual's belief system, and is diachronic (in linguistics this is understood as oriented in real time). This view is helpful for understanding different forms of theory, not only for linguistics but also for broad areas of human enquiry, including educational research. In this book I take the view that action research leads to the generation of I-theories of knowledge, theories which are already located within the practitioner's tacit forms of knowing, and which emerge in practice as personal forms of acting and knowing. These theories are linked with other I-belief systems – values, for example. The way the theories manifest as living practices is congruent with the belief systems of the knower.

Debates like this, to do with how we understand the process of research and the generation of theory, however, give rise to struggles about the nature and practice of action research – what it is and who owns it (or, when action research is taken as a term denoting people in company with one another, who we are and who creates our identities).

The struggle for action research as a living practice

At the moment three distinct developmental trends are visible in the literature of action research: an interpretive, a critical theoretic and a living theory approach (see Chapter 3). Interpretive and critical theoretic approaches clearly work at the levels of observation and description: while they also offer explanations for practice, these explanations are offered within sets of propositional relationships. It also seems that many people offering action research courses in higher- and formal-education contexts tend to operate within interpretive and critical theoretic rather than living theory frameworks. It is less problematic to observe other people doing action research than to do it oneself.

Engaging with living theory approaches means, as Whitehead says, placing the 'living I' at the centre of our enquiries and recognising ourselves potentially as living contradictions. We might believe we are working in an effective and morally committed manner and then find from our own self-evaluation that we are denying much of what we believe in.

Here is an example from the doctoral work of Caroline Clarke as she speaks about trying to live out her values of care. Outlining her research (Clarke, 2000: 1–2), she says:

My study focuses on two main areas: my personal and professional journey as an educator and my attempt to change and influence the culture of my school

with regard to discipline . . . I describe the 'epiphanies' that brought me to realise that I was outside my 'value world' and consequently experiencing a drain on my emotional and spiritual energy as a result of my workplace role. Following these realisations I began searching not only for answers but also for understanding of what was happening to me in those moments where my values were compromised and I became what Jack Whitehead (1989) describes as a 'living contradiction'. The answers came in the form of reading, observation and action on reflection, and the solution came in the form of change. The change was two-fold, in me and in the wider educational system of which I was a part. My diary of the time (June, 2000) reads: 'To hope for a change is essential but it takes courage to go beyond hope and bring about change. It must be the kind of courage which not only seeks to change oneself, but also the circumstances and people around you, despite the opposition.'

Self-study is now widely recognised as a powerful influence for personal and social renewal (see the foreword by Douglas Barnes, in Hamilton, 1998; Zeichner, 1999). It does mean accepting the responsibility of accounting for our own practice, and, in work contexts, accounting for our own professionalism. We offer descriptions and explanations for our work by producing professional narratives to show that the work did impact beneficially on others. We gather and test data of our practice and produce evidence to show that our claims are well founded. Those with whom we work state that they have benefited (or not as the case may be), and those with whom they are working testify that they in turn are benefiting (or not) (see, for example, Delong, 2000; Lillis, 2000b). So it is possible to trace lines of influence from ourselves to others with whom we might have no personal contact, but whose lives we can claim to have touched. There are, says Bakhtin (1986), voices in everything. I am alone as I write, but I am influenced by the voices in the texts I have read and the seminars I have attended, as well as the voices in the supermarket and at the airport. You are listening to my voice as you read, and responding, and in turn others will hear your voice and be stirred. How can we ensure that we are speaking well, and using our influence for others' benefit? In some instances the lines of influence are too complex and it is impossible to know the extent of our influence. An implication is that, in all the contexts of our lives, whether its effects are visible or not, we need to ensure that our influence leads to life-enhancing growth for all.

Descriptive E-approaches cannot do this. They work from a behaviourist orientation in which an external researcher offers accounts of other people's action. In this view, as McNamara and O'Hara (2000) and Zuber-Skerritt (1996) rightly say, it is difficult to show how action research can influence organisational growth or collective action. The process of influencing social change begins with the process of personal change: 'change can only come about when the individuals who belong to a particular organization can see the point in changing' (Rizvi, 1989: 227). It is pointless to produce abstract models of social change and expect other people to apply them to their own circumstances or locate themselves within the models (as, for example, Zuber-Skerritt does in 1992a and 1996). Bourdieu's (1990) idea

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about the reality of the model being more powerful than the model of reality becomes very real.

I am not saying that observations and descriptions are unimportant. They are important, but they do not go far enough. It is not enough for an external observer to describe another person's actions and then to present an account of those actions as if to give a full explanation of their reality. The practice is also ethically questionable. I am saying that, while observation and description are essential first steps, it is important to go beyond and offer explanations. Explanations are the I-theories people generate to show their own process of learning and development. Moving on like this is a generative transformational process in which present forms transform into increasingly robust forms; observations turn into descriptions which turn into explanations. The whole developmental process is integrated within the life of the person who is telling the story. An approach which might be deemed educational would perhaps be to place evidence from living theory accounts alongside the propositional theories generated from spectator research, and so show the enhanced validity of those living theories which explain the practices and learning of individuals.

Action research for explanatory adequacy

Here is an example of how descriptions can turn into explanations, how propositional theory can turn into real-world action. The example is taken from the action research literature about the nature of action research.

There are many well-known descriptions of action research. Here are two of the most famous.

Description 1

Action research is a form of *collective* self-reflective enquiry undertaken by participants in social situations in order to improve the rationality and justice of their own social or educational practices, as well as their understanding of these practices and the situations in which these practices are carried out.

(Kemmis and McTaggart, 1988: 5; emphasis in original)

Description 2

If yours is a situation in which

- people reflect and improve (or develop) their own work and their own situations
- by tightly interlinking their reflection and action
- and also making their experience public not only to other participants but also to other persons interested in and concerned about the work and the situations (i.e. their (public) theories and practices of the work and the situation)

and if yours is a situation in which there is increasingly

- data-gathering by participants themselves (or with the help of others) in relation to their own questions
- participation (in problem-posing and in answering questions) in decision-making
- power-sharing and the relative suspension of hierarchical ways of working towards industrial democracy
- collaboration among members of the group as a 'critical community'
- self-reflection, self-evaluation and self-management by autonomous and responsible persons and groups
- learning progressively (and publicly) by doing and by making mistakes in a 'self reflective spiral' of planning, acting, observing, reflecting, replanning, etc.
- reflection which supports the idea of the (self-) reflective practitioner

then yours is a situation in which action research is occurring.

(inclusive working definition drawn up collaboratively at the International Symposium on Action Research, Brisbane, March 1989, and reproduced in Zuber-Skerritt, 1992b: 14; emphasis in original)

So far, these are linguistic descriptions of action research. However, some of the authors go on to show how they turn their linguistic descriptions into real-life explanations (see Atweh *et al.*, 1998); they show how they lived out the principles they spell out. More of such accounts are needed.

Future directions in action research

These issues are important for future developments. Action researchers need to show their collective intent to live out the values which inform their work. Because they write about action research they inevitably position themselves as action researchers, so they need to take care that they do not stay at the level of abstract analysis. If they write about practice but do not explain their own they are not engaging with the issues they are speaking about. Contradictory situations arise. The contradictions are methodological, in the same way as when we try to teach people how to swim on dry land; and also ethical, as when we talk at people about the value of dialogue. Action researchers cannot afford to be armchair philosophers if they wish to maintain their professional and ethical integrity. Action research means action, not by some, but by all, but this means honesty and courage, and is not easy for those positioned as members of intelligentsias. We are all judged by our actions, especially when action is part of our trade mark. We all make our own decisions about these things.

So what do we know?

The community of educational action researchers knows a great deal about the procedures and principles of action research. We do not know so much about how action research can be used as a form of living practice in the evolution of good social orders, although a good deal of work has appeared recently in this regard. Jack Whitehead, Pam Lomax, I and others have supported the development of networks of practitioners who have produced accounts of educational development and who in turn support others to produce accounts of how they do the same. The support of this networking is managed in a non-hierarchical, non-coercive way. It is a question of educational influence, a dialogue of equals. We constitute educational communities who are hoping to transform themselves through learning for social benefit.

I like Lynn Davies's (1990: 210) view of the management of learning communities: 'to achieve equity and efficiency, out go coercion, streaming, hierarchies and leadership, and in come federalism, power-sharing, organizational responsiveness'. This view is shown in our educational networks. Impressive bodies of validated case studies now exist in the Universities of Bath, Kingston and the West of England in the UK; in Brock and Nipissing in Canada; in Limerick in Ireland. These case studies constitute a major body of educational research literature. The influence of practitioners' ideas is being felt in their contexts of practice (for example, Delong 2000; Evans, 1996; Lomax, 1996; McNiff *et al.*, 2000).

Action research has been legitimated by the Academy as a powerful and valid form of learning. The task is now to extend the range of influence. While it is not too difficult to show influence within supportive communities (see Chapters 5 and 9–12), it is more problematic when it is a question of influencing others who are indifferent or hostile, or whose interests are to do with careerism and profit-making rather than education. How the knowledge can be disseminated, and the influence intensified, is discussed in Chapter 4.

I will now move on to consider the relationships between theory and practice

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2011

How to do Your Case Study

A guide for students and
researchers

Gary Thomas

(Ch. 1)

 SAGE

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1

What is a case study?

The case study method is a kind of research that concentrates on one thing, looking at it in detail, not seeking to generalise from it. When you do a case study, you are interested in that thing in itself, as a whole.

I'm using the word 'thing' advisedly since the thing may be a person, group, an institution, a country, an event, a period in time or whatever. You may be looking at the process of a medical diagnosis with one patient or you may be looking at relationships among a gang of teenagers or you may be looking at one student's learning in class or a family or you may be looking at IKEA's development as a business or the USA's political stance at the time of the Cuba Missile Crisis. Any of these 'things', these phenomena, could form the subject of a case study. What is of interest is the uniqueness of the thing and the thing in its completeness.

Being about one thing, it is about the particular, rather than the general. You can't generalise from one thing, so there is no point in trying to do so – no point, in other words, in trying to say, 'This is the case here, so it is also the case there, there and there.' You cannot, for example, say, 'The treatment of this patient with influenza with our new drug *Flugone* has worked. Hooray! Let's give it to everyone with influenza.'

Because you are not able to generalise from this one case, there is no point in thinking about all of the sampling techniques that are used with other kinds of research. The only sense in which 'sampling' is relevant in a case study is the sense in which you go out and find the subject of your case study.

A case study is about the particular rather than the general. You cannot generalise from a case study.

This is your 'sample' and you have to justify your choice.

The main feature of your choice of case will be the interest that you have in the subject of the study. Why are you interested in it? It could be interesting because ...

- you are intimately connected with it – perhaps it is a child in your class, a patient in your hospital or a project in your company
- it is a conspicuously good example of something in which you are interested – for example, IKEA as an example of a successful business

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- it is different from what is typical – it is an ‘outlier’ that shows something remarkable by virtue of its difference from what seems to be the norm, such as, perhaps, Kerala as a state in India where, despite widespread poverty, people have a far longer life expectancy than they do elsewhere in India.

Singleness is the watchword with a case study.

Should you worry about this singleness? You may, after all, have heard people being snooty about ‘anecdotal evidence’ – in other words, evidence that comes from just one situation or one event. Such anecdotal evidence is uncorroborated by evidence from different experiences. Social science is about getting beyond what is merely anecdotal, some would say.

Certainly, when we want to generalise – to say that this phenomenon has occurred in this sample of people and we can generalise this finding to the wider population – clearly we cannot use just one person’s experience or a single set of uncorroborated observations as the basis for this. That is why such trouble has to be taken in many kinds of research to establish the sample as being *representative* of the wider population. If we want to generalise we need to make the basis of our generalisation clear: what gives us grounds to make the claim that we *can* generalise?

What we are talking about with a case study, though, is a different kind of inquiry from those where generalisation is expected to follow. Here we are talking about understanding *how* and *why* something might have happened or why it might be the case. The assumption in a case study is that, with a great deal of intricate study, looking at our subject from many and varied angles, we can get closer to the ‘why’ and the ‘how’. You’ll notice, though, that although the assumptions about valid processes of inquiry and analysis differ from those in many kinds of social research (for we are not working from samples that enable us to generalise), assumptions about the use of evidence do not change. With a case study there is still the assumption that we must collect good evidence and lots of it. In the modern phrase, we must ‘drill down’ as deep as we possibly can to get evidence, penetrating into every nook and cranny and squeezing out every little bit that can be found.

To mix our metaphors even more, we must look at our subject from many and varied angles, to develop what the great historian-philosopher Michel Foucault (1981) called ‘a polyhedron of intelligibility’. By this he meant that inquiries in the humanities and social sciences are too often one-dimensional, as if we are looking at our subject just from one direction. In looking from *several* directions, a more rounded, richer, more balanced picture of our subject is developed – we get a three-dimensional view.

Two important things about a case study:

- 1 you drill down further
- 2 you create a three-dimensional picture – or what Foucault called a ‘polyhedron of intelligibility’.

It is not just in the social sciences and the humanities that this multifaceted view is valuable. In what are taken to be 'harder' sciences, such as biology, astronomy and geology, and in many applied sciences, such as palaeoanthropology (the study of humanoid fossils), the case study is one of the principal methods used.

Let's look at astronomers. They can't experiment with the stars and, although there are a lot of stars (around 10^{22} – that's 1 with 22 noughts after it), when something unusual happens in the sky, astronomers have to make inferences from a very small number of cases – often, only one case. Their work is carried out by means of the case study method. As data accumulates, they patch bits of it together. They try things out with the data, trialling various models to attempt to explain it.

For example, in 1967, an astronomical phenomenon (to which the name 'pulsar' was later given) was discovered by the astronomer Jocelyn Bell Burnell and her colleagues with the then new radio telescope at Jodrell Bank. As she was drinking her tea one morning and eating her Penguin biscuit for elevenses, gazing into the green cathode-ray tube, Jocelyn noticed something unusual. Something in a distant galaxy was giving off massive pulses of radio energy at fantastically regular intervals. The rhythm was as regular as an atomic clock.

How could this amazingly regular pulse be accounted for? At first, it was thought by these hard-nosed scientists that this was a message from intelligent life and the pulsar was given the name LGM-1 (Little Green Men 1). There was no better explanation for this regular as clockwork beep beep beeping; nothing else in the known universe did anything like this. The astronomers had to make judgments based on their existing knowledge of phenomena such as this and the best they could come up with at the time was little green men trying to communicate with us.

This pulsing phenomenon was a case – *The Case of the Beeping Astronomical Phenomenon*, Sherlock Holmes might have called it – and astronomers had to go about studying it idiographically (see right for definition). In fact, the case study is sometimes called idiographic study since the ideas are all based on and rooted in a single picture – the picture drawn by the inquirer. Given that there was only this case to study and little green men provided a possible, yet unlikely, source of explanation, the scientists let this explanation suffice for the time being.

Idiograph – a symbol, a drawing or figure that stands for an idea (from the Greek *idea* meaning ... er ... 'idea', and *graphein* meaning 'to write').

To go beyond this simplistic (that is to say, too simple) explanation, Jocelyn and her astronomer colleagues had to come up with different kinds of observations and work on different explanations. They had to look in different ways at the pulse to work out what it might be. By using their existing knowledge and theoretical speculation – putting forward and rejecting or accepting plausible hypotheses – they eventually worked out these regular pulses were the flashes of radiation from a

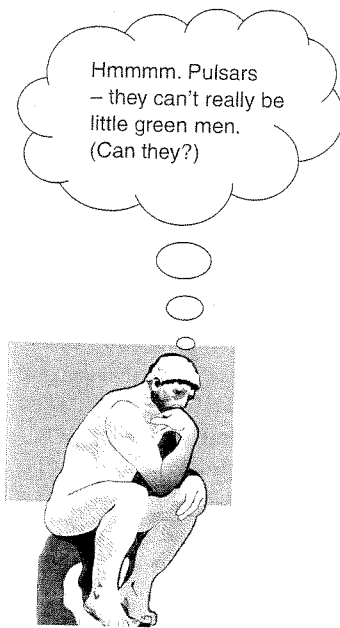


Figure 1.1 Problems are solved by *thinking*, not by a special method

collapsed neutron star that was spinning very fast to give out a directed and extraordinarily condensed beam of light, like a lighthouse.

The important point about this story is that this conclusion was arrived at not by studying thousands or hundreds or even tens of these pulsars, but by studying just one or two pulsars in a great deal of detail and employing a great deal of thinking. Hugely significant scientific conclusions were drawn from this case. Much of the most important natural scientific endeavour and discovery has been achieved in similar ways. Indeed, it is worth noting that Einstein's *annus mirabilis* papers (including his conclusions on special relativity and mass-energy equivalence) were produced with little more than 'thought experiments' and a pencil, while Darwin's theory of evolution could be said to have been based on the insights he gained from a series of meticulously conducted case studies. From Einstein's 'thought experiments' to Darwin's finches to Jocelyn Bell Burnell's pulsar, the study was of *cases*.

The key, I think – and I am now moving back more single-mindedly to social research – is in doing what Flyvbjerg (2001: 132) calls 'getting close to reality'. By this he means keeping in contact with the subject of study and thinking with your own experience and your own intelligence. It is this 'staying real' that the case study is particularly good at encouraging, for it eschews methodological formulae and endorses and stimulates a critical, creative approach to problemsolving.

Flyvbjerg puts the emphasis in this 'getting close to reality' on 'little questions' and 'thick description' (of which more on page 135). We sometimes ignore these little questions in the 'Big Science' view of what research should be like. Flyvbjerg (2001: 133) quotes Nietzsche as saying, 'all the problems of politics, of social organization, and of education have been falsified through and through ... because one learned to despise "little" things, which means the basic concerns of life itself.' As Flyvbjerg points out, small questions often lead to big answers.

For me, the truth of Flyvbjerg's point is exemplified in one of the best case studies I have ever read: *A Glasgow Gang Observed* (Patrick, 1973). It was written by a young sociologist named James Patrick (a pseudonym, for reasons that will become obvious), who had infiltrated a gang in the Maryhill district of Glasgow. By hoodwinking members of the gang into believing that he was like them, then joining the gang and becoming a participant in its activities, Patrick was able to paint a detailed picture of

the way that it operated. Although it is a case study – there is only a single gang, not 50, from which to generalise – it gives, through sparkling analysis, an understanding of gangs. Even though there is no pretence that this is a representative picture or all gangs are like this, we can nevertheless get a rich understanding of the dynamics, tensions and motivation of gangs in general.

I'll let Flyvbjerg (2001: 135–6) have the last word on this since he puts it so nicely:

practical rationality and judgment evolve and operate primarily by virtue of deep-going case experiences. Practical rationality, therefore, is best understood through cases – experienced or narrated – just as judgment is best cultivated and communicated via the exposition of cases ... which is why ... John Dewey [says]: 'the way to re-enchant the world ... is to stick to the concrete.'

A lot can be achieved by recourse to the concrete. We escape from a tendency too often found in academic writing to obfuscate with abstractions rather than clarify with specificity; to bring a fog over the topic in hand with abstract words and the seeking of generalisation where none is possible and none is helpful.

The great writer Harold Evans (2000: 32) said that the abstract should be 'chased out' in favour of the specific. He was talking of journalism rather than academic inquiry and writing, but I think his point is just as relevant in the academy, if not more so. It is a point that is especially germane to the use of the case study, where the reason-for-being concerns the validity of the concrete and the specific. Evans (2000: 33) quotes from C.E. Montague: 'The great escape should be from "mere intellectualism, with its universals and essences, to concrete particulars, the smell of human breath, the sound of voices, the stir of living".'

You should, in a case study, be able to smell human breath and hear the sound of voices. Nothing is lost in their refraction through our own understanding as interpreting inquirers. In fact, much is gained as we add a separate viewpoint – one that moulds and melds the experiences of others through our own understandings.

Is the case study scientific?

If people are ever sniffy about the case study with you – or, indeed, if ever you have doubts yourself about the validity of this method – then you should remind yourself of the pedigree of the case study in the natural sciences. It has a lineage of highly significant advances coming out of case studies – idiographic analysis. The interesting thing about scientists' own reflections on their work and the methodological traditions in which it grew is that it is generally accepted there is no particular, correct or proper way of generating or marshalling evidence and undertaking

inquiries. As Einstein put it, the creative scientist must be an 'unscrupulous opportunist'. The essence of science, he said, is the seeking 'in whatever manner is suitable, a simplified and lucid image of the world ... There is no logical path, but only intuition' (cited in Holton, 1995: 168).

Some students in the social sciences seem unaware of this methodological eclecticism.

In fact, they may assume that 'serious' scientific inquiry is of a different character from that of the case study and necessarily

There are many ways of being scientific. The case study is one of them.

involves quantification, isolation and manipulation of variables and carefully controlled experiments. This, however, is a product only of a particular kind of mid-twentieth-century thinking on research and method.

In a classic article reviewing this mindset, two social scientists, Parlett and Hamilton (1987), suggested that social science – particularly education and psychology – had assumed the methods of social science should, at their best, properly involve work with large datasets from which generalisations could be made.

Parlett and Hamilton saw this attitude as wrong-headed. They saw the mistake as having emerged from an expropriation by the social sciences of the outlook and methods of certain branches of the biological sciences. In particular, they said, the methods of agricultural scientists and medics had been seen by large sections of the social science establishment as the template within which to fit. They described this outlook, this way of doing social science, as conforming to what they called the *agricultural-botany paradigm*. Plant scientists do most of their work comparing the growth of fields of wheat or potatoes or soya beans that have been subjected to different treatments, but you can't, they suggested, treat social analysis in the same way that you treat plant analysis.

Their point was that the evaluative methods of agricultural scientists are fine and dandy for studying agriculture, but the trouble is that people are not ears of wheat, nor are they potatoes or even soya beans. We cannot, when we set up social scientific experiments, make assumptions about 'before' and 'after' conditions in the same way that agricultural scientists do, because there aren't before, middles and afters as there are in fields of crops being given extra doses of fertiliser or different insecticides.

People do odd things, in a way that potatoes don't. People are, in the jargon, *agents* in their own destiny (in the way that potatoes aren't) and in the habit of making subtle or even drastic changes to the conditions for a trial. The measures used to assess change can't be taken with a tapemeasure or a pair of calipers; they have to be undertaken with test scores, attitude ratings and so on. The trouble is that the latter are assumed to be of the same order of robustness as the assessments – simple measures of length and weight – that are made of plant growth. They're not. These social and psychological measures are not the same as centimetres and grams (which get gold star status in the measuring stakes because a centimetre is always a centimetre); they are far less trustworthy – in fact, they can be downright deceitful. In short, Parlett and Hamilton argued, studies undertaken in education and social sciences in this tradition fall short of

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their own claims that they are controlled, exact and unambiguous. (See also my own musings on this subject, Thomas, 2009b.)

The long and the short of this is that there is no one way to be scientific and the case study is just as scientific as the next way to carry out an inquiry. As Einstein said (see above), science is not about a method, but about intuition or thinking – it's about supplying answers to questions with good evidence and good reasoning, which can be done in a variety of ways, with the principal feature of importance being the thought and analysis that goes into providing those answers.

We sometimes get too hung up, I think, on the shibboleths of science – that is, the things taken to be core to the enterprise of science, such as causation and generalisation. Einstein's point is, it is *not* these that are at the core of good scientific thinking. Rather, it's about making connections and having insights and testing these out, in whatever way. The great sociologist Howard Becker (1998: 41) put it this way when he wrote about how the connections we seek are multistranded and multidirectional, such that causality is a less than helpful concept in social science:

there are many modes of connection, for which we use words like 'influence' or 'causality' or 'dependence'. All these words point to variation. Something will vary and something else, dependent on what happens to the first thing, will undergo some change as well. The things that so vary will often influence each other in complicated ways, so that 'causality' is not really an appropriate way to talk about what we want to emphasize.

Some definitions

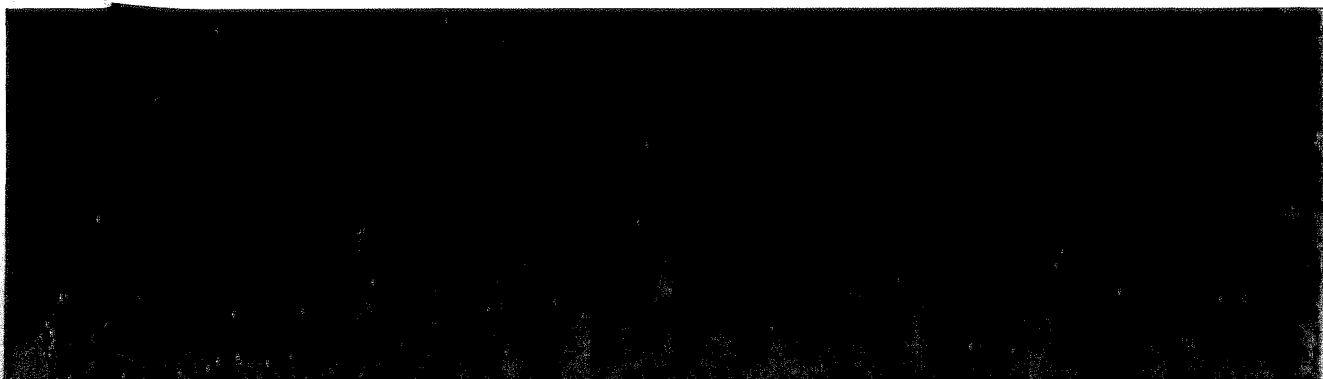
The case study is not a method in itself. Rather, it is a focus and the focus is on one thing, looked at in depth and from many angles. Bob Stake (2005: 443) puts it this way:

Case study is not a methodological choice but a choice of what is to be studied ... By whatever methods we choose to study *the case*. We could study it analytically or holistically, entirely by repeated measures or hermeneutically, organically or culturally, and by mixed methods – but we concentrate, at least for the time being, on the case.

So, you have the focus for the case and you choose methods to help you inquire into the subject. I'll discuss some of those methods in Chapters 9 and 10.

Stake goes on to emphasise the importance of the singular in the case. So, a doctor may be a case to be studied, but it is difficult to see how methods of 'doctoring' could be a case. Nor could the reasons for child neglect be considered a case, says Stake (2005: 443), since, 'We think of those topics as generalities rather than specificities. The case is a specific One' – note the upper case 'O'.

Helen Simons (2009: 21) sums this up well in her definition of the case study:



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Case study is an in-depth exploration from multiple perspectives of the complexity and uniqueness of a particular project, policy, institution, programme or system in a 'real life' context. It is research-based, inclusive of different methods and is evidence-led. The primary purpose is to generate in-depth understanding of a specific topic (as in a thesis), programme, policy, institution or system to generate knowledge and/or inform policy development, professional practice and civil or community action.

Stake (1995: xi) makes a similar point: 'Case study is the study of the particularity and complexity of a single case, coming to understand its activity within important circumstances.'

Both of these experts stress particularity and complexity and the real-life circumstances within which the research occurs. Looking at a number of definitions of the case study, Helen Simons says that what unites them is a commitment to studying the complexity that is involved in real situations and defining the case study other than by methods.

Still on the question of definition, Hammersley and Gomm (2000: 2) make the point that all research is, in a sense, case study because, 'there is always some unit, or set of units, in relation to which data are collected and/or analysed.' They suggest that the important distinction between the case study and other kinds of research is the number of cases investigated and the amount of detailed information which can therefore be collected about each one. Whereas with social surveys, for example, you find a little bit – perhaps just one set of questionnaire responses – from each of the many people participating, with a case study you are finding much, much more, but about a very limited number.

A summary of Hammersley and Gomm's table outlining the differences between the case study and two other forms of research (experiment and survey) is given in Table 1.1.

Table 1.1 A comparison of the case study with other forms of inquiry (liberally adapted from Hammersley and Gomm, 2000)

	Case study	Experiment	Survey
Investigates ...	one case or a small number of cases	a relatively large number of cases	a relatively large number of cases
Data collected and analysed about ...	a large number of features of each case	a small number of features of each case	a small number of features of each case
Study of ...	naturally occurring cases where the aim is not to control variables	cases where the aim is to control the important variables	naturally occurring cases selected to maximise the sample's representativeness of a wider population
Quantification of data ...	is not a priority	is a priority	is a priority

To Hammersley and Gomm's table I would add ...

	Case study	Experiment	Survey
Using ...	many methods and sources of data	one method	one method
Aiming to ...	look at relationships and processes	look at causation	look for generalisation

The choice of one case (or a small number) is made with a trade-off in mind, they say. You choose your very restricted sample so that you can gain greater detail, but this is at the expense of being able to make generalisations about your findings.

Like Hammersley and Gomm, Ragin (1992: 5) also offers a definition by presenting a contrast with what we might call 'variable-led research':

The ... case-oriented approach places cases, not variables, center stage. But what is a case? Comparative social science has a ready-made, conventionalized answer to this question: Boundaries around places and time periods define cases (e.g., Italy after World War II).

Ragin's definition does two things. It contrasts the emphasis on cases with an emphasis on variables in other kinds of research, but it also introduces the important idea of the *boundary*.

What is a case?

When I was writing this book, a moment of delightful serendipity occurred when, by mistake, I pressed the thesaurus button on my word processor while the cursor was stationed over 'case'. I give a summary of the meanings it came up with in Table 1.2.

Table 1.2 Meanings given for 'case'

Container	Situation	Argument
box	instance	reason
casing	event	defence
crate	occurrence	justification
suitcase	state of affairs	rationale
folder	circumstances	basis

I had thought that I knew what 'case' meant, but this serendipitous opening led me to some personal research into the meanings of 'case'. It turns out (if my less-than-scrupulously-thorough research is correct) that

the rather different meanings of 'case' arise because the word is taken from two similar-sounding Latin roots that have arrived, after corruption, at the same word, 'case', in English. The two roots are:

- *capsa*, meaning 'box', 'container' and, thus, 'case'
- *casus*, meaning 'event', 'fall', 'accident' and, thus, 'case'.

Kelly (1993: 35) notes that the *Oxford Latin Dictionary* lists 11 different meanings for the word 'case', suggesting that, 'the term was just as confusing in Latin as in English'.

Anyway, this personal etymological quest led me to realise that the use of 'case' in 'case study' ticked two important boxes – by good luck, probably, rather than good planning – about the essence of this kind of inquiry. Quite fortuitously, therefore, it is an entirely appropriate term for what is done in a case inquiry. Let's look at these two meanings, plus the other one identified by my computer's thesaurus.

The case as container

The first column in Table 1.2, headed 'Container', is about the physical, concrete meaning of 'case'. While I hadn't at first considered that this was relevant to the case study, interestingly it emphasises the *containment* of a case study. A case, in the example of a crate or suitcase or even a pencil case, is 'bounded': you close it and clip the latches and that's it. The case – think 'suitcase' – is everything that is in it: T-shirts, jeans, socks, underwear, washbag, flip-flops, sunglasses, electric shaver (but not if you're a lady, obviously).

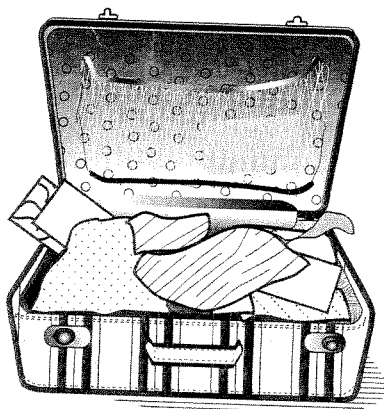


Figure 1.2 The case as a container

At the risk of going from the ridiculous to the sublime, this is, in fact, similar to something the great philosopher Ludwig Wittgenstein said in the opening line of his famous *Tractatus*: 'The world is everything that is the case' (see Kenny, 1993: 3). Wittgenstein's language is sometimes thought to be a little mysterious, enigmatic or even poetic – as if he were teasing his merely mortal readers with riddles, saying, 'I'm not giving you answers, peasants: go away and think about it.'

Wittgenstein's enigmatic statement is relevant to the case study. Wittgenstein scholars (see Biletzki, 2009) have understood the great philosopher's 'The world is everything that is the case' to mean that the world in which we are interested, as inquirers, is composed of facts and states of affairs and objects, all of which are in a constant interrelationship

with one another. Like zillions of atoms in a box, they bounce into one another, they fit or don't fit with each other and intermesh in myriad ways, and the result is inherently highly complex and could have taken any one of a number of other forms in our box. This is, in a sense, what the case study is about: the focus is on the complexity. That is, we have a state of affairs bounded by the case (think 'suitcase' again) and we study the complexity of what is in there.

So, there we are: the case is like a suitcase or possibly a wrapper.

The case as situation, event

Here, 'case' means a particular instance, an event, a happening, and the set of circumstances that surround this. So, this is different from the bounded (suit)case since it is less about the parameters that define the extent of the subject and more about a set of conditions or a state of affairs.

This, too, very nicely defines what happens when a case study is undertaken. A case study is about a set of circumstances in its completeness and the case is described – marked out – by those circumstances. It is the circumstances of the instance that are being studied. Where did it happen? When? What had happened before? Who was around? What was in the news? How did all of this affect what was going on and how events turned out?

An important feature of the case here is that, as Mitchell (2006: 31) puts it, there is, in ordinary use of English, 'a strong connotation that the word "case" implies a chance or haphazard occurrence' and this haphazardness entails specificity or particularity. This is an important point to make, for, as I discuss in Chapter 4 regarding 'samples', there can be no assumption that the case is in any way representative of a wider whole – it is a one-off, defined by the peculiar circumstances that you, the researcher, describe.

The case as argument

The clue to the meaning of 'case' here is in statements such as 'The case for ... is as follows ...' or, 'Here is the case for the prosecution'. 'Case' is thus now being taken to mean a rationale, an argument.

This is not, if I am honest, likely to be relevant to the origins of the term 'case study'; nor is it likely to have been influential in the term's take-up as describing a form of inquiry. It is interestingly relevant, however, since a case study involves the rationale of one thing relating to another or possibly causing another. It involves your train of reasoning about the interrelationships between the elements of your study. It involves you justifying your reasoning and the conclusions with which you emerge, using evidence drawn from your empirical work. It is essentially about the arguments that you make to connect the elements of

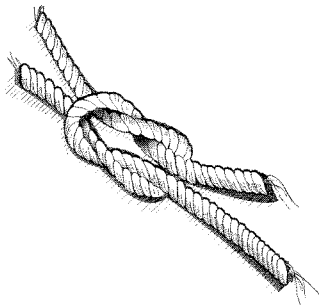


Figure 1.3 Your arguments are the fibres that hold your study together

your observations. The arguments that you pose are like the fibres and ropes that hold all of the disparate elements together (see Figure 1.3).

What is and is not a case?

I noted earlier Stake's distinction between a study of a *doctor*, who could form the subject of a case study, and *doctoring*, which could not. On his or her own, though, even the doctor would not be a case – at least, not a very interesting or informative one. To be a case – an interesting one – we would have to be able to say that the doctor was a case of something (you should have an idea of what he or she is a case of at the outset). It will then be this analytical focus that crystallises, thickens or develops as the study proceeds. Indeed, it's the way this emerges, grows and develops that is at the heart of the study.

So, the Korean War would not be a case unless it was a case of something. Is it a case of a war? If so, can you say that it is a case of an especially remarkable or unusual *kind* of war? Perhaps it may be, by contrast, essentially a case of a border dispute, but one that has blown up out of proportion. Is it a case of US resistance to what was assumed to be communist expansion? Just chronicling the story of the Korean War would not be a case study. An exploration of any one of these 'ofs' – any of these analytical backdrops – however, *would* make a fascinating case study.

Wieviorka (1992: 159) explains the distinctions I have drawn in my Korean War example by noting that when we talk about a case we, in fact, are talking about two elements:

- 1 what he calls a 'practical, historical unity' (the Korean War in my example) – this is, in essence, the *subject*
- 2 what he calls the 'theoretical, scientific basis' of the case (such as US resistance to communist expansion, in my example) – this is the *analytical frame* or we could call it the *object*.

So, just extending the war theme for a moment, we could not have a case study entitled 'World War II: a case study'. It is not a case study of anything. 'World War II: a case study of a "just war"', however, would contain both of the elements outlined by Wieviorka. It would make an interesting piece of work since it could examine in detail the notion of a just war, played out and exemplified through the case of World War II.

Case study research comprises two parts:

- 1 a subject
- 2 an analytical frame or object.

Alternatively, take the example of a hospital ward. Would a description of this ward constitute a case study? In my opinion, it would not – it's not a case *of* anything. It becomes a case *of* something, as I explain in more detail in Chapter 5, when you can explain the analytical frame through which you might be viewing it. It might:

- be a good example of that analytical frame
- demonstrate something interesting in terms of your analysis because of its peculiarity
- be an example of an analytical focus that arises by virtue of your personal experience.

As I indicate in Table 1.3, the analytical focus must extend beyond mere description.

Table 1.3 What is and is *not* a case?

Subject (the 'practical unity')	What would <i>not</i> (on its own) constitute a case study?	What would be a case study?
Jesson Ward at Parktown Children's Hospital	a simple description of the ward	an analysis of why it is thought to be an outstanding children's ward
Editorials in the <i>Daily Globe</i> for the six days in week beginning 5 March	a content analysis of each editorial	a case study analysis of a newspaper proprietor's influence over the content of editorials
Aleksandr the meerkat	the rise of this advertising phenomenon	a case study analysis of the exemplary use of personality and a storyline in advertising

So, a case study is like one of those capsules with two halves: each half, each ingredient, is necessary in order for the other half to work, as shown in Figure 1.4. It has to contain the 'practical, historical unity', as Wieviorka (p.159) puts it (or 'the subject' in plain English), *and* it has to contain the analytical frame (or 'object'). It is not complete without both parts in place: one will not work without the other.

Wieviorka (1992: 160) continues:

For a 'case' to exist, we must be able to identify a characteristic unit ... This unit must be observed, but it has no meaning in itself. It is significant only if an observer ... can refer it to an analytical category or theory. It does not suffice to observe a social phenomenon, historical event, or set of behaviors in order to declare them to be 'cases'. If you want to talk about a 'case', you also need the means of interpreting it or placing it in a context.

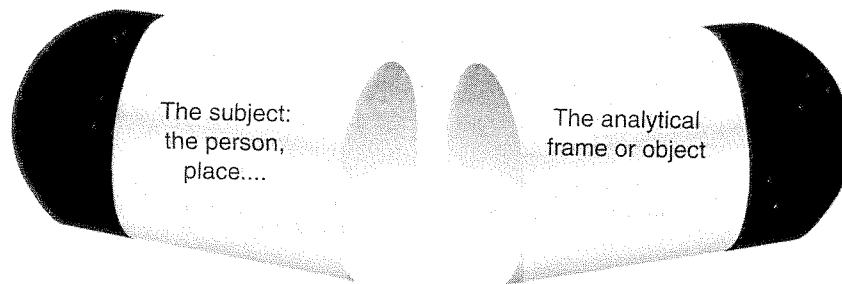


Figure 1.4 The two parts of a case study, each part needing the other

Let's take one more example and put it in the two-part capsule. Let's take as our subject the 1923 hung parliament in the UK. A narrative of this on its own is not a case study. How can we put it in the context of an analytical frame?

