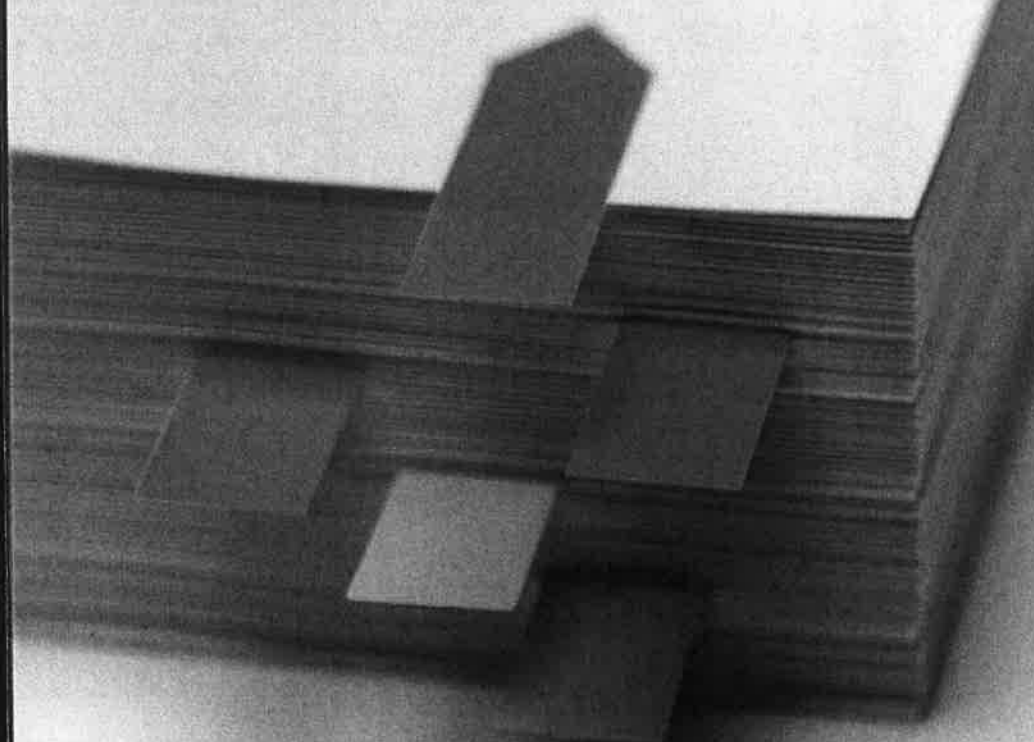


Richard Pring

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Research methods: philosophical issues they raise

Two traditions?

Carr (1995) draws a distinction between two quite different and warring philosophical traditions which have dominated educational research. On the one hand, there is a tradition that sees educational research as a subset of the social sciences. Thus, it seeks the general laws or conditions which will enable teachers or policy-makers to predict what will happen if . . . It seeks to establish empirically the most efficient and effective ways of attaining certain goals. The goals might be established by government (e.g. that X per cent of 16-year-olds achieve examination Grades A–C); these are the measured 'outcomes' to be pursued. Researchers would show what schools must do to reach those targets – how they might be *effective*.

Therefore, educational research has been dominated by empirical enquiries, initially from within educational psychology, more recently from within sociology, and more recently still from what is referred to the 'evidence-based' tradition pioneered in medicine.

'Learning theory' used to be included in the training of teachers. Such theory or theories included, for example, reference to the

'instinct theory' of McDougal who defined psychology as 'the positive science of the conduct of living creatures', and to Hull's *Principles of Behaviour*. The behaviourist traditions of Pavlov, Watson, Thorndike and Skinner aimed to produce effective ways of managing classrooms through behaviour modification or through the establishment of principles of reinforcement and reward (see Peters, 1974, chapters 1 and 2). But, apart from questions about the theories themselves, the verb 'to learn', as explained in Chapter 3, is far too complex in terms of the logically different kinds of concepts, actions, skills and so on, which are learnt, for it to be fitted into something called 'learning theory'.

Although such theories are now largely neglected, the scientific model is not. Indeed, it is often assumed to be the most appropriate one in the introductions to textbooks on educational research. Cohen and Mannion's popular text quotes Kerlinger's definition of research as 'the systematic, controlled, empirical and critical investigation of hypothetical propositions about the presumed relationships among natural phenomena' (1985, p. 5). Indeed, educational progress is seen to have been 'slow and unsure' because of this failure to be truly scientific, relying upon unreflective experience, common sense, 'subjective' views and untested opinion.

Such views are assumed in not a little of the research into effective schools. They influence, too, the advocacy of evidence-based educational policy and practice where the model of randomized controlled tests (RCTs) in medicine is being translated into other public services including education.