We first of all have to remember that we are looking at this *one* hung parliament – the 1923 one. We are looking at it as one of a set of hung parliaments. *Why* are we looking at this particular one? Perhaps it is an especially interesting one or an unusual one concerning the negotiations and the 'horse-trading' that occur among party leaders at the beginning of a hung parliament, just after the election result has been finalised and announced. This, then, would be our analytical frame. Clearly, a range of analytical frames *could* potentially accompany the subject, but we would need to choose one (or more) of these (see Figure 1.5).

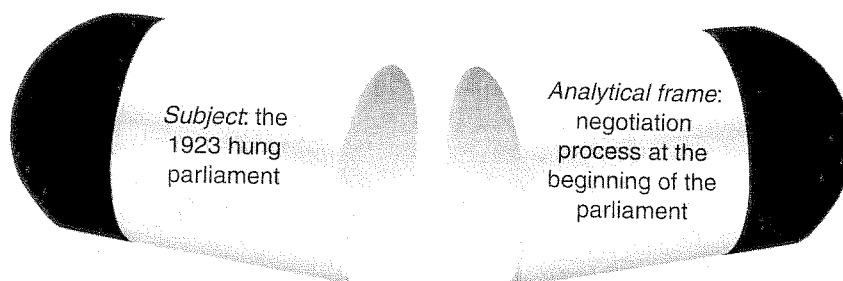


Figure 1.5 Putting the two parts together

What is the case study good and not good for?

Let's start with what the case study is *not* good for. It's not good for generalising from ...

It's not good for generalising from (yes, I said 'not')

In the history of social inquiry there have been two broad lines of thought about the way that inquiries can and should be done. One suggests that we should collect lots of data about an issue and then generalise from it. The other suggests that, with certain questions, we are better learning from a specific example. As I shall discuss in Chapter 3, the ancestry for those lines of thought is as old as the Athenian hills. As far as I am concerned, both of these forms of inquiry are perfectly legitimate for their own purposes, but it is important not to get them mixed up. We cannot, for example, come up with potential reasons for Billy hitting Ryan repeatedly by doing an experiment – at least not in the formal sense of the word 'experiment'. Nor can we generalise about the success of a drug after observing its success in the case of one patient.

Throughout this book I shall be making a bit of a meal of the difference between generalisation and particularisation. I do so because a misunderstanding of the purposes of research and what we can get from inquiries is at the root of many a wild goose chase by students and other first-time researchers.

The assumption made by some is that *all* research has to be governed by identical ground rules – ones that stress sampling, generalisation and induction. Induction is the process of deriving a general principle on the basis of many observations gathered from experience.

I feel that I need to make this point so emphatically and repeatedly because students often misunderstand the purpose of the case study. The case study is about one thing, as I keep saying, but the unitary nature of the thing we are studying is unsatisfactory in itself for it to be called a case study. The work needs to have some purpose and direction behind it and the purpose and direction comes from the analytical frame I have just discussed (see Figure 1.4).

Let's imagine Alex and Zac are horticultural (that is, gardening) researchers at an horticultural college – one that specialises in breeding roses.

One morning, on their routine daily tour of the glasshouses, Zac finds a stunning new white rose; the buds having just burst into flower. The flowers are fabulous; they're almost luminous. He bends to sniff one and immediately swoons as its perfume is hypnotic.

Zac cuts the stem from the plant (don't worry, there are plenty more buds) and runs to Alex, calling: 'Hey, Alex, come and see this!' As he reaches her, he holds out the rose for her to smell. She nearly faints as she takes in the perfume – Zac has to hold out a hand to support her.

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Here, Zac is offering Alex the rose as an example of a new and particularly beautiful rose. (It will certainly become a garden classic.)

Now, if you will, replace Alex and Zac in your mind with Paula, who is a lecturer in plant science in the botany department of Gotham University. Quentin is among a group of students listening to Paula give a lecture.

Paula holds up a white rose to the class. The point she wants to make is that roses are not alone in the family *Rosaceae* – they are but one particular genus, *Rosa*, of the family *Rosaceae*, which also contains blackberries, apples and strawberries (yes, extraordinary, but true).

In both of these cases, the roses are offered for a purpose, but *not* as examples from which we can generalise. Because on each occasion it is *one* rose, we can make no generalisations about it. We cannot look at each rose and say, 'All roses are white'. Nor can we look at it and say, 'All roses are perfumed' or 'All roses have thorns'. It would be wrong, in other words, to generalise from this one rose. In each case, the rose is serving a particular exemplary function of an analytical category.

I'll come back to this in a moment (under the next heading 'It's good for uniqueness'), but before I do so I'd just like to spend a moment looking at a real-life example that I think shows why we should restrict the conclusions we draw from case studies. The case study has a fine history in medical education, particularly in psychiatry, where Sigmund Freud unpacked many of his new ideas through descriptions of cases. Freud, however, does something inappropriate, in my opinion, in one of his more famous cases and this serves as a good example of what should *not* be done with a case study.

The case was of a young woman to whom he gave the pseudonym Anna O. She had developed a set of neurotic behaviours, including a repeated paralysis, an inability to speak and a fear of drinking water. Of this latter characteristic, Freud (1957/2009) writes:

It was in the summer during a period of extreme heat, and the patient was suffering very badly from thirst; for, without being able to account for it in any way, she suddenly found it impossible to drink. She would take up the glass of water that she longed for, but as soon as it touched her lips she would push it away like someone suffering from hydrophobia. As she did this, she was obviously in an absence for a couple of seconds. She lived only on fruit, such as melons, etc., so as to lessen her tormenting thirst. This had lasted for some six weeks, when one day during hypnosis she grumbled about her English 'lady-companion', whom she did not care for, and went on to describe, with every sign of disgust, how she had once gone into this lady's room and how her little dog – horrid creature! – had drunk out of a glass there. The patient had said nothing, as she had wanted to be polite. After giving further energetic expression to the anger she had held back, she asked for something to drink, drank a large quantity of water without any difficulty, and awoke from her hypnosis with the glass at her lips; and thereupon the disturbance vanished, never to return.

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The idea in the popular mind that personal acknowledgement and acceptance assist in the process of 'healing' is linked to many and varied contributory ideas stemming from psychoanalysis. Freud concludes:

Ladies and Gentlemen, if I may be allowed to generalize – which is unavoidable in so condensed an account as this – I should like to formulate what we have learned so far as follows: *our hysterical patients suffer from reminiscences*. Their symptoms are residues and mnemonic symbols of particular (traumatic) experiences.

Freud asks here if he 'may be allowed to generalize'. Well, no, sorry Sigmund, you can't generalise. You can't generalise – at least not in a useful way – from this case study. We can certainly take inferences and interpret these in the context of our personal knowledge and that which comes by virtue of our reading. It is wrong, however, to generalise and conclude, as Freud is encouraging us to do here, that reminiscence is the basis of an hysterical reaction. How could this be a useful generalisation? You or I would not develop a fear of drinking water if, like Anna O, we had witnessed a friend's dog drinking from a glass (well, I wouldn't – I would think it was rather sweet, though you may be a more sensitive soul).

No, Anna O's reaction was in the context of the era in which the episode happened and Anna's own history, which, of course, Freud went on to analyse in some detail. His analysis in these terms is as particular and as positioned as it is fascinating. However fascinating, though, Freud was not right to generalise from this one case to others and we can now appreciate, as twenty-first-century readers why this is so, why it was incorrect to generalise.

We can appreciate that such a reaction was not a function simply of the individual psychological history of this patient, but also of the social, moral and political context of the age. It was a time, in our terms now, of heightened sensitivity to what would have been considered vulgarity. If Anna O could be taken in a time machine to watch one of today's stand-up comedians, she would not just go off her water, she would have a fit of the vapours and probably never recover.

It's good for uniqueness

Going back to the rose, what would have been the purpose of offering the rose as a single case? For Zac the horticulturalist, its uniqueness would be defined by its beauty and fragrance, placing it in a special position in relation to other roses. For Paula, the botanist, its uniqueness is defined by its exemplification of the unique genus, *Rosa*, of the family *Rosaceae*. The list of potential uniquenesses could go on, as shown in Table 1.4.

The point I am trying to make is that, if we are using the rose as an object for an inquiry, we are looking at its particular features in their

Table 1.4 One thing, one unique story

	Unique relative to what?	How might a case study, in showing the rose in its completeness, be interesting or instructive?
As a rose	All other garden plants	Showing how the rose is different from other garden plants, such as in repeat flowering Showing how it is particularly susceptible to disease, compared with others
As a white rose	All other roses when used as emblems	A study of the significance of the white rose and its symbolic place in the history of different nations – in the Wars of the Roses, for example. Its cultivation and its popularity following this emblematic use
As a fragrant rose	All other roses	A case study of this rose and its selective breeding vis-à-vis fragrance. Its particular features that mark it out as an example of genetic mutation, including a discussion of the special difficulties in getting this variety to graft to the usual rootstocks

variety and their completeness. We are, in other words, seeing the rose in terms of broader analytical categories, for, to quote from Wieviorka (1992: 160) again, the 'case' in itself (the rose) has no meaning in itself: 'It is significant only if an observer ... can refer it to an analytical category'. Wieviorka continues:

If you want to talk about a 'case', you also need the means of interpreting it or placing it in a context ... Regardless of the practical approach for studying it, a case is an opportunity of relating facts and concepts, reality and hypotheses. But do not make the mistake of thinking that it is, in itself, a concept. A case draws its unity not from the theoretical tools used to analyze it, but from the way it takes shape. (1992: 160)

Ragin (1992) makes an interesting point here, drawing from a seminar that he organised with Howard Becker – author of the celebrated case study *Boys in White* (1961/1980). He says that Becker would continually pull the rug from under people's feet at the seminar by saying that cases should not be defined at the outset but, rather, *emerge* as the inquiry progresses. We should always be asking, Becker is suggesting, 'What is this a case of?' We should ask this question again and again, Becker advises, as our evidence accumulates around potential explanations (or 'theories'). The 'object' is then the explanation.

It's interesting to look at Becker's insight in the case of the three approaches to the rose set out in Table 1.4 because it puts the emphasis on the analytical part of the study – the explanation. It is the explanation and the theorising that are key in foci (roses in this case) that are quite similar.

A rich picture – with boundaries

A case study will not tell you the kinds of things that an experiment will tell you about causation (although you may choose to use an experiment as part of a case study and this is covered in Chapter 7). What it does offer you is a rich picture with many kinds of insights coming from different angles, from different kinds of information. So, you may go into your case study and do interviews, make observations, keep a diary, look at statistics. Nothing is ruled out. The case study is a frame that offers a boundary to your research. Equally, you could think of the case study as representing the end of a searchlight beam. Everything at the end of the beam is seen in bright light and thrown into sharp relief, shadows and all. You study what is in the beam of light and your subject of interest in it is, 'What happened here? How did it happen? What was connected to what? Why did it happen?'

The end of a beam of light has a boundary, an edge, just as a case study has a boundary. Your case study is defined not so much by the methods that you are using to do the study, but the edges you put around your case – the direction and extent to which you want your search to go. You will define these at the outset. Perhaps you will say, 'I want to look at how IKEA grew to be the biggest furniture retailer in the world'. In making this examination you will be defining the subject of your case study by its growth and growth implies development over time. Time implies history, with a beginning, middle and end.

Let's look at this particular example, defined by these parameters.

In this case study your principal theme of interest, we have just decided, will be growth. This case study will be focusing on change over time, beginning perhaps with a reconnaissance of the available statistics, to establish the grounds for your case. You will want to establish that IKEA really *is* the biggest furniture retailer in the world by comparing it with other retailers. You will want to show how it has grown over time, examining its turnover and profit figures since its creation by Ingvar Kamprad in Sweden in 1943. What does the graph of turnover figures look like over the years? Alternatively, you could show, on a series of maps, each representing a ten-year period, the countries in which IKEA stores have existed. You may be able to find out the figures you need quite easily or you may need to go to some quite obscure sources.

This gathering and plotting of basic data on turnover and profit over the years since 1943 provides the *boundary* for your case study. It says, 'This has happened, IKEA really is the biggest and it has become the biggest in a period of less than 50 years'.

This, though, is not enough for a case study. Simply to show the growth over time is not sufficient. You have the boundaries defined by the growth, but now you want to know how and why the growth has happened and, while you will not be able to provide a definitive answer, the case study should provide you

(Continued)

(Continued)

with enough evidence to make a convincing argument. You will need to click together bits of information like pieces in a jigsaw, bits of evidence that you collect in support of any one of a number of tentative ideas – sometimes called hypotheses or theories – about the growth of the company.

If you are thinking about the growth of the company, what might be plausible ideas to explain its growth? Let's have a look at a few.

The company may have grown because its founder had one big idea or a series of good ideas, such as realising the:

- potential of sawdust and chipboard for making cheap furniture
- willingness of consumers to build their own furniture from a kit if the price and design are right
- ability and willingness of consumers to carry away their own flat-packed furniture with the advent of mass car ownership.

If any of these were important, where did Kamprad's ideas come from? Did he work in a sawmill and notice the abundance of sawdust? Did he work in retailing and realise the wastage involved in 'transporting air' in assembled furniture? Was it, instead, down to the retailing technique employed in IKEA stores, wherein consumers are encouraged to walk along a certain route through the store, being encouraged to buy as they make their extended and unexpected walk? (How many times have I come away from IKEA with something like a never-to-be-used Benthölme drawer tidy precisely because I was steered along such a route?)

How could you go about finding out which of these potential ideas was important? Clearly the ideal would be an interview with Ingvar Kamprad, to ask him about your tentative theories, but I doubt if he gives many interviews. You will almost certainly have to rely on printed biographies of him. The Wikipedia entry for him, for example, tells us that in 1976 he wrote a manifesto detailing the IKEA concept of frugality and enthusiasm. We learn that he also worked with Swedish journalist Bertil Torekull on the book *Leading by Design: The IKEA Story* (HarperCollins, 2000). In the autobiographical account, he further describes his philosophies and the trials and triumphs of the founding of IKEA. Also, that he began to make a lot of money when he was a child by buying matches cheaply in bulk and then reselling them at a profit. He expanded to fish, ballpoint pens and pencils.

Already we can see that he seems to be one of those people who almost can't help but make money. It's not as if he had an especially good idea about furniture or an especially wonderful insight into how it could be mass produced. Rather, the seed seems to have come from his dynamism in finding ways to sell. Alongside this, he apparently drives a 15-year-old Volvo, flies only economy class and encourages IKEA employees to always write on both sides of a piece of paper. An obsession with economy, paring expenses down to the very last little bit, then also comes through.

This is the kind of emerging information on which the narrative in a case study is built. The analysis begins to 'thicken' in this way around one theme.

If you take only one thing from this chapter,
take this ...

A case study is about seeing something in its completeness, looking at it from many angles. This is good science. In fact it is the *essence* of good science.

Although we can not generalise from a case study, generalisation is not always what is wanted from the inquiry process. We don't always want or need to generalise and some of the most inspired and insightful research, of any kind, has come about as the result of case studies.

What the case study is especially good for is getting a rich picture and gaining analytical insights from it. The 'analytical' bit is important: each study has a subject of interest (a person, place, event or phenomenon) and an analytical frame within which it is studied.

For these reasons, the definition that I shall adopt is as follows:

Case studies are analyses of persons, events, decisions, periods, projects, policies, institutions or other systems which are studied holistically by one or more methods. The case that is the subject of the inquiry will be an instance of a class of phenomena that provides an analytical frame – an object – within which the study is conducted and which the case illuminates and explicates.

Further Reading

Bailey, M.T. (1992) 'Do physicists use case studies? Thoughts on public administration research', *Public Administration Review*, 52 (1): 47–55.

A widely cited article that provides an interesting contrast to the stance I have taken in this chapter, in that it defends case study research as meeting scientific criteria of generalisability, transferability and replicability.

Campbell, Donald (1975) "'Degrees of freedom" and the case study', *Comparative Political Studies*, 8 (2): 178–93.

Classic paper in which Campbell – one of the most respected proponents of experimental method – comes round to seeing the value of the case study.

Cohen, M.F. (2007) *An Introduction to Logic and Scientific Method*. New York: Harcourt, Brace.

Originally published (with Ernest Nagel) in 1934, this is a magnificent, if slightly dated, outline of what constitutes science. It is good on the duty of doubt in science. See especially Chapters 2 and 12.

Gomm, R., Hammersley, M. and Foster, P. (eds) (2000) *Case Study Method*. London: Sage.

This book offers, as the editors put it, the most influential and important articles on the case study. See especially the introduction by Hammersley and Gomm for an excellent overview.

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Hammersley, M. (1992) *What's wrong with ethnography?* London: Routledge.
See Chapter 11, 'So, what are case studies?' By comparing the case study with surveys and experiments, Hammersley aims to show that these methods are not as different from each other as we sometimes assume and the differences that do exist between these ways of structuring research hinge on *case selection*.

Patrick, J. (1973) *A Glasgow Gang Observed*, London: Methuen.
This is a book I never tire of recommending. Like Becker et al.'s *Boys in White* (1961/1980) it is as good as a novel, describing the author's infiltration of a gang of young men in Glasgow.

Ragin, C.C. and Becker, H.S. (eds) (1992) *What is a Case?* Cambridge: Cambridge University Press.
Many excellent articles are included here. See especially Ragin's Introduction.

Simons, H. (2009) *Case Study Research in Practice*. London: Sage.
An excellent, practical book on conducting a sociologically framed case study. Taken from the point of view of a student making her way through the process.

Stake, R.E. (1995) *The Art of Case Study Research*. Thousand Oaks, CA: Sage.

Stake, R.E. (2005) 'Qualitative case studies', in N.K. Denzin and Y.S. Lincoln (eds) *The Sage Handbook of Qualitative Research* (3rd edn). Thousand Oaks, CA: Sage.

Stake is one of the doyens of the case study method and his work is well worth reading.

2011

How to do Your Case Study

A guide for students and
researchers

Gary Thomas

(CH. 3)

 SAGE

Los Angeles | London | New Delhi
Singapore | Washington DC

Models of the whole

Why am I reading this chapter?

There are two chapters in this book that are not 'How to' chapters. This is the first of them (Chapter 12 is the other one) and it doesn't really give practical advice, if I'm honest. What it does do is offer an intellectual history of the case study – a pedigree, showing its scholarly ancestors. This may not help much with the nuts and bolts of doing your case study research, but it will help, if you can bear to read it, in understanding where the case study has come from and where it currently stands as an approach to inquiry. You do need to know this if you want to get a good mark for your research project. More importantly, you need to know it if you want to understand why the case study is appropriate and meaningful for your work. One of the important chapters in your thesis will be your methodology chapter and if you understand what is in this chapter (that is, this chapter in *this* book), you will write a better methodology chapter in your own dissertation or thesis ... I promise.

Looking at the whole represents something of a new – or at least a reawakened – worldview (a *Weltanschauung*, in the jargon). It is one that has emerged during the twentieth century with the purpose of understanding phenomena not as so many disconnected parts but, rather, as interconnected elements. Anyone wanting to do a case study implicitly shares this worldview, though it should be said that the case study does not boast an intellectual allegiance to any one strand of thought but many. I shall review some of these in this chapter.

Break things down or see them as wholes?

That the case study is not superglued on to one of these schools of thought may make it seem like a bit of an intellectual orphan, which is part of its problem as it attempts to establish its credibility. While it may not adhere to a particular stream of thought or school of thinking, its emphasis on the whole – the holistic – puts it in some respected

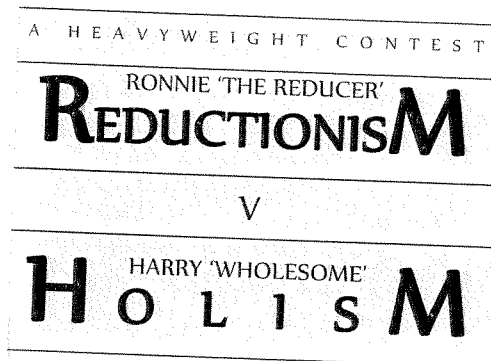


Figure 3.1 Reductionism and holism: each has its own advantages

terms of their constituent parts. It has, in other words, been about reduction, so is known to philosophers as *reductionism*.

Reductionism has done wonders in the natural sciences and the technology that springs from it – people on the Moon, refrigeration, antibiotics, computers, to name but ... er ... four. Viewing the world from this position, phenomena are nothing more than the sum of their parts.

The starting point taken in the case study, by contrast, is that certain phenomena are *more* than the sum of their parts and have to be understood as a whole, rather than as a set of interrelating variables. How has this holistic position fared next to the super-successful reductionism? Also, given that reductionism has been so successful, why would anyone want to study things differently?

This chapter is really about the attempt to view and analyse phenomena as *more* than the sum of their parts and about the intellectual traditions, mainly in the twentieth century rather than before, that have competed against the highly productive reductionist tradition. This has, of course, been a bit of a David versus Goliath contest. There are a number of themed but unrelated elements to this understanding of things-as-a-whole that I shall look at briefly – each in its own different way about the importance of wholeness in inquiry.

The issue is more complex than simply seeing the world as indivisibly complete *versus* the world as necessarily divisible and reducible. Associated with the assumptions being made here are beliefs about the purpose of inquiry. In other words, why are we doing the research?

Some have said that the purpose of research is to develop laws and theories with which we can explain the world and predict what is likely to happen next. Others have said that this is fine and dandy in understanding the things of the natural sciences (and it has got us to the Moon and so on, as I mentioned before), but it is less adequate in understanding the things of the social world. Here, reductionism is of less value.

The issue about whether or not reductionism is of value is as old as the hills – the Athenian hills. Plato, Socrates and Aristotle had major

scholarly company. All of this company shares a wish to see things as wholes.

The wish to see things more as wholes does present a number of challenges, however, since the thrust of most scientific inquiry since the Enlightenment (that is, since the beginnings of rational and scientific thought in the mid-seventeenth century) has been about breaking things down. It has been about reducing them and understanding them in

disagreements about this. It was pistols at dawn. Well, it would have been, except that pistols weren't to be invented for another 2000 years, so they probably had naked wrestling. Not a pretty sight, but, if your imagination will allow you, I'd like you to imagine the ring. In the blue corner, will you welcome Mr Plato, from Athens, who thinks that universal truths developed from generalisation are the bee's knees – they are what we should all be searching for if we want true knowledge. With his friend Mr Socrates, also from Athens, he says, 'I am looking for the "*simile in multis*".' He is looking, in other words for the *essence* that captures the truth evinced by many cases – for example, I've only ever seen white swans, so *all* swans must be white.

And ... in the red corner, will you welcome Mr Aristotle, also from Athens – clearly a popular place for budding intellectuals. Mr Aristotle disagrees with Messrs Plato and Socrates on the value of *simile in multis*. For many kinds of knowledge, says Mr Aristotle, we progress only by using our practical reasoning, craft knowledge or tacit knowing – the stuff we know because of our experience. It's the stuff we learn 'on the job' or by simply being alive and it is explicable only in terms of the particular – the case. It cannot be communicated by reducing it to general principles. 'You are wrong, Mr Socrates and Mr Plato,' says Mr Aristotle, 'to dismiss the value of cases in the production of knowledge.' (I made up this quote, by the way, using artistic licence, but he *might* have said it.) Seconds out ... and let's have a good clean fight, boys.

The punch-up on this theme continues to the present day. It proceeded through into Base Camp 1 of the Renaissance, with Francis Bacon – the first scientist, some would say – averring that the generalisers and regularisers, the followers of Plato, 'hasted to their theories and dogmaticals, and were imperious and scornful toward particulars.' Today, the argument has unfortunately come to be known as the 'paradigm wars' (Oakley, 1999) and shows little sign of abating. Any argument that goes on for 2500 years has clearly got legs and we are not going to resolve it any time soon.

It was the occupants of the blue corner, Plato and Socrates, whose ideas won (broadly speaking) and whose thinking took hold so successfully for the natural sciences. Poor old Aristotle sat dejected in the red corner, disconsolately staring at his feet and licking his wounds. This was a big defeat and a big loss for our ways of understanding, asserts an astute commentator on these issues, Bent Flyvbjerg (2001). Flyvbjerg says that the mistake led humankind on two millennia of false starts in understanding social phenomena. The mistake was in the failure to distinguish between different kinds of inquiry for different purposes and it leads us, in extremis, to the absurd position that it is inappropriate to argue or learn from particular examples for fear that this might be thought 'anecdotal' and, therefore, unscientific.

The distinction, then, has been on a long journey intellectually. In essence, it is about the relevance of particular, individual events to a larger picture and the extent to which we can use separate events to

establish laws or theories by which we can explain and predict. Some say that it's legitimate to generalise in the natural sciences, but not in the social sciences. In the former, physics and chemistry, the stability of the events studied is such that laws and theories can be worked out in a way that they will prove useful for explanation and prediction. In the human sciences, however, events follow such random twists and turns, any attempt at establishing stable laws and theories that will reliably explain and predict is meaningless.

The classic argument here has been posed by philosopher Alasdair MacIntyre (1985) in his book *After Virtue*. If you are interested, read Chapter 8 in that book, 'The character of generalizations in social science and their lack of predictive power'. It's as entertaining as it is enlightening.

I've said that this argument about reductionism versus holism has been going on for 2500 years and reductionism has certainly won as far as the main method used for the natural sciences is concerned. In fact, it's only in the last 100 years or so that protagonists of Aristotle's views have come to the fore again in thinking about the human sciences.

Earlier, right at the beginning of the book, I mentioned the idiograph – a drawing or figure that stands not for a sound, as do symbols in the Roman alphabet, but for an idea, from the Greek word *idea*, meaning (unsurprisingly) 'idea' or 'own' or 'private', and *gramma*, meaning 'drawing'. It was the philosopher Wilhelm Windelband who, at the cusp of the nineteenth and twentieth centuries drew on the notion of the idiograph to separate what he called the 'idiographic' and the 'nomothetic' (from the Greek *nomos*, meaning 'law') in social inquiry.

I personally think that it would have made life easier if Windelband had called the 'ideographic' the 'pictographic' since 'pictograph' is a perfectly acceptable alternative to 'idiograph' and it more straightforwardly explains what he was trying to get at. A pictogram is a small picture that has become stylised, so that it represents the thing in written form. The Chinese pictogram for 'person', for example, looks a little like a matchstick person, with a trunk and two 'legs'. (Interestingly, these picture representations eventually take on sound-like representations as well, developing into our modern system of alphabetic writing.)

With the idiographic, the approach is to specify and study individual phenomena in detail – we have a 'picture' in front of us. With the nomothetic, by contrast, the approach is to generalise from many cases and derive laws from such generalisation.

Windelband was not the first to notice this difference but, as is so often the case in the history of ideas, he encapsulated something nicely in his separation of the idiographic from the nomothetic and the distinction he

Holism is seeing things as a whole. (Why hasn't it got a 'w'? Don't ask me.) There is an assumption that the whole is more than the sum of its parts. It's about seeing things in their entirety, their totality.

drew has stuck. It has proved enormously helpful in summarising the differences between these kinds of inquiry.

Gestalt psychology

Not long after Windelband, one interesting avenue down which this discussion travelled was that of Gestalt psychology early in the twentieth century. This was key for the case study since the essence of Gestalt thinking is that things should be seen in their totality, as we try to do in a case study. The key idea to emerge here was that the mind works not by perceiving things in isolation, separately, but, rather, as wholes – as integrated units. We humans, alone among animals, invest meaning in what appear to be unrelated phenomena and make patterns, make sense, out of these. It is almost as if our minds are patternmaking or sensemaking machines.

The key figures here – psychologists such as Max Wertheimer and Wolfgang Köhler (and, later, the great social psychologist Kurt Lewin, the inventor of action research) – suggested that the methods psychologists use to study psychological phenomena distort our understanding of them. By always reducing, atomising the subject matter of psychology into constituent variables, the end result is a misunderstanding of the way the mind works, they said. The mind works not in the convenient building blocks of psychological science – variables such as gender, age, reaction speed, time, class – but *wholes*.

It is certainly true that *Homo sapiens* is a pattern-finding animal. The fertility of our brains in divining patterns is boundless – it requires only the mildest encouragement for shape and form to crystallise out of what may seem at first to be chaotic stimuli. Look at Figure 3.2. After looking for a while, you will see a Dalmatian dog. (Can't see it? Its nose is in the middle of the picture and its backside towards the top right.) In fact, perhaps dangerously for certain kinds of inquiry, we even seem to be predisposed to preferring to see form where there is none rather than not see any form at all.



Figure 3.2 We have a wonderful capacity for seeing shapes and making meaning

Philosopher Karl Popper suggested that this facility may be inborn. It may be due, he proposed (1977: 270), to 'mechanisms which make us search for regularities' and be responsible for what he called 'the dogmatic [ouch] way of thinking'. In a similar vein, Barrow (1997) points out that the human brain has an evolutionary imperative to *over* see patterns – that is, occasionally see patterns where they don't exist (as 'false positives' – for instance, 'seeing' slender shadows as tiger stripes) as this has historically given us an evolutionary advantage over the opposite state of not recognising patterns where they in fact exist (where the slender shadows are actually the stripes of a tiger).

The bottom line is that we must be cautious about this precious ability to see pattern since, today, harbouring a predilection to make shape and theory out of that which is shapeless may not be quite so useful. We may overdo generalisation, tending to generalise from insufficient information. It is this tendency that has to be guarded against in all of the sampling procedures that have been developed with what has been called probabilistic research (of which more on page 62).

If we are looking at idiographs (that is, cases) rather than representative samples, however, we are not expecting to generalise. Rather, we are expecting to employ to best effect our ready-made excellence at making sense *without* generalising. It's back to the naked wrestling again and not getting the two kinds of inquiry mixed up.

No, you can't generalise from a case study, but you *can* use your ability to put things together, to draw from experience, make informed judgments about cause and effect in this particular case. It's what the human brain is good at. It's what got us out of the trees. It's what Newton, Darwin and Einstein did. We shouldn't be ashamed of it. It's about connecting ideas, like the ganglia between neurons.

There have been a number of superstructures that have been used to accommodate wholeness, intellectually speaking, and I shall look at these in the sections that follow.

Dramas, theatres and stages

All the world's a stage,

And all the men and women merely players;

They have their exits and their entrances

This quotation is from Shakespeare's *As You Like It* (1599, act 2, scene 7, line 139), the words spoken by the character Jaques, and it sums up several streams of thought about how the world – the natural as well as the social world – should be seen. So, it's worth saying a little more about the stage analogy.

In fact, the *As You Like It* quotation could perhaps be the motto of the case researcher because the ways of thinking about social situations that

I am going to review in this section are all characterised by seeing the person in context, where the action is defined by interactions between people and situations, which, of course, is the *sine qua non* of case study research. Each person is affected by the environment around them: no one is an island.

There's no better place to begin to explore this idea than with the work of sociologist Erving Goffman, who actually called his view of social life *dramaturgy*. In the dictionary definition, 'dramaturgy' is the art of writing and producing plays and, for Goffman, life is a set of dramatic performances in which people take on roles and change the way they behave depending on their interactions with other actors. I am a different person with different folk. I take on a different character with my sister Adrienne from the one I take on with my friend Peter, from the one I take on with my boss Edward. The core perhaps is the same, but if ever I were to be faced with the three of them in one room (heaven forbid) I would suffer from an acute case of triple personality implosion syndrome – or PIS³, as it is known in the psychiatric literature (that's meant to be a joke, by the way).

Interpretivists say that there is no 'objective' social world 'out there'. Rather, it is *constructed* differently by each person in each situation they face, so it is useful sometimes to see the world as a stage on which we play out characters.

Goffman differentiated between the kinds of situations in which such performances are necessary, noting that there is a 'front stage', where the 'act' is always used in order to 'impression-manage', and a 'backstage' where people can be more 'themselves'.

Goffman's dramaturgical perspective was itself an offshoot, an important one, of a branch of sociology known as symbolic interactionism. Here's how one of symbolic interactionism's main proponents, Herbert Blumer (1992: 82) presents what he calls the 'basic premises' and 'methodological consequences' of this school of sociology:

... human beings interpret or 'define' each other's actions instead of merely reacting to each other's actions. Their 'response' is not made directly to the actions of one another but instead is based on the meaning which they attach to such actions. Thus, human interaction is mediated by the use of symbols, by interpretation, or by ascertaining the meaning of one another's actions.

Symbolic interactionism is, in turn, a tributary of a wider stream of thinking known as *interpretivism*. The basic assumption here is that the social world is *constructed* by each of us differently, with words and events carrying different meanings for each person and in each situation. Interpretivism started, broadly speaking, with the American sociologist George Herbert Mead and I'll examine it in more detail in Chapter 7.

Though not directly linked, a forerunner to these lines of thought was the phenomenology of Edmund Husserl, which emerged at the beginning of the twentieth century. This is generally thought of as a philosophical rather than a sociological movement, but it is connected to the thinking of the interpretivists and the symbolic interactionists in its emphasis on meanings. Actions, Husserl would say, cannot be broken down into their constituent elements to understand them. They are understandable only in terms of the meanings that actors impose on them. They are understandable only, in other words, in terms of the wholeness of the context in which they occur.

In fact, a clutch of philosophers, and those in the infant disciplines of sociology and psychology, were saying much the same kind of thing at this time. Wilhelm Dilthey came up with the idea of *verstehen* (meaning 'understanding'), more adequately to demarcate between the stuff of the natural and the human sciences and our ways of inquiring into them. We cannot, he said, ignore the historical context and the context of meaning in all that human beings do and say.

Ecological psychology

Another manifestation of the need for wholeness is seen in ecological psychology. In its 'green' sense, ecology refers to the relationship between living things in the environments that they inhabit. It is the study of the complex interrelationships that take place in those environments.

A starting point here is that the lives of organisms inhabiting these environments are inextricably intertwined with the nature and quality of those environments. Changes in any small aspect of the environment will have knock-on effects that will affect the lives of the organisms populating it. An important aspect of this is that there will be an equilibrium in this environment and it will be maintained. If foxes become blighted, their main meal, rabbits, breed like ... er, rabbits, with few predators, so the remaining foxes do well, eating the superfluity of rabbits, so then rabbits decline, causing foxes, in turn, to decline, then rabbits breed ... and so the cycle goes on (see Figure 3.3). Thus, the equilibrium is maintained and the same happens in the environments (family, school, factory) that we inhabit as people.

So, ecological psychologists would say, people – that is, *people* ... humans, persons, *Homo sapiens* – live in a multicoloured world. It's an ecology where an increase in one stimulus invokes, not an automatic reflex or a tropism, but a thought-out reaction or an overreaction.

The difficulty is that it's not as if you could set up a meaningful research study to assess the effect of turning up the volume of music on the volume of people's conversation. Of course, you could do it physically – you could get some volunteers conversing in a room where rock

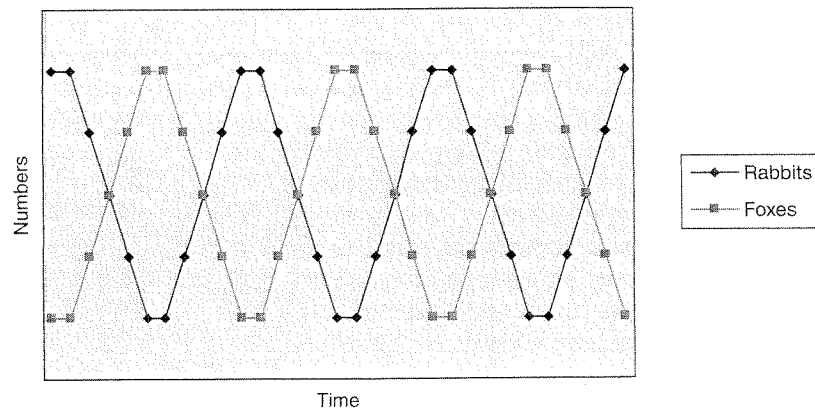


Figure 3.3 Equilibrium is maintained in an ecology

music is playing quietly and start turning up the volume and measuring the volume of the volunteers' voices. I guess that you would find that the volunteers' voice levels rose (much psychological experimentation is of this kind). An ecological psychologist would say, however, that the real world is not like this. In the real world, people would just leave the room to talk or someone would go and turn the volume down again. Then someone would go and turn it up again and perhaps there would be an argument, then maybe a fight, then someone would be taken to hospital ... and so on.

The real world, then, is complex and it is the real world in which we are interested. The understanding that ecological psychology offers is that the way people behave in a psychological laboratory, with its tight control of a small number of variables, does not remotely represent the way people behave in the real world.

The basic idea is the same as the one behind symbolic interactionism, which is that no behaviour occurs in a vacuum; it occurs in the context of others' activity, in the context of the family, place of work or school or community and the context of the linguistic, cultural, legal and physical environments. Because of the way that psychology and its inquiries typically proceed – by isolating variables, controlling important ones and manipulating one or two – we can get into a rut of thinking that they, using these controls and manipulations, are valid representations of what happens in our everyday lives. We come to think that the world in which we live is really like this – like a psychological laboratory with a simple set of variables that are all more or less inert in their relationships with one another.

The traditional focus on the individual in psychological endeavour permeates the applied field, where interventions also often take place in a vacuum, with scant consideration of the physical and social environments within which individuals learn and behave. The ecological metaphor used by some psychologists since the mid-twentieth century enables an

understanding that things are not that simple. Behaviours may emerge in an environment that are a consequence of conditions outside the narrow ambit of the individual. Changing one or two features of this system may have unanticipated consequences or may have no effect at all.

Adopting an ecological model is like using a wide-angle lens, enabling an examination of the wider situation that surrounds the target and encouraging analysis in the wider situation. In essence, an ecological view enables a recognition that people inhabit a variety of contexts, each one impinging on the other. Any attempt to oversimplify the richness of the ways in which these contexts interplay is bound to have shortcomings. An ecological view

says that it is impossible to disentangle the functioning of a constellation of phenomena simultaneously interrelating in any social arena.

It is Roger Barker (1968) who has generally been credited with introducing the notion of ecological psychology. He suggested that behaviour settings have both static and dynamic attributes. On the static side, the setting consists of one or more standing patterns of behaviour and milieu – the latter comprising elements such as house, classroom, a windy day. On the dynamic side, the setting comprises the interpersonal relationships among people and groups. Science, Barker says, attempts to keep apart the static and the dynamic, the physical and the behavioural, but their interaction is the central focus of his attention. He gives the name *synomorphs* to the settings in which these interactions occur.

Barker clearly intended his model to be useful in the real world rather than the laboratory. By taking as a starting point the integration of behaviour and environment, he intends to be able to predict real-world events and solve real-world problems. Unfortunately, as you will realise if you follow up his work, the Byzantine complexity of his model and the impenetrability of his language render his aims pretty well unachievable – his aims are on a different numbered bus from his model; in fact, the buses are going in opposite directions, one going to Moscow, the other to Timbuktu. This is a pity, since his central thesis is as powerful as it is important.

Amidst Barker's complex analysis is a continual reiteration of the influence of the physical environment and its interaction with a range of other influences, such as the ideologies of the participants, and the procedures, practices or rituals associated with certain 'synomorphs'. It is notable that Barker and others, such as Gregory Bateson (1972/1999), who promoted an ecological thrust to understanding people in their environments, had a multidisciplinary scholarly background themselves and promoted intellectual interchange with different fields.