However, in the previous chapter's account of educational practice, I referred to that nurturing of the mental capacities through which the learners come to know, understand, judge, reflect and behave intelligently. There is something logically distinctive about such mental descriptions, indicating the apparent inappropriateness of the scientific model. A proper description of an *educational* activity (viz. the transaction between teacher and learner through which the learner comes to see and to understand in a different, more definitive manner) cannot avoid reference to the 'mental state' (the perceptions, understandings, feelings and indeed valuings) of the learner. In a sense, these have a 'life of their own'. Present thoughts shape future reactions. Future understandings to

some degree are adaptations or reconstructions of previous ones. Therefore, it is concluded that the scientific model is simply not appropriate. There is a world of difference between the sort of enquiry appropriate for understanding physical reality and the sort of enquiry for understanding the mental life of individual persons. 'Man' is not 'a subject of science', despite what the philosopher A. J. Ayer (1963) argued.

For that reason, a quite different tradition of educational research has prevailed, one which purports to reveal the understandings and perceptions of the subjects of research, namely, 'the phenomenology' of the mind. Such are the peculiarities of each person's perceptions and interpretations of events that significant generalization is impossible. Persons cannot be the object of scientific enquiry (though no doubt their biological functioning can be). Since an 'educational practice' is where individuals 'make sense' (starting from their different perspectives) of experience, struggle to understand and come to find value in different things and activities, then it cannot be grasped within general laws or theories. Educational enquiry becomes focused upon individuals, making explicit what is unique and distinctive of the 'thinking life' of each, and interpreting what is seen through the personal ideas which make each action intelligible.

Such a clear dichotomy between different research traditions (what Dewey, 1916, p. 323, referred to as 'false dualisms') permeates so much of research writing. It is reflected in the contrast between the objective world of physical things and the subjective world of 'meanings', between the public world of outer reality and the private world of inner thoughts, and therefore between the quantitative methods based on a scientific model and the qualitative methods based on a kind of phenomenological exposure.

One purpose of this book is to show that such dichotomies are mistaken, that researchers have fallen into a philosophical trap, which is very old indeed. It is the ancient dualism between mind and body, between the publicly accessible and the privately privileged. Educational research is both and neither. This I shall explain in greater detail in the next chapter. Meanwhile, I need to indicate the range of research approaches which, unless one is careful, are distorted by this assumed dualism.

The variety of educational research

A feature of educational research is the variety of it. Different approaches are used to answer different questions. That, of course, makes sense where what is being researched into (viz. educational practice) is a complex phenomenon. Different sorts of question require different sorts of research. Researchers must be eclectic in their search for the truth.

But behind these different approaches may also lie more fundamental differences of a philosophical kind. Indeed, disagreement between researchers is as often as much a disagreement over the assumptions behind a research method as it is disagreement over the most effective way of proceeding. This I want to illustrate by reference to a range of approaches which one might think of adopting.

Observing what happens

It may seem common sense that, if one wants to know something, one goes out and has a look. To know what works requires careful observation, the systematic recording of those observations and the attempt to generalize from them. The more observations there are which support the generalization, the more confident one might be in the conclusions reached. Thus, theory is gradually built up inductively from what is systematically recorded. Such a theory, when confirmed again and again, can then be used to predict observable outcomes and to guide practice. One can imagine such observation-based theory on such matters as how to maintain control of a class, how to teach irregular verbs or how to create the 'effective school'.

The key feature in undertaking such observations would be consistency of approach. There is an awful amount to observe, and there is the danger that different observers might be looking for different things, or that the same observer might change the viewpoint from which he or she observes, thereby not allowing general conclusions to be drawn. For that reason, there were created 'observation schedules', the most famous of which was that by

Flanders (see Flanders, 1970). Such a schedule divided the lesson into timed sections, and there were clear instructions to observe only certain things and to record behaviours for each of those sections. Thus, one would be able to observe accurately the exact amount of teacher talk as opposed to student talk or silences. Moreover, the schedule could be used by other observers, and, with the same instructions, they would be able to arrive at the same conclusions. Or, using the same schedule, they would be able to observe other lessons and, on the basis of records, make useful comparisons. These observations, when extended to enough cases, would enable the researcher to make generalizations which could be tested out against further observations, obtained and recorded in exactly the same way. Thus, a theoretical position could be built up.

It was thus hoped that there could be a science of teaching, just as there had been a science (constructed through careful observation, generalization and verification) of the physical world. What was wanted was an agreed base for making the observations and a painstaking effort to gather and to check the evidence. The spirit of this aspiration is caught in the observation of one researcher who claimed that in any one lesson a thousand interactions take place between teacher and learner. It is as though an 'interaction' is some thing which can be isolated, observed and added to all the other interactions.

There are many examples of influential research which emphasize observation conceived in this way. The research into 'behaviour modification' required the careful defining of that which was to be observed in precise, behavioural and thereby observable terms (see Gurney, 1980). The researchers felt able therefore to correlate certain behaviours with certain conditions and to test out, through systematic observation, what happened once those conditions were changed. Or, again, examination output is seen as an observable behaviour which, in pursuit of the generalizations about 'effective schools', is to be correlated with observations about how those schools are managed or how the teachers behave. Thus, inspectors circulate with observation schedules, and teachers receive numerical scores on the basis of what is observed.

In one sense, of course, observation is necessary. But that necessity should not be allowed to obscure certain things. First,

observations do not come independent of concepts and theories, apart from the prejudices and preferences we bring to the observing. Take for example the request to observe the different objects in the room. Very soon one finds oneself in the logical difficulty of deciding what is an object for this purpose. Are these several people or one family? Is this one object (a chair) or five objects (four chair legs and a seat)? Is that picture on the wall one thing (a picture) or several things (the painting, the frame and the cord with which to hang it or them)? Second, as I shall show, it is not clear that it is the *behaviour* that I am observing. For example, two teachers who appear to be doing the same thing may in fact be doing two very different things – as when both seem to be teaching but one is mimicking the other. Two pupils write the same answer in the same circumstances, but one has worked it out through a complex formula, the other has guessed or relied on memory. What appear on the surface to be the same behaviour are in fact two very different ones, but this we know only by reference to the intentions and mental activities of those who are being observed.

Therefore, we are beginning to see difficulties in too much dependence on observation as such. First, observations are 'filtered', as it were, through the understandings, preferences and beliefs of the observer. Second, what is observed is not open to immediate acquaintance; the meanings and motives of those who are observed need to be taken into account.

I return to this point in Chapter 6 (see 'Key Concepts') and Chapter 7 (see 'Interpretive Theory'). But one can, no doubt, already see how far-reaching are the philosophical issues. Is this the *real* world that I am observing – or one that is interpreted through my own personal (and subjective?) scheme of things? What is the connection between the language through which I choose to describe the world and the world itself, existing independently of me? And does the *human* world, which I want to describe, actually exist independently of me, that is, of my interactions with it and of my interpretations of it? We take for granted that our 'common-sense' account of the world is accurate enough. But it may be the case that such a world is as distorted in its conception of things as was the pre-Copernican world which was convinced from experience that the sun went round the earth.

Experimenting

One aspect of the scientific paradigm, which educational research might emulate, is the experimental design in which there are control and experimental groups. One might say that the experimental approach is a more systematic approach to observation, as one carefully adjusts different elements and carefully observes, in a very controlled way, the results of the adjustment.

Such control and experimental groups need to be carefully selected and sufficiently large for conclusions to be drawn about the larger population. For this to happen it is seen to be desirable for the groups to be selected on a randomized basis; one compares the performance of the two groups in the light of a specific intervention in the experimental group. Research based on randomized controlled groups is well established in medical sciences (see below). Thus, if one wants to know the impact of a particular drug, then one randomly selects two groups of patients, and then, keeping all other variables constant, observes carefully the effect on one group of the use of this particular drug. The groups have to be large to play down the significance of 'rogue factors' or particular exceptions to the general case.

The history of evidence-based medical practice is explained by Peile (2004, p. 102).

The core activities are: a questioning approach to practice leading to scientific experimentation; meticulous observation, enumeration and analysis replacing anecdotal case description; recording and cataloguing the evidence for systematic retrieval.

The most influential articulation of this position was by Professor Arthur Cochrane whose book *Effectiveness and Efficiency: Random Reflections on Health Services* in 1972 promoted properly designed evaluations, the most reliable of which were RTCs. The consequent Cochrane Centre, established in Oxford to help policy-makers make decisions on proven effectiveness of interventions, inspired the Canadian, Donald Campbell, to set up the Campbell Collaboration, an international body for making systematic reviews of evidence-based policy and practice (see Davies, 2004 and Peile, 2004). Significant developments of the Campbell Collaboration lay in the extension of the evidence-based approaches to social policy more widely,

including education. However, Campbell expressed reservations of philosophical significance at the exclusive of quantitative methods in the experimental approach (see below).

There are many examples of such research in the area of education, and one can see the temptation to extend the approach more widely. For example, Sylva and Hurry's (1995) research into intervention in reading difficulties compares two different interventions with a control group. Therefore, it might be concluded that, if one group scores significantly more high on reading scores after the period of intervention, then the intervention itself was a significant factor. It might even be said that the intervention itself was the *cause* of the difference.

The concept of 'cause' is one we shall need to examine more closely in Chapter 6. But we seem now to be shifting into the very scientific paradigm which has been questioned. Is there not a danger of ignoring those individual differences, reflected in their own distinctive consciousness, in order to treat each of the several thousand children as identical units to be added together, subtracted and compared? How can this approach to research be reconciled with the apparent uniqueness of each individual?

The answer must lie in recognizing that the uniqueness of each individual in certain respects does not entail uniqueness in every respect. That is what I refer to as the 'uniqueness fallacy'. There would seem to be certain aspects of being human which enable us to make tentative generalizations about how individuals will perform or react, while at the same time recognizing that there will inevitably be exceptions to the rule. Does the recognition of the distinctively personal mode of consciousness preclude any commonality and therefore generalization (howsoever tentative) about human motive, aspiration, valuing, mode of learning and so on?