Another important figure in this area is Kurt Lewin, whom I mentioned earlier. Lewin was a bit of a psychological polymath – brilliant

Ecological psychology is about the complexity of the psychological world. Everything is connected, so it is difficult to assess the effect of changing one variable. Things tend to stay in equilibrium.

ideas came sparking off him almost as if he were a human Van der Graaf generator. He was just a fizzing lightning ball of brilliance and, among other things (in his spare time, probably), he invented action research.

In the current context, he came up with an idea that he called 'field theory', which is linked as much to Gestalt psychology as it is to ecological psychology, but I'll mention it here. Lewin suggested the 'field' of an individual's psyche comprises a range of social and psychological factors and changes with the person's experience. An alternative term he uses for fields is 'lifespaces' – home, job, school, sports club – that we occupy at different times and in different ways. All are joined by 'vectors' that operate at different levels of strength.

Lewin, with his mercurial (and maybe slightly manic) mind, perhaps tried to make things too mathematically precise, but he offered another stratum to the developing bundle of ideas about holism.

One of the main proponents of ecological thinking was Urie Bronfenbrenner, but since he developed his model from a hybrid of ecological and systems thinking (covered next), I shall deal with the latter before going on to talk about his ideas in more detail.

Systems thinking

Systems thinking is a way of viewing the world that lends itself naturally to the case study since it aims to see the system as a whole, in much the same way that you aim to do in a case inquiry. The difference is that systems theory and its various branches are bounded by some quite firm frameworks about viewing the interrelationships that exist.

With the case study, there are no methodological prescriptions. By contrast, systems theory proffers methodological tools and route maps to assist analysis. It's a *particular* way of studying the whole, unlike the case study. That is not to say you can't use the methods of systems thinking *in* a case study – you can and they may prove to be very useful for structuring your thinking. For this reason, I go into some detail on a branch of systems thinking in Chapter 10.

Systems theory was developed in a variety of contexts, from biology to engineering, and thus can be relevant for a range of foci and adapted for use in each of these. If your interests are in engineering or biology or an applied science or if you are interested in the interrelationship between one of these and the social world, it may well be worth you exploring aspects of systems theory.

The models for understanding that have been advanced in systems thinking were developed specifically to address the need for holism in getting to grips with complex dynamics. As with the parallel trains of thought in psychology and sociology that I have just looked at, the finger is pointed at the barrenness of attempts to understand complex behaviour by reductionist methods. Our understanding, systems theorists

say, has to push that complexity to the front: we have to try to understand the ability of the complex system to adapt, change and 'learn' from experience. Systems theory used ideas from biology, such as *negative entropy* (the ability of a system to make sense, re-form itself and maintain an equilibrium), and from mechanics, such as *feedback* (the effect of a signal going back into a loop and magnifying itself each time it returns). These are characteristics of *systems* that are not seen in *individual parts* of the system.

One of the first proponents of systems theory was Ludwig von Bertalanffy (1950) with his *general systems theory*. At the risk of getting repetitive, the same theme emerges – one of complex interactions – though von Bertalanffy didn't leave it at this.

He made an important distinction between what he called open systems and closed systems. A *closed system* will be one such as an engineering system (think of a diesel engine), that has very little in the way of a change to its movement once it is working, aside perhaps from a movement of the throttle by the operator. By contrast, an *open system* is one such as a biological system (think of any living thing, even a bacterium) that seeks to maintain itself in equilibrium by responding to its environment.

Apart from features such as feedback and negative entropy, von Bertalanffy spoke about features such as *isomorphism* (that is, being the same shape) between biological and social systems. So, a beehive could be seen (from some points of view) as being a bit like a town.

His thinking led naturally to interdisciplinarity, with insights from one field providing clues for thinking about how things work in another. In fact, von Bertalanffy expected to see more of an integration of the work of science – natural and social. Sadly, there is not a huge amount of evidence that this is occurring today, despite attempts to promote it by all and sundry. He said that the human sciences were too often atomistic, they were a kind of 'social physics' of the worst kind.

Beyond all of this, systems theory has been taken forward by others in some interesting ways that may be quite useful to the case researcher. One of these is the development of *soft systems theory*, expounded by Peter Checkland (1981) and his colleagues at Lancaster University.

Checkland tried to incorporate the basic tenets of systems thinking while offering a framework for seeing the ways that the elements interconnect. He thus advanced a model that was simpler than the one promoted by Barker (see page 54), but still offered a framework of a more holistic understanding of social environments. Developing his 'soft systems' within the context of industrial psychology, he suggested the matters with which we are concerned are complex wholes that maintain themselves in a changing environment. They do this by means of adaptation and control action.

Although the concepts used in Checkland's model are borrowed from biology and engineering, he does not explicitly use the ecological metaphor, but his aims are congruent with those of the ecological psychologists.

He suggests a model that draws a distinction between the 'real world' and 'systems thinking' and forces an analysis of both. He asserts that the 'soft, messy problems of human beings in their everyday lives' are not amenable to the methods of the 'hard' systems tradition of engineering. He suggests that the hard systems tradition can handle natural (that is, biological) systems and designed systems (such as bicycles, computers, mathematics), but cannot cope in situations dominated by human perceptions. He calls the latter 'human activity systems'.

His soft systems method suggests a sequence of events and processes for analysing social settings. I describe this in detail in Chapter 10, which is on forms of analysis.

Ecological systems theory

I've already mentioned Bronfenbrenner (1979) in the context of ecological psychology, but his work is also interesting as a fusion of ecological psychology and systems thinking. Being a psychologist rather than a sociologist, Bronfenbrenner was not so fond of theatres and stages as metaphors (with which sociologists seem fascinated, if not mildly obsessed). No, rather than seeing human behaviour and activity as a drama, he saw it as an ecology. The metaphor was biological rather than theatrical (or 'performative', in sociological jargon).

Bronfenbrenner has perhaps contributed most systematically to furthering the ways in which the ecological approach can be used practically. It was he who could also be said to have been the most assertive proponent of ecological approaches, with some quite direct attacks on the more narrowly scientific approaches to psychology of his contemporaries. For him, a parallel track was not sufficient. He saw psychology's traditional methods actually doing harm to our understanding of people and he wanted to force a reappraisal of its traditions, habits and methods. It could almost be said that Bronfenbrenner was aiming for a paradigm shift in the way that psychology proceeded as a form of science.

For him, culture and society provide a set of 'instructions' for how social settings are made and he conjoined all of his ideas about context into a formal framework that he developed as a form of 'systems theory' (Bronfenbrenner and Morris, 1998). He saw ecological environments as being composed of what he called micro-, meso-, exo- and macrosystems. A *microsystem* is a pattern of activities, roles and interpersonal relations of an individual in a given setting. A *mesosystem* is a system of microsystems. An *exosystem* is a setting where the individual is not involved but events occur that affect or are affected by the individual's setting. A *macrosystem* comprises the belief systems or ideology that structure other, lower systems (see Figure 3.4).

So, taking an ecological and Bronfenbrenner-type view, if Samantha decides not to get up and go to work today, how can we explain this? Looking at the situation in just one-dimensional terms, we might see it

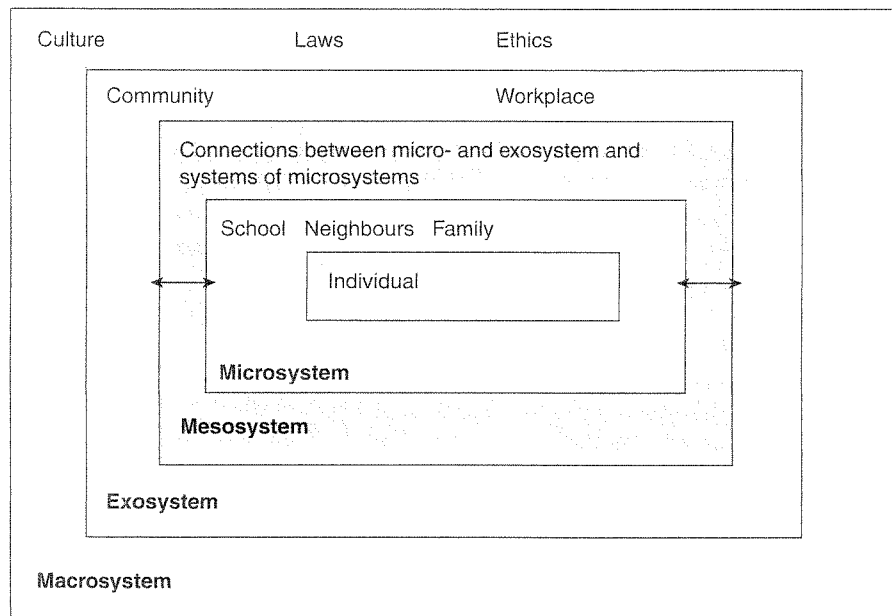


Figure 3.4 Bronfenbrenner's ecological systems

in terms of depression. Looking more widely, we might see it in terms of friendships and Sam's lack of contacts, having just moved here from Australia. Looking out even more widely, we might find a problem that she has been having at work. Even more broadly, she may be affected by an overdue tax return and a demand to her employers from the inland revenue to take tax directly from her salary, leaving her mortified with shame.

This kind of understanding is a million miles from traditional psychological inquiry and is now encouraged by a new wave of clinical psychologists such as David Smail. Indeed, in what sounds like an echo of Bronfenbrenner, Smail (1993: 13) asserts that, 'There is certainly no evidence that the wider availability of psychological theories and techniques is leading to a decrease in psychological distress.' It is the oversimplifying models that are used to approach distress that are to blame, says Smail.

Thinking particularly about children, Bronfenbrenner's comment on traditional psychological inquiry was that it sacrificed far too much to gain experimental control and led to 'the science of strange behaviour of children in strange situations with strange adults for the briefest possible periods of time (1979: 19).' The links with symbolic interactionism are manifest. There's nothing new in the world.

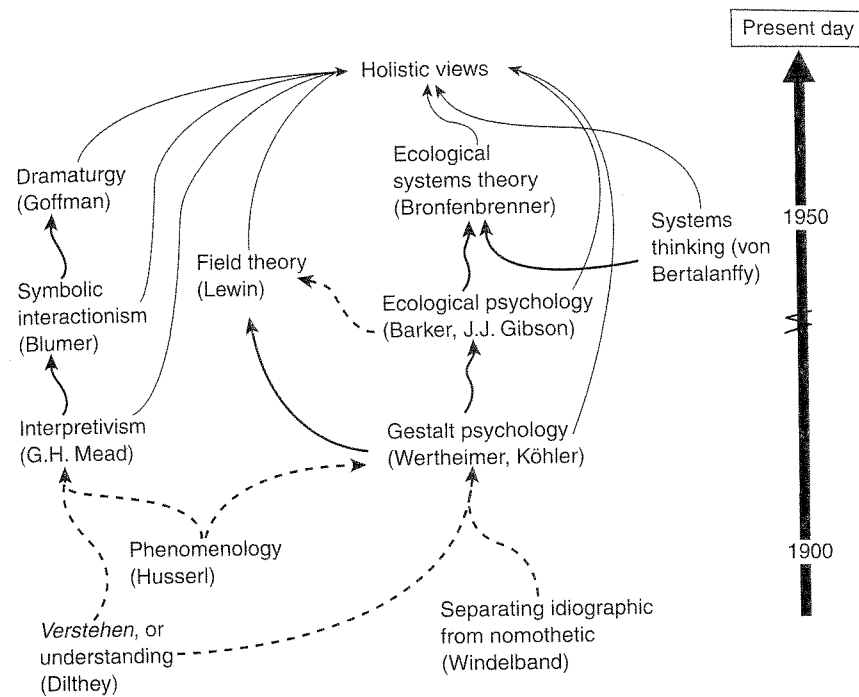


Figure 3.5 Holism and its schools of thought

If you take only one thing from this chapter,
take this ...

I hope you can see that the stress on the context and the 'wholeness' of behaviour is many stranded and is particularly suited to the case study.

Arguments underlining the need to look at the whole picture have a long intellectual history. Figure 3.5 has rather too many arrows in it, but summarises, I hope, some of the schools of thought that have led to today's more eclectic attitude to inquiry. It's an attitude that accepts the relevance and utility of the idiographic and provides ways of addressing wholeness by emphasising rather than denying the interconnectedness of the strands of psychological and social life.

Further Reading

Barker, R.G. (1968) *Ecological Psychology*. Palo Alto, CA: Stanford University Press. One of the most important pioneers in the movement to make psychology more responsive to the whole, Barker insists that behaviour is always *situated* – in other words, it doesn't exist in a vacuum. A useful platform for the case study.

Bateson, G. (1972/1999) *Steps to an Ecology of Mind*. Chicago, IL: University of Chicago Press.

A collection of Bateson's essays and lectures. Really, his thesis is about *ideas* rather than the 'mind' of the title and the book is about what he calls 'an ecology of ideas'. It's rather mysterious and enigmatic, but good for thinking further about the ways in which ideas interact – and we must make them interact in a case study.

Burgess, E.W. (1927) 'Statistics and case studies as methods of sociological research', *Sociology and Social Research*, 12: 103–20.

A classic article that presages many of today's debates about the distinctions between research involving small and large numbers.

Checkland, P. (1981) *Systems Thinking, Systems Practice*. Chichester: John Wiley.
Checkland's original thinking on soft systems.

Flyvbjerg, B. (2001) *Making Social Science Matter: Why social inquiry fails and how it can succeed again*. Cambridge: Cambridge University Press.

See especially Chapter 6 – 'The power of example' – for a comparison of case study with methods that seek to establish generalisation.

MacIntyre, A. (1985) *After Virtue: A study in moral theory*. London: Duckworth.

Chapter 8 is the key one in this book – 'The character of generalizations in social science and their lack of predictive power'. This is taken to be a key critique of the methods of social science and their attempt to establish generalisation.

Travers, M. (2001) *Qualitative Research through Case Studies*. London: Sage.

This is essentially about the interpretative tradition and the case study and has a good chapter (Chapter 3) on Goffman's dramaturgical analysis.

SECOND EDITION

A SHORT GUIDE TO ACTION RESEARCH

ANDREW P. JOHNSON

Minnesota State University, Mankato

2005



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RESEARCH PARADIGMS AND THE NATURE OF REALITY

The scientific knowledge that has been gained influences the way we perceive the world. But the way the world is experienced in a particular culture influences what kind of science gets developed by that society.

—Willis Harman, 1998, p. 28

This chapter examines how our metaphysical views and research perspectives influence our educational philosophy, which in turn affects educational practices.

METAPHYSICAL PERSPECTIVES

Willis Harman (1998) describes three views of reality that he calls metaphysical perspectives. *Metaphysical* here refers to ontology or the question of the origins of the universe and the nature of reality. These perspectives are materialistic monism, dualism, and transcendental monism. They determine the types of questions that get asked and answered and the types of data that are deemed valid in academic research.

Materialistic Monism

The materialistic monism perspective views the universe as being made up only of matter and energy. Consciousness is something that arises out of matter. That is, our sense of self is solely an end product of neurological activity, which in turn is an end product of millions of years of evolution. All things in this purely materialistic universe consist of the sum of their parts and can be understood by breaking them down into their most basic components and measuring that which is observed and experienced. Newtonian law of cause and effect governs all events and can be used to know, predict, and ultimately control all things.

Positivism, a philosophy consistent with this view, states that knowledge exists outside the individual. Truth can only be derived through objective observations that are proven through reliable tests and predictions (Alkove & McCarty, 1992). It is only by collecting empirical data, isolating variables, testing, and retesting that we obtain truth or say that a thing exists. Quantitative research methodologies are used to piece together small bits of knowledge in order to understand the world in which we live. The type of thinking valued here is deductive thinking or thinking in lines.

From this perspective, learning consists of receiving knowledge from outside the individual, which must then be demonstrated outwardly for it to have occurred. The purpose of schools is to supply students with a designated body of knowledge and skills in a predetermined order. Teaching is a matter of transmitting knowledge from point A (teacher's head) to point B (students' heads). Academic achievement is students' ability to demonstrate or retransmit this designated body of knowledge back to the teacher (point A) or some other measuring agency or entity. Quantitative psychometric methods are used exclusively to determine and describe intelligence.

Dualism

The dualistic perspective views the universe as comprising two distinctly different kinds of stuff: matter and energy, as well as consciousness. Here consciousness exists separately from matter. From this perspective, matter and energy are studied using the traditional tools of science described above. Consciousness (people's subjective interpretation of the outer phenomenal world) is studied using observations, descriptions, ethnographies, and interviews. Consciousness also pertains to the subjective interpretation of one's inner world of feelings, memories, and impressions. These are studied using interviews, gestalt, inner exploration, dream analysis, and art. Some will posit that consciousness also includes super-consciousness or spirituality.

Constructivism is an educational philosophy consistent with this view. Here, true knowledge is seen as personally constructed by individuals as they interact with the environment. Truth requires a value judgement and is different for each individual. We construct our truths by observing and combining events in the objective outer world with images and impressions occurring in the subjective inner world. Both quantitative and qualitative research methodologies are used here, but qualitative research using inductive thinking is more reflective of this metaphysical perspective. Inductive thinking can be thought of as thinking in circles. It is the process of inducing order on a field.

From this perspective, learning is an inner activity that uses both objective and subjective knowledge to constantly build and revise our cognitive webs. Learning then becomes a transaction between the learner and what is to be learned. The purpose of schools is to help students construct knowledge and develop the skills they will need to successfully live in their worlds. Teaching is a matter of creating conditions whereby students are able to transact with knowledge. Academic

Transcendental Monism

Thus, consciousness is the cause and physical phenomena are the effect.

A helpful analogy can be found in music and the images created in the heads of listeners as they engage in the act of consciously attending to the music. The music can be thought of as holomovement or implicate reality. As the consciousness of the human listener interacts with the music, various mental images are created. These mental images, which do not exist in the absence of a conscious listener, are analogous to explicate reality. In this sense, phenomenal reality does not exist in the absence of an observer. This reflects the Copenhagen interpretation (Herbert, 1985), which posits that if a tree falls in a forest and nobody is there to hear it, not only does it not make a sound, but the tree does not exist in the first place.

Consistent with this perspective, states that the universe is

Holism, a thesis consistent with this perspective, states that the universe is made up of integrated wholes that cannot be reduced to the sum of their parts. We can never come to know the whole of reality by isolating variables in order to examine small parts. Instead, the whole is best understood by examining the principles that govern behavior within the system. Both quantitative and qualitative methodologies are used here; however, they must always be combined with a greater perspective in the form of transcendental methodology in order to approach truth. Transcendental methodologies are those activities that allow an individual to move beyond personal consciousness (emotion, will, and logic) to approach universal consciousness and include: meditation, systematic contemplation, vision questing, the arts, journal writing, poetry and creative writing, personal narratives, stories,

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religious traditions, spirituality, sacred writing, deep silence, sensory deprivation, ritual, and chanting. These can all be used as a way of seeing and re-presenting reality. From this metaphysical perspective, all types of thinking are valued, although deep reflection, intuition, and associative thinking are used to provide the ultimate context.

Holistic education is based on theories of holism. It is constructed around the principle of interconnectedness and seeks to integrate multiple levels of meaning and experience (Miller, 1996). Making connections is central to the curriculum process here. These connections occur between concepts, subject areas, communities, cultures, humanity, the arts, the sciences, mythology, religion, ecological systems, and history. True knowledge is seen as ultimately residing within each individual. Thus, teachers strive to create experiences whereby students are able to encounter phenomena, both internal and external, so that this knowledge can be realized. To learn, one must transcend one's culture, bias, values, ego, past experience, and sense of self in order to see things as they really are. Learning is said to have occurred when this view elicits a transformation of consciousness that leads to a greater nurturing of self, others, and the environment. From this perspective, a school's fundamental purpose is the creation of better human beings, which occurs through self-actualization and self-transcendence (Maslow, 1971).

Intelligence here consists of two parts: the ability to perceive the whole and the ability to think and act in ways that nurture the self, others, and the environment. Intelligence, from this perspective, cannot be separated from these nurturing values. Classes are small and curriculums are flexible and multifaceted so that these students' particular talents can be easily displayed and nurtured. Teachers are then empowered and have the knowledge and skills to enable all students to develop their particular strengths; discover their passions; and engage in relevant, complex, real-world activities.

Three Perspectives in Perspective

Figure 2.1 provides a very general overview of how these metaphysical perspectives project into various areas. It is important to note that there are very few people who identify with or exist in only one column.

IMPLICATIONS

The purpose of this chapter is not to suggest that any one perspective should be adopted. Indeed, one of the current problems in education is that there is an overreliance on materialistic monism and an overuse of quantitative research methodologies to define academic achievement and describe the quality of our learning experiences. Instead, I wish to make three points: First, we must recognize how our culture's predominant, some would say parochial, metaphysical perspective serves to shape the questions that get asked relative to education and educational

religious traditions, spirituality, sacred writing, deep silence, sensory deprivation, ritual, and chanting. These can all be used as a way of seeing and re-presenting reality. From this metaphysical perspective, all types of thinking are valued, although deep reflection, intuition, and associative thinking are used to provide the ultimate context.

Holistic education is based on theories of holism. It is constructed around the principle of interconnectedness and seeks to integrate multiple levels of meaning and experience (Miller, 1996). Making connections is central to the curriculum process here. These connections occur between concepts, subject areas, communities, cultures, humanity, the arts, the sciences, mythology, religion, ecological systems, and history. True knowledge is seen as ultimately residing within each individual. Thus, teachers strive to create experiences whereby students are able to encounter phenomena, both internal and external, so that this knowledge can be realized. To learn, one must transcend one's culture, bias, values, ego, past experience, and sense of self in order to see things as they really are. Learning is said to have occurred when this view elicits a transformation of consciousness that leads to a greater nurturing of self, others, and the environment. From this perspective, a school's fundamental purpose is the creation of better human beings, which occurs through self-actualization and self-transcendence (Maslow, 1971).

Intelligence here consists of two parts: the ability to perceive the whole and the ability to think and act in ways that nurture the self, others, and the environment. Intelligence, from this perspective, cannot be separated from these nurturing values. Classes are small and curriculums are flexible and multifaceted so that these students' particular talents can be easily displayed and nurtured. Teachers are then empowered and have the knowledge and skills to enable all students to develop their particular strengths; discover their passions; and engage in relevant, complex, real-world activities.

Three Perspectives in Perspective

Figure 2.1 provides a very general overview of how these metaphysical perspectives project into various areas. It is important to note that there are very few people who identify with or exist in only one column.

IMPLICATIONS

The purpose of this chapter is not to suggest that any one perspective should be adopted. Indeed, one of the current problems in education is that there is an overreliance on materialistic monism and an overuse of quantitative research methodologies to define academic achievement and describe the quality of our learning experiences. Instead, I wish to make three points: First, we must recognize how our culture's predominant, some would say parochial, metaphysical perspective serves to shape the questions that get asked relative to education and educational

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FIGURE 2.1
Metaphysical Perspectives

	MATERIALISTIC MONISM	DUALISM	TRANSCENDENTAL MONISM
1. worldview	universe consists of only matter and energy	universe consists of matter and energy, and mind-consciousness-spirit	the primary stuff of the universe is mind-consciousness-spirit
2. consciousness	consciousness arises from matter	exists separate from matter	matter arises from consciousness
3. reality	objective universe separate from observer	a personal construction	a dream
4. knowledge	<i>positivist</i> : exist outside self	<i>constructivist</i> : is constructed by individuals as they interact with the environment	<i>holistic</i> : exists within each individual, that which has the ability to transform
5. truth	objective examination of knowledge	requires a value judgment and is different for each individual	requires reflection and examination, an inward journey
6. learning	demonstrated outwardly for it to exist	inner state, constructing cognitive webs	free one's self from illusion, expand consciousness, transcendence
7. intelligence	ability to use linear thinking and deductive reasoning	ability to solve problems, create products, and shape environment	ability to perceive wholes and nurture self, others, and environment
8. purpose, teaching style	transmission		transformation
9. educational model	factory model	Dewey, social constructivist	Waldorf, Montessori, holistic education
10. primary psychological perspective	behaviorism	cognitive psychology	transpersonal psychology
11. primary research perspective	quantitative methodology	qualitative methodology	transcendental methodology
12. primary mode of thinking	deductive, lines	inductive, fields	intuitive, images, balls, B-cognition
13. scientific paradigm	phenomenological, cause-effect, natural laws, causal reality, atomistic, reductionist, positivistic	observational, objective, interpretive, contextual, subjective, ethnographic	quantum physics, holomovement, transcendentalism

practices. Recognizing this, the field of education needs to move toward a more expansive research agenda in terms of questions, methodology, and subject matter.

Second, we should reexamine and redefine the purpose of our schools. We do not ask the "why" questions often enough. It is our educational philosophies and assumptions about the nature of reality and the purpose of education that become the basis for our educational practice. I posit, as does Abraham Maslow (1971), that the fundamental purpose of education is self-actualization, which is to be fully human and to realize one's full potential. In this sense, our schools should be designed to help students identify and develop their abilities and discover their passions and interests. However, we cannot become fully human if we do not first recognize that which makes us so: our emotions, imagination, intuition, ideals, values, and creativity, none of which conveniently lend themselves to measurement. The Indian philosopher Jiddu Krishnamurti said, "The function of education is to create human beings who are integrated and therefore intelligent. . . . Intelligence is the capacity to perceive the essential, the what is; and to awaken this capacity, in oneself and in others, is education" (1953, p. 14).

Third, I am hoping that the three perspectives laid out in this chapter will move readers toward a broader conception of research. Again, research is a way of seeing and re-presenting reality. It runs the gamut from (a) strict, empirical, scientific experiments with controls used to isolate variables; to (b) descriptive, qualitative research; to (c) photography, the arts, poetry and creative writing, journal writing, and various forms of transcendental methodology. All of these can be used in action research projects to help you systematically observe your teaching practice with the goal of understanding and ultimately enhancing your ability to meet the various needs of your students.

SUMMARY

- Three primary metaphysical perspectives are materialistic monism, dualism, and transcendental monism.
- Our metaphysical perspectives influence educational research, which in turn affect our teaching practices.
- Materialistic monism suggests a universe that consists of only matter and energy. Knowledge exists outside the individual. Quantitative research methods align most closely with this perspective.
- Dualism suggests a universe that consists of matter and energy, and also mind, consciousness, and spirit. Knowledge is constructed by individuals as they interact with knowledge found in the environment. Qualitative research methods align most closely with this perspective.
- Transcendental monism suggests a universe that consists primarily of consciousness. True knowledge exists within each individual. Transcendental research methods align most closely with this perspective.

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QUESTIONS AND ACTIVITIES

1. Which metaphysical perspective do you align with most closely? Within the perspective, which things do you disagree with?
2. How does your teaching philosophy align with your metaphysical perspective?
3. What do you perceive to be the purpose of our schools?
4. What are some important things relative to school, teaching, and learning that cannot be measured and quantified? How else might these things be expressed or represented?
5. If grades or standardized tests could not be used, how else might you describe a student's learning or growth?
6. Describe an instance in which intuition has served you well.
7. Find an interesting place. Use as many numbers as you can to describe that place quantitatively. Then, use words to describe that place qualitatively. Finally, use pictures, metaphors, poetry, or symbols to describe that place. What does the difference in your three descriptions tell you?

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INTRODUCTION TO ACTION RESEARCH

Chapter 1 described research and the two major research categories. This chapter defines and describes action research and its various parts and examines the importance of action research in education.

RESEARCH IN ACTION

Action research can be defined as the process of studying a real school or classroom situation to understand and improve the quality of actions or instruction (Hensen, 1996; McTaggart, 1997; Schmuck, 1997). It is a systematic and orderly way for teachers to observe their practice or to explore a problem and a possible course of action (Dinkelman, 1997; McNiff, Lomax, & Whitehead, 1996). Action research is also a type of inquiry that is preplanned, organized, and can be shared with others (Foshy, 1998; Tomlinson, 1995).

A Quick Overview of Action Research

The action research process involves five essential steps or parts. First, ask a question, identify a problem, or define an area of exploration. Determine what it is you want to study. Second, decide what data should be collected, how they should be collected, and how often. Third, collect and analyze data. Fourth, describe how your findings can be used and applied. You create your plan for action based on your findings. And finally, report or share your findings and plan for action with others.

Action research is a recursive process that does not always proceed in a linear fashion (Patterson & Shannon, 1993). You may find yourself repeating some of these steps several times or doing them in a different order (see Figure 3.1). Also, some action research projects incorporate a sixth step, which is to put the findings or the question in a theoretical context called the literature review. This and the other steps are described in detail in the chapters that follow.

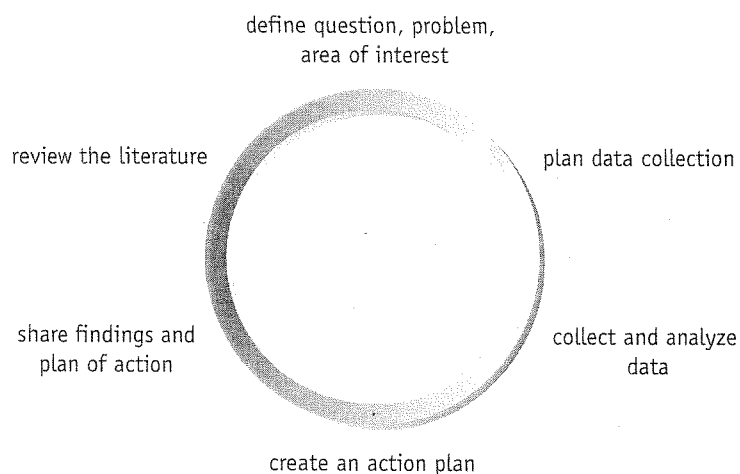


FIGURE 3.1 Steps of the Action Research Process

Descriptors of Action Research

The following nine descriptors may help bring this type of research into clearer focus.

1. *Action research is systematic.* Although data can be collected, analyzed, and presented with a certain amount of freedom, you must create a systematic way of looking. Action research is not a type of "anything goes" methodology. Nor is it a matter of simply describing what you think about an issue, depicting an interesting project or unit you have created, or explaining a pedagogical method that works well in your classroom. Action research is a planned, methodical observation related to one's teaching.

2. *You do not start with an answer.* An assumption underlying any research is that you do not know what you are going to find when you start; you are an unbiased observer. After all, if you had the answer, why would you be doing the research? Thus, even though you think Method X is the best way to teach reading, it is *not* appropriate to do a study describing why Method X is good and why Method Y is bad. Instead, you become an impartial observer (to the greatest degree possible) and study the effects of Method X as it is used in your classroom. The goal is to understand fully Method X and its effect (both positive and negative) on students' reading performance.

3. *An action research study does not have to be complicated or elaborate to be rigorous or effective.* Many beginning action researchers make the mistake of creating overly thick descriptions of every detail of their study in an attempt to be rigorous. This may often defeat the purpose. If too much material or too many details are

included, you may miss what you are looking for and the report will probably not be read or understood. Thus, what you do not include is often just as important as what you do include in your observing and in your reporting. A well-organized, concisely described study is always preferable to a confusing, complicated study.

4. *You must plan your study adequately before you begin to collect data.* Having a plan and a schedule for collecting data before you start is what separates a systematic inquiry from an impressionistic view. However, plans and the type of data you collect can change as you get into your study.

5. *Action research projects vary in length.* The length of data collection in an action research study is determined by your question, the nature of your inquiry, the research environment, and the parameters of your data collection. For smaller action research projects I suggest that my graduate students who are classroom teachers consider 2 weeks as a minimum length. For undergraduate students, there are some very interesting sorts of projects that can be done in 1 or 2 class periods. Major studies, such as a master's thesis or an article for an academic journal, generally have a duration of 2 months to a whole school year. Keep in mind that if your data collection period is too short, you run the risk of presenting an unrealistic view of that educational setting.

6. *Observations should be regular, but they do not necessarily have to be long.* The duration of observations might be anywhere from 1 minute to an hour or more. Many of your observations might be a quick note with the date and time recorded, whereas others might be longer and more formal. Although they do not have to be long, observations do have to be done on a consistent, preplanned schedule. Also, observations are only one form of data collection. Other data collection forms and methods are described in Chapter 8.

7. *Action research projects exist on a continuum from simple and informal to detailed and very formal.* As stated above, some very interesting projects are short and simple. I suggest that beginning action researchers practice with these kinds of projects before tackling larger, more complicated ones. Action research can also be used for more formal kinds of studies. Many of my graduate students who are teachers have done action research projects for their master's theses. A sample guideline for this purpose is described in Chapter 15. Because these projects directly related to what was happening in their classrooms, these teachers were able to link theory to practice in a meaningful way. Graduate students have been far more enthusiastic about investing themselves in these kinds of projects than in the kinds of secondary research often done as part of a master's thesis. And not surprisingly, action research projects always seem to be more interesting and enjoyable to read.

8. *Action research is sometimes grounded in theory.* Relating the questions, results, and conclusions to existing theory provides a context in which to understand your research. Let the reader know how your data relate to what others have said about the same topic. Some action researchers do a short review of the literature before

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they begin collecting data, to provide a framework for the data collection. Other wait until the data are collected, then compare their conclusions to what others have written. Opinions as to the place and extent of a literature review in action research vary. This topic is discussed further in Chapter 7.

9. *Action research is not a quantitative study.* Many beginning action researchers have to divest themselves of their previous notions of the nature of research. In an action research project you are not trying to prove anything. You are not comparing one thing to another to determine the best possible thing. Also, there are no experimental or control groups, independent or dependent variables, or hypotheses to be supported. The goal is simply to understand. As an action researcher you are creating a series of snapshots in various forms and in various places to help understand exactly what is going on.

To illustrate this concept of understanding, one night in a graduate class I asked a group of teachers to describe an event that happened that day. Alison Reynolds wrote of her interaction with a particular student that morning. Although she had described her teaching situation in early classes, it was not until I saw her snapshot of this student and the nature of their interactions that I began to understand some of the dynamics of her teaching situation. Her descriptions went beyond the numbers and test scores in helping to comprehend the nature of teaching in an alternative high school and provided insight as to what an action research project might be. If she were to continue with this and make it an action project, she would create a plan for describing a number of students and situations in her school.

10. *The results of quantitative action research projects are limited.* While quantitative methods are sometimes used in action research, caution must be exercised in coming to generalized conclusions or in applying the results to larger populations. This is because of the small sample size of most action research projects and the many variables that are uncontrolled or unaccounted for.

For example, Mr. Miller did an action research project using quantitative methods to compare two approaches to reading instruction in his second-grade classroom. His statistical analysis found that the group using Method A scored significantly better than the group using Method B; however, Mr. Miller cannot reasonably conclude that Method A is the superior approach. The only conclusion he can reasonably make in this case is that Method A appears to be more effective than Method B. We do not know if the two groups were equal to begin with, we do not know if the samples were representative, it was conducted in an uncontrolled environment, and there was nothing to address the bias and subjectivity of the researcher. In order to make a stronger conclusion, each of these issues would need to be addressed.

In another example, Ms. Reuter wanted to see if her new approach to spelling instruction was effective in improving students' ability to spell words correctly. Using quantitative methods, she found that when using her new approach, students showed a statistically significant gain in their ability to spell words over the period of three months. However, like Mr. Miller she has to be very careful in the

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kinds of conclusions she can reasonably make. In this case, we do not know how much of this gain was a result of general learning and maturity. That is, students naturally become better spellers as they write and are exposed to more print. Also, she cannot reasonably compare her class to another class because of the differences in teaching styles and approaches as well as many other unaccounted variables. At best, she might be able to say that her new approach appears to be effective in helping students learn new words.

THE IMPORTANCE OF ACTION RESEARCH

This section describes how action research can be used to fill the gap between theory and practice in education, empower teachers to become agents of change, and enhance the professional growth and development of teachers.

The Gap between Theory and Practice

Among other things, research is used to build theories that in turn help determine the best practices in education. These practices are then used to help teachers create effective learning experiences. However, sometimes a gap exists between researchers conducting and reporting their education research and teachers practicing in the field (Hensen, 1996; Patterson & Shannon, 1993; Tomlinson, 1995). That is, what goes on in public schools often does not reflect the wealth of research related to best practice in teaching and learning.

There are two possible reasons for this gap: First, it may be because research in education is sometimes written in a way that does not respect the demands of teachers' daily schedules where time is a precious commodity. Research articles in academic journals can sometimes get overly descriptive, use jargon common to a very few, and focus on methodology and hypothetical notions that are not germane to the daily needs of teachers. This leads to the belief held by many teachers that education research is practically irrelevant (Barone, Berliner, Blanchard, Casanova, & McGown, 1996; Patterson & Shannon, 1993).

Second, the gap between research and practice might also be a result of the Moses Effect. This is when researchers hand down research edicts from on high with the expectation that teachers will be passive receivers of these edicts (see Figure 3.2). This creates a one-way flow of information that often does not value teachers' points of view, reflect the complexities of teaching, or address the concrete problems and concerns that teachers face in their classrooms on a daily basis (Patterson & Shannon, 1993).

Action research is one solution to use in bridging this gap (Hensen, 1996; Knight, Wiseman, & Cooner, 2000). The Action Research Quadrant (Figure 3.3) illustrates the two-way flow of information in action research. Theories and research related to best practice are used to understand and observe what is happening in a classroom setting. At the same time, this data is used to understand or inform theories and research related to best practice.

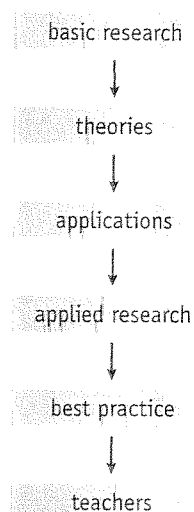


FIGURE 3.2 The Moses Effect

Teacher Empowerment

Action research also facilitates teacher empowerment. Teachers are empowered when they are able to collect their own data to use in making decisions about their schools and classrooms (Book, 1996; Erickson, 1986; Hensen, 1996). Empowered teachers are able to bring their talents, experiences, and creative ideas into the classroom, and implement programs and strategies that best meet the needs of their students. They also are able to use the methodologies that complement their own particular philosophy and teaching style. When teachers are allowed to take risks and make changes related to teaching and learning, student achievement is enhanced (Marks, & Louis, 1997; Sweetland & Hoy, 2002) and schools become

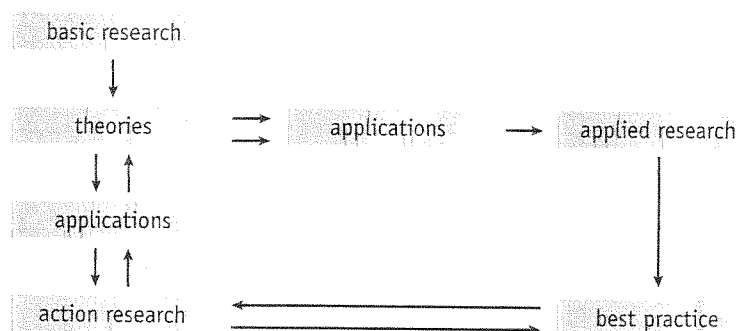


FIGURE 3.3 The Action Research Quadrant

more effective learning communities (Detert, Louis, & Schroeder, 2001). The top-down approaches that are sometimes used to manage schools and solve classroom problems create an external locus of control that inhibits teacher empowerment and thereby lessens the effectiveness of schools (Book, 1996).

Teacher Inservice and Professional Growth

Action research can also be used to replace teacher inservices as a means of professional growth and development. Traditional teacher inservices are often ineffective (Barone et al., 1996). Teachers are gathered, usually after a long day of teaching or on a busy workshop day, to listen to an expert describe an approach or methodology that often does not relate to their classroom situation or align with their teaching style. These traditional inservices generally do not give teachers sufficient time, activities, or content to increase their knowledge or affect their practice (Birman, Desimone, Porter, & Garet, 2000). To be effective, inservices need to be longer or extended over multiple sessions, contain active learning to allow teachers to manipulate the ideas and enhance their assimilation of the information, and align the concepts presented with the current curriculum, goals, or teaching concerns.

In regard to teachers' professional growth and development, Hensen (1996) describes the following benefits of action research: It (a) helps teachers develop new knowledge directly related to their classrooms, (b) promotes reflective teaching and thinking, (c) expands teachers' pedagogical repertoire, (d) puts teachers in charge of their craft, (e) reinforces the link between practice and student achievement, (f) fosters an openness toward new ideas and learning new things, and (g) gives teachers ownership of effective practices. Thus, providing teachers time and incentive to engage in action research projects and giving them a platform to present their findings and engage in professional dialogue with peers enhances their professional growth and development, which in turn moves the field of education forward. Using action research as a form of teacher development also increases the sense of professionalism in education (Tomlinson, 1995), and it enables teachers to become agents of change (Hensen, 1996). (Using action research for professional development is described further in Chapter 5.)

S U M M A R Y

- Action research is the process of studying a real school problem or situation.
- The goal of action research is to improve one's teaching practice or to enhance the functioning of a school.
- Action research is a preplanned and systematic observation of one's teaching practice or teaching situation.
- The essential steps of action research are to (a) define a question or area of study; (b) decide on a method for collecting data; (c) collect and analyze data; (d) describe how those findings can be used and applied; and (e) share your findings and plan for action with others.

- A review of the relevant literature related to your topic or question might also be included in action research.
- Action research can be used to bridge the gap between education research and teaching practice.
- Teacher empowerment can be facilitated with action research.
- Action research can be used as a replacement for traditional inservices to enhance teachers' professional growth and development.

QUESTIONS AND ACTIVITIES

1. List or describe three to five things that interest you in your classroom or school.
2. What are some problems in your classroom or school that you would like to solve?
3. What are some aspects of your teaching that you would like to improve?
4. Describe some theory or bit of research that seems to have no direct link to your teaching situation.
5. Describe some theory or bit of research that seems to have a direct link to your teaching situation.
6. Describe a situation where someone else has made a decision regarding your classroom practice. Would you have made the same decision?
7. Describe an instance where you would have made a different decision than what was made regarding your school or classroom. What reasons or evidence do you have to support your idea?
8. Think about the teacher inservices you have attended. What characteristics were effective? What characteristics were less effective? Use the T-chart in Appendix C to record your ideas.
9. What topic or topics would be of interest to you in a teacher inservice? Where might you go to get information on this topic?
10. Design a method that you think would be effective as a means for teacher growth and development.

LIFE HISTORY RESEARCH IN EDUCATIONAL SETTINGS

Learning from lives

Ivor F. Goodson and Pat Sikes

2001

Open University Press

I | Developing life histories

The story of life history: origins of the life history method

Searching for the origins of the life history method, we find that the first life histories, in the form of autobiographies of American Indian chiefs, were collected by anthropologists at the beginning of the twentieth century (for example, Barrett 1906; Radin 1920). Since then, the approach has been increasingly adopted by sociologists and other scholars working in the humanities, although its popularity and acceptance as a research strategy has tended to wax and wane. As we write, life history and other biographical and narrative approaches are widely seen as having a great deal to offer, and in this chapter we argue that they should be employed in educational studies. But, in examining their scholarly fate, we have to scrutinize their use to date within sociology, as this has been a major 'battleground' in their evolution.

For sociologists, the main landmark in the development of life history methods came in the 1920s, following the publication of Thomas and Znaniecki's (1918–1920) mammoth study, *The Polish Peasant in Europe and America*. In exploring the experience of Polish peasants migrating to the United States, Thomas and Znaniecki relied mainly on migrants' autobiographical accounts, alongside extant diaries and letters. For these authors, life histories were the data *par excellence* of the social scientist, and they present a strident case for using life histories above all other methods:

In analysing the experiences and attitudes of an individual, we always reach data and elementary facts which are exclusively limited to this individual's personality, but can be treated as mere incidences of more or less general classes of data or facts, and can thus be used for the determination of laws of social becoming. Whether we draw our materials for sociological analysis from detailed life records of concrete individuals or from the observation of mass phenomena, the problems

of sociological analysis are the same. But even when we are searching for abstract laws, life records, as complete as possible, constitute the *perfect* type of sociological material, and if social science has to use other materials at all it is only because of the practical difficulty of obtaining at the moment a sufficient number of such records to cover the totality of sociological problems, and of the enormous amount of work demanded for an adequate analysis of all the personal materials necessary to characterise the life of a social group. If we are forced to use mass phenomena as material, or any kind of happenings taken without regard to the life histories of the individuals who participated, it is a defect, not an advantage, of our present sociological method.

(Thomas and Znaniecki 1918-1920: 1831-3)

Thomas and Znaniecki's pioneering work established the life history as a bona fide research device (although, as Miller (2000) points out, its foundations can be seen in the notion of historicism as expressed by Wilhelm Dilthey). The prominent position of the life history was further consolidated by the flourishing tradition of sociological research stimulated at Chicago, particularly by Robert Park.

In the range of studies of city life completed under Park, the life history method was strongly in evidence: *The Gang* (Thrasher 1928), *The Gold Coast and the Slum* (Zorbaugh 1929), *The Hobo* (Anderson 1923) and *The Ghetto* (Wirth 1928). But perhaps the zenith was reached in the 1930s with publications such as Clifford Shaw's account of a 'mugger' in *The Jack-Roller* (Shaw 1930), and Edwin Sutherland's *The Professional Thief* (Cornwell and Sutherland 1937). Howard Becker's comments on Shaw's study underline one of the major strengths of the life history method:

By providing this kind of voice from a culture and situation that are ordinarily not known to intellectuals generally and to sociologists in particular, *The Jack Roller* enables us to improve our theories at the most profound level: by putting ourselves in Stanley's skin, we can feel and become aware of the deep biases about such people that ordinarily permeate our thinking and shape the kinds of problems we investigate. By truly entering into Stanley's life, we can begin to see what we take for granted (and ought not to) in designing our research – what kinds of assumptions about delinquents, slums and Poles are embedded in the way we set the questions we study.

(Becker 1970: 71)

Becker's argument goes to the heart of the appeal of life history methods at their best: for life history data disrupts the normal assumptions of what is 'known' by intellectuals in general, and sociologists in particular. Conducted successfully, the life history forces a confrontation with other people's subjective perceptions. This confrontation can be avoided, and so often is

avoided in many other social scientific methods: one only has to think of the common rush to the quantitative indicator or theoretical construct, the statistical table or the ideal type. This sidesteps the messy confrontation with human subjectivity which we believe should comprise the heartland of the sociological enterprise. Behind and/or coterminous with this methodological sidestep, there is often a profound substantive and political sidestep. In the avoidance of human subjectivity, quantitative assessment and theoretical commentaries can so easily service powerful constituencies within the social and economic order. As we shall see, the tendency to favour and support power is always a potential problem in social science and later we will discuss some of the safeguards that can be employed in life history work.

From the statement about 'putting ourselves in Stanley's skin', Becker leads on to the assertion that Stanley's story offers the possibility 'to begin to ask questions about delinquency from the point of view of the delinquent' (*ibid.*: 71), so that it follows that questions will be asked not from the point of view of the powerful actors but from the perspective of those who are 'acted upon' in professional transactions. These are, then, some important reasons, which move beyond the issues of methodological debate, as to why life history methods might be unpopular in some quarters.

Life history, by its nature, asserts and insists that 'power' should listen to the people it claims to serve, as Becker notes:

If we take Stanley seriously, as his story must impel us to do, we might well raise a series of questions that have been relatively little studied – questions about the people who deal with delinquents, the tactics they use, their suppositions about the world, and the constraints and pressures they are subject to.

(*ibid.*: 71)

However, this contention should be read in the light of Shaw's own 'ear warning' in his preface, where he cautions the reader 'against drawing conclusions' about general causes of delinquency on the basis of a single case record.

One of the best early attempts to analyse the methodological base of the life history method was Dollard's (1949) *Criteria for the Life History*. Following shadowing Becker, he argued that: 'detailed studies of the lives of individuals will reveal new perspectives on the culture as a whole which are not accessible when one remains on the formal cross sectional plane of observation' (p. 4). A lot of Dollard's arguments have a familiar ring, perhaps reflecting the influence of George Herbert Mead. He notes that:

as soon as we take the post of observer on the cultural level the individual is lost in the crowd and our concepts never lead us back to him. After we have 'gone cultural' we experience the person as a fragment of

a (derived) culture pattern, as a marionette dancing on the strings of (reified) culture forms.

(Dollard 1949: 5)

In contrast to this, the life historian: 'can see his life history subject as a link in a chain of social transmission' (*ibid.*: 5). This linkage should ensure that life history methods will ameliorate the 'presentism' which exists in so much sociological theory and a good deal of symbolic interactionism (this is explored later in the chapter). Dollard describes this linkage between historical past, present and future:

There were links before him from which he acquired his present culture. Other links will follow him to which he will pass on the current of tradition. The life history attempts to describe a unit in that process: it is a study of one of the strands of a complicated collective life which has historical continuity.

(*ibid.*: 15)

Dollard is especially good, though perhaps unfashionably polemical, in his discussion of the tension between what might be called the 'cultural legacy', the weight of collective tradition and expectation, and the individual's unique history and capacity for interpretation and action. By focusing on this tension, Dollard argues, the life history offers a way of exploring the relationship between the culture, the social structure and individual lives. This is a similar argument to that developed later in Chapter 5 on life stories and social context, where structure and agency are shown to collide around cultural storylines. Thus, Dollard believed that in the best life history work:

we must constantly keep in mind the situation both as defined by others and by the subject, such a history will not only define both versions but let us see clearly the pressure of the formal situation and the force of the inner private definition of the situation.

(*ibid.*: 32)

This resolution, or attempt to address a common tension, is seen as valuable because:

whenever we encounter difference between our official or average or cultural expectation of action in a 'situation' and the actual conduct of the person this indicates the presence of a private interpretation.

(*ibid.*: 32)

In fact, Dollard was writing some time after a decline set in for life history methods (one of the unfortunate side-effects of this is that Dollard's work is not as well known as it should be). After reaching its peak in the 1930s, the life history approach fell from grace and was largely abandoned by social

statistical methods gained them a growing number of adherents among sociologists; also, ethnographically inclined sociologists began to place more emphasis on situation than on biography as the basis for understanding human behaviour.

Then, in the 1970s, something of a 'minor resurgence' (Plummer 1990) could be observed, starting, particularly and significantly, among deviancy sociologists. Thus, there were studies of a transsexual (Bogdan 1974), a professional fence (Klockars 1975) and, with a fine sense of history following Shaw's 1930 study, once again, a professional thief (Chambliss 1972).

Whilst life history methods have long been popular with journalists-cum-sociologists like Studs Terkel in the United States, Jeremy Seabrook and Ronald Blythe in the UK, and a growing band of 'oral Historians' (Thompson 1978, 1988), Daniel Bertaux's (1981) collection, *Biography and Society*, marked a significant step in the academic rehabilitation of the approach. This book was closely followed by Ken Plummer's (1983) important *Documents of Life* (a revised version of which was published in 2000), and Tierney's (1998) special issue of *Qualitative Inquiry* is also of interest here.

Feminist researchers have been particularly vociferous in their support of the approach, owing mainly to the way in which it can be used to give expression to, and celebration of, hidden or 'silenced' lives (cf. McLaughlin and Tierney 1993): lives which are lived privately and without public accomplishment – the sorts of lives most women (and, it has to be said, most men) live (cf. Personal Narratives Group 1989; Stanley 1990, 1992; Gluck and Patai 1991; Middleton 1997; Munro 1998; Weiler and Middleton 1999). Similarly, those researching issues and aspects of sexuality, notably, Plummer (1995) and Sparkes (1994), have also made considerable use of the approach.

Within the field of educational studies, working with teachers and pupils who are, again, arguably marginal in terms of their social power, life history has been seen as particularly useful and appropriate because, as Bullough (1998: 20–1) points out, 'public and private cannot . . . be separated in teaching . . . The person comes through when teaching'. Life history does not ask for such separation: indeed it demands holism. The growing number of life history studies dealing with educational topics is testimony to this (for example, Ball and Goodson 1985; Sikes *et al.* 1985; Goodson 1992a; Casey 1993; Middleton 1993; Goodson and Hargreaves 1996; Osler 1997; Sikes 1997; Erben 1998b; Kridel 1998).

Among these scholars, albeit in marginal or fragmented groups, a debate is under way which promises a thoroughgoing re-examination of the potential of life history methods. But, before we consider the contemporary appeal of the life history, it is important to discover why life history method was for so long eclipsed by social theory, social survey and by participant observation. In this examination, the emphasis will be on distinguishing

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fundamental methodological stumbling blocks from professional, micro-political and personal reasons for the decline of life history work. Often the latter are far more important than those involved in the methodological 'paradigm wars' acknowledge.

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Reasons for the decline of the life history in early sociological study

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By 1966, Becker was able to summarize the fate of the life history method among American sociologists in this manner: 'given the variety of scientific uses to which the life history may be put, one must wonder at the relative neglect into which it has fallen' (Becker 1970: 71-2).

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Becker noted that sociologists have never given up life histories altogether, but neither have they made it one of their standard research tools. The general pattern was, and by and large continues to be, that: 'they know of life history studies and assign them for their students to read. But they do not ordinarily think of gathering life history documents or of making the technique part of their research approach' (*ibid.*: 71-2).

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The reasons for the decline of life history methods are partly specific to the Chicago department. From the late 1920s, life histories came under increasing fire as the debate within the department between the virtues of case study (and life histories) and statistical techniques intensified. Faris (1967), in his study of Chicago sociology, records a landmark within this debate:

To test this issue, Stouffer had hundreds of students write autobiographies instructing them to include everything in their life experiences relating to school usage and the prohibition law. Each of these autobiographies was read by a panel of persons presumed to be qualified in life history research, and for each subject the reader indicated on a scaled line the position of the subject's attitude regarding prohibition. Inter-reader agreement was found to be satisfactory. Each of the same subjects had also filled out a questionnaire that formed a scale of the Thurstone type. The close agreement of the scale measurement of each subject's attitude with the reader's estimate of the life history indicated that, as far as the scale score was concerned, nothing was gained by the far more lengthy and laborious process of writing and judging a life history.

(Faris 1967: 114-15)

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One might note that the experiment does beg the question of why use life history simply in order to measure attitude. No doubt the autobiographies *did* contain explanations of 'why' the informants' attitudes were of a particular degree: such information could be valuable for purposes other than attitude measurement and would, moreover, not be accessible by means of a questionnaire.

Even within Chicago case study work, the life history declined against other ethnographic devices, notably participant observation. One element of the explanation of this lies, perhaps, in the orientations of Blumer and Hughes. These two sociologists provide a bridge between the Chicago school of the 1920s and 1930s and those Matza has termed the 'neo-Chicagoans', such as Becker (1970). Blumer's symbolic interactionism places primary emphasis on process and situation, and explanations in terms of biography, like those in terms of social structural forces, are regarded with considerable suspicion. Hughes's comparative approach to the study of occupations may have tended to limit interest in biography in favour of concern with the typical problems faced by occupational practitioners and the strategies they adopt for dealing with them. An additional factor, which hastened the decline of the methodological eclecticism of Chicago sociology with the life history playing a central role, was the decline of Chicago itself as a dominant centre for sociological studies.

The fate of life history methods has been inextricably linked to the historical emergence of sociology as a discipline. Hence, the methodological weaknesses of the approach came to be set against the need to develop abstract theory. When sociology was highly concerned with providing detailed accounts of specific communities, institutions or organizations such weaknesses were clearly of less account. But in the life history of sociology, the pervasive drift of academic disciplines towards abstract theory has been irresistibly followed: in this evolutionary imperative it is not difficult to discern the desire of sociologists to gain parity of esteem with other academic disciplines. The resulting pattern of mainstream sociology meant that sociologists came to pursue 'data formulated in the abstract categories of their own theories rather than in the categories that seemed most relevant to the people they studied' (Becker 1970: 72).

Alongside the move towards abstract academic theory, sociological method became more 'professional'. Essentially, this led towards a model of 'single-study' research, defined by Becker in this way:

I use the term to refer to research projects that are conceived of as self-sufficient and self-contained, which provide all the evidence one needs to accept or reject the conclusions they proffer . . . The single study is integrated with the main body of knowledge in the following way: it derives its hypotheses from an inspection of what is already known: then, after the research is completed, if those hypotheses have been demonstrated, they are added to the wall of what is already scientifically known and used as the basis for further studies. The important point is that the researcher's hypothesis is either proved or disproved on the basis of what he has discovered in doing that one piece of research.
(Becker 1970: 72)

The imperative towards this pattern of sociological research can be clearly

evidenced in the traditions and organizational format of emergent professional sociology. The PhD student must define and test their hypothesis; the journal article must test the author's own or other academics' hypotheses; the research project or programme must state the generalizable aims and locate the burden of what has to be proved. But this dominant experimental model, so fruitful in analogies with other *sciences* and, hence, so crucial in legitimating sociology as a fully-fledged academic discipline, led to the neglect of sociology's full range of methodology and data sources.

It has led people to ignore the other functions of research and particularly to ignore the contribution made by one study to an overall research enterprise even when the study, considered in isolation, produced no definitive results of its own. Since, by these criteria, the life history did not produce definitive results, people have been at a loss to make anything of it and by and large have declined to invest the time and effort necessary to acquire life history documents.

(*ibid.*: 73)

Becker ends by holding out the hope that sociologists will, in the future, develop a 'further understanding of the complexity of the scientific enterprise' (p. 73); that this will rehabilitate the life history method and lead to a new range of life history documents as generative as those produced by the Chicago sociologists in the 1920s and 1930s.

In the period following Becker's strictures in 1970, sociology was subject to a number of new directions which sought to re-embrace some of the elements lost in the 'positivist', theory-testing models. (Morris 1977; Cuff and Payne 1979). One new direction which clearly stressed biography, the phenomenological sociology of Berger and Luckmann (Berger 1963; Berger and Luckmann 1967), actually resulted in little empirical work.

Hence, research in interpretive sociology has displayed a heavy emphasis on situation under the influence of interactionism and ethnomethodology. The paradox is that these new directions in sociology moved away from the 'positivist' model, but directly to situation and occasion; as a result, life history and biography have tended to remain at the sidelines of the sociological enterprise. For instance, interactionist studies have focused on the perspectives and definitions emerging among groups of actors in particular situations, the backcloth to this being presented as a somewhat monolithic 'structural' or 'cultural legacy' which constrains, in a rather disconnected manner, the actors' potentialities. In overreacting to more deterministic models, this situational emphasis most commonly fails to make any connection with historical process. Thus, whilst interactionists retained their interest in the meaning that objects had for actors, these meanings increasingly came to be seen as collectively generated to deal with specific situations, rather than as the product of individual or even collective biography.

Viewing sociology's evolution over half a century or so provides a number

development or abstract theory, life history work came to be seen as having serious methodological flaws. In addition, since life history studies often appeared to be only 'telling tales', these methodological reservations were enhanced by the generally low status of this as an 'academic' or 'scientific' exercise. Paradoxically, even when antidotes to the experimental mode of sociology developed, these took the form of interactionism and ethnographic methodology, both of which stressed situation and occasion rather than biography and background. Moreover, since these new directions had their own problems of their own, life history work was unattractive on this count as well. At the conference, where Ivor Goodson's early work on life history was originally delivered as a seminar paper, a classroom interactionist rejected the exhortation to consider life history work by saying: 'we should not suggest new methodologies of this sort . . . because of the problems of our academic careers. Christ! Ethnography is low status enough as it is' (Goodson 1983).

Set against the life history of the aspirant academic, keen to make a career in the academy as it is, or as it has been shaped and ordered, we clearly see the unattractiveness of the life history method at particular stages in the evolution of sociology. But, by the 1980s, matters were beginning to change markedly in ways that have led to a re-embracing of life history methodology.

From modernism to postmodernism

Under modernism, life history languished because it persistently failed 'objectivity tests': numbers were not collected and statistical aggregates were not produced and, since studies were not judged to be representative or exemplary, contributions to theory remained parsimonious. In the historical aspiration to be a social 'science', life history failed its membership test.

However, as Harvey (1989) and others have documented, the 'conditions of postmodernity' provides both new dilemmas and new directions. In some ways, the new possibilities invert the previous deficits which were perceived in life history work. In moving from objectivities to subjectivities, new prospects open for life history work and, as a result, a range of new studies has begun to appear (see Denzin and Lincoln 1994a, 2000). As is often the case, educational studies have been slow to follow new directions, although new work has recently begun to emerge.

Life history work has accompanied the turn to postmodernism and poststructuralism, particularly as evidenced in sociological studies, gender studies, cultural studies, literacy theory and even psychology. Such work facilitates the move away from modernist master narratives which are viewed as social productions of the Enlightenment Project. Alongside the

move, the notion of a singular, knowable, essential self is judged as part of the social production of individualism and one linked to agentic selves in pursuit of progress, knowingness and emancipation. Assumptions of linearity of chronological timelines and storylines are challenged in favour of more multiple, disrupted notions of subjectivity. Foucault's work, for instance, has focused sociological attention on the way in which institutions like hospitals and prisons regulate and constitute our subjectivities. Likewise, discourse studies have focused on the role of language in constructing identities in producing textual representations which purport to 'capture' the essential selves of others (Shotter and Gergen 1989).

These new syntagms in sociological work have led to a revival in the use of life history work:

The current focus on acknowledging the subjective, multiple and partial nature of human experience has resulted in a revival of life history methodology. What were previously criticisms of life history, its lack of representativeness and its subjective nature, are now its greatest strength.

(Munro 1998: 8)

Yet, the postmodern concern with disrupting constructed selves and stories is itself not without difficulty, as Munro reflectively notes:

In collecting the life histories of women teachers I find myself situated in a paradoxical position. I know that I cannot 'collect' a life. Narrative does not provide a better way to locate truth, but in fact reminds us that all good stories are predicated on the quality of the fiction. We live many lives. Consequently, the life histories in this book do not present neat, chronological accounts of women's lives. This would be an act of betrayal, a distortion, a continued form of 'fitting' women's lives into the fictions, categories and cultural norms of patriarchy. Instead, my understanding of a life history suggests that we need to attend to the silences as well as what is said, that we need to attend to how the story is told as well as what is told or not told, and to attend to the tensions and contradictions rather than succumb to the temptations to gloss over these in our desire for 'the' story.

(*ibid.*: 12-13)

Here, Munro begins to address the methodological and, indeed, ethical minefield which potentially confronts, confuses and confounds the researcher and the researched. Michelle Fine has written of some of the issues to be confronted:

Self and Other are knottily entangled. This relationship, as lived between researchers and informants, is typically obscured in social science texts, protecting privilege, securing distance, and laminating the

ism, persona-logic theorizing, and de-contextualization, we inscribe the Other, strain to white out Self, and refuse to engage the contradictions that litter our texts.

(Fine 1994: 72)

Fine's warnings are of inestimable value as we approach life history work. But in the end we do face the inevitable closure of the text that is produced or are forever caught in the politics of infinite regress where every closure must be reopened. For Fine warns us that the search for the complete and coherent is a delusion; we produce a snapshot of transgressions in process when we write up life history work.

Furthermore, the relationship of the researcher and informant is much concerned in the postmodern predilection for 'rejection of the unitary subject for a more complex, multiple and contradictory notion of subjectivity' (Munro 1998: 35). What does such researcher rejection mean in the face of an informant who narrates their life as a search for coherence? For it remains the case that many people narrate their lives according to an aspiration for coherence, for a unitary self. Should we, in Munro's words 'reject this social construction of self? Rejection is not the issue here, for life history work should, where possible, refuse to play postmodern God. Life history work is interested in the way people *do* narrate their lives, not in the way they *should*. Here it seeks to avoid the fate of some postmodern fundamentalists.

Life stories, then, are the starting point for our work. Such stories are, in their nature, already removed from life experiences: they are lives interpreted and made textual. They represent a partial, selective commentary on lived experience. Freeman has explored some of the issues that are raised here:

For what we will have before us are not lives themselves, but rather *texts* of lives, literary artifacts that generally seek to recount in some fashion what these lives were like. In this respect, we will be – we must be – at least one step removed from the lives that we will be exploring: we can only proceed with our interpretive efforts on the basis of what has been written [or related] by those whose lives they are.

The basic situation, I hasten to emphasize, obtains not only in the case of literary texts of the sort we will be examining here, but in the case of interviews and the like along with the observation of human action more generally. Interviews, of the sort that social scientists often gather, are themselves texts, and while they may not have quite as much literary flourish as those we buy in bookstores, they are in their own right literary artifacts, taking the form of words, designed to give shape to some feature of experience. As for the observation of human action, the story is actually much the same: human action, which occurs in time

and yields consequences the significance of which frequently extend beyond the immediate situation in which it takes place, is itself a kind of text; it is a constellation of meanings which, not unlike literary texts or interviews, calls forth the process of interpretation (see especially Ricoeur 1980). In any case, the long and short of this brief excursion into 'textuality' is that our primary interpretive takeoff point will not be lives as such but the words used to speak them.

(Freeman 1998: 7)

The rendering of lived experience into a 'life story' is one interpretive layer, but the move to 'life history' adds a second layer and a further interpretation. Goodson (1992a) has written before of the distinction between stage one, where the informant relates their 'life story' and stage two, where a 'life history' is constructed employing a new range of interviews and documentary data. The move from life story to life history involves the range of methodologies and ethical issues noted earlier. Moving from personal life stories to life histories involves issues of process and power as Bertaux has noted (1981: 9): 'What is really at stake is the relationship between the sociologist and the people who make his work possible by accepting to be interviewed on their life experiences'.

Moving from life story to life history involves a move to account for historical context – a dangerous move, for it offers the researcher considerable 'colonizing' power to 'locate' the life story with all its inevitable selections, shifts and silences. Nonetheless, we hold to the need for providing historical contexts for reading 'life stories'.

Dannefer (1992) has written of the various meanings of context in studying developmental discourse. Here, our concern is to provide communications that cover the social histories and, indeed, social geographies in which life stories are embedded: without contextual commentary on issues of time and space, life stories remain uncoupled from the conditions of their social construction. This, above all, is the argument for life histories rather than life stories.

Whilst rightly concerned about the colonizing dangers of contextual commentary, even poststructuralist accounts often end up moving from life stories to life histories, and they confront issues surrounding the changing contexts of time and space. For instance, Middleton's early work on women teachers' lives relates a substantive account of one feminist teacher's pedagogy within the specific sociocultural setting of post-Second World War New Zealand (Middleton 1992). Likewise Munro, an avowed feminist post-structuralist, argues that:

Since this study is concerned with placing the lives of women teachers within a broader historical context, historical data regarding the communities and the time period in which they taught was also collected. Although I am not an educational historian an attempt was made to

(Munro 19)

The distinction between life stories or narratives and life histories is a crucial one. By providing contextual data, the life stories can be set in light of changing patterns of time and space in testimony and action constructions.

In this chapter, our aim has been to provide a general overview of the development and use of life history method. In a number of places, anticipated issues and themes which will be revisited in subsequent chapters. We now move on to consider pragmatical issues by looking at techniques for doing life history.

2 | Techniques for doing life history

Introduction

The sections dealing with practical procedures and considerations have been placed towards the beginning of the book because we believe that an awareness of what life history research and data can be like will help to inform and illustrate subsequent discussions focusing on more theoretical matters. Having said this, we do want to emphasize a point to which we will regularly return: namely, that we see any separation of theoretical, methodological, practical, epistemological, ethical, ontological (and so on) aspects of research to be in essence artificial. All are, or should be, a considered part of any researcher's whole philosophy of, and approach to, their work. This is true regardless of the paradigm they adopt or methods they use. Addressing each aspect individually, however, does help to provide an organizational framework for comprehension and enables readers to refer more easily and quickly to specific issues or areas. Similarly, whilst at times we may adopt a sequential approach in describing the various stages and phases involved when doing life history research, we know that research does not often happen in a neat, linear manner, and nor is this necessarily desirable.

We are also keen to make it clear that we do not believe that there is only one, 'proper' way of doing life history research. Different projects will have their own features and requirements and each researcher is likely to have their own personal style and a unique emotional engagement with any particular project (see Coffey 1999). Indeed, the extent to which life history methodology is individualistic and personal, relying as it does on 'intensely idiosyncratic personal dynamics' (Sikes *et al.* 1996: 43) is a defining characteristic of the approach. This does mean, though, that it is a methodology that cannot easily be taught 'because . . . personal dynamics are themselves unpackagable' (*ibid.*: 44). An important implication of this is that not everyone can or should do life history research or, as is discussed in a later chapter, can or should use life history to investigate particular topics at particular

'should not', then they are unlikely to succeed for the following reasons:

- 1 They are unlikely to develop the sort of relationship with informants that tends to lead to 'quality' data.
- 2 They are unlikely to be sufficiently sensitive to the central tenet of the approach – that, potentially at least, all aspects of life interact with and have implications for each other – to be able to make insightful use of the data.
- 3 They may even have a negative effect (to a greater or lesser degree, and in a variety of possible life areas) on their informants.

It may seem somewhat contradictory to be writing this book having raised these points because we seem to be saying: 'if you have not got the right sort of personal characteristics then you cannot do this type of research'. While we stand by our claim that not everyone can do life history work, we believe that people can develop and improve their practice by learning from the experiences of others as presented here. Furthermore, one of our key aims is to alert readers to these sensitive issues and, thereby, provide them with a basis for deciding whether or not it is an approach they consider to be appropriate to their particular interests, personality and life stage.

As a general rule, life history research is more likely to appeal to the incurably curious who are interested in, and fascinated by, the minutiae of other lives, and particularly in how people make sense of their experiences and of the world around them. At the risk of stereotyping (yet on the basis of an informal survey), life historians are likely to prefer novels such as: *A Suitable Boy*, *The Magic Mountain* and *A la Recherche du temps perdu*, to exciting action-packed yarns. Life history is an approach best suited to people who are able to listen attentively and beyond what is actually being said, and who can ask pertinent questions in a non-threatening manner. It demands the willingness to share one's own experiences, if this seems appropriate, and, of supreme importance, it requires the researcher to be the sort of person that people want to talk to.

Reasons for choosing research approaches and methods

Personal preferences and predilections aside, the key reason for using any research method has to be that it is the most appropriate one, the one most likely to produce data which address, answer or otherwise meet and fulfil the questions, aims and purposes of a specific enquiry. Methods also have to be feasible in terms of time, cost, resources, and within the various parameters of particular research contexts. Tempting though it may be to indulge oneself, as Robson (1993: 26) notes: one of the things that 'unsuccessful research starts with ... [is] Method or technique. Using it as a vehicle to

carry out a specific method of investigation'. Research which is 'method-led' can be uneconomical, inappropriate and unjustifiably biased.

However, aficionados of life history would argue that the method can be used effectively to provide useful data on practically every social issue and aspect of life: as we have already noted in Chapter 1, Thomas and Znaniecki (1918–1920) suggested that personal life records do appear to constitute the perfect form of sociological data. To take a slightly ridiculous example, the approach could be used to find out why people buy a specific brand and type of frozen peas. For instance, life history interviews can reveal that some people just pick the nearest package out of the freezer because they have little time to shop, are in a hurry and, because they have a demanding and well-paid job, do not have to worry about cost; others may choose a brand because their mother always bought them and impressed upon her children that these were the best peas to have. Thus, buying Bird's Eye™ petit pois may be tied up with conceptions of what good mothers do based on positive childhood experiences. Pragmatically, brand choice may be to do with where people live and their access to certain shops carrying particular ranges. In 'poorer' areas, for example, choice tends to be more restricted. Buying a supermarket's own brand, as opposed to Bird's Eye, may be to do with economic circumstances and, thereby, occupational status. Alternatively, as a result of advertising campaigns, shoppers might see a certain brand as fitting in with their lifestyle, or their lifestyle aspirations. And, similarly associated with advertising, their children may put pressure on them to buy fashionable brands, perhaps because they want to present a particular image to friends who come to tea. We may say this is a ridiculous example, but frozen food manufacturers no doubt do conduct biographical type research, often using focus groups in an attempt to achieve maximum sales.

Which research method to use depends on what you want to know. If you are a shopkeeper, then your only concern may be to know what will sell best and, therefore, what you should stock. A straightforward and simple survey to find out the most popular brand would, therefore, meet your needs effectively and economically.

When the focus of enquiry is something more far-reaching and significant than a consumer-preference issue, when it is something like why someone becomes a teacher, or how they cope with imposed change, or why they adopt a particular pedagogical style, or how being a teacher fits in with other aspects of a person's life such as parenthood, or what it means to be a gay or lesbian teacher, or a teacher from an ethnic minority group, the potential of life history is enormous. There are likely to be many influences, experiences and relationships within any teacher's life which have led to their developing a particular philosophy of education and taking on a specific professional identity which informs their work. Then there are the various contexts and conditions within which teachers have to work which further

front biography'. If the researcher wants to know 'why', 'how', 'what's it like' and 'what does it mean to you', then they may be well advised to include life history methods among their modes of enquiry. If they want to know 'who', 'where', 'how many' and 'what kind', then it would probably be unnecessary and uneconomical in terms of time and resources to embark on detailed interviewing, resulting in vast quantities of data which have then to be transcribed and analysed.

Of course, life history does not have to be an either/or approach. If circumstances allow, and if it is appropriate, then life history can be combined with other methods to provide yet another perspective on a topic. For instance, a study of how a particular subject – let us take religious education – is taught may use surveys, observation, analysis of textbooks and initial and in-service teacher education syllabi to see trends, patterns and frequencies in curriculum content and pedagogical style; it may also use life history interviews to explore whether and how a teacher's own experiences of religion generally, and religious education specifically, might impact upon how they perceive, experience and relate to their work.

As with all types of research, having decided on a focus and on the appropriate approach, there are a number of practical issues which have to be considered and dealt with. It is these to which we now turn.

Research populations

Sample size

Research samples for life history research are usually quite small. Interviewing, transcription and analysis are time consuming and expensive activities. When there is only one researcher, working on a personal, unfunded project, the resources to interview large numbers of people are rarely available – and this is often the case, because one of the difficulties with life history is persuading funders of its appropriateness. As well as the quantitative predilections that are common, this problem is related to some of the historical battles fought over the legitimacy of life history, especially in Chicago in the 1940s (see Chapter 1). Regardless of economic considerations though, life historians usually, although not inevitably, use life history because they take a particular epistemological position which values the subjective, emic and idiographic (see Goodson 1992b: 9). Thus, they may well argue that large samples are unnecessary and even inappropriate because objective, etic and nomothetic generalization is not the ultimate aim.

The fact that life history samples tend to be small, allied with the sort of philosophical and, therefore, epistemological stance that life historians often

take, means that they will rarely talk in terms of samples or research populations, and almost never of subjects. 'Respondent', 'informant', 'participant', or just pseudonyms, are likely to be preferred because they do not have the same 'othering' and homogenizing implications that the traditional research designations do. When, as is usually the case, the research is collaborative, or at least has an interactive dimension, then co-researcher, collaborator, or research partner may be the terms used.

It is impossible to say how many informants should be involved in any project. So much depends on the aims of the research, on the topic, and on what is actually possible. Many life histories, including most of the 'original', 'foundational' or 'germinal' ones, undertaken by members of the Chicago school in the 1920s and 1930s, were of one person and aimed to give detailed insight into a specific individual's perception and experience of their life. A study of one individual is rare in education, partly because of the essentially social and collective nature of the enterprise, although the work of Elbaz (1983), Wolcott (1983) Bullough (1989), Sparkes (1994) and Bullough and Baughman (1997), are among the notable exceptions which demonstrate the value of such research.

If the aim is to reveal shared patterns of experience or interpretation within a group of people who have some characteristic, attribute or experience in common, then ideally sample size will be adequate when:

sufficient data have been collected and saturation occurs and variation is both accounted for and understood . . . In qualitative research, the investigator samples until repetition from multiple sources is obtained.

(Morse 1994: 230)

Thus, adequacy is dependent not upon quantity but upon the richness of the data and the nature of the aspect of life being investigated. Researchers should, however, try to ensure that they include some negative or discrepant examples. Pat's (Sikes 1997) research, which focused on teachers' perceptions of the ways in which parenthood had influenced all aspects of their professional lives, involved 25 informants. This was quite a large group because she wanted to include male and female teachers of various ages from as many sectors of the educational system as possible. However, without exception, every single one of them talked about how their feelings for their students changed once they had their own child so, with regard to this theme, saturation could have been considered to have been attained after five or six interviews.

Bertaux (1981) has written about the discovery of the saturation process. In his work on bakers, he describes how they became aware of the point at which saturation had been achieved:

while we were conducting our fieldwork, however, we came to realize that a process was taking shape, which seemed to indicate that we had

the story was confirming what the preceding ones had shown. Again and again we were collecting the same story about poor, usually rural backgrounds, about very hard exploitation and training during apprenticeship; about moving from village to town, from town to city, from city to Paris (of course this last feature was to be expected). Again and again we heard about some specific health problems – which many workers, especially the young ones, related to their own physical constitution instead of to their working conditions. And despite our efforts we still could not find a single adult bakery worker born in Paris or even in its suburbs. What was taking place was a process of *saturation*: on it rests the validity of our sociological assumptions. One life story is only one life story. Thirty life stories of thirty men or women scattered in the whole social structure are only thirty life stories. But thirty life stories of thirty men who have lived their lives in one and the same sector of production (here bakery workers) represent more than thirty isolated life stories; taken together, they tell a different story, at a different level: the history of this sector of production, at the level of its pattern of sociostructural relationships. A single life story stands alone, and it would be hazardous to generalize on the ground of that one alone, as a second life story could immediately contradict those premature generalizations. But several life stories *taken from the same set of sociostructural relations* support each other and make up, all together, a strong body of evidence.

(Bertaux 1981: 187, original emphasis)

Sample selection

Life history research rarely involves a random sample of informants. For start, the sole aim is seldom to make generalizations and so, therefore, such a group is not required. More particularly, however, it is essential that informants are prepared and able, in terms of both time and articulacy, to talk for extended periods. In addition, the research topic is likely to be focused on a specific social situation, thus requiring informants to have the appropriate knowledge and experience (Erben 1998b: 5). Consequently, sampling is frequently one or more of the following types:

- 1 *Purposive*. The research is concerned with specific characteristics, attributes or experiences and informants are 'selected' because they meet the criteria.
- 2 *Opportunistic*. For example, by chance the researcher meets someone who volunteers or who is willing to be an informant.
- 3 *Convenience*. The researcher has easy access to the informants.