Such puzzles make one query the rather rigid separation of the quantitative from the qualitative approaches to research – and the clear distinction often made between the objective, observable and measurable world of science, on the one hand, and, on the other, the subjective and non-measurable world of individual consciousness. Perhaps we exaggerate the uniqueness of each person. Perhaps we make too sharp a break between the 'private' and 'subjective' consciousness, on the one hand, and, on the other, the 'public'

and 'objective' world, both physical and social. Given human nature being what it is and given the physical conditions necessary for mental operations, then maybe generalizations are possible, verified by observations howsoever guarded these may be. This subtle interconnection between the public and the private, the objective and the subjective, the physical and the mental, the personal and the social, is too often neglected by those who espouse 'research paradigms' which embrace one side of the dichotomy to the exclusion of the other. As Campbell said,

There is a mistaken belief that quantitative measures replace qualitative knowledge. Instead qualitative knowledge is absolutely essential as a prerequisite for quantification in any science. Without competence at the qualitative level, one's computer print-out is misleading or meaningless. (Campbell, 1999, p. 141)

Surveying what is the case

One way of evading some of these difficulties would seem to be the survey. This calls upon the views of those who matter, but does so in a way that can lead to generalization.

The survey does not depend upon an outside observer who, recording what is observable, fails to take into account the views of those who are observed, which views might affect the conclusions drawn. The ideas, and not just the observable behaviours, of the teachers or learners might be wanted. Putting together a survey would seem to be the obvious way forwards. That is, one simply asks people through a questionnaire.

Moreover, when, for example, a researcher wants to know a great deal from several schools, observation would not seem to be practically possible. One could, of course, employ an enormous number of observers. But, first, this would be expensive; second, there would be problems over reliability between so many observers. In any case, it might not be a matter of observation alone. The survey, however, does not escape completely the problems raised by straightforward observation. Very often it seeks to build up the evidence which can then be quantified – 'X percentage say this, but Y percentage say that'. There are sophisticated ways of getting

this information and of checking the reliability of the answers. But there are limitations built into the approach. Thus, just as the researchers bring their own distinctive ways of looking at the world to their observations, so do those being researched bring their own understandings to answering the questions. Two parents might both answer 'yes' to the question 'Is your child's school a successful one?', but mean different things by that, one associating success with achievements against national norms in public examinations, the other interpreting success in terms of prowess at sport. Or, even if they both agree with what is meant, they may be ignorant in different ways of the implications of what they agree to. If they knew the implications, they might have given different answers. There *can* be something logically odd, therefore, in simply adding together the different answers to questionnaires and surveys and giving a score. It is as though all the answers added together are all of the same logical kind. But, if the same mark on the paper represents different understandings, then they should not be added up as though they mean the same thing.

There is, therefore, a tendency in much criticism of such research to reject it out of hand. It is seen to 'reify' (i.e. make it into a 'thing', existing independently of my or others' thinking about it) what is significant only in relation to the thoughts, feelings, intentions of the people concerned. It quantifies that which is not open to adding, subtracting and multiplying. It is, in other words, an extension of the 'scientific/mathematical' paradigm which seems inappropriate to an understanding of human beings.

Once again, however, one must be careful in pursuing this line too far. In saying this, I am indicating my general criticism of so much discourse on educational research, which, not attending to the deeper philosophical issues, polarizes the debate between the scientific and the humanistic, between the quantitative methodology and the qualitative. Thus, there are many questions which are unambiguous to fluent users of the English language. The meanings which respondents attribute to the questions are not something private and subjective, but the meanings which anyone conversant with the language would attribute to them. Therefore, where one can be sure that there is no ambiguity in the questions or where personal beliefs are unlikely to change the significance of

the questions, it makes sense to tot up how many people agree with this or disagree with that. But such uses must be limited, because many questions do raise issues on which there is disagreement over interpretation as well as over the facts. It is always reasonable to ask further what a person meant by answering the question in the way that he or she did.

Interviewing

The answer to the problems, referred to above, connected with the systematic observation of behaviours, or with the wide-scale surveys of what people do or believe, would seem to lie in letting the 'objects' of research (e.g. the teachers or students) speak for themselves. What significance do *they* attach to the action observed or the activity pursued? Typically the researcher, in seeking answers to certain questions, will structure the interview so that the answers will be relevant to the researcher's interests. But the interview will normally be only semi-structured because otherwise there would not be the scope for those interviewed to expound the full significance of their actions. If you believe that the significance or the 'meaning' of what is done lies in the ideas, intentions, values and beliefs of the agent, then those ideas, and so on, have to be taken into account. The good interviewer is able to draw out from the person interviewed the deeper significance of the event, so much so that it seems ever more difficult to generalize – to see this or that individual as simply an instance of a generalization. The individual's consciousness and intentions are the significant factors in explaining why things happen as they do.

This seems very plausible, and the result has been the growth of studies which seek an understanding of particular events through the eyes of the participants. But there are difficulties.

First, of a practical kind, such studies can be of interest only to those who are in that unique situation (unique because to be understood only through the ideas and beliefs of the 'actors' within the situation). But quite clearly those who seek to improve educational practice or those who are responsible for developing educational policy want more than a collection of studies which, however interesting in themselves, have nothing to say beyond the

particular events and contexts studied. Hence, the quite justified criticism that educational research is often too small-scale and fragmented to serve policy and professional interests.