- 4 *Snowball*. The researcher works with an informant who tells them of friends or colleagues who might be prepared to participate.
- 5 *Homogeneous*. Everyone who has a common experience, attribute or characteristic. This is likely to occur only when the research focuses on a small group, for example all the black women teachers who belong to a group within a local education authority (Rakhit 1999).
- 6 *Extreme case*. When the informant's characteristics, attributes or experiences are strikingly different from or in some other way noteworthy compared with others in the potential research population.

As with all types of research, researchers need to think carefully before embarking on a study which involves colleagues, friends, acquaintances or relatives. This is, perhaps, especially pertinent here, given that life history work is likely to involve a non-probability sample. Doing research 'in your own backyard' can have unintended consequences with implications going far beyond the data that are collected. For all sorts of reasons, informants may be cautious about what they reveal, and this can be especially so when they are already in some sort of relationship with the enquirer. When the research solicits information of a personal nature, the potential 'power' that such knowledge gives to the researcher can be considerable: as Madelaine Grumet (1991: 69) notes: 'telling a story to a friend is a risky business; the better the friend, the riskier the business'.

It is possible to take the view that non-probability sampling is biased but, in any case, the concept of bias, and particularly of bias as being negative in a research context, is contested by many qualitative researchers generally, and by life historians in particular (see various authors in Denzin and Lincoln (1994a), for example). We would argue that all human knowledge and experience as expressed through verbal accounts is in essence biased. Everyone sees the world through frames of reference which are developed as a result of their possessing particular attributes, or being situated in particular social, historical, geographical, political, religious (or whatever) contexts which, consequently, lead to various and differing experiences. Researchers have to be reflexive in accounting for their own biases, and reflective and enquiring in identifying possible biases in their informants' stories. Rather than seeking to pretend that any aspect of research can ever be bias free, our recommendation is to acknowledge bias and make every attempt to indicate where it may occur. (For further discussion on these issues, see Shacklock and Smyth 1998; Hammersley 2000.)

Negotiating access and participation

Having identified potential informants, the next stage is to invite them to take part. This may seem a relatively straightforward matter but it does raise

a number of questions and issues. Perhaps the most significant of these concerns the research bargain: that is the understanding between the researcher and the informant about what the nature of their relationship is and what each can expect from their mutual participation (see Measor and Sikes 1992: 213; Goodson and Fliesser 1994). Of course, this varies from study to study but, in some cases, participation can involve a considerable commitment on the part of the informant. In the first place, life history interviews can take up many hours. For instance, Bascia (1996: 5) reports a project where informants were interviewed for between 2 and 24 hours. Thus, her work with one male immigrant teacher involved twelve sessions, each of, on average, 2 hours' duration followed by journal and reflection entries that constituted further data. Sikes *et al.*'s (1985) life history study of art and science, secondary school teachers involved informants in two to seven interviews, each lasting, on average, 1 to 1½ hours. Pat's (1997) parent teachers talked for, on average, 4 hours each. Researchers may be reluctant to say that so much time is involved for fear of putting people off. In any case, given that so much depends on the relationship that is developed and on the loquacity of the informant, it is not easy to tell with any accuracy before the interviews start, how long they are going to take. Furthermore, when initial contact with potential informants is made by letter or over the telephone, it may not seem appropriate or be possible to go into detail. Perhaps the best and most honest policy is simply to explain the nature of the work and that it can stretch over a considerable period, and leave it at that, giving the informant the assurance that they can quit at any time.

A similar question about how much to reveal, concerns the potential consequences of reflecting on and talking about one's life to an interested yet dispassionate listener. Undoubtedly, there are some similarities between Rogerian counselling and life history interviewing, in that interviewers, like counsellors, listen, reflect back, ask questions which encourage further reflection, and are non-judgemental. Both are also often dealing with intimate aspects of life. However, researchers are not (usually) counsellors: they are researching, not practising therapy (see Butt *et al.* 1992; Goodson 1992a). Yet, these characteristics of the approach can have implications for researchers and informants, and, occasionally, being involved in life history research can have life-changing effects. However, given that such outcomes are not common, it may be acceptable not to raise the possibility of anything of this kind happening to any particular informant. Also, these things are not predictable (or not predictable to researchers who initially are unlikely to be aware of events in an informant's life which may make them prone to radical life change) so, again, talking about it may be unacceptably pre-empting the issue. What is important is that, before they start work, researchers should have thought about the possibilities, should know that some informants may take advantage of the therapeutic potential of life

history interviews, and should have considered their basic human responsibilities to other people (see Goodson 1992c: 245-8; Measor and Sikes 1992: 226). They need to be emotionally sensitive and intelligent and should exercise caution. Ethical questions and issues arising from relationships between researchers and informants are dealt with in detail in Chapter 6.

When the research has an essentially collaborative nature and involves informants as co-researchers, perhaps when a key focus of the project is professional and personal development, then exactly what is required of them has to be spelt out. Everyone has a notion of what research is, of what researchers want and expect, and of what research 'subjects' do. Often, this notion is based on ideas associated with 'traditional' research within the modernist paradigm. If informants come to a project with this notion colouring their expectations and responses, then misunderstandings can arise. Most researchers, particularly those who use interviewing, are used to informants saying something along the lines of, 'I don't know if this is relevant', or 'I'm not sure that this is the sort of answer you're looking for'. Responses of this kind reveal the influence that informants can inadvertently have on data through their eagerness to please. Clarity is, therefore, of the essence.

It is a good idea to give informants a written document to which they can refer, setting down expectations, 'rules', clauses and so on. If there is to be any deviation from this initial agreement, then the onus is on the researcher to negotiate the change. Such a document might cover the following areas:

- 1 Confidentiality and anonymity: the researcher should be clear about who is going to listen to tape recordings, have access to interview transcripts and other types of data and so on. They should explain how they are going to disguise, anonymize or otherwise protect the identity of informants. An approach which often proves popular is giving people the opportunity to choose their own pseudonym.
- 2 Anything about 'work' the researcher would like the informant to do, such as keeping a diary or writing accounts of particular experiences.
- 3 'Ownership' of any tapes and transcripts.
- 4 The informant's 'rights' to change, comment on, contribute to analysis and the eventual presentation of findings.
- 5 Where and when interviews will take place.
- 6 Contact numbers and addresses.

Strategies for collecting data

Interview-conversations

A one-to-one interview-conversation between informant and researcher is perhaps the most commonly used strategy for collecting life history data

(Goodson (2001) has referred to this as a 'grounded conversation'). Definitions of research interviews usually put the emphasis on their being conversations with the *purpose* of eliciting the information that the researcher wants (see, for example, Denscombe 1984; Powney and Watts 1987: vii; Robson 1993: 228–9; Fontana and Frey 1994), and various strategies and techniques are advocated for achieving this aim. Most of these strategies and techniques are concerned with establishing and maintaining a positive and trusting relationship between interviewer and informant, which takes us back to what we said at the start of the chapter about the importance of personal dynamics in life history work. Thus, researchers are advised to share their own experiences and perceptions (Oakley 1981), and to establish common ground through the clothes they wear, the interests they profess, the company they are seen to keep, the language they use and how they present themselves. In subsequent chapters we consider the ethics of manipulating relationships in order to get 'good' data, as well as look at epistemological issues around the 'truth' and validity of the accounts that people give in a research context.

Our general preference is for relatively unstructured, informal, conversation-type encounters. Of course, much depends on the particular focus of the research in general and, specifically, on the topics to be covered in a particular session, but – and this is a key characteristic of the approach – a researcher can never know for certain which experiences have been influential and relevant in a particular sphere of life, for sometimes connections are apparent only to the individual concerned. Conversely, it may be that events, experiences or personal characteristics, which the researcher expects to have been important, are not seen in the same way by the informant. Too tight a structure and schedule, and relevant information may be lost or, alternatively, may be given disproportionate emphasis by the researcher. 'On one level, perhaps, life historians have to accept that people tell the story that they, for whatever reason, want to tell to the person who is listening' (Sikes *et al.* 1996: 51).

Informants also respond variously to different approaches. Some prefer to be given detailed prompts, whereas others are quite happy to take their cue from key words or phrases.

Group work

The intensity and intimacy that is usually involved in life history research means that the study is generally carried out by the researcher and an informant working together. On occasion, group work can be used either as an additional strand of a project and perhaps focusing on a specific area, or with a collaborative and/or developmental aim.

Much depends upon the relationships between the various individuals who constitute a specific group and there is no way in which a researcher

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convening a group can know how the dynamics will work on any particular occasion. It may be that people are more likely to be frank and open with people that they do not know than they are with friends or colleagues. Equally, it is possible that a group who share a common experience will take a great deal of shared understanding for granted and will, therefore, leave out significant, or useful, explanatory details. Researchers have to try to consider the range of eventualities as thoroughly as possible.

If the relationships and consequent dynamics are conducive, group work can be very productive, in that accounts given by one person may jog others' memories about similar or contrasting experiences or perceptions. Having a number of people from different backgrounds and with different perceptions can mean that a wider range of questions is asked than if the researcher had been working alone.

The work of Frika Haug and her colleagues (1999) on collective memory work is especially important as a model for life history workers. Frika *et al.* have developed this memory work in collective settings as a way of investigating feminist topics, especially female sexualization. The use of these techniques in educational settings is long overdue, but as yet little work has been undertaken. A valuable exception are the Australian studies undertaken by Fitzclarence (1991). He has employed collective memory work as a strategy in involving student teachers in understanding the patterns of authority as they undertake the transition from the role of student to that of fully fledged teacher.

Pat Sikes and Barry Troyna (1991) used a group approach in their life history work which investigated student teachers' experiences and perceptions of schooling. The students first worked in triads, alternating the roles of interviewer, informant and recorder, and then came together for a full group (containing up to 30 participants) discussion. This was a particularly productive use of the approach because group members' experiences of schooling were so diverse. For example, between them, they had attended a vast range of types of school in a variety of countries. They were of different ages and belonged to different socioeconomic, ethnic and religious groups, and all of these differences were reflected in their perceptions, experiences, assumptions and expectations. Not only did the group-work have benefits for the researchers, it also contributed to the professional development of the student teachers, broadening their awareness and challenging their taken-for-granted understandings.

Group-work may also have a part to play in 'prosopographical' research. Prosopography is collective life history which aims to investigate 'common background characteristics of a group of actors in history by means of a collective study of their lives' (Stone 1987: 45). Since prosopographical research is historical, its sources are, primarily, documentary, but there may be occasions when oral evidence can be collected and group discussions could be useful.

A useful way to start life history research is by inviting respondents to construct a time-line of key events in their life with, if appropriate, an emphasis on those experiences which relate to any focus the project may have. This can be done prior to the interview and is useful in prompting memories and concentrating attention. The time-line can then be used as a structure for interviews, and to alert the researcher to experiences or phases of life which it might be productive to explore. Time-lines can be developed and expanded as the research progresses: alternatively they could be used just for their prompting value.

The sort of information that time-lines could touch on includes:

- Place and date of birth.
- Family background, birthplace and date.
- Parents' occupations during the informant's life; general character and interests.
- Brothers' and sisters' place and date of birth; occupations or school location; general character and interests.
- Extended family; occupations and character.
- Informant's childhood: description of home and general discussion of experiences.
- Community and context: character and general status and 'feel'.
- Education, preschool experience, school experience: courses taken, subjects favoured, credentials achieved; general character of school experience; peer relations; teachers; 'good' and 'bad' experiences.
- Occupation, general work history, changes of job, types of school, type of position.
- Marriage and own family: dates and locations.
- Other interests and pursuits.
- Future ambitions and aspirations.

(For a more detailed definition of life history time-lines, see Goodson 2001)

Journals, diaries and other personal writings

Journals or diaries kept by informants can be an extremely rich, although not unproblematic, source of data. As Woods notes:

why should somebody keep a diary? Hardly ever, I suspect, to preserve an objective view of facts. More likely it is to be for reasons like personal satisfaction in wishing to remember interesting events that have brought pleasure; or as a kind of celebration of self in annotating one's deeds, lest one forget; or as an apologia; or a kind of therapy in working one's way through a series of events that have brought personal diminishment, pain or embarrassment; or with a view to later publication and

public view. So one needs to know the basis on which the diary has been compiled.

(Woods 1986: 107–8)

Researchers using a life history approach are unlikely to be seeking an objective account but, acutely conscious of the need to contextualize and to know 'the basis' on which diaries are written, addressing the sorts of concerns Woods raises will probably be built into the research design. Similarly, such researchers are likely to interrogate and take an analytical approach to the language that informants use in both their written and oral accounts. And if they are not, then they should be.

In most cases, though, having access to diaries and journals will be a bonus, unless, that is, the researcher uses them as a primary source of data and asks informants to keep them for the duration of the project. In their 1991 study which focused on the socialization of new teachers, Bullough *et al.* used journals as one strand of their year-long case study approach. Unfortunately, but perhaps not surprisingly:

early on in the year a problem surfaced with the journals . . . perhaps not trusting us, the teachers wrote the journals for us, and not for themselves . . . *another problem was* not all of the teachers continued throughout the year to keep up their writing. Indeed, two of the teachers found journal writing to be a source of increased anxiety and frustration and by mid-year had stopped writing all together. The other teachers found in journal writing a useful means for thinking about and making sense of their experience and faithfully maintained them.

(Bullough *et al.* 1991: 15)

Journal writing is clearly an activity which some people take to more easily than others. Some find it an extremely useful device for personal and professional reflection and development (cf. Holly 1989; Weiner and Rosenwald 1993); others quite the opposite. In their ongoing research, Judith Everington and Pat Sikes asked religious education students to keep journals and had a similar response to that experienced by Robert Bullough and his colleagues. In the first year of their study, whilst their informants were taking their Post-Graduate Certificate of Education qualification, Everington and Sikes were able to build written assignments into the course which they also used as data. Some of these assignments were assessed and, to meet accreditation requirements, had to take a relatively traditional academic format. The potential influence that these factors had on both the content and style of what was written had to be taken into consideration when it came to analysis.

As well as making use of journals and diaries as data sources, researchers are well advised to keep their own research diary, recording such things as who has been seen, what has been read, trains of thought, hunches and so

tactical information, it can also help with analysis and interpretation, in that it can jog memory and indicate patterns and trends which might have been lost if confined to the mind.

Using documents

Documents of various kinds, including syllabi, prospectuses, school reports, agenda, memos, letters, publicity material, school magazines, newspaper accounts and programmes of events, may cast further light on the life or lives being considered. Sometimes, researchers are able to collect relevant documents for themselves, but often they have to rely on informants to produce them. An interesting example of the use of supplementary documentation was provided by one of Pat's students who had undertaken a life history study of two 'delinquent' pupils. One of these pupils had attended an expensive independent girls' convent school. Publicity material and magazines produced by the school made it clear that girls were expected to conform to particular standards and types of lady-like behaviour. Correspondence between the school and the pupil's father, and her yearly reports during the time she was at the convent, documented the increasing mismatch between what the girl did and the school's expectations. The last straw was reached when the girl got very drunk whilst on a trip to the theatre. The final letter from the headteacher gave all the details and culminated in a request that the girl be withdrawn immediately.

Working with life history data

Recording data

Life history interviewing requires concentration. 'Listening beyond', picking up on clues and hints about what might be a productive line of enquiry, simply knowing what someone has said, all depend on the interviewer giving their total attention to the conversation and the social situation generally.

This means that most life historians prefer to use tape recorders rather than rely solely on notetaking. Inevitably, taping is not without its problems. Machines break down or have faults, batteries fail, power cuts happen, people speak softly and extraneous sounds make it difficult to hear clearly. Then there is human error of various kinds. Most researchers have had the experience of forgetting to switch the machine on to record. For this reason, and also in order to provide an aide-memoire it is good practice to make some brief notes as well.

Researchers also need to consider the extent to which using a tape recorder influences the nature and content of what informants say. Some

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people may be inhibited by the knowledge that their words could 'come back to haunt them', and there are those who find it extremely difficult to speak fluently in the presence of a tape recorder. Often, in the course of recorded interview-conversations, people will ask for the machine to be switched off while they talk about a 'sensitive' issue or make comments about a particular person. Then there are those exchanges which take place before or after the interview has started. Thus, researchers frequently hear things relating to their project which are not part of their formally, or officially, collected data. This raises the question of what one should do with such information. If an informant has said something off the record and, what is more, has made it clear that they do not want their words to be attributable to them, then ethically the researcher should not ignore the request.

It can be argued that using a tape recorder introduces an element of artificiality into the situation. However, unless research, of whatever type, is undertaken covertly – an unacceptable approach for life history work – it is bound to be 'artificial'. Researchers and informants alike come into the research situation with certain expectations and preconceptions. These may include that interviews are recorded and that researchers can be trusted to use recordings responsibly. In our experience, the benefits of taping usually far outweigh any drawbacks, and most informants are, or become, reasonably comfortable with its use.

It is important to be aware that a recording only captures what is said, it cannot be a perfect, total and faithful representation of an interview, and even video recordings can only ever be partial. ✓

Although, in our view, tape recording is to be recommended, there may be times when it is not possible. In these cases, the onus is on the researcher to make it clear that they are not working directly from the informant's words. |

Transcribing

Having made a recording, the next stage is usually to make a written summary or complete transcript. This stage is time consuming and can be expensive in terms of transcription costs. There is no doubt that doing your own transcribing enables you to become familiar with the data. It can also aid analysis in that ideas and themes can emerge or be developed as a consequence of repetitive listening and intimate engagement with the data. However, even if someone else does the transcription, researchers should listen to the tape and follow the script to ensure that there are as few errors as possible. Such close listening is important because intent and meaning are conveyed as much through how things are said as through the actual words that are used. Annotations concerning tone of voice (and, if they can be remembered, or if a note has been made, body language and gestures) can add considerably to subsequent readings and interpretations.

... using key words and phrases. It is important that a note is made of whereabouts on the tape particular things are said in order to facilitate verbatim transcription, if required, at a later date. Playback should, therefore, always be on a machine with a counter (this is a specification to bear in mind when buying new equipment). Making a summary transcript is, inevitably, analytical because it involves making decisions about how particular utterances are classified. Researchers should ensure that they note enough of what was said to enable them to make alternative interpretations if appropriate.

Analysis

Traditionally, following the positivistic paradigm of research, analysis of evidence took place at a particular stage of the research process: namely, after all the evidence had been collected and processed, in whatever form and by whatever means. This timing was to avoid the introduction of any contaminating bias. The extent to which neutrality of this kind is possible, or even desirable, is debatable (does/can anyone ever embark on any social research completely free of expectations or assumptions?), and life historians tend to the view that analysis begins as soon as they start working with an informant. Interview-conversations are not tightly structured and researchers will take opportunities to check out ideas, themes and thoughts as they proceed.

Analysis is about making sense of, or interpreting, the information and evidence that the researcher has decided to consider as data. This usually involves fitting the evidence and information into a framework of some kind. This framework may take the form of classifications, categories, models, typologies or concepts. The nature and origins of the framework and the extent to which it can be demonstrated that the evidence does actually fit and, thereby, the explanation holds, has been the central, the defining, research task. In the next chapter, we go on to consider epistemological questions of interpretation which focus on the relationship of life history accounts to the lives they represent. At this point, though, we do want to note that what constitutes a framework can be variously interpreted and, therefore, that it is up to the researcher to be explicit about their particular position. In itself, any story or life narrative is a more-or-less structured and ordered framework, regardless of whether it is someone relating their own life or a researcher retelling other people's (albeit through their own frame). In our view, the following observation applies to all parties involved in any narrative enterprise:

Narratives select the elements of the telling to confer meaning on prior events – events that may not have had such meaning at the time. This is

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a narrative transposition of Kirkegaard's famous statement that we live life forwards but understand it backwards. In understanding ourselves, we choose those facets of our experience that lead to the present and remain our life story coherent. Only from a hermeneutic position are we poised to study the genesis and revision of people making sense of themselves.

Narrative models of knowing are models of process *in process* ... personal narratives describe the road to the present and point the way to the future. But the as-yet-unwritten future cannot be identified with the emerging plot and so the narrative is revised.

(Josselson 1995: 35)

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Increasing awareness of researcher reflectivity and reflexivity means that more people take the view that interpretations/explanations/analyses are, inevitably, coloured and shaped by a range of influences, not least of which is the background, interests, in short, the biography, of the researcher. It is for this reason that some commentators have gone as far as suggesting that 'research biographies' should be compulsory because they provide readers with more evidence by which to evaluate research accounts (see Ball 1990, and various authors in Denzin and Lincoln 1994a). Indeed, since the 1980s, it has become common practice for qualitative researchers in general to 'write themselves into' their research, on the grounds that personal, background information will enhance the rigour of their work by making potential biases explicit (see, for example, Atkinson 1990). Whether or not it actually can do this is open to question (see Troyna 1994). In some cases there may be an element of what Mary Maynard (1993: 329) describes as 'vanity ethnography', that is when a researcher tells their story in a desire for self-publicity as much as, or more than, to support their work. It may also sometimes be true that, as Cotterill and Letherby (1993) suggest, some researchers do it in the hope that introducing a personal element will protect them from criticism. In life history work, where informants' lives are revealed, perhaps it is only 'fair' that researchers' lives are too – at least, in so far as what is told is really relevant to the project in question. Then, there is also the issue of actually and explicitly giving voice to the researcher themselves which is, in effect, a further acknowledgement of the 'polyvocality of social life' (Coffey 1999: 118; see also Ellis and Bochner 2000). It would, perhaps, be inconsistent to fail to acknowledge that the researcher's voice is there among all others, especially if claims are made for the egalitarian broadcasting properties of the approach.

Analysis using computer programs

In recent years, an increasing number of computer programs (for example Atlas-ti, Ethnograph, Kwalitan, Nud.ist) which analyse qualitative data

have become available. We shall do no more than refer to their existence and note that there is considerable controversy over the extent to which they can aid analysis, as distinct from their obvious capabilities when it comes to the storage and retrieval of data (for further discussion see Denscombe 1998: 218–22; Blaxter *et al.* 1999: 132–6; Miller 2000: 150–3).

The way in which researchers go about analysing evidence is, in itself, influenced by the epistemological and philosophical position they take. Whether they use analytic induction, constant comparative method, grounded theory, varieties of content or discourse analysis, thematic field analysis or some other approach, it is important that researchers are explicit about their practices and their reasons for them.

Respondent validation

Peter Woods (1996: 40) defines respondent validation as ‘insiders confirming the correctness of analysis’. Yvonna Lincoln and Egon Guba (1985) argue that the standard for qualitative work is reached if informants find researchers’ interpretations of their perceptions, experiences, or whatever it is that is being investigated, credible. However, as Norman Denzin (1970) has pointed out, it is not always that straightforward because informants might not like the interpretation or, if it is couched in specialist language, they may not recognize or understand it. Bev Skeggs, for example, reports that when she passed draft chapters and articles on to informants involved in one project she worked on, the most common response was, ‘Can’t understand a bloody word it says’ (Skeggs 1994: 86).

Since life history work is so often collaborative, with researcher and informant seeking meanings and explanations together, respondent validation may well be built into the research design. If it is not, it is usually a good idea to ask informants what they think about any analyses or written accounts. However, researchers should bear in mind that, sometimes, informants do not expect or even want to be involved in any way other than by being interviewed. Researchers may find that, having passed transcripts and papers on to informants for their comments, they hear no more. On following up, they are likely to find that lack of time, or acceptance of the researcher’s ‘specialist’ position is the usual reason given for non-response.

Researchers should also consider what they would do if informants disagree or ask for alterations. In some cases, the researcher may believe such changes to be appropriate, but in others they may not. What happens then? One way forward may be publication with a note to the effect that the informant(s) took a different view.

Data presentation

When all the relevant data have been analysed, the researcher is faced with the question of how to present their findings. (In Chapter 3, we look in more detail at some of the epistemological issues associated with turning a life story into a life history and in crafting a representation of informants' experiences and perceptions, but here we concentrate on practicalities.) So much depends upon the nature, scope and focus of any particular project, and on the type of presentation, its purpose, and the audience for whom it is intended. If it constitutes work to be submitted for a qualification (for example an assessed undergraduate project, a masters or doctoral dissertation), then there are likely to be official requirements and criteria, as well as conventional expectations to be met, and it is important for the student researcher to be clear about these.

When using data from interviews, and when the aim is to represent and reflect as closely as possible what an informant said, a key question tends to be how much direct quotation to use. Any synthesis or rewording by the researcher is a step away from the original, even when it is simply a matter of leaving out the 'ums' and 'errs'. Then there is the issue of how far the informant's words are left to speak for themselves and how much commentary and analysis there should be. Whatever decisions are made, the researcher needs to be able to justify what they have done. Thus, in some circumstances, it may be possible to leave a verbatim transcript to stand entirely alone (or with a minimum of comment) and in others, not to include any reported speech.

Some degree of editing is usual and is generally undertaken in order to support the researcher's case. This raises the issue of what is left out and why it has been omitted: an issue that is pertinent to many types of research, not just life history.

On occasion, researchers have explicitly presented their data in a fictionalized form (see Banks and Banks 1998). They may have amalgamated accounts given by a number of informants (see, for example, Clough 1999), or drawn on their knowledge of particular social situations to create a composite character. In some ways, this device is similar to describing types and there may be strong reasons for doing it. Andrew Sparkes (1995), for instance, created Alex, a gay physical education teacher, because he was unable to find such an individual who was prepared to take part in his study which focused on the sexuality of PE teachers.

In conclusion

In this chapter we have looked at some of the practicalities of doing life history research. It is important to note that there is only one way of doing

such work, and each specific project will have its own idiosyncratic needs and aspects. For this reason, we have focused on what we see as the major concerns of widespread applicability. Above all else, the decision to take a life history approach should not be made lightly. This is research that can have an emotional effect on all parties involved. Life historians have to take seriously their responsibilities to their informants and readers: only if this is done can the march through the ethical and methodological minefield be completed successfully.

Research Methods in Education

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The nature of inquiry - Setting the field

Introduction

This chapter explores the context of educational research. It sets out several foundations on which different kinds of empirical research are constructed:

- scientific and positivistic methodologies
- naturalistic and interpretive methodologies
- methodologies from critical theory
- feminist educational research.

Our analysis takes an important notion from Hitchcock and Hughes (1995: 21) who suggest that ontological assumptions give rise to epistemological assumptions; these, in turn, give rise to methodological considerations; and these, in turn, give rise to issues of instrumentation and data collection. This view moves us beyond regarding research methods as simply a technical exercise and as concerned with understanding the world; this is informed by how we view our world(s), what we take understanding to be, and what we see as the purposes of understanding. The chapter also acknowledges that educational research, politics and decision-making are inextricably intertwined, and it draws attention to the politics of educational research and the implications that this has for undertaking research (e.g. the move towards applied and evaluative research and away from 'pure' research). Finally, we add a note about methodology.

The search for truth

People have long been concerned to come to grips with their environment and to understand the nature of the phenomena it presents to their senses. The means by which they set

out to achieve these ends may be classified into three broad categories: *experience*, *reasoning* and *research* (Mouly 1978). Far from being independent and mutually exclusive, however, these categories must be seen as complementary and overlapping, features most readily in evidence where solutions to complex modern problems are sought.

In our endeavours to come to terms with the problems of day-to-day living, we are heavily dependent upon experience and authority. It must be remembered that as tools for uncovering ultimate truth they have decided limitations. The limitations of personal experience in the form of *common-sense knowing*, for instance, can quickly be exposed when compared with features of the scientific approach to problem-solving. Consider, for example, the striking differences in the way in which theories are used. Laypeople base them on haphazard events and use them in a loose and uncritical manner. When they are required to test them, they do so in a selective fashion, often choosing only that evidence that is consistent with their hunches and ignoring that which is counter to them. Scientists, by contrast, construct their theories carefully and systematically. Whatever hypotheses they formulate have to be tested empirically so that their explanations have a firm basis in fact. And there is the concept of *control* distinguishing the layperson's and the scientist's attitude to experience. Laypeople generally make no attempt to control any extraneous sources of influence when trying to explain an occurrence. Scientists, on the other hand, only too conscious of the multiplicity of causes for a given occurrence, resort to definite techniques and procedures to isolate and test the effect of one or more of the alleged causes. Finally, there is the difference of

attitude to the relationships among phenomena. Laypeople's concerns with such relationships are loose, unsystematic and uncontrolled. The chance occurrence of two events in close proximity is sufficient reason to predicate a causal link between them. Scientists, however, display a much more serious professional concern with relationships and only as a result of rigorous experimentation will they postulate a relationship between two phenomena.

People attempt to comprehend the world around them by using three types of reasoning: *deductive reasoning*, *inductive reasoning* and the *combined inductive-deductive* approach. Deductive reasoning is based on the syllogism which was Aristotle's great contribution to formal logic. In its simplest form the syllogism consists of a major premise based on an a priori or self-evident proposition, a minor premise providing a particular instance, and a conclusion. Thus:

All planets orbit the sun.

The earth is a planet.

Therefore the earth orbits the sun.

The assumption underlying the syllogism is that through a sequence of formal steps of logic, from the general to the particular, a valid conclusion can be deduced from a valid premise. Its chief limitation is that it can handle only certain kinds of statement. The syllogism formed the basis of systematic reasoning from the time of its inception until the Renaissance. Thereafter its effectiveness was diminished because it was no longer related to observation and experience and became merely a mental exercise. One of the consequences of this was that empirical evidence as the basis of proof was superseded by authority and the more authorities one could quote, the stronger one's position became. Naturally, with such abuse of its principal tool, science became sterile.

The history of reasoning was to undergo a dramatic change in the 1600s when Francis Bacon began to lay increasing stress on the observational basis of science. Being critical of the model of deductive reasoning on the grounds that its major premises were often preconceived notions which

inevitably bias the conclusions, he proposed in its place the method of inductive reasoning by means of which the study of a number of individual cases would lead to an hypothesis and eventually to a generalization. Mouly (1978) explains it by suggesting that Bacon's basic premise was that, with sufficient data, even if one does not have a preconceived idea of their significance or meaning, nevertheless important relationships and laws would be discovered by the alert observer. Bacon's major contribution to science was thus that he was able to rescue it from the death-grip of the deductive method whose abuse had brought scientific progress to a standstill. He thus directed the attention of scientists to nature for solutions to people's problems, demanding empirical evidence for verification. Logic and authority in themselves were no longer regarded as conclusive means of proof and instead became sources of hypotheses about the world and its phenomena.

Bacon's inductive method was eventually followed by the inductive-deductive approach which combines Aristotelian deduction with Baconian induction. Here the researcher is involved in a back-and-forth process of induction (from observation to hypothesis) and deduction (from hypothesis to implications) (Mouly 1978). Hypotheses are tested rigorously and, if necessary, revised.

Although both deduction and induction have their weaknesses, their contributions to the development of science are enormous and fall into three categories:

- the suggestion of hypotheses
- the logical development of these hypotheses
- the clarification and interpretation of scientific findings and their synthesis into a conceptual framework.

A further means by which we set out to discover truth is *research*. This has been defined by Kerlinger (1970) as the systematic, controlled, empirical and critical investigation of hypothetical propositions about the presumed relations among natural phenomena. Research has three characteristics in

particular which distinguish it from the first means of problem-solving identified earlier, namely, experience. First, whereas experience deals with events occurring in a haphazard manner, research is systematic and controlled, basing its operations on the inductive-deductive model outlined above. Second, research is empirical. The scientist turns to experience for validation. As Kerlinger (1970) puts it, subjective, personal belief has to have a reality check against objective, empirical facts and tests. And third, research is self-correcting. Not only does the scientific method have built-in mechanisms to protect scientists from error as far as is humanly possible, but also their procedures and results are open to public scrutiny by fellow professionals. Incorrect results in time will be found and either revised or discarded (Mouly 1978). Research is a combination of both experience and reasoning and must be regarded as the most successful approach to the discovery of truth, particularly as far as the natural sciences are concerned (Borg 1963).¹

Educational research has absorbed several competing views of the social sciences – the established, traditional view and an interpretive view, and several others that we explore in this chapter – critical theory, feminist theory and complexity theory. The established, traditional view holds that the social sciences are essentially the same as the natural sciences and are therefore concerned with discovering natural and universal laws regulating and determining individual and social behaviour; the interpretive view, however, while sharing the rigour of the natural sciences and the same concern of traditional social science to describe and explain human behaviour, emphasizes how people differ from inanimate natural phenomena and, indeed, from each other. These contending views – and also their corresponding reflections in educational research – stem in the first instance from different conceptions of social reality and of individual and social behaviour. It will help our understanding of the issues to be developed subsequently if we examine these in a little more detail (see <http://www.routledge.com/textbooks/9780415368780> – Chapter 1, file 1.1. ppt).

Two conceptions of social reality

The views of social science that we have just identified represent strikingly different ways of looking at social reality and are constructed on correspondingly different ways of interpreting it. We can perhaps most profitably approach these conceptions of the social world by examining the explicit and implicit assumptions underpinning them. Our analysis is based on the work of Burrell and Morgan (1979), who identified four sets of such assumptions.

First, there are assumptions of an ontological kind – assumptions which concern the very nature or essence of the social phenomena being investigated. Thus, the authors ask, is social reality external to individuals – imposing itself on their consciousness from without – or is it the product of individual consciousness? Is reality of an objective nature, or the result of individual cognition? Is it a given 'out there' in the world, or is it created by one's own mind? These questions spring directly from what philosophy terms the nominalist–realist debate. The former view holds that objects of thought are merely words and that there is no independently accessible thing constituting the meaning of a word. The realist position, however, contends that objects have an independent existence and are not dependent for it on the knower.

The second set of assumptions identified by Burrell and Morgan (1979) are of an epistemological kind. These concern the very bases of knowledge – its nature and forms, how it can be acquired, and how communicated to other human beings. How one aligns oneself in this particular debate profoundly affects how one will go about uncovering knowledge of social behaviour. The view that knowledge is hard, objective and tangible will demand of researchers an observer role, together with an allegiance to the methods of natural science; to see knowledge as personal, subjective and unique, however, imposes on researchers an involvement with their subjects and a rejection of the ways of the natural scientist. To subscribe to the former is to be positivist; to the latter, anti-positivist.

The third set of assumptions concern human nature and, in particular, the relationship between human beings and their environment. Since the human being is both its subject and object of study, the consequences for social science of assumptions of this kind are indeed far-reaching. Two images of human beings emerge from such assumptions – the one portrays them as responding mechanically and deterministically to their environment, i.e. as products of the environment, controlled like puppets; the other, as initiators of their own actions with free will and creativity, producing their own environments. The difference is between *determinism* and *voluntarism* respectively (Burrell and Morgan 1979).

It would follow from what we have said so far that the three sets of assumptions identified above have direct implications for the methodological concerns of researchers, since the contrasting ontologies, epistemologies and models of human beings will in turn demand different research methods. Investigators adopting an objectivist (or positivist) approach to the social world and who treat it like the world of natural phenomena as being hard, real and external to the individual will choose from a range of traditional options – surveys, experiments, and the like. Others favouring the more subjectivist (or anti-positivist) approach and who view the social world as being of a much softer, personal and humanly created kind will select from a comparable range of recent and emerging techniques – accounts, participant observation and personal constructs, for example.

Where one subscribes to the view that treats the social world like the natural world – as if it were a hard, external and objective reality – then scientific investigation will be directed at analysing the relationships and regularities between selected factors in that world. It will be predominantly quantitative and will be concerned with identifying and defining elements and discovering ways in which their relationships can be expressed. Hence, they argue, methodological issues, of fundamental importance, are thus the concepts themselves, their measurement and the identification of

underlying themes in a search for universal laws that explain and govern that which is being observed (Burrell and Morgan 1979). An approach characterized by procedures and methods designed to discover general laws may be referred to as *nomothetic*.

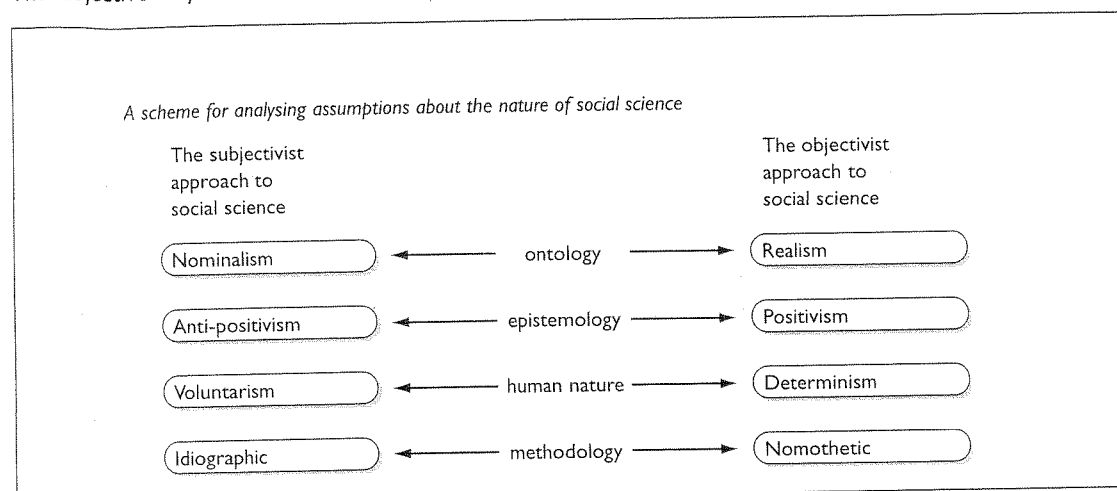
However, if one favours the alternative view of social reality which stresses the importance of the subjective experience of individuals in the creation of the social world, then the search for understanding focuses upon different issues and approaches them in different ways. The principal concern is with an understanding of the way in which the individual creates, modifies and interprets the world in which he or she finds himself or herself. The approach now takes on a qualitative as well as quantitative aspect. As Burrell and Morgan (1979) and Kirk and Miller (1986: 14) observe, emphasis here is placed on explanation and understanding of the unique and the particular individual case rather than the general and the universal; the interest is in a subjective, relativistic social world rather than an absolutist, external reality. In its emphasis on the particular and individual this approach to understanding individual behaviour may be termed *idiographic*.

In this review of Burrell and Morgan's analysis of the ontological, epistemological, human and methodological assumptions underlying two ways of conceiving social reality, we have laid the foundations for a more extended study of the two contrasting perspectives evident in the practices of researchers investigating human behaviour and, by adoption, educational problems. Box 1.1 summarizes these assumptions along a subjective-objective dimension. It identifies the four sets of assumptions by using terms we have adopted in the text and by which they are known in the literature of social philosophy.