Second, however, what lies behind this apparently practical difficulty and criticism is a difficulty of a more philosophical kind. Given the claimed uniqueness of each individual's understanding of an event or an activity, it would seem impossible for the interviewer to grasp the significance of what is said. The interviewer inhabits his or her own unique world of beliefs and understandings. The responses would necessarily be filtered through those, and thereby become different from the beliefs and understandings of the person interviewed. This difficulty and the answer to it are developed in the following chapter. All sorts of device are invoked by those researchers who want to preserve the uniqueness and essential subjectivity of the private worlds of the people researched into. For example, meanings are 'negotiated' between researcher and researched, as indeed they are claimed to be between any persons engaged in conversation. But such metaphors do not really get us out of the hole already dug between the objective world espoused by those within the 'scientific paradigm' and the subjective world of intentions, beliefs and meanings espoused by those who reject such a paradigm.

Let us leave it there for the moment. We have seen the importance attached to a particular way of engaging in research (the semi-structured interview) which meets some of the criticisms, partly philosophical, arising from reliance on observation, survey and experiment. But, unless one is careful, this antidote is equally problematic. This is because, in my view, which will be argued at greater length later, a dichotomy between the public and the private, between the objective and the subjective, has been created which cannot be justified. This dichotomy is re-enforced by the frequent and mistaken use of the phrase 'different paradigms'.

Case studying

This emphasis on the uniqueness of events or actions, arising from their being shaped by the meanings of those who are the participants in the situation, points to the importance of 'case study' – the study

of the unique case or the particular instant. This is reflected in the rather paradoxical title of Helen Simons's edited book on case study, *Towards a Science of the Singular* (Simons, 1981). Such a study would start from the premise that any unit of investigation in which persons were involved could only be understood if the perspectives of those involved (and the interaction of those perspectives) were taken into account. Indeed, these would be central to the research. But, of course, that would make what was being researched unique and not open to generalization.

The 'unit' studied might be a person, an institution or a collection of institutions (such as a local School Board or Education Authority). Obviously, the larger the unit the more complex becomes the unravelling of the interactions and the perspectives. None the less, the claim would be made that, without such in-depth detective work, one would not really understand what was going on. Examples of thoroughgoing 'studies of the singular' in education would be Peshkin's (1978) *Growing Up American* and Hargreaves's (1967) *Social Relations in a Secondary School*.

These studies reveal a variety of methods: observations, surveys, interviews and so on. It is difficult to generalize. What they tend to have in common (although there are exceptions) are: first, intensity in the examination of the particular; second, belief that the unit under investigation cannot be understood except within the broader context of the understandings shared and not shared by the participants; third, disinclination to import an alien and theoretical language not used by the participants; fourth, responsiveness to the experience of 'the case'; fifth, narrowing of distance between researcher and researched such that the resulting study is more a 'negotiation' than a discovery of what is the case.

Thus, the case studies, despite the differences among them, tend to make assumptions which raise philosophical questions.

First, it is often assumed that the researcher, in coming to the investigation with an open mind, lets the data 'speak for themselves'. Hence, the popularity of Glaser and Strauss's (1967) 'grounded theory'. Only gradually, in trying to make sense of one's experience, does one develop a theoretical position, and that 'emerging theory' is constantly tested out against further experience, data and questionings. Furthermore, that theoretical position remains close to

the concepts and language shared with those being researched into. There is no privileging of a theoretical position imported from outside through which the data are looked for or selected.

Second, from such an intense study of the particular, it is not possible to generalize to other situations, although the graphic descriptions may alert one to similar possibilities in other situations. They, as it were, 'ring bells', but general links between particular events or conditions and subsequent happenings cannot be established. That would not make sense.

Third, questions emerge about the *objectivity* of the research, the *reality* which is exposed and the *truth* of the claims being made. Indeed, these concepts, as we shall see in later chapters, are interlinked. The objectivity is challenged precisely because the researchers cannot stand aside as though their presence had no effect upon the situation. Furthermore, the situation has to be described in a language which is employed by those who are being researched into. Otherwise, so the argument goes, it would not be *their* situation which is being investigated. The reality being researched has to be the reality as defined by the participants. Thus, there is talk of 'multiple realities', reflecting the different definitions of reality by the people involved in the research – that is, the 'realities' of different individuals whose 'definitions' of the situation are necessarily different from each other's. 'Objectivity' in the sense of getting at what exists 'out there', independently of researcher and researched, makes no sense.

The interrelated concepts of 'objectivity', 'reality' and 'truth' require careful consideration which I give in Chapter 6. Indeed, how one makes sense of these concepts or employs them in educational research affects profoundly the significance attached to that research. A particular interpretation of them arises in some accounts of case study. I believe that many of these accounts are philosophically mistaken. However, despite this, I still insist upon the insights which case study, the study of the singular, brings, that is, the study of the distinctive features of a particular situation without which that situation cannot be fully understood. (On the other hand, there is something peculiar in the phrase 'fully understood'. Events are understood for different purposes, and the degree of understanding required depends on those purposes.)

But the value of the 'study of the singular' should not blind us to those features of the study which limit the singularity. All situations are unique in some aspects, not in others. Remember the 'uniqueness fallacy'. There is something distinctive about each individual, and yet something in common between this individual and that. The language through which we describe the singularity of one situation is the same language through which we describe the singularity of another. Concepts are necessarily general in their application. And so we must be careful, in studying the 'singular', not to draw mistaken philosophical conclusions about the inability to generalize.