Each of the two perspectives on the study of human behaviour outlined above has profound implications for research in classrooms and schools. The choice of problem, the formulation of questions to be answered, the characterization of pupils and teachers, methodological concerns, the kinds of data sought and their mode of treatment,

Box 1.1

The subjective-objective dimension



Source: Burrell and Morgan 1979

all are influenced by the viewpoint held. Some idea of the considerable practical implications of the contrasting views can be gained by examining Box 1.2 which compares them with respect to a number of critical issues within a broadly societal and organizational framework. Implications of the two perspectives for research into classrooms and schools will unfold in the course of the text.

Because of its significance for the epistemological basis of social science and its consequences for educational research, we devote much discussion in this chapter to the positivist and anti-positivist debate.

Positivism

Although positivism has been a recurrent theme in the history of western thought from the Ancient Greeks to the present day, it is historically associated with the nineteenth-century French philosopher, Auguste Comte, who was the first thinker to use the word for a philosophical position (Beck 1979). His positivism turns to observation and reason as means of understanding behaviour; explanation proceeds by way of scientific description. In

his study of the history of the philosophy and methodology of science, Oldroyd (1986) says:

It was Comte who consciously 'invented' the new science of society and gave it the name to which we are accustomed . . . For social phenomena were to be viewed in the light of physiological (or biological) laws and theories and investigated empirically, just like physical phenomena.

(Oldroyd 1986)

Comte's position was to lead to a general doctrine of positivism which held that all genuine knowledge is based on sense experience and can be advanced only by means of observation and experiment. Following in the empiricist tradition, it limited inquiry and belief to what can be firmly established and in thus abandoning metaphysical and speculative attempts to gain knowledge by reason alone, the movement developed what has been described as a 'tough-minded orientation to facts and natural phenomena' (Beck 1979).

Although the term positivism is used by philosophers and social scientists, a residual meaning is always present and this derives from an acceptance of natural science as the paradigm of human knowledge (Duncan 1968). This includes

Box 1.2

Alternative bases for interpreting social reality

<i>Dimensions of comparison</i>	<i>Conceptions of social reality</i>	
	<i>Objectivist</i>	<i>Subjectivist</i>
Philosophical basis	Realism: the world exists and is knowable as it really is. Organizations are real entities with a life of their own.	Idealism: the world exists but different people construe it in very different ways. Organizations are invented social reality.
The role of social science	Discovering the universal laws of society and human conduct within it.	Discovering how different people interpret the world in which they live.
Basic units of social reality	The collectivity: society or organizations.	Individuals acting singly or together.
Methods of understanding	Identifying conditions or relationships which permit the collectivity to exist. Conceiving what these conditions and relationships are.	Interpretation of the subjective meanings which individuals place upon their action. Discovering the subjective rules for such action.
Theory	A rational edifice built by scientists to explain human behaviour.	Sets of meanings which people use to make sense of their world and behaviour within it.
Research	Experimental or quasi-experimental validation of theory.	The search for meaningful relationships and the discovery of their consequences for action.
Methodology	Abstraction of reality, especially through mathematical models and quantitative analysis.	The representation of reality for purposes of comparison. Analysis of language and meaning.
Society	Ordered. Governed by a uniform set of values and made possible only by those values.	Conflicted. Governed by the values of people with access to power.
Organizations	Goal oriented. Independent of people. Instruments of order in society serving both society and the individual.	Dependent upon people and their goals. Instruments of power which some people control and can use to attain ends which seem good to them.
Organizational pathologies	Organizations get out of kilter with social values and individual needs.	Given diverse human ends, there is always conflict among people acting to pursue them.
Prescription for change	Change the structure of the organization to meet social values and individual needs.	Find out what values are embodied in organizational action and whose they are. Change the people or change their values if you can.

Source: adapted from Barr Greenfield 1975

the following connected suppositions, identified by Giddens (1975). First, the methodological procedures of natural science may be directly applied to the social sciences. Positivism here implies a particular stance concerning the social scientist as an observer of social reality. Second, the end-product of investigations by social

scientists can be formulated in terms parallel to those of natural science. This means that their analyses must be expressed in laws or law-like generalizations of the same kind that have been established in relation to natural phenomena. Positivism here involves a definite view of social scientists as analysts or interpreters of their subject

matter. Positivism claims that science provides us with the clearest possible ideal of knowledge.

Where positivism is less successful, however, is in its application to the study of human behaviour where the immense complexity of human nature and the elusive and intangible quality of social phenomena contrast strikingly with the order and regularity of the natural world. This point is nowhere more apparent than in the contexts of classroom and school where the problems of teaching, learning and human interaction present the positivistic researcher with a mammoth challenge (see <http://www.routledge.com/textbooks/9780415368780> – Chapter 1, file 1.2. ppt).

For further information on positivism within the history of the philosophy and methodology of science, see Oldroyd (1986). We now look more closely at some of its features.

The assumptions and nature of science

We begin with an examination of the tenets of scientific faith: the kinds of assumptions held by scientists, often implicitly, as they go about their daily work. First, there is the assumption of *determinism*. This means simply that events have causes, that events are determined by other circumstances; and science proceeds on the belief that these causal links can eventually be uncovered and understood, that the events are explicable in terms of their antecedents. Moreover, not only are events in the natural world determined by other circumstances, but also there is regularity about the way they are determined: the universe does not behave capriciously. It is the ultimate aim of scientists to formulate laws to account for the happenings in the world, thus giving them a firm basis for prediction and control.

The second assumption is that of *empiricism*. We have already touched upon this viewpoint, which holds that certain kinds of reliable knowledge can only derive from experience. In practice, this means scientifically that the tenability of a theory or hypothesis depends on the nature of the empirical evidence for its support. Empirical here means that which is verifiable by observation and

direct experience (Barratt 1971); and evidence, data yielding proof or strong confirmation, in probability terms, of a theory or hypothesis in a research setting.

Mouly (1978) identifies five steps in the process of empirical science:

- 1 *experience*: the starting point of scientific endeavour at the most elementary level
- 2 *classification*: the formal systematization of otherwise incomprehensible masses of data
- 3 *quantification*: a more sophisticated stage where precision of measurement allows more adequate analysis of phenomena by mathematical means
- 4 *discovery of relationships*: the identification and classification of functional relationships among phenomena
- 5 *approximation to the truth*: science proceeds by gradual approximation to the truth.

The third assumption underlying the work of the scientist is the principle of *parsimony*. The basic idea is that phenomena should be explained in the most economical way possible, as Einstein was known to remark – one should make matters as simple as possible, but no simpler! The first historical statement of the principle was by William of Occam when he said that explanatory principles (entities) should not be needlessly multiplied. It may, of course, be interpreted in various ways: that it is preferable to account for a phenomenon by two concepts rather than three; that a simple theory is to be preferred to a complex one.

The final assumption, that of *generality*, played an important part in both the deductive and inductive methods of reasoning. Indeed, historically speaking, it was the problematic relationship between the concrete particular and the abstract general that was to result in two competing theories of knowledge – the rational and the empirical. Beginning with observations of the particular, scientists set out to generalize their findings to the world at large. This is so because they are concerned ultimately with explanation. Of course, the concept of generality presents much less of a problem to natural scientists working

chiefly with inanimate matter than to human scientists who, of necessity having to deal with samples of larger human populations, have to exercise great caution when generalizing their findings to the particular parent populations.

We come now to the core question: What is science? Kerlinger (1970) points out that in the scientific world itself two broad views of science may be found: the *static* and the *dynamic*. The *static* view, which has particular appeal for laypeople, is that science is an activity that contributes systematized information to the world. The work of the scientist is to uncover new facts and add them to the existing corpus of knowledge. Science is thus seen as an accumulated body of findings, the emphasis being chiefly on the present state of knowledge and adding to it.² The *dynamic* view, by contrast, conceives science more as an activity, as something that scientists *do*. According to this conception it is important to have an accumulated body of knowledge of course, but what really matter most are the discoveries that scientists make. The emphasis here, then, is more on the heuristic nature of science.

Contrasting views exist on the functions of science. We give a composite summary of these in Box 1.3. For the professional scientists, however, science is seen as a way of comprehending the world; as a means of explanation and understanding, of prediction and control. For them the ultimate aim of science is theory.

Theory has been defined by Kerlinger as 'a set of interrelated constructs [concepts], definitions, and propositions that presents a systematic view of phenomena by specifying relations among variables, with the purpose of explaining and predicting the phenomena' (Kerlinger 1970). In a sense, theory gathers together all the isolated bits of empirical data into a coherent conceptual framework of wider applicability. More than this, however, theory is itself a potential source of further information and discoveries. It is in this way a source of new hypotheses and hitherto unasked questions; it identifies critical areas for further investigation; it discloses gaps in our knowledge; and enables a researcher to postulate the existence of previously unknown phenomena.

Box 1.3

The functions of science

- 1 Its problem-seeking, question-asking, hunch-encouraging, hypotheses-producing function.
- 2 Its testing, checking, certifying function; its trying out and testing of hypotheses; its repetition and checking of experiments; its piling up of facts.
- 3 Its organizing, theorizing, structuring function; its search for larger and larger generalizations.
- 4 Its history-collecting, scholarly function.
- 5 Its technological side; instruments, methods, techniques.
- 6 Its administrative, executive and organizational side.
- 7 Its publicizing and educational functions.
- 8 Its applications to human use.
- 9 Its appreciation, enjoyment, celebration and glorification.

Source: Maslow 1954

Clearly there are several different types of theory, and each type of theory defines its own kind of 'proof'. For example, Morrison (1995a) identifies *empirical theories*, *'grand' theories* and *'critical theory*. Empirical theories and critical theories are discussed below. 'Grand theory' is a metanarrative defining an area of study, being speculative, clarifying conceptual structures and frameworks, an creatively enlarging the way we consider behavior and organizations (Layder 1994). It uses fundamental ontological and epistemological postulates which serve to define a field of inquiry (Hughes 1976). Here empirical material tends to be used by way of illustration rather than 'proof'. This is the stuff of some sociological theories, for example Marxism, consensus theory and functionalism. While sociologists may be excited by the totalizing and all-encompassing nature of such theories, they have been subject to considerable undermining. For example, Merton (1949), Coseriu and Rosenberg (1969), Doll (1993) and Layder (1994) contend that while they might possess the attraction of large philosophical systems of considerable – Byzantine – architectonic splendour a logical consistency, nevertheless they are scientifically sterile, irrelevant and out of touch with the world that is characterized by openness, fluidity

heterogeneity and fragmentation. This book does not endeavour to refer to this type of theory.

The status of theory varies quite considerably according to the discipline or area of knowledge in question. Some theories, as in the natural sciences, are characterized by a high degree of elegance and sophistication; others, perhaps like educational theory, are only at the early stages of formulation and are thus characterized by great unevenness. Popper (1968), Lakatos (1970),³ Mouly (1978), Laudan (1990) and Rasmussen (1990) identify the following characteristics of an effective empirical theory:

- A theoretical system must permit deductions and generate laws that can be tested empirically; that is, it must provide the means for its confirmation or rejection. One can test the validity of a theory only through the validity of the propositions (hypotheses) that can be derived from it. If repeated attempts to disconfirm its various hypotheses fail, then greater confidence can be placed in its validity. This can go on indefinitely, until possibly some hypothesis proves untenable. This would constitute indirect evidence of the inadequacy of the theory and could lead to its rejection (or more commonly to its replacement by a more adequate theory that can incorporate the exception).
- Theory must be compatible with both observation and previously validated theories. It must be grounded in empirical data that have been verified and must rest on sound postulates and hypotheses. The better the theory, the more adequately it can explain the phenomena under consideration, and the more facts it can incorporate into a meaningful structure of ever-greater generalizability. There should be internal consistency between these facts. It should clarify the precise terms in which it seeks to explain, predict and generalize about empirical phenomena.
- Theories must be stated in simple terms; that theory is best that explains the most in the simplest way. This is the law of parsimony. A theory must explain the data adequately

and yet must not be so comprehensive as to be unwieldy. On the other hand, it must not overlook variables simply because they are difficult to explain.

- A theory should have considerable explanatory and predictive potential.
- A theory should be able to respond to observed anomalies.
- A theory should spawn a research enterprise (echoing Siegel's (1987) comment that one of the characteristics of an effective theory is its fertility).
- A theory should demonstrate precision and universality, and set the grounds for its own falsification and verification, identifying the nature and operation of a 'severe test' (Popper 1968). An effective empirical theory is tested in contexts which are different from those that gave rise to the theory, i.e. they should move beyond simply corroboration and induction and towards 'testing' (Laudan 1990). It should identify the type of evidence which is required to confirm or refute the theory.
- A theory must be operationalizable precisely.
- A test of the theory must be replicable.

Sometimes the word *model* is used instead of, or interchangeably with, *theory*. Both may be seen as explanatory devices or schemes having a broadly conceptual framework, though models are often characterized by the use of analogies to give a more graphic or visual representation of a particular phenomenon. Providing they are accurate and do not misrepresent the facts, models can be of great help in achieving clarity and focusing on key issues in the nature of phenomena.

Hitchcock and Hughes (1995) draw together the strands of the discussion so far when they describe a theory thus:

Theory is seen as being concerned with the development of systematic construction of knowledge of the social world. In doing this theory employs the use of concepts, systems, models, structures, beliefs and ideas, hypotheses (theories) in order to make statements about particular types of actions, events or activities, so as to make analyses of their causes, consequences and process. That is, to explain

events in ways which are consistent with a particular philosophical rationale or, for example, a particular sociological or psychological perspective. Theories therefore aim to both propose and analyze sets of relations existing between a number of variables when certain regularities and continuities can be demonstrated via empirical enquiry.

(Hitchcock and Hughes 1995: 20-1)

Scientific theories must, by their very nature, be provisional. A theory can never be complete in the sense that it encompasses all that can be known or understood about the given phenomenon. As Mouly (1978) argues, one scientific theory is replaced by a superior, more sophisticated theory, as new knowledge is acquired.

In referring to theory and models, we have begun to touch upon the tools used by scientists in their work. We look now in more detail at two such tools which play a crucial role in science – the concept and the hypothesis.

The tools of science

Concepts express generalizations from particulars – anger, achievement, alienation, velocity, intelligence, democracy. Examining these examples more closely, we see that each is a word representing an idea: more accurately, a concept is the relationship between the word (or symbol) and an idea or conception. Whoever we are and whatever we do, we all make use of concepts. Naturally, some are shared and used by all groups of people within the same culture – child, love, justice, for example; others, however, have a restricted currency and are used only by certain groups, specialists, or members of professions – idiosyncrasy, retroactive inhibition, anticipatory socialization.

Concepts enable us to impose some sort of meaning on the world; through them reality is given sense, order and coherence. They are the means by which we are able to come to terms with our experience. How we perceive the world, then, is highly dependent on the repertoire of concepts we can command. The more we have, the more sense data we can pick up and the surer will be our perceptual (and cognitive) grasp of

whatever is 'out there'. If our perceptions of the world are determined by the concepts available to us, it follows that people with differing sets of concepts will tend to view the 'same' objective reality differently – a doctor diagnosing an illness will draw upon a vastly different range of concepts from, say, the restricted and simplistic notions of the layperson in that context.

So, you may ask, where is all this leading? Simply to this: that social scientists have likewise developed, or appropriated by giving precise meaning to, a set of concepts which enable them to shape their perceptions of the world in a particular way, to represent that slice of reality which is their special study. And collectively, these concepts form part of their wider meaning system which permits them to give accounts of that reality, accounts which are rooted and validated in the direct experience of everyday life. These points may be exemplified by the concept of social class. Hughes (1976) says that it offers

a rule, a grid, even though vague at times, to use in talking about certain sorts of experience that have to do with economic position, life-style, life-chances, and so on. It serves to identify aspects of experience, and by relating the concept to other concepts we are able to construct theories about experience in a particular order or sphere.

(Hughes 1976: 34)

There are two important points to stress when considering scientific concepts. The first is that they do not exist independently of us: they are indeed our inventions enabling us to acquire some understanding at least of the apparent chaos of nature. The second is that they are limited in number and in this way contrast with the infinite number of phenomena they are required to explain.

A second tool of great importance to the scientist is the *hypothesis*. It is from this that much research proceeds, especially where cause-and-effect or concomitant relationships are being investigated. The hypothesis has been defined by Kerlinger (1970) as a conjectural statement of the relations between two or more variables or 'an educated guess', though it is unlike

an educated guess in that it is often the result of considerable study, reflective thinking and observation. Medawar (1972) writes of the hypothesis and its function thus:

All advances of scientific understanding, at every level, begin with a speculative adventure, an imaginative preconception of *what might be true* – a preconception which always, and necessarily, goes a little way (sometimes a long way) beyond anything which we have logical or factual authority to believe in. It is the invention of a possible world, or of a tiny fraction of that world. The conjecture is then exposed to criticism to find out whether or not that imagined world is anything like the real one. Scientific reasoning is therefore at all levels an interaction between two episodes of thought – a dialogue between two voices, the one imaginative and the other critical; a dialogue, if you like, between the possible and the actual, between proposal and disposal, conjecture and criticism, between what might be true and what is in fact the case.

(Medawar 1972)

Kerlinger (1970) has identified two criteria for 'good' hypotheses. The first is that hypotheses are statements about the relations between variables; and second, that hypotheses carry clear implications for testing the stated relations. To these he adds two ancillary criteria: that hypotheses disclose compatibility with current knowledge; and that they are expressed as economically as possible. Thus if we conjecture that social class background determines academic achievement, we have a relationship between one variable, social class, and another, academic achievement. And since both can be measured, the primary criteria specified by Kerlinger can be met. Neither do they violate the ancillary criteria proposed by Kerlinger (see also Box 1.4).

He further identifies four reasons for the importance of hypotheses as tools of research. First, they organize the efforts of researchers. The relationship expressed in the hypothesis indicates what they should do. They enable them to understand the problem with greater clarity and provide them with a framework for collecting, analysing and interpreting their data.

Box 1.4 The hypothesis

Once one has a hypothesis to work on, the scientist can move forward: the hypothesis will guide the researcher on the selection of some observations rather than others and will suggest experiments. Scientists soon learn by experience the characteristics of a good hypothesis. A hypothesis that is so loose as to accommodate any phenomenon tells us precisely nothing; the more phenomena it prohibits, the more informative it is.

A good hypothesis must also have *logical immediacy*, i.e. it must provide an explanation of whatever it is that needs to be explained and not an explanation of other phenomena. Logical immediacy in a hypothesis means that it can be tested by comparatively direct and practicable means. A large part of the *art of the soluble* is the art of devising hypotheses that can be tested by practicable experiments.

Source: adapted from Medawar 1981

Second, they are, in Kerlinger's words, the working instruments of theory. They can be deduced from theory or from other hypotheses. Third, they can be tested, empirically or experimentally, thus resulting in confirmation or rejection; and there is always the possibility that a hypothesis, once supported and established, may become a law. Fourth, hypotheses are powerful tools for the advancement of knowledge because, as Kerlinger (1970) explains, they enable us to get outside ourselves. Hypotheses and concepts play a crucial part in the scientific method and it is to this that we now turn our attention.

The scientific method

If the most distinctive feature of science is its empirical nature, the next most important characteristic is its set of procedures which show not only how findings have been arrived at, but are sufficiently clear for fellow-scientists to repeat them, i.e. to check them out with the same or other materials and thereby test the results. As Cuff and Payne (1979) say: 'A scientific approach necessarily involves standards and procedures for demonstrating the "empirical

warrant" of its findings, showing the match or fit between its statements and what is happening or has happened in the world' (Cuff and Payne 1979: 4). These standards and procedures we will call for convenience 'the scientific method', though this can be somewhat misleading for the following reason: the combination of the definite article, adjective and singular noun conjures up in the minds of some people a single invariant approach to problem-solving, an approach frequently involving atoms or rats, and taking place within the confines of a laboratory. Yet there is much more to it than this. The term in fact cloaks a number of methods which vary in their degree of sophistication depending on their function and the particular stage of development a science has reached. Box 1.5 sets out the sequence of stages through which a science normally passes in its development or, perhaps more realistically, that are constantly present in its progress and on which scientists may draw depending on the kind of information they seek or the kind of problem confronting them. Of particular interest in our efforts to elucidate the term 'scientific method' are stages 2, 3 and 4. Stage 2 is a relatively uncomplicated point at which the researcher is content to observe and record facts and possibly arrive at some system of classification. Much research in the field of education, especially at classroom and school level, is conducted in this way, e.g. surveys and case studies. Stage 3 introduces a note of added sophistication as attempts are made to establish relationships between variables within a loose framework of inchoate theory. Stage 4 is the most sophisticated stage and often the one that many people equate exclusively with the scientific method. In order to arrive at causality, as distinct from mere measures of association, researchers here design experimental situations in which variables are manipulated to test their chosen hypotheses. This process moves from early, inchoate ideas, to more rigorous hypotheses, to empirical testing of those hypotheses, thence to confirmation or modification of the hypotheses (Kerlinger 1970).

With stages 3 and 4 of Box 1.5 in mind, we may say that the scientific method begins

Box 1.5

Stages in the development of a science

- 1 Definition of the science and identification of the phenomena that are to be subsumed under it.
- 2 Observational stage at which the relevant factors, variables or items are identified and labelled, and at which categories and taxonomies are developed.
- 3 Correlational research in which variables and parameters are related to one another and information is systematically integrated as theories begin to develop.
- 4 The systematic and controlled manipulation of variables to see if experiments will produce expected results, thus moving from correlation to causality.
- 5 The firm establishment of a body of theory as the outcomes of the earlier stages are accumulated. Depending on the nature of the phenomena under scrutiny, laws may be formulated and systematized.
- 6 The use of the established body of theory in the resolution of problems or as a source of further hypotheses.

consciously and deliberately by selecting from total number of elements in a given situation. More recently Hitchcock and Hughes (1995: 23) suggest an eight-stage model of the scientific method that echoes Kerlinger. This is represented in Box 1.6.

The elements the researchers fasten on to naturally be suitable for scientific formulation; means simply that they will possess quantita-

Box 1.6

An eight-stage model of the scientific method

- Stage 1: Hypotheses, hunches and guesses
- Stage 2: Experiment designed; samples taken; variables isolated
- Stage 3: Correlations observed; patterns identified
- Stage 4: Hypotheses formed to explain regularities
- Stage 5: Explanations and predictions tested; falsifiability
- Stage 6: Laws developed or disconfirmation (hypothesis rejected)
- Stage 7: Generalizations made
- Stage 8: New theories.

aspects. Their principal working tool will be the hypothesis which, as we have seen, is a statement indicating a relationship (or its absence) between two or more of the chosen elements and stated in such a way as to carry clear implications for testing. Researchers then choose the most appropriate method and put their hypotheses to the test.

Criticisms of positivism and the scientific method

In spite of the scientific enterprise's proven success using positivism – especially in the field of natural science – its ontological and epistemological bases have been the focus of sustained and sometimes vehement criticism from some quarters. Beginning in the second half of the nineteenth century, the revolt against positivism occurred on a broad front, attracting some of the best intellectuals in Europe – philosophers, scientists, social critics and creative artists. Essentially, it has been a reaction against the world picture projected by science which, it is contended, undermines life and mind. The precise target of the anti-positivists' attack has been science's mechanistic and reductionist view of nature which, by definition, defines life in measurable terms rather than inner experience, and excludes notions of choice, freedom, individuality, and moral responsibility, regarding the universe as a living organism rather than as a machine (e.g. Nesfield-Cookson 1987).

Another challenge to the claims of positivism came from Søren Kierkegaard, the Danish philosopher, one of the originators of existentialism. Kierkegaard was concerned with individuals and their need to fulfil themselves to the highest level of development. This realization of a person's potential was for him the meaning of existence which he saw as 'concrete and individual, unique and irreducible, not amenable to conceptualization' (Beck 1979). Characteristic features of the age in which we live – democracy's trust in the crowd mentality, the ascendancy of reason, scientific and technological progress – all militate against the achievement of this end and contribute to the dehumanization of the individual. In his desire to free people from

their illusions, the illusion Kierkegaard was most concerned about was that of objectivity. By this he meant the imposition of rules of behaviour and thought, and the making of a person into an observer set on discovering general laws governing human behaviour. The capacity for subjectivity, he argued, should be regained. This he regarded as the ability to consider one's own relationship to whatever constitutes the focus of inquiry. The contrast he made between objectivity and subjectivity is brought out in the following passage:

When the question of truth is raised in an objective manner, reflection is directed objectively to the truth as an object to which the knower is related. Reflection is not focused on the relationship, however, but upon the question of whether it is the truth to which the knower is related. If only the object to which he is related is the truth, the subject is accounted to be in the truth. When the question of truth is raised subjectively, reflection is directed subjectively to the nature of the individual's relationship; if only the mode of this relationship is in the truth, the individual is in the truth, even if he should happen to be thus related to what is not true.

(Kierkegaard 1974: 178)

For Kierkegaard, 'subjectivity and concreteness of truth are together the light. Anyone who is committed to science, or to rule-governed morality, is benighted, and needs to be rescued from his state of darkness' (Warnock 1970).

Also concerned with the dehumanizing effects of the social sciences is Ions (1977). While acknowledging that they can take much credit for throwing light in dark corners, he expresses serious concern at the way in which quantification and computation, assisted by statistical theory and method, are used. He argues that quantification is a form of collectivism, but that this runs the risk of depersonalization. His objection is not directed at quantification *per se*, but at quantification when it becomes an end in itself – 'a branch of mathematics rather than a humane study seeking to explore and elucidate the gritty circumstances of the human condition' (Ions 1977). This echoes Horkheimer's (1972) powerful

critique of positivism as the mathematization of concepts about nature.

Another forceful critic of the objective consciousness has been Roszak (1970; 1972), who argues that science, in its pursuit of objectivity, is a form of alienation from our true selves and from nature. The justification for any intellectual activity lies in the effect it has on increasing our awareness and degree of consciousness. This increase, some claim, has been retarded in our time by the excessive influence that the positivist paradigm has exerted on areas of our intellectual life. Holbrook (1977), for example, affording consciousness a central position in human existence and deeply concerned with what happens to it, condemns positivism and empiricism for their bankruptcy of the inner world, morality and subjectivity.

Hampden-Turner (1970) concludes that the social science view of human beings is biased in that it is conservative and ignores important qualities. This restricted image of humans, he contends, comes about because social scientists concentrate on the repetitive, predictable and invariant aspects of the person; on 'visible externalities' to the exclusion of the subjective world; and on the parts of the person in their endeavours to understand the whole.

Habermas (1972), in keeping with the Frankfurt School of critical theory (critical theory is discussed below), provides a corrosive critique of positivism, arguing that the scientific mentality has been elevated to an almost unassailable position – almost to the level of a religion (scientism) – as being the only epistemology of the west. In this view all knowledge becomes equated with scientific knowledge. This neglects hermeneutic, aesthetic, critical, moral, creative and other forms of knowledge. It reduces behaviour to technicism.

Positivism's concern for control and, thereby, its appeal to the passivity of behaviourism and for instrumental reason is a serious danger to the more open-ended, creative, humanitarian aspects of social behaviour. Habermas (1972; 1974) and Horkheimer (1972) argue that scientism silences an important debate about values, informed

opinion, moral judgements and beliefs. Scientific explanation seems to be the only means of explaining behaviour, and, for them, this seriously diminishes the very characteristics that make humans human. It makes for a society without conscience. Positivism is unable to answer many interesting or important areas of life (Habermas 1972: 300). Indeed this is an echo of Wittgenstein's (1974) famous comment that when all possible scientific questions have been addressed they have left untouched the main problems of life.

Other criticisms are commonly levelled at positivistic social science from within its own ranks. One is that it fails to take account of our unique ability to interpret our experiences and represent them to ourselves. We can and do construct theories about ourselves and our world; moreover, we act on these theories. In failing to recognize this, positivistic social science is said to ignore the profound differences between itself and the natural sciences. Social science, unlike natural science, stands in a subject–subject rather than a subject–object relation to its field of study, and works in a pre-interpreted world in the sense that the meanings that subjects hold are part of their construction of the world (Giddens 1976).

The difficulty in which positivism finds itself is that it regards human behaviour as passive, essentially determined and controlled, thereby ignoring intention, individualism and freedom. This approach suffers from the same difficulties that inhere in behaviourism, which has scarcely recovered from Chomsky's (1959) withering criticism where he writes that a singular problem of behaviourism is our inability to infer causes from behaviour, to identify the stimulus that has brought about the response – the weakness of Skinner's stimulus–response theory. This problem with positivism also rehearses the familiar problem in social theory, namely the tension between agency and structure (Layder 1994): humans exercise agency – individual choice and intention – not necessarily in circumstances of their own choosing, but nevertheless they do not behave simply or deterministically like puppets.

Finally, the findings of positivistic social science are often said to be so banal and trivial that they are of little consequence to those for whom they are intended, namely, teachers, social workers, counsellors, personnel managers, and the like. The more effort, it seems, that researchers put into their scientific experimentation in the laboratory by restricting, simplifying and controlling variables, the more likely they are to end up with a 'pruned, synthetic version of the whole, a constructed play of puppets in a restricted environment.'⁴

These are formidable criticisms; but what alternatives are proposed by the detractors of positivistic social science?

Alternatives to positivistic social science: naturalistic approaches

Although the opponents of positivism within social science itself subscribe to a variety of schools of thought each with its own subtly different epistemological viewpoint, they are united by their common rejection of the belief that human behaviour is governed by general, universal laws and characterized by underlying regularities. Moreover, they would agree that the social world can be understood only from the standpoint of the individuals who are part of the ongoing action being investigated and that their model of a person is an autonomous one, not the plastic version favoured by positivist researchers. In rejecting the viewpoint of the detached, objective observer – a mandatory feature of traditional research – anti-positivists would argue that individuals' behaviour can only be understood by the researcher sharing their frame of reference: understanding of individuals' interpretations of the world around them has to come from the inside, not the outside. Social science is thus seen as a subjective rather than an objective undertaking, as a means of dealing with the direct experience of people in specific contexts, and where social scientists understand, explain and demystify social reality through the eyes of different participants; the participants themselves define the social reality (Beck 1979) (see <http://www.routledge.com/textbooks/9780415368780> – Chapter 1, file 1.3. ppt).

The anti-positivist movement has influenced those constituent areas of social science of most concern to us, namely, psychology, social psychology and sociology. In the case of psychology, for instance, a school of humanistic psychology has emerged alongside the coexisting behaviouristic and psychoanalytic schools. Arising as a response to the challenge to combat the growing feelings of dehumanization which characterize many social and cultural milieux, it sets out to study and understand the person as a *whole* (Buhler and Allen 1972). Humanistic psychologists present a model of people that is positive, active and purposive, and at the same time stresses their own involvement with the life experience itself. They do not stand apart, introspective, hypothesizing. Their interest is directed at the intentional and creative aspects of the human being. The perspective adopted by humanistic psychologists is naturally reflected in their methodology. They are dedicated to studying the individual in preference to the group, and consequently prefer idiographic approaches to nomothetic ones. The implications of the movement's philosophy for the education of the human being have been drawn by Carl Rogers.⁵

Comparable developments within social psychology may be perceived in the 'science of persons' movement. It is argued here that we must use ourselves as a key to our understanding of others and conversely, our understanding of others as a way of finding out about ourselves, an anthropomorphic model of people. Since anthropomorphism means, literally, the attribution of human form and personality, the implied criticism is that social psychology as traditionally conceived has singularly failed, so far, to model people as they really are. As some wry commentators have pleaded, 'For scientific purposes, treat people as if they were human beings' (Harré and Secord 1972), which entails treating them as capable of monitoring and arranging their own actions, exercising their agency.

Social psychology's task is to understand people in the light of this anthropomorphic model. Proponents of this 'science of persons' approach place great store on the systematic and painstaking analysis of social episodes, i.e. behaviour in context.

In Box 1.7 we give an example of such an episode taken from a classroom study. Note how the particular incident would appear on an interaction analysis coding sheet of a researcher employing a positivistic approach. Note, too, how this slice of classroom life can be understood only by knowledge of the specific organizational background and context in which it is embedded.

The approach to analysing social episodes in terms of the 'actors' themselves is known as the 'ethogenic method'.⁶ Unlike positivistic social psychology, which ignores or presumes its subjects' interpretations of situations, ethogenic social psychology, concentrates upon the ways in which persons construe their social world. By probing at their accounts of their actions, it endeavours to come up with an understanding of what those persons were doing in the particular episode.

As an alternative to positivist approaches, naturalistic, qualitative, interpretive approaches

of various hue possess particular distinguishing features:

- People are deliberate and creative in their actions, they act intentionally and make meanings in and through their activities (Blumer 1969).
- People actively construct their social world – they are not the 'cultural dopes' or passive dolls of positivism (Garfinkel, 1967; Becker 1970).
- Situations are fluid and changing rather than fixed and static; events and behaviour evolve over time and are richly affected by context – they are 'situated activities'.
- Events and individuals are unique and largely non-generalizable.
- A view that the social world should be studied in its natural state, without the intervention of, or manipulation by, the researcher (Hammersley and Atkinson 1983).

Box 1.7

A classroom episode

Walker and Adelman describe an incident in the following manner:

In one lesson the teacher was listening to the boys read through short essays that they had written for homework on the subject of 'Prisons'. After one boy, Wilson, had finished reading out his rather obviously skimmed piece of work, the teacher sighed and said, rather crossly:

T: Wilson, we'll have to put you away if you don't change your ways, and do your homework. Is that all you've done?
P: Strawberries, strawberries. (Laughter)

Now at first glance this is meaningless. An observer coding with Flanders Interaction Analysis Categories (FIAC) would write down:

'7' (teacher criticizes) followed by a,
'4' (teacher asks question) followed by a,
'9' (pupil irritation) and finally a,
'10' (silence or confusion) to describe the laughter

Such a string of codings, however reliable and valid, would not help anyone to *understand* why such an interruption was funny. Human curiosity makes us want to know *why* everyone laughs – and so, I would argue, the social scientist needs to know too. Walker and Adelman (1976), asked subsequently why 'strawberries' was a stimulus to laughter and were told that the teacher frequently said the pupils' work was 'like strawberries – good as far as it goes, but it doesn't last nearly long enough'. Here a casual comment made in the past has become an integral part of the shared meaning system of the class. It can be comprehended only by seeing the relationship as developing over time.

- Fidelity to the phenomena being studied is fundamental.
- People interpret events, contexts and situations, and act on the bases of those events (echoing Thomas's (1928) famous dictum that if people define their situations as real then they are real in their consequences – if I believe there is a mouse under the table, I will act as though there is a mouse under the table, whether there is or not (Morrison 1998)).
- There are multiple interpretations of, and perspectives on, single events and situations.
- Reality is multilayered and complex.
- Many events are not reducible to simplistic interpretation, hence 'thick descriptions' (Geertz 1973b) are essential rather than reductionism, that is to say thick descriptions representing the complexity of situations are preferable to simplistic ones.
- We need to examine situations through the eyes of participants rather than the researcher.

The anti-positivist movement in sociology is represented by three schools of thought – phenomenology, ethnomethodology and symbolic interactionism. A common thread running through the three schools is a concern with phenomena, that is, the things we directly apprehend through our senses as we go about our daily lives, together with a consequent emphasis on qualitative as opposed to quantitative methodology. The differences between them and the significant roles each phenomenon plays in research in classrooms and schools are such as to warrant a more extended consideration of them in the discussion below.

A question of terminology: the normative and interpretive paradigms

So far we have introduced and used a variety of terms to describe the numerous branches and schools of thought embraced by the positivist and anti-positivist viewpoints. As a matter of convenience and as an aid to communication, we clarify at this point two generic terms conventionally used to describe these two

perspectives and the categories subsumed under each, particularly as they refer to social psychology and sociology. The terms in question are 'normative' and 'interpretive'. The normative paradigm (or model) contains two major orienting ideas (Douglas 1973): first, that human behaviour is essentially rule-governed, and second, that it should be investigated by the methods of natural science. The interpretive paradigm, in contrast to its normative counterpart, is characterized by a concern for the individual. Whereas normative studies are positivist, all theories constructed within the context of the interpretive paradigm tend to be anti-positivist. As we have seen, the central endeavour in the context of the interpretive paradigm is to understand the subjective world of human experience. To retain the integrity of the phenomena being investigated, efforts are made to get inside the person and to understand from within. The imposition of external form and structure is resisted, since this reflects the viewpoint of the observer as opposed to that of the actor directly involved.

Two further differences between the two paradigms may be identified at this stage: the first concerns the concepts of 'behaviour' and 'action'; the second, the different conceptions of 'theory'. A key concept within the normative paradigm, behaviour refers to responses either to external environmental stimuli (another person, or the demands of society, for instance) or to internal stimuli (hunger, or the need to achieve, for example). In either case, the cause of the behaviour lies in the past. Interpretive approaches, on the other hand, focus on action. This may be thought of as behaviour-with-meaning; it is intentional behaviour and as such, future oriented. Actions are meaningful to us only in so far as we are able to ascertain the intentions of actors to share their experiences. A large number of our everyday interactions with one another rely on such shared experiences.

As regards theory, normative researchers try to devise general theories of human behaviour and to validate them through the use of increasingly complex research methodologies which, some believe, push them further and further from the

experience and understanding of the everyday world and into a world of abstraction. For them, the basic reality is the collectivity; it is external to the actor and manifest in society, its institutions and its organizations. The role of theory is to say how reality hangs together in these forms or how it might be changed so as to be more effective. The researcher's ultimate aim is to establish a comprehensive 'rational edifice', a universal theory, to account for human and social behaviour.

But what of the interpretive researchers? They begin with individuals and set out to understand their interpretations of the world around them. Theory is emergent and must arise from particular situations; it should be 'grounded' in data generated by the research act (Glaser and Strauss 1967). Theory should not precede research but follow it. Investigators work directly with experience and understanding to build their theory on them. The data thus yielded will include the meanings and purposes of those people who are their source. Further, the theory so generated must make sense to those to whom it applies. The aim of scientific investigation for the interpretive researcher is to understand how this glossing of reality goes on at one time and in one place and compare it with what goes on in different times and places. Thus theory becomes sets of meanings which yield insight and understanding of people's behaviour. These theories are likely to be as diverse as the sets of human meanings and understandings that they are to explain. From an interpretive perspective the hope of a universal theory which characterizes the normative outlook gives way to multifaceted images of human behaviour as varied as the situations and contexts supporting them.

Phenomenology, ethnomethodology and symbolic interactionism

There are many variants of qualitative, naturalistic approaches (Jacob 1987; Hitchcock and Hughes 1995). Here we focus on three significant 'traditions' in this style of research – phenomenology, ethnomethodology and symbolic interactionism.

In its broadest meaning, phenomenology is a theoretical point of view that advocates the study of direct experience taken at face value; and one which sees behaviour as determined by the phenomena of experience rather than by external, objective and physically described reality (English and English 1958). Although phenomenologists differ among themselves on particular issues, there is fairly general agreement on the following points identified by Curtis (1978) which can be taken as distinguishing features of their philosophical viewpoint:

- a belief in the importance, and in a sense the primacy, of subjective consciousness
- an understanding of consciousness as active, as meaning bestowing
- a claim that there are certain essential structures to consciousness of which we gain direct knowledge by a certain kind of reflection: exactly what these structures are is a point about which phenomenologists have differed.