Historical research

McCulloch and Richardson (2000), in their book *Historical Research in Educational Settings*, point to the dearth of literature on the application of historical research to the field of education, even though there is so much written on the history of education. But it is important, where historical research is pursued, to explore what it means to think historically – not simply the distinctive skills which need to be acquired (let us say, in searching for and in interpreting documentary evidence), but also the competing philosophical traditions of what constitutes 'doing history' or 'thinking historically'. For example, there is the inevitable problem of understanding the text within the belief system of the time, the attempt, for example, to enter imaginatively into the mindset of people and institutions in a very different era and situation. The notion of 'historical imagination', or what the philosopher of history, R. G. Collingwood, referred to as 're-enactment', would seem to be crucial, but constantly open to revision when tested out against further evidence. But, more significantly for my purposes, is the centrality of historical understanding to a valid appreciation of the present, a realization that how we see ourselves, others and the world around us is itself (in part) a historical product, which can therefore be fully grasped only within a historical narrative. The way in which we conceptualize experience is not (and never has been) fixed for all eternity, but has a history, or has its roots in particular economic and social circumstances and to some extent has evolved through critical reflection and discussion.

But Collingwood's ideas of history (see Collingwood, 1946, but also Hughes-Warrington's (1996 and 1997) excellent accounts of Collingwood's historical/philosophical understanding of education) would not be universally accepted by philosophers of history. Historical research gets caught up in the ideological disputes which permeate the social sciences generally – Marxist, modernist and postmodernist.

There are, of course, other research approaches, which we should bear in mind, concerned with: studying other societies and groups (from their point of view); documentary enquiry; comparisons across cultures; action research. Some of these I shall be examining later in this book. But the ones above reveal the tendency to divide research into two different paradigms, namely, the 'warring camps' to which Carr referred. To this, therefore, we must turn in greater detail.

5

Quantitative and qualitative research: a false dualism

Warring philosophical traditions introduced

There is a danger in educational research, as indeed in everything, of drawing too sharp a contrast between different kinds of activity or different kinds of enquiry. These sharp divisions are frequently 'institutionalized', with members of one 'institution' sniping at members of the other. Thus, in many theses and books on research, a sharp distinction is made between quantitative and qualitative research. The distinction is made on the basis not of 'appropriateness to task' but of 'epistemology' and even 'ontology'. Thus, quantitative researchers are seen to have a distinctive view about the nature of our knowledge of the physical and social worlds. And qualitative researchers question that view, and very often reject the whole quantitative enterprise as 'epistemologically flawed'. The two sorts of researcher are said to work within 'different paradigms'.

The differences are reflected in the respective languages of each and in the way in which key concepts take on a different logical character. By that I mean that these concepts, which seem unavoidable when we reflect philosophically upon the nature of enquiry, link together in logically different ways and take on different

meanings. Such words as 'objectivity' (and, by contrast, 'subjectivity'), 'reality' (and 'multiple realities'), 'truth' and 'verification', 'knowledge' and 'meaning', though interrelated, are defined differently.

These concepts shall be examined more carefully in Chapter 6. Here I wish to show how differences in use and application frequently arise out of sharply contrasted ways of engaging in research. Thus Hodgkinson (1998), in condemning Tooley and Darby's report on educational research, refers to 'the deep ontological and epistemological problems that lie behind his essentially Cartesian approach'. It is not clear why Tooley was judged to be a follower of Descartes. Nor is it stated why, if that were the case, such a discipleship should be considered wrong. But evidently Tooley was understood to be resurrecting a clear distinction between researcher and that which is researched, between a knowable world existing independently of the knower and the knower him or herself. By contrast, qualitative researchers would blur these distinctions. The world researched is affected by the research itself; our knowledge is a 'construction', reflecting the world, not as independent of our deliberations, but as something constructed by them.

In a nutshell, the contrast is drawn between quantitative research which is seen to be appropriate to the physical world (and wrongly applied to the personal and social) and qualitative research which addresses that which is distinctive of the personal and social, namely, the 'meanings' through which personal and social reality is understood. The latter simply cannot be quantified; it is not that sort of thing. Furthermore, the former kind of research is referred to as 'positivist', a word which has had a bad press among educational researchers and which therefore signals strong disapproval.

In Chapter 7, I shall examine more closely what we mean by 'positivism'. It requires a more subtle and charitable understanding than many of its detractors afford it. But for my present purposes, it is enough to show how a clear distinction is made between explanations which generally pertain to the physical world, which are characterized by quantifiable generalizations, and which are frequently called positivist, and explanations which pertain to the

non-physical world of personal and social meaning. The divide between the two sorts of explanation is seen to be 'epistemological'. That is, there are considered to be different underlying theories of explanation, of truth and of verification. And the divide is also seen to reflect 'ontological differences'. By that, I think, is meant that a particular philosophical position (viz. that the research gets at the world as it really is) applies to the first but does not to the second (viz. that the 'reality' researched can never be independent of the person researching it).

Here we see signs of the very Cartesian dualism which Hodgkinson accuses Tooley of. And, indeed, it demonstrates the difficulty of avoiding such perennial dualism, namely, the clear distinction so often drawn between the physical and the mental, between body and soul, between the objective world (independent of our thinking about it) and the subjective world ('in our heads' and personally constructed), between 'things' and 'meanings'.