Various strands of development may be traced in the phenomenological movement: we shall briefly examine two of them – the transcendental phenomenology of Husserl, and existential phenomenology, of which Schutz is perhaps the most characteristic representative.

Husserl, regarded by many as the founder of phenomenology, was concerned with investigating the source of the foundation of science and with questioning the commonsense, 'taken-for-granted' assumptions of everyday life (see Burrell and Morgan 1979). To do this, he set about opening up a new direction in the analysis of consciousness. His catch-phrase was 'Back to the things!' which for him meant finding out how things appear directly to us rather than through the media of cultural and symbolic structures. In other words, we are asked to look beyond the details of everyday life to the essences underlying them. To do this, Husserl exhorts us to 'put the world in brackets' or free ourselves from our usual ways of perceiving the world. What is left over from this reduction is our consciousness of which there are three elements – the 'I' who thinks, the mental acts of this thinking subject, and the intentional

objects of these mental acts. The aim, then, of this method of *epoché*, as Husserl called it, is the dismembering of the constitution of objects in such a way as to free us from all preconceptions about the world (see Warnock 1970).

Schutz was concerned with relating Husserl's ideas to the issues of sociology and to the scientific study of social behaviour. Of central concern to him was the problem of understanding the meaning structure of the world of everyday life. The origins of meaning he thus sought in the 'stream of consciousness' – basically an unbroken stream of lived experiences which have no meaning in themselves. One can impute meaning to them only retrospectively, by the process of turning back on oneself and looking at what has been going on. In other words, meaning can be accounted for in this way by the concept of reflexivity. For Schutz, the attribution of meaning reflexively is dependent on the people identifying the purpose or goal they seek (see Burrell and Morgan 1979).

According to Schutz, the way we understand the behaviour of others is dependent on a process of typification by means of which the observer makes use of concepts resembling 'ideal types' to make sense of what people do. These concepts are derived from our experience of everyday life and it is through them, claims Schutz, that we classify and organize our everyday world. As Burrell and Morgan (1979) observe, we learn these typifications through our biographical locations and social contexts. Our knowledge of the everyday world inheres in social order and this world itself is socially ordered.

The fund of everyday knowledge by means of which we are able to typify other people's behaviour and come to terms with social reality varies from situation to situation. We thus live in a world of multiple realities, and social actors move within and between these with ease (Burrell and Morgan 1979), abiding by the rules of the game for each of these worlds.

Like phenomenology, ethnomethodology is concerned with the world of everyday life. In the words of its proponent, Harold Garfinkel, it sets out

to treat practical activities, practical circumstances, and practical sociological reasonings as topics of empirical study, and by paying to the most commonplace activities of daily life the attention usually accorded extraordinary events, seeks to learn about them as phenomena in their own right.

(Garfinkel 1967)

He maintains that students of the social world must doubt the reality of that world; and that in failing to view human behaviour more sceptically, sociologists have created an ordered social reality that bears little relationship to the real thing. He thereby challenges the basic sociological concept of order.

Ethnomethodology, then, is concerned with how people make sense of their everyday world. More especially, it is directed at the mechanisms by which participants achieve and sustain interaction in a social encounter – the assumptions they make, the conventions they utilize and the practices they adopt. Ethnomethodology thus seeks to understand social accomplishments in their own terms; it is concerned to understand them from within (see Burrell and Morgan 1979).

In identifying the taken-for-granted assumptions characterizing any social situation and the ways in which the people involved make their activities rationally accountable, ethnomethodologists use notions like 'indexicality' and 'reflexivity'. Indexicality refers to the ways in which actions and statements are related to the social contexts producing them; and to the way their meanings are shared by the participants but not necessarily stated explicitly. Indexical expressions are thus the designations imputed to a particular social occasion by the participants in order to locate the event in the sphere of reality. Reflexivity, on the other hand, refers to the way in which all accounts of social settings – descriptions, analyses, criticisms, etc. – and the social settings occasioning them are mutually interdependent.

It is convenient to distinguish between two types of ethnomethodologists: linguistic and situational. The linguistic ethnomethodologists focus upon the use of language and the ways in which conversations in everyday life are

structured. Their analyses make much use of the unstated taken-for-granted meanings, the use of indexical expressions and the way in which conversations convey much more than is actually said. The situational ethnomethodologists cast their view over a wider range of social activity and seek to understand the ways in which people negotiate the social contexts in which they find themselves. They are concerned to understand how people make sense of and order their environment. As part of their empirical method, ethnomethodologists may consciously and deliberately disrupt or question the ordered taken-for-granted elements in everyday situations in order to reveal the underlying processes at work.

The substance of ethnomethodology thus largely comprises a set of specific techniques and approaches to be used in studying what Garfinkel (1967) has described as the 'awesome indexicality' of everyday life. It is geared to empirical study, and the stress which its practitioners place upon the uniqueness of the situation encountered, projects its essentially relativist standpoint. A commitment to the development of methodology and fieldwork has occupied first place in the interests of its adherents, so that related issues of ontology, epistemology and the nature of human beings have received less attention than perhaps they deserve.

Essentially, the notion of symbolic interactionism derives from the work of Mead (1934). Although subsequently to be associated with such noted researchers as Blumer, Hughes, Becker and Goffman, the term does not represent a unified perspective in that it does not embrace a common set of assumptions and concepts accepted by all who subscribe to the approach. For our purposes, however, it is possible to identify three basic postulates. These have been set out by Woods (1979) as follows. First, human beings act towards things on the basis of the meanings they have for them. Humans inhabit two different worlds: the 'natural' world wherein they are organisms of drives and instincts and where the external world exists independently of them, and the social world where the existence of symbols, like language, enables them to give meaning to objects. This attribution of meanings, this interpreting, is

what makes them distinctively human and social. Interactionists therefore focus on the world of subjective meanings and the symbols by which they are produced and represented. This means not making any prior assumptions about what is going on in an institution, and taking seriously, indeed giving priority to, inmates' own accounts. Thus, if pupils appear preoccupied for too much of the time – 'being bored', 'mucking about', 'having a laugh', etc. the interactionist is keen to explore the properties and dimensions of these processes.

Second, this attribution of meaning to objects through symbols is a continuous process. Action is not simply a consequence of psychological attributes such as drives, attitudes or personalities, or determined by external social facts such as social structure or roles, but results from a continuous process of meaning attribution which is always emerging in a state of flux and subject to change. The individual constructs, modifies, pieces together, weighs up the pros and cons and bargains.

Third, this process takes place in a social context. Individuals align their actions to those of others. They do this by 'taking the role of the other', by making indications to 'themselves' about the likely responses of 'others'. They construct how others wish or might act in certain circumstances, and how they themselves might act. They might try to 'manage' the impressions others have of them, put on a 'performance', try to influence others' 'definition of the situation'.

Instead of focusing on the individual, then, and his or her personality characteristics, or on how the social structure or social situation causes individual behaviour, symbolic interactionists direct their attention at the nature of interaction, the dynamic activities taking place between people. In focusing on the interaction itself as a unit of study, the symbolic interactionist creates a more active image of the human being and rejects the image of the passive, determined organism. Individuals interact; societies are made up of interacting individuals. People are constantly undergoing change in interaction and society is changing through interaction. Interaction implies human beings acting in relation to each other, taking

each other into account, acting, perceiving, interpreting, acting again. Hence, a more dynamic and active human being emerges rather than an actor merely responding to others. Woods (1983: 15–16) summarizes key emphases of symbolic interaction thus:

- individuals as constructors of their own actions
- the various components of the self and how they interact; the indications made to self, meanings attributed, interpretive mechanisms, definitions of the situation; in short, the world of subjective meanings, and the symbols by which they are produced and represented
- the process of negotiation, by which meanings are continually being constructed
- the social context in which they occur and whence they derive
- by taking the 'role of the other' – a dynamic concept involving the construction of how others wish to or might act in a certain circumstance, and how individuals themselves might act – individuals align their actions to those of others.

A characteristic common to the phenomenological, ethnomethodological and symbolic interactionist perspectives, which makes them singularly attractive to the would-be educational researcher, is the way they fit naturally to the kind of concentrated action found in classrooms and schools. Yet another shared characteristic is the manner in which they are able to preserve the integrity of the situation where they are employed. Here the influence of the researcher in structuring, analysing and interpreting the situation is present to a much smaller degree than would be the case with a more traditionally oriented research approach.

Criticisms of the naturalistic and interpretive approaches

Critics have wasted little time in pointing out what they regard as weaknesses in these newer qualitative perspectives. They argue that while it is undeniable that our understanding of the actions of our fellow-beings necessarily requires knowledge

of their intentions, this, surely, cannot be said to comprise the purpose of a social science. As Rex (1974) has observed:

While patterns of social reactions and institutions may be the product of the actors' definitions of the situations there is also the possibility that those actors might be falsely conscious and that sociologists have an obligation to seek an objective perspective which is not necessarily that of any of the participating actors at all.... We need not be confined purely and simply to that... social reality which is made available to us by participant actors themselves.

(Rex 1974)

While these more recent perspectives have presented models of people that are more in keeping with common experience, some argue that anti-positivists have gone too far in abandoning scientific procedures of verification and in giving up hope of discovering useful generalizations about behaviour (see Mead 1934). Are there not dangers in rejecting the approach of physics in favour of methods more akin to literature, biography and journalism? Some specific criticisms of the methodologies are well directed, for example Argyle (1978) questions whether, if carefully controlled interviews such as those used in social surveys are inaccurate, then the less controlled interviews carry even greater risks of inaccuracy. Indeed Bernstein (1974) suggests that subjective reports may be incomplete and misleading.

Bernstein's criticism is directed at the overriding concern of phenomenologists and ethnomethodologists with the meanings of situations and the ways in which these meanings are negotiated by the actors involved. What is overlooked about such negotiated meanings, observes Bernstein (1974), is that the very process whereby one interprets and defines a situation is itself a product of the circumstances in which one is placed. One important factor in such circumstances that must be considered is the power of others to impose their own definitions of situations upon participants. Doctors' consulting rooms and headteachers' studies are locations in which inequalities in power are regularly imposed on unequal participants. The ability of certain individuals, groups, classes and

authorities to persuade others to accept their definitions of situations demonstrates that while – as ethnomethodologists insist – social structure is a consequence of the ways in which we perceive social relations, it is clearly more than this. Conceiving of social structure as external to ourselves helps us take its self-evident effects upon our daily lives into our understanding of the social behaviour going on about us. Here is rehearsed the tension between agency and structure of social theorists (Layder 1994); the danger of interactionist and interpretive approaches is their relative neglect of the power of external – structural – forces to shape behaviour and events. There is a risk in interpretive approaches that they become hermetically sealed from the world outside the participants' theatre of activity – they put artificial boundaries around subjects' behaviour. Just as positivistic theories can be criticized for their macro-sociological persuasion, so interpretive and qualitative theories can be criticized for their narrow micro-sociological perspectives.

Critical theory and critical educational research

Positivist and interpretive paradigms are essentially concerned with understanding phenomena through two different lenses. Positivism strives for objectivity, measurability, predictability, controllability, patterning, the construction of laws and rules of behaviour, and the ascription of causality; the interpretive paradigms strive to understand and interpret the world in terms of its actors. In the former observed phenomena are important; in the latter meanings and interpretations are paramount. Habermas (1984: 109–10), echoing Giddens (1976), describes this latter as a 'double hermeneutic', where people strive to interpret and operate in an already interpreted world. An emerging approach to educational research is the paradigm of *critical educational research*. This regards the two previous paradigms as presenting incomplete accounts of social behaviour by their neglect of the political and ideological contexts of much educational research. Positivistic and interpretive paradigms are seen as preoccupied

with technical and hermeneutic knowledge respectively (Gage 1989). The paradigm of critical educational research is heavily influenced by the early work of Habermas and, to a lesser extent, his predecessors in the Frankfurt School, most notably Adorno, Marcuse, Horkheimer and Fromm. Here the expressed intention is deliberately political – the emancipation of individuals and groups in an egalitarian society (see <http://www.routledge.com/textbooks/9780415368780> – Chapter 1, file 1.4. ppt).

Critical theory is explicitly prescriptive and normative, entailing a view of what behaviour in a social democracy *should* entail (Fay 1987; Morrison 1995a). Its intention is not merely to give an account of society and behaviour but to realize a society that is based on equality and democracy for all its members. Its purpose is not merely to understand situations and phenomena but to change them. In particular it seeks to emancipate the disempowered, to redress inequality and to promote individual freedoms within a democratic society.

In this enterprise critical theory identifies the 'false' or 'fragmented' consciousness (Eagleton 1991) that has brought an individual or social group to relative powerlessness or, indeed, power, and it questions the legitimacy of this. It holds up to the lights of legitimacy and equality issues of repression, voice, ideology, power, participation, representation, inclusion and interests. It argues that much behaviour (including research behaviour) is the outcome of particular illegitimate, dominatory and repressive factors, illegitimate in the sense that they do not operate in the general interest – one person's or group's freedom and power is bought at the price of another's freedom and power. Hence critical theory seeks to uncover the *interests* at work in particular situations and to interrogate the legitimacy of those interests, identifying the extent to which they are legitimate in their service of equality and democracy. Its intention is *transformative*: to transform society and individuals to social democracy. In this respect the purpose of critical educational research is intensely practical, to bring about a more just, egalitarian society

n which individual and collective freedoms are practised, and to eradicate the exercise and effects of illegitimate power. The pedigree of critical theory in Marxism, thus, is not difficult to discern. For critical theorists, researchers can no longer claim neutrality and ideological or political innocence.

Critical theory and critical educational research, then, have their substantive agenda – for example examining and interrogating: the relationships between school and society – how schools perpetuate or reduce inequality; the social construction of knowledge and curricula, who defines worthwhile knowledge, what ideological interests this serves, and how this reproduces inequality in society; how power is produced and reproduced through education; whose interests are served by education and how legitimate these are (e.g. the rich, white, middle-class males rather than poor, non-white females).

The significance of critical theory for research is immense, for it suggests that much social research is comparatively trivial in that it *accepts* rather than *questions* given agendas for research, compounded by the funding for research, which underlines the political dimension of research sponsorship (discussed later) (Norris 1990). Critical theorists would argue that the positivist and interpretive paradigms are essentially technicist, seeking to understand and render more efficient an existing situation, rather than to question or transform it.

Habermas (1972) offers a useful tripartite conceptualization of interests that catches the three paradigms of research in this chapter. He suggests that knowledge – and hence research knowledge – serves different interests. Interests, he argues, are socially constructed, and are 'knowledge-constitutive', because they shape and determine what counts as the objects and types of knowledge. Interests have an ideological function (Morrison 1995a), for example a 'technical interest' (discussed below) can have the effect of keeping the empowered in their empowered position and the disempowered in their powerlessness – i.e. reinforcing and perpetuating the status quo. An 'emancipatory interest' (discussed below) threatens the status quo. In this view knowledge – and research

knowledge – is not neutral (see also Mannheim 1936). What counts as worthwhile knowledge is determined by the social and positional power of the advocates of that knowledge. The link here between objects of study and communities of scholars echoes Kuhn's (1962) notions of paradigms and paradigm shifts, where the field of knowledge or paradigm is seen to be only as good as the evidence and the respect in which it is held by 'authorities'. Knowledge and definitions of knowledge reflect the interests of the community of scholars who operate in particular paradigms. Habermas (1972) constructs the definition of worthwhile knowledge and modes of understanding around three cognitive interests (see <http://www.routledge.com/textbooks/9780415368780> – Chapter 1, file 1.5. ppt):

- prediction and control
- understanding and interpretation
- emancipation and freedom.

He names these the 'technical', 'practical' and 'emancipatory' interests respectively. The technical interest characterizes the scientific, positivist method outlined earlier, with its emphasis on laws, rules, prediction and control of behaviour, with passive research objects – instrumental knowledge. The 'practical' interest, an attenuation of the positivism of the scientific method, is exemplified in the hermeneutic, interpretive methodologies outlined in the qualitative approaches earlier (e.g. symbolic interactionism). Here research methodologies seek to clarify, understand and interpret the communications of 'speaking and acting subjects' (Habermas 1974: 8).

Hermeneutics focuses on interaction and language; it seeks to understand situations through the eyes of the participants, echoing the *verstehen* approaches of Weber and premised on the view that reality is socially constructed (Berger and Luckmann 1967). Indeed Habermas (1988: 12) suggests that sociology must understand social facts in their cultural significance and as socially determined. Hermeneutics involves recapturing the *meanings* of interacting others, recovering and reconstructing the *intentions* of the other in a situation. Such an enterprise involves the

of meaning in a social context (Held 1980). Gadamer (1975: 273) argues that the hermeneutic sciences (e.g. qualitative approaches) involve the *fusion of horizons* between participants. Meanings rather than phenomena take on significance here.

The emancipatory interest subsumes the previous two paradigms; it requires them but goes beyond them (Habermas 1972: 211). It is concerned with *praxis* – action that is informed by reflection with the aim to emancipate (Kincheloe 1991: 177). The twin intentions of this interest are to expose the operation of power and to bring about social justice as domination and repression act to prevent the full existential realization of individual and social freedoms (Habermas 1979: 14). The task of this knowledge-constitutive interest, indeed of critical theory itself, is to restore to consciousness those suppressed, repressed and submerged determinants of unfree behaviour with a view to their dissolution (Habermas 1984: 194–5).

What we have in effect, then, in Habermas's early work is an attempt to conceptualize three research styles: the scientific, positivist style; the interpretive style; and the emancipatory, ideology critical style. Not only does critical theory have its own research agenda, but also it has its own research methodologies, in particular ideology critique and action research. With regard to ideology critique, a particular reading of ideology is being adopted here, as the *suppression of generalizable interests* (Habermas 1976: 113), where systems, groups and individuals operate in rationally indefensible ways because their power to act relies on the disempowering of other groups, i.e. that their principles of behaviour cannot be generalized.

Ideology – the values and practices emanating from particular dominant groups – is the means by which powerful groups promote and legitimize their particular – sectoral – interests at the expense of disempowered groups. Ideology critique exposes the operation of ideology in many spheres of education, the working out of vested interests under the mantle of the general good. The task of ideology critique is to uncover the vested interests at work which may be occurring consciously or subliminally, revealing to participants how they may be acting to

perpetuate a system which keeps them either empowered or disempowered (Geuss 1981), i.e. which suppresses a generalizable interest. Explanations for situations might be other than those 'natural', taken for granted, explanations that the participants might offer or accept. Situations are not natural but problematic (Carr and Kemmis 1986). They are the outcomes or processes wherein interests and powers are protected and suppressed, and one task of ideology critique is to expose this (Grundy 1987). The interests at work are uncovered by ideology critique, which, itself, is premised on reflective practice (Morrison 1995a; 1995b; 1996a). Habermas (1972: 230) suggests that ideology critique through reflective practice can be addressed in four stages:

- Stage 1: a description and interpretation of the existing situation – a hermeneutic exercise that identifies and attempts to make sense of the current situation (echoing the *verstehen* approaches of the interpretive paradigm) (see <http://www.routledge.com/textbooks/9780415368780> – Chapter 1, file 1.6. ppt).
- Stage 2: a penetration of the reasons that brought the existing situation to the form that it takes – the causes and purposes of a situation and an evaluation of their legitimacy, involving an analysis of interests and ideologies at work in a situation, their power and legitimacy (both in micro- and macro-sociological terms). Habermas's (1972) early work likens this to psychoanalysis as a means for bringing into the consciousness of 'patients' those repressed, distorted and oppressive conditions, experiences and factors that have prevented them from a full, complete and accurate understanding of their conditions, situations and behaviour, and that, on such exposure and examination, will be liberatory and emancipatory. Critique here reveals to individuals and groups how their views and practices might be ideological distortions that in their effects, perpetuate a social order or situation that works against their democratic freedoms, interests and empowerment (see also Carr and Kemmis 1986: 138–9).

- Stage 3: an agenda for altering the situation – in order for moves to an egalitarian society to be furthered.
- Stage 4: an evaluation of the achievement of the situation in practice.

In the world of education Habermas's stages are paralleled by Smyth (1989) who, too, denotes a four-stage process:

- *description* (what am I doing?)
- *information* (what does it mean?)
- *confrontation* (how did I come to be like this?)
- *reconstruction* (how might I do things differently?)

It can be seen that ideology critique here has both a reflective, theoretical and a practical side to it; without reflection it is hollow and without practice it is empty.

As ideology is not mere theory but impacts directly on practice (Eagleton 1991) there is a strongly practical methodology implied by critical theory, which articulates with action research (Callawaert 1999). Action research (discussed in Chapter 14), as its name suggests, is about research that impacts on, and focuses on, practice. In its espousal of practitioner research, for example teachers in schools, participant observers and curriculum developers, action research recognizes the significance of contexts for practice – locational, ideological, historical, managerial, social. Furthermore it accords power to those who are operating in those contexts, for they are both the engines of research and of practice. In that sense the claim is made that action research is strongly empowering and emancipatory in that it gives practitioners a 'voice' (Carr and Kemmis 1986; Grundy 1987), participation in decision-making, and control over their environment and professional lives. Whether the strength of the claims for empowerment are as strong as their proponents would hold is another matter, for action research might be relatively powerless in the face of mandated changes in education. Here action research might be more concerned with the intervening in existing practice to ensure

that mandated change is addressed efficiently and effectively.

Morrison (1995a) suggests that critical theory, because it has a practical intent to transform and empower, can – and should – be examined and perhaps tested empirically. For example, critical theory claims to be empowering; that is a testable proposition. Indeed, in a departure from some of his earlier writing, in some of his later work Habermas (1990) acknowledges this; he argues for the need to find 'counter examples' (p. 6), to 'critical testing' (p. 7) and empirical verification (p. 117). He acknowledges that his views have only 'hypothetical status' (p. 32) that need to be checked against specific cases (p. 9). One could suggest, for instance, that the effectiveness of his critical theory can be examined by charting the extent to which equality, freedom, democracy, emancipation, empowerment have been realized by dint of his theory; the extent to which transformative practices have been addressed or occurred as a result of his theory; the extent to which subscribers to his theory have been able to assert their agency; the extent to which his theories have broken down the barriers of instrumental rationality. The operationalization and testing (or empirical investigation) of his theories clearly is a major undertaking, and one which Habermas has not done. In this respect critical theory, a theory that strives to improve practical living, runs the risk of becoming merely contemplative (see <http://www.routledge.com/textbooks/9780415368780> – Chapter 1, file 1.7. ppt).

Criticisms of approaches from critical theory

There are several criticisms that have been voiced against critical approaches. Morrison (1995a) suggests that there is an artificial separation between Habermas's three interests – they are drawn far more sharply (Hesse 1982; Bernstein 1983: 33). For example, one has to bring hermeneutic knowledge to bear on positivist science and vice versa in order to make meaning of each other and in order to judge

their own status. Further, the link between ideology critique and emancipation is neither clear nor proven, nor a logical necessity (Morrison 1995a: 67) – whether a person or society can become emancipated simply by the exercise of ideology critique or action research is an empirical rather than a logical matter (Morrison 1995a; Wardekker and Miedama 1997). Indeed one can become emancipated by means other than ideology critique; emancipated societies do not necessarily demonstrate or require an awareness of ideology critique. Moreover, it could be argued that the rationalistic appeal of ideology critique actually obstructs action designed to bring about emancipation. Roderick (1986: 65), for example, questions whether the espousal of ideology critique is itself as ideological as the approaches that it proscribes. Habermas, in his allegiance to the view of the social construction of knowledge through ‘interests’, is inviting the charge of relativism.

While the claim to there being three forms of knowledge has the epistemological attraction of simplicity, one has to question this very simplicity (e.g. Keat 1981: 67); there are a multitude of interests and ways of understanding the world and it is simply artificial to reduce these to three. Indeed it is unclear whether Habermas, in his three knowledge-constitutive interests, is dealing with a conceptual model, a political analysis, a set of generalities, a set of transhistorical principles, a set of temporally specific observations, or a set of loosely defined slogans (Morrison 1995a: 71) that survive only by dint of their ambiguity (Kolakowski 1978). Lakowski (1999: 179–82) questions the acceptability of the consensus theory of truth on which Habermas’s work is premised; she argues that Habermas’s work is silent on social change, and is little more than speculation, a view echoed by Fendler’s (1999) criticism of critical theory as inadequately problematizing subjectivity and ahistoricity.

More fundamental to a critique of this approach is the view that critical theory has a deliberate political agenda, and that the task of the researcher is not to be an ideologue or to have an agenda, but to be dispassionate, disinterested and objective (Morrison 1995a). Of course, critical

theorists would argue that the call for research to be ideologically neutral is itself ideological: saturated with laissez-faire values which allow status quo to be reproduced, i.e. that the call for researchers to be neutral and disinterested is just as value laden as is the call for them to intrude their own perspectives. The rights of a researcher to move beyond disinterestedness are clearly contentious, though the safeguard here is that the researcher’s is only one voice in a community of scholars (Kemmis 1982). Critical theorists as researchers have been hoisted by their own petard, for if they are to become more than merely negative Jeremiahs and sceptics, bearing witness to a particular social order that is dominated by scientism and instrumental rationality (Eagleton 1991; Wardekker and Miedama 1997), then they have to generate a positive agenda, but in so doing they are violating the traditional objectivity of researchers. Because their focus is on an ideological agenda, they themselves cannot avoid an ideological stance (Morrison 1995a).

Claims have been made for the power of action research to empower participants as researchers (e.g. Carr and Kemmis 1986; Grundy 1987). This might be over-optimistic in a world in which power is often through statute; the reality of political power seldom extends to teachers. That teachers might be able to exercise some power in schools but that this has little effect on the workings of society at large was caught in Bernstein’s (1970) famous comment that ‘education cannot compensate for society’. Giving action researchers a small degree of power (to research their own situations) has little effect on the real workings of power and decision-making, which often lie outside the control of action researchers. Is action research genuinely and full-bloodedly empowering and emancipatory? Where is the evidence?

Critical theory and curriculum research

For research methods, the tenets of critical theory suggest their own substantive fields of enquiry and their own methods (e.g. ideology critique and action research). Beyond that the contribution of this text on empirical research methods is

limited by the fact that the agenda of critical theory is highly particularistic, prescriptive and, as has been seen, problematical. Though it is an influential paradigm, it is influential in certain fields rather than in others. For example, its impact on curriculum research has been far-reaching.

It has been argued for many years that the most satisfactory account of the curriculum is given by a modernist, positivist reading of the development of education and society. This has its curricular expression in Tyler's (1949) famous and influential rationale for the curriculum in terms of four questions:

- 1 What educational purposes should the school seek to attain?
- 2 What educational experiences can be provided that are likely to attain these purposes?
- 3 How can these educational experiences be effectively organized?
- 4 How can we determine whether these purposes are being attained?

Underlying this rationale is a view that the curriculum is controlled (and controllable), ordered, predetermined, uniform, predictable and largely behaviourist in outcome – all elements of the positivist mentality that critical theory eschews. Tyler's rationale resonates sympathetically with a modernist, scientific, managerialist mentality of society and education that regards ideology and power as unproblematic, indeed it claims the putative political neutrality and objectivity of positivism (Doll 1993); it ignores the advances in psychology and psychopedagogy made by constructivism.

However, this view has been criticized for precisely these sympathies. Doll (1993) argues that it represents a *closed* system of planning and practice that sits uncomfortably with the notion of education as an *opening* process and with the view of postmodern society as open and diverse, multidimensional, fluid and with power less monolithic and more problematical. This view takes seriously the impact of chaos and complexity theory and derives from them some important features for contemporary curricula. These are

incorporated into a view of curricula as being *rich, relational, recursive and rigorous* (Doll 1993) with an emphasis on *emergence, process epistemology and constructivist psychology*.

Not all knowledge can be included in the curriculum; the curriculum is a selection of what is deemed to be worthwhile knowledge. The justification for that selection reveals the ideologies and power in decision-making in society and through the curriculum. Curriculum is an ideological selection from a range of possible knowledge. This resonates with Habermas's (1972) view that knowledge and its selection is neither neutral nor innocent.

Ideologies can be treated unpejoratively as sets of beliefs or, more sharply, as sets of beliefs emanating from powerful groups in society, designed to protect the interests of the dominant. If curricula are value-based then why is it that some values hold more sway than others? The link between values and power is strong. This theme asks not only *what* knowledge is important but also *whose* knowledge is important in curricula, *what* and *whose* interests such knowledge serves, and *how* the curriculum and pedagogy serve (or do not serve) differing interests. Knowledge is not neutral (as was the tacit view in modernist curricula). The curriculum is ideologically contestable terrain.

The study of the sociology of knowledge indicates how the powerful might retain their power through curricula and how knowledge and power are legitimized in curricula. The study of the sociology of knowledge suggests that the curriculum should be both subject to ideology critique and itself promote ideology critique in students. A research agenda for critical theorists, then, is how the curriculum perpetuates the societal status quo and how can it (and should it) promote equality in society.

The notion of ideology critique engages the early writings of Habermas (1972), in particular his theory of three knowledge-constitutive interests. His *technical interest* (in control and predictability) resonates with Tyler's (1949) model of the curriculum and reveals itself in technicist, instrumentalist and scientific views of curricula that are to be 'delivered' to passive recipients – the

curriculum is simply another commodity in a consumer society in which differential cultural capital is inevitable. Habermas's *hermeneutic interest* (in understanding others' perspectives and views) resonates with a *process* view of the curriculum. His *emancipatory interest* (in promoting social emancipation, equality, democracy, freedoms and individual and collective empowerment) requires an exposure of the ideological interests at work in curricula in order that teachers and students can take control of their own lives for the collective, egalitarian good. Habermas's emancipatory interest denotes an inescapably political reading of the curriculum and the purposes of education – the movement away from authoritarianism and elitism and towards social democracy.

Habermas's work underpins and informs much contemporary and recent curriculum theory (e.g. Grundy 1987; Apple 1990; UNESCO 1996) and is a useful heuristic device for understanding the motives behind the heavy prescription of curriculum content in, for example, the United Kingdom, New Zealand, Hong Kong and France. For instance, one can argue that the National Curriculum of England and Wales is heavy on the technical and hermeneutic interests but very light on the emancipatory interest (Morrison 1995a) and that this (either deliberately or in its effects) supports – if not contributes to – the reproduction of social inequality. As Bernstein (1971: 47) argues: 'how a society selects, classifies, distributes, transmits and evaluates the educational knowledge it considers to be public, reflects both the distribution of power and the principles of social control'.

Several writers on curriculum theory (e.g. McLaren 1995; Leistyna *et al.* 1996) argue that power is a central, defining concept in matters of the curriculum. Here considerable importance is accorded to the political agenda of the curriculum, and the empowerment of individuals and societies is an inescapable consideration in the curriculum. One means of developing student and societal empowerment finds its expression in Habermas's (1972) emancipatory interest and critical pedagogy.

In the field of critical pedagogy the argument is advanced that educators must work with, and on, the lived experience that students bring to the pedagogical encounter rather than impose a dominatory curriculum that reproduces social inequality. In this enterprise teachers are to transform the experience of domination in students and empower them to become 'emancipated' in a full democracy. Students' everyday experience of oppression, of being 'silenced', of having their cultures and 'voices' excluded from curricular decision-making are to be examined for the ideological messages that are contained in such curricula. Raising awareness of such inequalities is an important step to overcoming them. Teachers and students together move forward in the process towards 'individual autonomy within a just society' (Masschelein 1991: 97). In place of prescribed and culturally biased curricula that students simply receive, critical pedagogy regards curriculum as a form of cultural politics in which *participants* in (rather than *recipients* of) curriculum question the cultural and dominatory messages contained in curricula and replace them with a 'language of possibility' and empowering, community-related curricula. In this way curriculum serves the 'socially critical' rather than the uncritically and ideologically passive school.

One can discern a utopian and general tenor in some of this work, and applying critical theory to education can be criticized for its limited comments on practice. Indeed, Apple and Wardekker (1999: 68) go so far as to say that critical pedagogy has had its day, and that it was a stillborn child and that critical pedagogy is a philosophy of science without a politics (p. 75)! Nevertheless it is an important contribution for it recognizes and makes much of the fact that curricula and pedagogy are problematic and political.

A summary of the three paradigm

Box 1.8 summarizes some of the broad differences between the three approaches that we have discussed so far (see <http://www.routledge.com/19780415368780> – Chapter 1, file 1.8. p1

Box 1.8

Differing approaches to the study of behaviour

<i>Normative</i>	<i>Interpretive</i>	<i>Critical</i>
Society and the social system	The individual	Societies, groups and individuals
Medium/large-scale research	Small-scale research	Small-scale research
Impersonal, anonymous forces regulating behaviour	Human actions continuously recreating social life	Political, ideological factors, power and interests shaping behaviour
Model of natural sciences	Non-statistical	Ideology critique and action research
'Objectivity'	'Subjectivity'	Collectivity
Research conducted 'from the outside'	Personal involvement of the researcher	Participant researchers, researchers and facilitators
Generalizing from the specific	Interpreting the specific	Critiquing the specific
Explaining behaviour/seeking causes	Understanding actions/meanings rather than causes	Understanding, interrogating, critiquing, transforming actions and interests
Assuming the taken-for-granted	Investigating the taken-for-granted	Interrogating and critiquing the taken for granted
Macro-concepts: society, institutions, norms, positions, roles, explanations	Micro-concepts: individual perspective, personal constructs, negotiated meanings, definitions of situations	Macro- and micro-concepts: political and ideological interests, operations of power
Structuralists	Phenomenologists, symbolic interactionists, ethnomethodologists	Critical theorists, action researchers, practitioner researchers
Technical interest	Practical interest	Emancipatory interest

The emerging paradigm of complexity theory

An emerging fourth paradigm in educational research is that of complexity theory (Morrison 2002a). Complexity theory looks at the world in ways which break with simple cause-and-effect models, linear predictability, and a dissection approach to understanding phenomena, replacing them with organic, non-linear and holistic approaches (Santonus 1998: 3) in which relations within interconnected networks are the order of the day (Youngblood 1997: 27; Wheatley 1999: 10). Here key terms are feedback, recursion, emergence, connectedness and self-organization. Out go the simplistic views of linear causality, the ability to predict, control and manipulate, and in come uncertainty, networks and connection, self-organization, emergence over time through feedback and the relationships of the internal and external environments, and survival and development through adaptation and change.

Chaos and complexity theories argue against the linear, deterministic, patterned, universalizable, stable, atomized, modernistic, objective, mechanist, controlled, closed systems of law-like behaviour which may be operating in the laboratory but which do not operate in the social world of education. These features of chaos and complexity theories seriously undermine the value of experiments and positivist research in education (e.g. Gleick 1987; Waldrop 1992; Lewin 1993).

Complexity theory suggests that phenomena must be looked at holistically; to atomize phenomena into a restricted number of variables and then to focus only on certain factors is to miss the necessary dynamic interaction of several parts. More fundamentally, complexity theory suggests that the conventional units of analysis in educational research (as in other fields) should move away from, for example, individuals, institutions, communities and systems (cf. Lemke 2001). These should merge, so that the unit of analysis becomes a web or ecosystem (Capra 1996: 301), focused on,

and arising from, a specific topic or centre of interest (a 'strange attractor'). Individuals, families, students, classes, schools, communities and societies exist in symbiosis; complexity theory tells us that their relationships are necessary, not contingent, and analytic, not synthetic. This is a challenging prospect for educational research, and complexity theory, a comparatively new perspective in educational research, offers considerable leverage into understanding societal, community, individual, and institutional change; it provides the nexus between macro- and micro-research in understanding and promoting change.

In addressing holism, complexity theory suggests the need for case study methodology, action research, and participatory forms of research, premised in many ways on interactionist, qualitative accounts, i.e. looking at situations through the eyes of as many participants or stakeholders as possible. This enables multiple causality, multiple perspectives and multiple effects to be charted. Self-organization, a key feature of complexity theory, argues for participatory, collaborative and multi-perspectival approaches to educational research. This is not to deny 'outsider' research; it is to suggest that, if it is conducted, outsider research has to take in as many perspectives as possible.

In educational research terms, complexity theory stands against simple linear methodologies based on linear views of causality, arguing for multiple causality and multidirectional causes and effects, as organisms (however defined: individuals, groups, communities) are networked and relate at a host of different levels and in a range of diverse ways. No longer can one be certain that a simple cause brings a simple or single effect, or that a single effect is the result of a single cause, or that the location of causes will be in single fields only, or that the location of effects will be in a limited number of fields.

Complexity theory not only questions the values of positivist research and experimentation, but also underlines the importance of educational research to catch the deliberate, intentional, agentic actions of participants and to adopt interactionist

and constructivist perspectives. Addressing complexity theory's argument for self-organization, the call is for the teacher-as-researcher movement to be celebrated, and complexity theory suggests that research in education could concern itself with the symbiosis of internal and external researchers and research partnerships. Just as complexity theory suggests that there are multiple views of reality, so this accords not only with the need for several perspectives on a situation (using multi-methods), but resonates with those tenets of critical research that argue for different voices and views to be heard. Heterogeneity is the watchword. Complexity theory not only provides a powerful challenge to conventional approaches to educational research, but also suggests both a substantive agenda and a set of methodologies. It provides an emerging new paradigm for research (see <http://www.routledge.com/textbooks/9780415368780> – Chapter 1, file 1.1.doc).

Feminist research

It is perhaps no mere coincidence that feminist research should surface as a serious issue at the same time as ideology-critical paradigms for research; they are closely connected. Usher (1996: 124), although criticizing Habermas for his faith in family life as a haven from a heartless, exploitative world, nevertheless sets out several principles of feminist research that resonate with the ideology critique of the Frankfurt School:

- acknowledging the pervasive influence of gender as a category of analysis and organization
- deconstructing traditional commitments to truth, objectivity and neutrality
- adopting an approach to knowledge creation which recognizes that all theories are perspectival
- using a multiplicity of research methods
- acknowledging the interdisciplinary nature of feminist research
- involving the researcher and the people being researched

- deconstructing the theory–practice relationship.

Her suggestions build on earlier recognition of the significance of addressing the 'power issue' in research ('whose research', 'research for whom', 'research in whose interests') and the need to address the emancipatory element of educational research – that research should be empowering to all participants. The paradigm of critical theory questioned the putative objective, neutral, value-free, positivist, 'scientific' paradigm for the splitting of theory and practice and for its reproduction of asymmetries of power (reproducing power differentials in the research community and for treating participants/respondents instrumentally – as objects).

Robson (1993: 64) suggests seven sources of sexism in research:

- *androcentricity*: seeing the world through male eyes and applying male research paradigms to females
- *overgeneralization*: when a study generalizes from males to females
- *gender insensitivity*: ignoring sex as a possible variable
- *double standards*: using male criteria, measures and standards to judge the behaviour of women and vice versa (e.g. in terms of social status)
- *sex appropriateness*: e.g. that child-rearing is women's responsibility
- *familism*: treating the family, rather than the individual, as the unit of analysis
- *sexual dichotomism*: treating the sexes as distinct social groups when, in fact, they may share characteristics.