However, 'the world of real life' or 'the world of common sense', of which Ryle (1954) writes, cannot be captured by either the one or the other, and indeed there must be an integration and overlapping of the two. It is important to explore this divide further and to show why that which is quite acceptable at the everyday commonsense level is to be declared unacceptable by researchers. None the less, I want to add that the distinction, which is drawn attention to, needs to be borne in mind lest research wrongly attempts to quantify that which cannot really be quantified.

Constructing reality and negotiating meanings

The 'false dualism' which I have in mind is most effectively demonstrated in an influential book by Guba and Lincoln, *Fourth Generation Evaluation* (1989). The book sharply distinguishes between different generations of research – the first being the adoption of a quite inappropriate scientific model. By 'scientific' the authors have in mind what would appear to many scientists as a rather impoverished

model of science and one which would no doubt be characterized as 'positivist'. However, the poverty of the scientific model did, over time, become clearer, and so there was gradual progression to the fourth and superior paradigm which Guba and Lincoln espouse. The contrast between the first and the fourth paradigm is hinted at in this passage:

Evaluation outcomes are not descriptions of the 'way things really are' or 'really work', or of some 'true' state of affairs, but instead represent meaningful constructions that individual actors or groups of actors form to 'make sense' of the situations in which they find themselves. The findings are not 'facts' in some ultimate sense but are, instead, literally *created* through an interactive process that *includes* the evaluator (so much for objectivity!) as well as the many stakeholders . . . What emerges from this process is one or more *constructions* that *are* the realities of the case. (p. 8, their italics)

Thus, the 'evaluation outcomes' do not describe 'the way things really are' or 'really work'. Such is, as it were, the 'false ontology' of the first and scientific paradigm. It no longer makes sense to talk of the 'true' state of affairs. What, one might ask, is truth? Rather is it the case that we each, in our research and evaluations, try to 'make sense' of the situation we find ourselves in. We do this through 'constructing' connections, meanings, frameworks through which experience is sieved and made intelligible. 'Facts' are not discovered, but created.

These 'creations' and 'constructions' are clearly influenced by the values which the researchers bring to the situation – something unrecognized, apparently, by those working within the scientific paradigm. None the less, despite the influence of such personal constructions and reconstructions, people of like minds and like values may, through a process of 'negotiation', come to share the same values and reach a consensus over the way in which experience is to be understood – and thus about the validity of the research and its findings.

The 'fourth generation' of evaluation, therefore, argues that 'realities' are not objectively 'out there' but 'constructed' by people

as they attempt 'to make sense' of their surrounds (which surrounds do not exist independently of them anyway). The new paradigm

exists in what we have come to call the constructivist paradigm [which] rests on a relativist rather than a realist ontology, and on a monistic, subjective rather than a dualistic, objective epistemology. [This 'paradigm'] is not, in sharp contrast to conventional methodology, a set of conclusions, recommendations, or value judgments, but rather an *agenda for negotiation* of those claims, concerns and issues that have not been resolved in the hermeneutic dialectic exchanges. (p. 13)

This view of research is to be contrasted with 'the first generation', the 'ontological' and 'epistemological' foundations of which are described in the following way:

The methodology employed in evaluation has been almost exclusively scientific, grounded *ontologically* in the positivist assumption that there exists an objective reality driven by immutable natural laws, and *epistemologically* in the counterpart assumption of a duality between observer and observed that makes it possible for the observer to stand *outside* the arena of the observed. (p. 12)

Contrasting paradigms

The contrast between qualitative and quantitative might be summarized as follows.

Premises of paradigm A

The premises of those who are within paradigm A would seem to be as follows:

- a There is a world which exists independently of me which is made up of 'objects' interacting causally with each other – 'objective reality'.

- b The researcher is able to observe that 'objective reality' as it really is.
- c There are different sciences of that world, partly depending on what is to count as an object (a 'behaviour', a 'physical object', even a 'social event').
- d Once, however, there is an agreement on what is to count as an 'object' (e.g. behaviour), such objects can be observed, their interrelations noted, regularities discovered, causal explanations given and tested, results quantified.
- e Other observers can check the conclusions through repeated experiments under similar conditions.
- f From many carefully conducted observations and experiments, following critical checking from others, a scientifically based body of knowledge can be built up.
- g That body of knowledge reflects the world as it is; the statements within it are true or false depending on their correspondence to the world as it is.

Premises of paradigm B

The premises of paradigm B are less easy to state, partly because truth lies not so much in correspondence between what we say and how things are, but in the 'consensus' which is 'negotiated'. But there is a paradox here. For the truth of this very position would itself be a matter of consensus. And those who share a different paradigm (let us say, paradigm A) might cheerfully state (using paradigm B's language) that they have socially constructed things differently, and happen to prefer the company of those who believe there is a reality 'out there' and who do believe that an account of the world is either true or false (whether or not it can be verified for certain). Indeed, even Guba and Lincoln are obliged to have recourse to words and phrases which, more obscurely, imply much the same. Thus, 'through a hermeneutic dialectic process, a new construction will emerge that is not 'better' or 'truer' than its predecessors, but simply more informed and sophisticated than either' (p. 17). This takes place (note the extension of the metaphor of 'negotiation') in the 'academic

marketplace of ideas'. Or, again, the hermeneutic/dialectic process 'creates a constructed reality that is as informed and sophisticated as it can be made at a particular point in time' (p.44). Thus, not any kind of negotiation will do, only one which is informed (as opposed, presumably, to mis-informed) and sophisticated (as opposed to naïve or lacking in subtlety).