Feminist research, too, challenges the legitimacy of research that does not empower oppressed and otherwise invisible groups – women. Ezzy (2002: 20) writes of the need to replace a traditional masculine picture of science with an emancipatory commitment to knowledge that stems from a feminist perspective, since, 'if women's experience is analysed using only theories and observations from the standpoint of men, the resulting theories

oppress women' (p. 23). Gender, as Ezzy (2002: 43) writes, is 'a category of experience'.

Positivist research served a given set of power relations, typically empowering the white, male-dominated research community at the expense of other groups whose voices were silenced. Feminist research seeks to demolish and replace this with a different substantive agenda – of empowerment, voice, emancipation, equality and representation for oppressed groups. In doing so, it recognizes the necessity for foregrounding issues of power, silencing and voicing, ideology critique and a questioning of the legitimacy of research that does not emancipate hitherto disempowered groups. In feminist research, women's consciousness of oppression, exploitation and disempowerment becomes a focus for research – the paradigm of ideology critique.

Far from treating educational research as objective and value-free, feminists argue that this is merely a smokescreen that serves the existing, disempowering status quo, and that the subject and value-laden nature of research must be surfaced, exposed and engaged (Haig 1999: 223). Supposedly value-free, neutral research perpetuates power differentials. Indeed Jayaratne and Stewart (1991) question the traditional, exploitative nature of much research in which the researchers receive all the rewards while the participants remain in their – typically powerless – situation, i.e. in which the status quo of oppression, under-privilege and inequality remain undisturbed. As Scott (1985: 80) writes: 'we may simply use other women's experiences to further our own aims and careers'. Cresswell (1998: 83), too, suggests that feminist research strives to establish collaborative and non-exploitative relationships. Indeed Scott (1985) questions how ethical it is for a woman researcher to interview those who are less privileged and more exploited than she herself is.

Changing this situation entails taking seriously issues of reflexivity, the effects of the research on the researched and the researchers, the breakdown of the positivist paradigm, and the raising of consciousness of the purposes and effects of the research. Ezzy (2002: 153) writes

that 'the personal experience of the researcher is an integral part of the research process' and reinforces the point that objectivity is a false claim by researchers.

Ribbens and Edwards (1997) suggest that it is important to ask how researchers can produce work with reference to theoretical perspectives and formal traditions and requirements of public, academic knowledge while still remaining faithful to the experiences and accounts of research participants. Denzin (1989), Mies (1993), Haig (1999) and De Laine (2000) argue for several principles in feminist research:

- The asymmetry of gender relations and representation must be studied reflexively as constituting a fundamental aspect of social life (which includes educational research).
- Women's issues, their history, biography and biology, feature as a substantive agenda/focus in research – moving beyond mere perspectival/methodological issues to setting a research agenda.
- The raising of consciousness of oppression, exploitation, empowerment, equality, voice and representation is a methodological tool.
- The acceptability and notion of objectivity and objective research must be challenged.
- The substantive, value-laden dimensions and purposes of feminist research must be paramount.
- Research must empower women.
- Research need not be undertaken only by academic experts.
- Collective research is necessary: women need to collectivize their own individual histories if they are to appropriate these histories for emancipation.
- There is a commitment to revealing core processes and recurring features of women's oppression.
- There is an insistence on the inseparability of theory and practice.
- There is an insistence on the connections between the private and the public, between the domestic and the political.

- There is a concern with the construction and reproduction of gender and sexual difference.
- Narrow disciplinary boundaries are rejected.
- The artificial subject/researcher dualism is rejected.
- Positivism and objectivity as male mythology are rejected.
- There is an increased use of qualitative, introspective biographical research techniques.
- The gendered nature of social research and the development of anti-sexist research strategies are recognized.
- There is a review of the research process as consciousness and awareness raising and as fundamentally participatory.
- The primacy of women's personal subjective experience is recognized.
- Hierarchies in social research are rejected.
- The vertical, hierarchical relationships of researchers, research community and research objects, in which the research itself can become an instrument of domination and the reproduction and legitimation of power elites, have to be replaced by research that promotes the interests of dominated, oppressed, exploited groups.
- The equal status and reciprocal relationships between subjects and researchers are recognized.
- There is a need to change the status quo, not merely to understand or interpret it.
- The research must be a process of conscientization, not research solely by experts for experts, but to empower oppressed participants.

Indeed Webb *et al.* (2004) set out six principles for a feminist pedagogy in the teaching of research methodology:

- reformulating the professor–student relationship (from hierarchy to equality and sharing)
- ensuring empowerment (for a participatory democracy)
- building community (through collaborative learning)
- privileging the individual voice (not only the lecturer's)

- respecting diversity of personal experience (rooted, for example, in gender, race, ethnicity, class, sexual preference)
- challenging traditional views (e.g. the sociology of knowledge).

Gender shapes research agendas, the choice of topics and foci, the choice of data collection techniques and the relationships between researchers and researched. Several methodological principles flow from a 'rationale' for feminist research (Denzin 1989; Mies 1993; Haig 1997, 1999; De Laine 2000):

- The replacement of quantitative, positivist, objective research with qualitative, interpretive, ethnographic reflexive research, as objectivity in quantitative research is a smokescreen for masculine interests and agendas.
- Collaborative, collectivist research undertaken by collectives – often of women – combining researchers and researched in order to break subject–object and hierarchical, non-reciprocal relationships.
- The appeal to alleged value-free, neutral, indifferent and impartial research has to be replaced by conscious, deliberate partiality – through researchers identifying with participants.
- The use of ideology-critical approaches and paradigms for research.
- The spectator theory or contemplative theory of knowledge in which researchers research from ivory towers has to be replaced by a participatory approach – perhaps action research – in which all participants (including researchers) engage in the struggle for women's emancipation – a liberatory methodology.
- The need to change the status quo is the starting point for social research – if we want to know something we change it. (Mies (1993) cites the Chinese saying that if you want to know a pear then you must chew it!).
- The extended use of triangulation and multiple methods (including visual techniques such as video, photograph and film).
- The use of linguistic techniques such as conversational analysis.
- The use of textual analysis such as deconstruction of documents and texts about women.
- The use of meta-analysis to synthesize findings from individual studies (see Chapter 13).
- A move away from numerical surveys and a critical evaluation of them, including a critique of question wording.

Edwards and Mauthner (2002: 15, 27) characterize feminist research as that which concerns a critique of dominatory and value-free research, the surfacing and rejection of exploitative power hierarchies between the researcher and the participants, and the espousal of close – even intimate – relationships between the researcher and the researched. Positivist research is rejected as *per se* oppressive (Gillies and Alldred 2002: 34) and inherently unable to abide by its own principle of objectivity; it is a flawed epistemology. Research, and its underpinning epistemologies, are rooted in, and inseparable from interests (Habermas 1972).

The move is towards 'participatory action research' in which empowerment and emancipation are promoted and which is an involved and collaborative process (e.g. De Laine 2000: 109 ff.). Participation recognizes 'power imbalances and the need to engage oppressed people as agents of their own change' (Ezzy 2002: 44), while action research recognizes the value of 'using research findings to inform intervention decisions' (p. 44). As De Laine (2000: 16) writes: the call is 'for more participation and less observation, of *being with* and *for* the other, not *looking at*', with relations of reciprocity and equality rather than impersonality, exploitation and power/status differentials between researcher and participants.

The relationship between the researcher and participant, De Laine argues, must break a conventional patriarchy. The emphasis is on partnerships between researchers and participants, to the extent that researchers are themselves participants rather than outsiders and the participants shape the research process as co-researchers (De Laine 2000: 107), defining the problem, the methods, the data collection and analysis, interpretation and dissemination. The

relationship between researchers and participants is one of equality, and outsider, objective, distant, positivist research relations are off the agenda; researchers are inextricably bound up in the lives of those they research. That this may bring difficulties in participant and researcher reactivity is a matter to be engaged rather than built out of the research.

Thapar-Björkert and Henry (2004) argue that the conventional, one-sided and unidirectional view of the researcher as powerful and the research participants as less powerful, with the researcher exploiting and manipulating the researched, could be a construction by western white researchers. They report research that indicates that power is exercised by the researched as well as the researchers, and is a much more fluid, shifting and negotiated matter than conventionally suggested, being dispersed through both the researcher and the researched. Indeed they show how the research participants can, and do, exercise considerable power over the researchers both before, during and after the research process. They provide a fascinating example of interviewing women in their homes in India, where, far from the home being a location of oppression, it was a site of their power and control.

With regard to methods of data collection, Oakley (1981) suggests that 'interviewing women' in the standardized, impersonal style which expects a response to a prescribed agenda and set of questions may be a 'contradiction in terms', as it implies an exploitative relationship. Rather, the subject-object relationship should be replaced by a guided dialogue. She criticizes the conventional notion of 'rapport' in conducting interviews (Oakley 1981: 35), arguing that they are instrumental, non-reciprocal and hierarchical, all of which are masculine traits. Rapport in this sense, she argues, is not genuine in that the researcher is using it for scientific rather than human ends (Oakley 1981: 55). Here researchers are 'faking friendship' for their own ends (Duncombe and Jessop 2002: 108), equating 'doing rapport' with trust, and, thereby, operating a very 'detached' form of friendship (p. 110). Similarly Thapar-Björkert and Henry (2004) suggest that attempts

at friendship between researchers and participants are disingenuous, with 'purported solidarity' being a fraud perpetrated by well-intentioned feminists.

Duncombe and Jessop (2002: 111) ask a very searching question when they question whether, if interviewees are persuaded to take part in an interview by virtue of the researcher's demonstration of empathy and 'rapport', this is really giving informed consent. They suggest that informed consent, particularly in exploratory interviews, has to be continually renegotiated and care has to be taken by the interviewer not to be too intrusive. Personal testimonies, oral narratives and long interviews also figure highly in feminist approaches (De Laine 2000: 110; Thapar-Björkert and Henry 2004), not least in those that touch on sensitive issues. These, it is argued (Ezzy 2002: 45), enable women's voices to be heard, to be close to lived experiences, and avoid unwarranted assumptions about people's experiences.

The drive towards collective, egalitarian and emancipatory qualitative research is seen as necessary if women are to avoid colluding in their own oppression by undertaking positivist, uninvolved, dispassionate, objective research. Mies (1993: 67) argues that for women to undertake this latter form of research puts them into a schizophrenic position of having to adopt methods which contribute to their own subjugation and repression by ignoring their experience (however vicarious) of oppression and by forcing them to abide by the 'rules of the game' of the competitive, male-dominated academic world. In this view, argue Roman and Apple (1990: 59), it is not enough for women simply to embrace ethnographic forms of research, as this does not necessarily challenge the existing and constituting forces of oppression or asymmetries of power. Ethnographic research, they argue, has to be accompanied by ideology critique; indeed they argue that the transformative, empowering, emancipatory potential of research is a critical standard for evaluating that piece of research.

This latter point resonates with the call by Lather (1991) for researchers to be concerned with the political consequences of their research (e.g. consequential validity), not only the conduct of the research and data analysis itself.

Research must lead to change and improvement, particularly, in this context, for women (Gillies and Alldred 2002: 32). Research is a political activity with a political agenda (Gillies and Alldred 2002: 33; see also Lather 1991). Research and action – praxis – must combine ‘knowledge for’ as well as ‘knowledge what’ (Ezzy 2002: 47). As Marx reminds us in his *Theses on Feuerbach*: ‘the philosophers have only interpreted the world, in various ways; the point, however, is to change it’. Gillies and Alldred (2002: 45), however, point out that ‘many feminists have agonized over whether politicizing participants is necessarily helpful’, as it raises awareness of constraints on their actions without being able to offer solutions or to challenge their structural causes. Research, thus politicized but unable to change conditions, may actually be disempowering and, indeed, patronizing in its simplistic call for enlightenment and emancipation. It could render women more vulnerable than before. Emancipation is a struggle.

Several of these views of feminist research and methodology are contested by other feminist researchers. For example, Jayaratne (1993: 109) argues for ‘fitness for purpose’, suggesting that exclusive focus on qualitative methodologies might not be appropriate either for the research purposes or, indeed, for advancing the feminist agenda (see also Scott 1985: 82-3). Jayaratne refutes the argument that quantitative methods are unsuitable for feminists because they neglect the emotions of the people under study. Indeed she argues for beating quantitative research on its own grounds (Jayaratne 1993: 121), suggesting the need for feminist quantitative data and methodologies in order to counter sexist quantitative data in the social sciences. She suggests that feminist researchers can accomplish this without ‘selling out’ to the positivist, male-dominated academic research community. Oakley (1998) suggests that the separation of women from quantitative methodology may have the unintended effect of perpetuating women as the ‘other’, and, thereby, discriminating against them.

De Laine (2000: 112) argues that shifting from quantitative to qualitative techniques may not

solve many ethical problems in research, as these are endemic in any form of fieldwork. She argues that some feminist researchers may not wish to seek either less participation or more detachment, and that more detachment and less participation are not solutions to ethical dilemmas and ‘morally responsible fieldwork’ as these, too, bring their own ethical dilemmas, e.g. the risk of threat. She reports work (p. 113) that suggests that close relationships between researchers and participants may be construed as just as exploitative, if more disguised, as conventional researcher roles, and that they may bring considerable problems if data that were revealed in an intimate account between friends (researcher and participant) are then used in public research. The researcher is caught in a dilemma: if she is a true friend then this imposes constraints on the researcher, and yet if she is only pretending to be a friend, or limiting that friendship, then this provokes questions of honesty and personal integrity. Are research friendships real, ephemeral, or impression management used to gather data?

De Laine (2000: 115) suggests that it may be misguided to privilege qualitative research for its claim to non-exploitative relationships. While she acknowledges that quantitative approaches may perpetuate power differentials and exploitation, there is no guarantee that qualitative research will not do the same, only in a more disguised way. Qualitative approaches too, she suggests, can create and perpetuate unequal relations, not least simply because the researcher is in the field *qua* researcher rather than a friend; if it were not for the research then the researcher would not be present. Stacey (1988) suggests that the intimacy advocated for feminist ethnography may render exploitative relationships *more* rather than *less* likely. We refer readers to Chapter 5 on sensitive educational research for a further discussion of these issues.

Gillies and Alldred (2002: 43-6) suggest that action research, an area strongly supported in some quarters of feminist researchers, is, itself, problematic. It risks being an intervention in people’s lives (i.e. a potential abuse of power), and the researcher typically plays a significant, if not

central, role in initiating, facilitating, crystallizing and developing the meanings involved in, or stemming from, the research, i.e. the researcher is the one exercising power and influence.

Ezzy (2002: 44) reports that, just as there is no single feminist methodology, both quantitative and qualitative methods are entirely legitimate. Indeed, Kelly (1978) argues that a feminist commitment should enter research at the stages of formulating the research topic and interpreting the results, but it should be left out during the stages of data collection and conduct of the research.

Thapar-Björkert and Henry (2004) indicate that the researcher being an outsider might bring more advantages than if she were an insider. For example, being a white female researching non-white females may not be a handicap, as many non-white women might disclose information to white women that they would not disclose to a non-white person. Similarly, having interviewers and interviewees of the same racial and ethnic background does not mean that non-hierarchical relationships will still not be present. They also report that the categories of 'insider' and 'outsider' were much more fuzzy than exclusive. Researchers are both 'subject' and 'object', and those being researched are both 'observed' and 'observers'.

De Laine (2000: 110) suggests that there is a division among feminists between those who advocate closeness in relationships between researchers and subjects – a human researching fellow humans – and those who advocate 'respectful distance' between researchers and those being studied. Close relationships may turn into quasi-therapeutic situations rather than research (Duncombe and Jessop 2002: 111), yet it may be important to establish closeness in reaching deeper issues. Further, one has to question how far close relationships lead to reciprocal and mutual disclosure (p. 120). The debate is open: should the researcher share, be close and be prepared for more intimate social relations – a 'feminist ethic of care' (p. 111) – or keep those cool, outsider relations which might objectify those being researched? It is a moral as well as a methodological matter.

The issue runs deep: the suggestion is that emotions and feelings are integral to the research,

rather than to be built out of the research in the interests of objectivity (Edwards and Mauthner 2002: 19). Emotions should not be seen as disruptive of research or as irrelevant (De Laine 2000: 151–2), but central to it, just as they are central to human life. Indeed emotional responses are essential in establishing the veracity of inquiries and data, and the 'feminist communitarian model' which De Laine (2000: 212–13) outlines values connectedness at several levels: emotions, emotionality and personal expressiveness, empathy. The egalitarian feminism that De Laine (2000: 108) and others advocate suggests a community of insiders in the same culture, in which empathy, reciprocity and egalitarianism are hallmarks.

Swantz (1996: 134) argues that there may be some self-deception by the researcher in adopting a dual role as a researcher and one who shares the situation and interests of the participants. She questions the extent to which the researcher may be able to be genuinely involved with the participants in other than a peripheral way and whether, simply because the researcher may have 'superior knowledge', a covert power differential may exist. De Laine (2000: 114) suggests that such superior knowledge may stem from the researcher's own background in anthropology or ethnography or simply more education. The primary purpose of the researcher is research, and that is different from the primary purpose of the participants.

Further, the researcher's desire for identification and solidarity with her research subjects may be pious but unrealistic optimism, not least because she may not share the same race, ethnicity, background, life chances, experiences or colour as those being researched. Indeed Gillies and Alldred (2002: 39–40) raise the question of how far researchers can, or should, try to represent groups to which they themselves do not belong – not least those groups without power or voice. As this, itself, is a form of colonization and oppression. Affinity, they argue (p. 40), is an authoritative basis for representative research. Even the notion of affinity becomes suspect when it overlooks, or underplays, the significance of difference, thereby homogenizing groups and t

particular experiences. In response to this, some feminist researchers (p. 40) suggest that researchers only have the warrant to confine themselves to their own immediate communities, though this is a contentious issue. There is value in speaking for others, not least for those who are silenced and marginalized, and in not speaking for others for fear of oppression and colonization. One has to question the acceptability and appropriateness of, and fidelity to, the feminist ethic, if one represents and uses others' stories (p. 41).

An example of a feminist approach to research is the Girls Into Science and Technology (GIST) action research project. This took place over three years, involving 2,000 students and their teachers in ten coeducational, comprehensive schools in one area of the United Kingdom, eight schools serving as the bases of the 'action', the remaining two acting as 'controls'. Several publications have documented the methodologies and findings of the GIST study (Kelly 1986; 1989a; 1989b; Kelly and Smail 1986; Whyte 1986), described by its co-director as 'simultaneous-integrated action research' (Kelly 1987) (i.e. integrating action and research). Kelly is open about the feminist orientation of the GIST project team, seeking deliberately to change girls' option choices and career aspirations, because the researchers saw that girls were disadvantaged by traditional sex-stereotypes. The researchers' actions, she suggests, were a small attempt to ameliorate women's subordinate social position (Kelly 1987).

Research and evaluation

The preceding discussion has suggested that research and politics are inextricably bound together. This can be taken further, as researchers in education will be advised to pay serious consideration to the politics of their research enterprise and the ways in which politics can steer research. For example, one can detect a trend in educational research towards more evaluative research, where, for example, a researcher's task is to evaluate the effectiveness (often of the implementation) of given policies and projects. This is particularly true in the

case of 'categorically funded' and commissioned research – research which is funded by policy-makers (e.g. governments, fund-awarding bodies) under any number of different headings that those policy-makers devise (Burgess 1993). On the one hand, this is laudable, for it targets research directly towards policy; on the other hand, it is dangerous in that it enables others to set the research agenda. Research ceases to become open-ended, pure research, and, instead, becomes the evaluation of *given* initiatives. Less politically charged, much research is evaluative, and indeed there are many similarities between research and evaluation. The two overlap but possess important differences. The problem of trying to identify differences between evaluation and research is compounded because not only do they share several of the same methodological characteristics but also one branch of research is called *evaluative research* or *applied research*. This is often kept separate from 'blue skies' research in that the latter is open-ended, exploratory, contributes something original to the substantive field and extends the frontiers of knowledge and theory whereas in the former the theory is *given* rather than *interrogated* or *tested*. One can detect many similarities between the two in that they both use methodologies and methods of social science research generally, covering, for example (see <http://www.routledge.com/textbooks/9780415368780> – Chapter 1, file 1.9. ppt), the following:

- the need to clarify the *purposes* of the investigation
- the need to *operationalize* purposes and areas of investigation
- the need to address principles of *research design* that include:
 - formulating *operational questions*
 - deciding appropriate *methodologies*
 - deciding which *instruments* to use for data collection
 - deciding on the *sample* for the investigation
 - addressing *reliability* and *validity* in the investigation and instrumentation

- addressing *ethical* issues in conducting the investigation
- deciding on *data analysis* techniques
- deciding on *reporting* and *interpreting* results.

Indeed Norris (1990) argues that evaluation applies research methods to shed light on a problem of action (Norris 1990: 97); he suggests that evaluation can be viewed as an extension of research, because it shares its methodologies and methods, and because evaluators and researchers possess similar skills in conducting investigations (see <http://www.routledge.com/textbooks/9780415368780> – Chapter 1, file 1.10. ppt). In many senses the eight features outlined above embrace many elements of the *scientific method*, which Smith and Glass (1987) set out in seven steps:

- 1 A *theory* about the phenomenon exists.
- 2 A *research problem* within the theory is detected and a *research question* is devised.
- 3 A *research hypothesis* is deduced (often about the relationship between constructs).
- 4 A *research design* is developed, *operationalizing* the research question and stating the *null hypothesis*.
- 5 The research is conducted.
- 6 The null hypothesis is tested based on the data gathered.
- 7 The original theory is revised or supported based on the results of the hypothesis testing.

Indeed, if steps 1 and 7 were removed then there would be nothing to distinguish between research and evaluation. Both researchers and evaluators pose questions and hypotheses, select samples, manipulate and measure variables, compute statistics and data, and state conclusions. Nevertheless there are important differences between evaluation and research that are not always obvious simply by looking at publications. Publications do not always make clear the background events that gave rise to the investigation, nor do they always make clear the uses of the material that they report, nor do they always make clear what the dissemination rights (Sanday 1993) are and who holds them. Several commentators

set out some of the differences between evaluation and research. For example Smith and Glass (1987) offer eight main differences (see <http://www.routledge.com/textbooks/9780415368780> – Chapter 1, file 1.11. ppt):

- *The intents and purposes of the investigation*: the researcher wants to advance the frontiers of knowledge of phenomena, to contribute to theory and to be able to make generalizations; the evaluator is less interested in contributing to theory or the general body of knowledge. Evaluation is more parochial than universal (Smith and Glass 1987: 33–4).
- *The scope of the investigation*: evaluation studies tend to be more comprehensive than research in the number and variety of aspects of a programme that are being studied (p. 34).
- *Values in the investigation*: research aspires to value neutrality, evaluations must represent multiple sets of values and include data on these values.
- *The origins of the study*: research has its origins and motivation in the researcher's curiosity and desire to know (p. 34). The researcher is answerable to colleagues and scientists (i.e. the research community) whereas the evaluator is answerable to the 'client'. The researcher is autonomous whereas the evaluator is answerable to clients and stakeholders. The researcher is motivated by a search for knowledge, the evaluator is motivated by the need to solve problems, allocate resources and make decisions. Research studies are public, evaluations are for a restricted audience.
- *The uses of the study*: the research is used to further knowledge, evaluations are used to inform decisions.
- *The timeliness of the study*: evaluations must be timely, research need not be. Evaluators' time scales are given, researchers' time scales need not be given.
- *Criteria for judging the study*: evaluations are judged by the criteria of utility and credibility, research is judged methodologically and by the contribution that it makes to the field (i.e. internal and external validity).

- *The agendas of the study:* an evaluator's agenda is given, a researcher's agenda is his or her own.

Norris (1990) reports an earlier piece of work by Glass and Worthen (1971) in which they identified eleven main differences between evaluation and research:

- *The motivation of the inquirer:* research is pursued largely to satisfy curiosity, evaluation is undertaken to contribute to the solution of a problem.
- *The objectives of the research:* research and evaluation seek different ends. Research seeks conclusions, evaluation leads to decisions.
- *Laws versus description:* research is the quest for laws (nomothetic), evaluation merely seeks to describe a particular thing (idiographic).
- *The role of explanation:* proper and useful evaluation can be conducted without producing an explanation of why the product or project is good or bad or of how it operates to produce its effects.
- *The autonomy of the inquiry:* evaluation is undertaken at the behest of a client, while researchers set their own problems.
- *Properties of the phenomena that are assessed:* evaluation seeks to assess social utility directly, research may yield evidence of social utility but often only indirectly.
- *Universality of the phenomena studied:* researchers work with constructs having a currency and scope of application that make the objects of evaluation seem parochial by comparison.
- *Salience of the value question:* in evaluation value questions are central and usually determine what information is sought.
- *Investigative techniques:* while there may be legitimate differences between research and evaluation methods, there are far more similarities than differences with regard to techniques and procedures for judging validity.
- *Criteria for assessing the activity:* the two most important criteria for judging the adequacy of research are internal and external validity, for evaluation they are utility and credibility.

- *Disciplinary base:* the researcher can afford to pursue inquiry within one discipline and the evaluator cannot.

A clue to some of the differences between evaluation and research can be seen in the definition of evaluation. Most definitions of evaluation include reference to several key features:

- answering specific, given questions
- gathering information
- making judgements
- taking decisions
- addressing the politics of a situation (Morrison 1993: 2).

(See <http://www.routledge.com/textbooks/9780415368780> – Chapter 1, file 1.12. ppt.) Morrison (1993: 2) provides one definition of evaluation as: *the provision of information about specified issues upon which judgements are based and from which decisions for action are taken*. This view echoes MacDonald (1987) in his comments that the evaluator

is faced with competing interest groups, with divergent definitions of the situation and conflicting informational needs He has to decide which decision-makers he will serve, what information will be of most use, when it is needed and how it can be obtained The resolution of these issues commits the evaluator to a political stance, an attitude to the government of education. No such commitment is required of the researcher. He stands outside the political process, and values his detachment from it. For him the production of new knowledge and its social use are separated. The evaluator is embroiled in the action, built into a political process which concerns the distribution of power, i.e. the allocation of resources and the determination of goals, roles and tasks When evaluation data influences power relationships the evaluator is compelled to weight carefully the consequences of his task specification The researcher is free to select his questions, and to seek answers to them. The evaluator, on the other hand, must never fall into the error of answering questions which no one but he is asking. (MacDonald 1987: 42)

MacDonald (1987) argues that evaluation is an inherently political enterprise. His much-used threefold typification of evaluations as autocratic, bureaucratic and democratic is premised on a political reading of evaluation (see also Chelinsky and Mulhauser (1993: 54) who refer to 'the inescapability of politics' in the world of evaluation). MacDonald (1987: 101), noting that 'educational research is becoming more evaluative in character', argues for research to be kept out of politics and for evaluation to square up to the political issues at stake:

The danger therefore of conceptualizing evaluation as a branch of research is that evaluators become trapped in the restrictive tentacles of research respectability. Purity may be substituted for utility, trivial proofs for clumsy attempts to grasp complex significance. How much more productive it would be to define research as a branch of evaluation, a branch whose task it is to solve the technological problems encountered by the evaluator.

(MacDonald 1987: 43)

However, the truth of the matter is far more blurred than these distinctions suggest. Two principal causes of this blurring lie in the *funding* and the *politics* of both evaluation and research. For example, the view of research as uncontaminated by everyday life is naive and simplistic; Norris (1990: 99) argues that such an antiseptic view of research ignores the social context of educational research, some of which is located in the hierarchies of universities and research communities and the funding support provided for some research projects but not all by governments. His point has a pedigree that reaches back to Kuhn (1962), and is commenting on the politics of research funding and research utilization. Since the early 1980s one can detect a huge rise in 'categorical' funding of projects, i.e. defined, *given* projects (often by government or research sponsors) for which bids have to be placed. This may seem unsurprising if one is discussing research grants by government bodies, which are deliberately policy-oriented, though one can also detect in projects that have been granted by non-governmental organizations

(e.g. the UK Economic and Social Research Council) a move towards sponsoring policy-oriented projects rather than the 'blue-skies' research mentioned earlier. Indeed Burgess (1993: 1) argues that 'researchers are little more than contract workers...research in education must become policy relevant...research must come closer to the requirement of practitioners'.

This view is reinforced by several chapters in the collection edited by Anderson and Biddle (1991) which show that research and politics go together uncomfortably because researchers have different agendas and longer time scales than politicians and try to address the complexity of situations, whereas politicians, anxious for short-term survival want telescoped time scales, simple remedies and research that will be consonant with their political agendas. Indeed James (1993) argues that

the power of research-based evaluation to provide evidence on which rational decisions can be expected to be made is quite limited. Policy-makers will always find reasons to ignore, or be highly selective of, evaluation findings if the information does not support the particular political agenda operating at the time when decisions have to be made.

(James 1993: 135)

The politicization of research has resulted in funding bodies awarding research grants for categorical research that specify time scales and the terms of reference. Burgess's (1993) view also points to the constraints under which research is undertaken; if it is not concerned with policy issues then research tends not to be funded. One could support Burgess's view that research must have some impact on policy-making.

Not only is *research* becoming a political issue, but also this extends to the use being made of *evaluation* studies. It was argued above that evaluations are designed to provide useful data to inform decision-making. However, as evaluation has become more politicized so its uses (or non-uses) have become more politicized. Indeed Norris (1990) shows how politics frequently overrides evaluation or research evidence. Norris (1990: 135) writes that the announcement of the decision

to extend the TVEI project was made without any evaluation reports having been received from evaluation teams in Leeds or the National Foundation for Educational Research. (The Technical and Vocational Education Initiative (TVEI) was a 1980s UK government-funded project frequently targeted to lower-attaining students.) This echoes James (1993) where she writes:

The classic definition of the role of evaluation as providing information for decision-makers... is a fiction if this is taken to mean that policy-makers who commission evaluations are expected to make rational decisions based on the best (valid and reliable) information available to them.

(James 1993: 119)

Where evaluations are commissioned and have heavily political implications, Stronach and Morris (1994) argue that the response to this is that evaluations become more 'conformative' (see <http://www.routledge.com/textbooks/9780415368780> – Chapter 1, file 1.13. ppt), possessing several characteristics:

- Being short-term, taking project goals as given and supporting their realization.
- Ignoring the evaluation of longer-term learning outcomes, or anticipated economic or social consequences of the programme.
- Giving undue weight to the perceptions of programme participants who are responsible for the successful development and implementation of the programme; as a result, tending to 'over-report' change.
- Neglecting and 'under-reporting' the views of classroom practitioners, and programme critics.
- Adopting an atheoretical approach, and generally regarding the aggregation of opinion as the determination of overall significance.
- Involving a tight contractual relationship with the programme sponsors that either disbars public reporting, or encourages self-censorship in order to protect future funding prospects.
- Undertaking various forms of implicit advocacy for the programme in its reporting style.
- Creating and reinforcing a professional schizophrenia in the research and evaluation

community, whereby individuals come to hold divergent public and private opinions, or offer criticisms in general rather than in particular, or quietly develop 'academic' critiques which are at variance with their contractual evaluation activities, alternating between 'critical' and 'conformative' selves.

The argument so far has been confined to large-scale projects that are influenced by and may or may not influence political decision-making. However, the argument need not remain there. Morrison (1993), for example, indicates how evaluations might influence the 'micro-politics of the school'. Hoyle (1986), for example, asks whether evaluation data are used to bring resources into, or take resources out of, a department or faculty. The issue does not relate only to evaluations, for school-based research, far from the emancipatory claims for it made by action researchers (e.g. Carr and Kemmis 1986; Grundy 1987), is often concerned more with finding out the most successful ways of organization, planning, teaching and assessment of a *given agenda* rather than *setting agendas* and following one's own research agendas. This is *problem-solving* rather than *problem-setting*. That evaluation and research are being drawn together by politics at both macro-level and micro-level is evidence of a growing interventionism by politics into education, thus reinforcing the hegemony of the government in power. Several points have been made here:

- There is considerable overlap between evaluation and research.
- There are some *conceptual* differences between evaluation and research, though, in practice, there is considerable blurring of the edges of the differences between the two.
- The funding and control of research and research agendas reflect the persuasions of political decision-makers.
- Evaluative research has increased in response to categorical funding of research projects.
- The attention being given to, and utilization of, evaluation varies according to the consonance between the findings and their political attractiveness to political decision-makers.

In this sense the views expressed earlier by MacDonald (1987) are now little more than an historical relic; there is very considerable blurring of the edges between evaluation and research because of the political intrusion into, and use of, these two types of study. One response to this can be seen in Burgess's (1993) view that a researcher needs to be able to meet the sponsor's requirements for evaluation while also generating research data (engaging the issues of the need to negotiate ownership of the data and intellectual property rights).

Research, politics and policy-making

The preceding discussion has suggested that there is an inescapable political dimension to educational research, both in the macro- and micro-political senses. In the macro-political sense this manifests itself in funding arrangements, where awards are made provided that the research is 'policy-related' (Burgess 1993) – guiding policy decisions, improving quality in areas of concern identified by policy-makers, facilitating the implementation of policy decisions, evaluating the effects of the implementation of policy. Burgess notes a shift here from a situation where the researcher specifies the topic of research and towards the sponsor specifying the focus of research. The issue of sponsoring research reaches beyond simply commissioning research towards the dissemination (or not) of research – who will receive or have access to the findings and how the findings will be used and reported. This, in turn, raises the fundamental issue of who owns and controls data, and who controls the release of research findings. Unfavourable reports might be withheld for a time, suppressed or selectively released! Research can be brought into the service of wider educational purposes – the politics of a local education authority, or indeed the politics of government agencies.

Though research and politics intertwine, the relationships between educational research, politics and policy-making are complex because research designs strive to address a complex social reality (Anderson and Biddle 1991); a

piece of research does not feed simplistically or directly into a specific piece of policy-making. Rather, research generates a range of different types of knowledge – concepts, propositions, explanations, theories, strategies, evidence, methodologies (Caplan 1991). These feed subtly and often indirectly into the decision-making process, providing, for example, direct inputs, general guidance, a scientific gloss, orienting perspectives, generalizations and new insights. Basic and applied research have significant parts to play in this process.

The degree of influence exerted by research depends on careful dissemination; too little and its message is ignored, too much and data overload confounds decision-makers and makes them cynical – the syndrome of the boy who cried wolf (Knott and Wildavsky 1991). Hence researchers must give care to utilization by policy-makers (Weiss 1991a), reduce jargon, provide summaries, and improve links between the two cultures of researchers and policy-makers (Cook 1991) and, further, to the educational community. Researchers must cultivate ways of influencing policy, particularly when policy-makers can simply ignore research findings, commission their own research (Cohen and Garet 1991) or underfund research into social problems (Coleman 1991; Thomas 1991). Researchers must recognize their links with the power groups who decide policy. Research utilization takes many forms depending on its location in the process of policy-making, e.g. in research and development, problem solving, interactive and tactical models (Weiss 1991b). Researchers will have to judge the most appropriate forms of utilization of their research (Alkin *et al.* 1991).

The impact of research on policy-making depends on its degree of consonance with the political agendas of governments (Thomas 1991) and policy-makers anxious for their own political survival (Cook 1991) and the promotion of their social programmes. Research is used if it is politically acceptable. That the impact of research on policy is intensely and inescapably political is a truism (Horowitz and Katz 1991; Kamin 1991; Selleck 1991; Wineburg 1991). Research

too easily becomes simply an 'affirmatory text' which 'exonerates the system' (Wineburg 1991) and is used by those who seek to hear in it only echoes of their own voices and wishes (Kogan and Atkin 1991).

There is a significant tension between researchers and policy-makers. The two parties have different, and often conflicting, interests, agendas, audiences, time scales, terminology, and concern for topicality (Levin 1991). These have huge implications for research styles. Policy-makers anxious for the quick fix of superficial facts, short-term solutions and simple remedies for complex and generalized social problems (Cartwright 1991; Cook 1991) – the Simple Impact model (Biddle and Anderson 1991; Weiss 1991a; 1991b) – find positivist methodologies attractive, often debasing the data through illegitimate summary. Moreover, policy-makers find much research uncertain in its effects (Cohen and Garet 1991; Kerlinger 1991), dealing in a *Weltanschauung* rather than specifics, and being too complex in its designs and of limited applicability (Finn 1991). This, reply the researchers, misrepresents the nature of their work (Shavelson and Berliner 1991) and belies the complex reality which they are trying to investigate (Blalock 1991). Capturing social complexity and serving political utility can run counter to each other.

The issue of the connection between research and politics – power and decision-making – is complex. On another dimension, the notion that research is inherently a political act because it is part of the political processes of society has not been lost on researchers. Usher and Scott (1996: 176) argue that positivist research has allowed a traditional conception of society to be preserved relatively unchallenged – the white, male, middle-class researcher – to the relative exclusion of 'others' as legitimate knowers. That this reaches into epistemological debate is evidenced in the issues of who defines the 'traditions of knowledge' and the disciplines of knowledge; the social construction of knowledge has to take into account the differential power of groups to define what is

worthwhile research knowledge, what constitutes acceptable focuses and methodologies of research and how the findings will be used.

Methods and methodology

We return to our principal concern, methods and methodology in educational research. By methods, we mean that range of approaches used in educational research to gather data which are to be used as a basis for inference and interpretation, for explanation and prediction. Traditionally, the word refers to those techniques associated with the positivistic model – eliciting responses to predetermined questions, recording measurements, describing phenomena and performing experiments. For our purposes, we will extend the meaning to include not only the methods of normative research but also those associated with interpretive paradigms – participant observation, role-playing, non-directive interviewing, episodes and accounts. Although methods may also be taken to include the more specific features of the scientific enterprise such as forming concepts and hypotheses, building models and theories, and sampling procedures, we will limit ourselves principally to the more general techniques which researchers use.

If methods refer to techniques and procedures used in the process of data-gathering, the aim of methodology then is to describe approaches to, kinds and paradigms of research (Kaplan 1973). Kaplan suggests that the aim of methodology is to help us to understand, in the broadest possible terms, not the products of scientific inquiry but the process itself.

We, for our part, will attempt to present normative and interpretive perspectives in a complementary light and will try to lessen the tension that is sometimes generated between them. Merton and Kendall (1946)⁷ express the same sentiment:

Social scientists have come to abandon the spurious choice between qualitative and quantitative data: they are concerned rather with that combination of both which makes use of the most valuable features of each. The problem becomes one of determining at

which points they should adopt the one, and at which the other, approach.

(Merton and Kendall 1946)

The term *research* itself may take on a range of meanings and thereby be legitimately applied to a variety of contexts from, say, an investigation into the techniques of Dutch painters of the seventeenth century to the problem of finding more efficient means of improving traffic flow in major city centres. For our purposes, however, we will restrict its usages to those activities and undertakings aimed at developing a science of behaviour, the word *science* itself implying both normative and interpretive perspectives. Accordingly, when we speak of social research,

we have in mind the systematic and scholarly application of the principles of a science of behaviour to the problems of people within their social contexts and when we use the term educational research, we likewise have in mind the application of these same principles to the problems of teaching and learning within the formal educational framework and to the clarification of issues having direct or indirect bearing on these concepts.

The particular value of scientific research in education is that it will enable educators to develop the kind of sound knowledge base that characterizes other professions and disciplines; and one that will ensure education a maturity and sense of progression it at present lacks.