It is difficult to state a philosophical position at this level of abstraction without falling victim to the implications of that position. There is something peculiar about an argument for the abolition of 'truth'. This is implicitly recognized by Guba and Lincoln, though explicitly denied, in their recourse to such words as 'better informed', 'more sophisticated', 'more reasonable' and 'more appropriate'. In seeing the implications of this, one is forced to acknowledge 'reality' as something not entirely 'created' or 'constructed' or 'negotiated', but constraining and limiting – something which *is* independent of us, which shapes the standards of what we can *justifiably* say, and which restricts the conclusions that can be *correctly* drawn from the evidence given. Such 'realism' must permeate both quantitative and qualitative research. None the less, in so arguing this, I am not thereby placing myself in paradigm A. Indeed, much that is labelled 'positivist' would not be 'realist' in the sense that I argue for. The difficulties which Guba and Lincoln create for themselves arise from a dichotomy, a 'false dualism', an opposition established between the quantitative and the qualitative, which leads to anti-realism in both camps.

The premises, therefore, of those within paradigm B are as follows:

- a Each person lives in a 'world of ideas', and it is through those ideas that the world (physical and social) is constructed. There is no way that one could step outside this world of ideas to check whether or not they accurately represent a world existing independently of the ideas themselves.
- b The distinction between researcher and the researched is blurred, the research findings being created (not discovered) through the interactions between researcher and researched.
- c Communication with others, therefore, lies in a 'negotiation' of their respective worlds of ideas whereby, for practical

reasons, they come to share the same ideas. That negotiation in 'a marketplace of ideas' never ceases. New consensuses constantly have to be reached.

- d The notion of 'truth', therefore, can be eliminated, or redefined in terms of 'consensus', because, given (a) above, there can be no correspondence between our conceptions of reality and that reality itself.
- e The distinction between 'objective' and 'subjective' needs to be redefined since there can be nothing 'objective' in the sense of that which exists independently of the world of ideas which either privately or in consensus with others has been constructed.
- f Since there is no sense in talking of reality independently of our conceiving it, there are as many realities as there are conceptions of it ('multiple realities').
- g What is researched is to be understood only within the context in which it has been constructed, thereby precluding generalization from one situation to another.

There is a danger of setting up 'straw men' to illustrate and to justify my accusation that researchers have created a 'false dualism' between the quantitative and the qualitative modes of research. There are many distinctions to be made within the qualitative tradition, each with its distinctive way of engaging in enquiry and of making intelligible the personal and social reality which is being portrayed. Indeed, the scientific paradigm, too, could be seen as a caricature, that is, as a view of science (narrow, impoverished, 'positivist') which by no means does justice to the distinctions and different perspectives marked out within the philosophy of science. The distinctions *within* the so-called paradigms are often as significant as the distinctions between them. Quantitative research, as was illustrated in the last chapter, covers enquiries which range from the detailed measurement and correlation of performances within a strictly behaviourist tradition to the large-scale surveys of social trends within the tradition of 'political arithmetic'. Qualitative research embraces symbolic interactionism, phenomenology, ethnography,

hermeneutics. And within any one piece of research there is often the employment of different approaches as different questions are addressed.

However, it is this failure to recognize the complexity of enquiry, and of the nature of that which is being enquired into, which causes the blurring of the distinctions within the so-called paradigms and results in the sharp dichotomy between them, characterized by contrasting conceptions of 'truth', 'reality' and 'objectivity'. The consequences for research are immense, with an inflated confidence in general explanations in the one case ('the causes of truancy', 'the science of teaching'), and with a total distrust of any general explanations in the other. One can see why those who look to research for guidance on the formulation of policy become disillusioned with an activity which denies the very possibility of providing it.

Rejection of the dualism

The 'false dualism' lies in the belief that, in rejecting paradigm A with its uncomplicated 'correspondence theory of truth', researchers must inevitably embrace paradigm B. This, however, is simply not the case. It is possible to reject what is referred to as the 'positivism' of paradigm A without abandoning the realism of the physical and social sciences and without therefore concluding that reality is but a social construction or that correspondence between language and reality is to be thrown overboard completely.

Certainly, how we see the world does depend upon the ideas we have inherited. It is correct that different societies and social groups do, in important respects, conceive the world differently. Thus, for example, we do in fact distinguish between different kinds of tree, but it is conceivable that we might not have done, for example, distinguishing vegetation in terms of colour or shape or size. But the fact that we do so distinguish, although in a sense a social phenomenon, depends on there being features of the world existing independently of me which makes such distinctions possible. The fact that there is an infinite number of ways in which we could divide up and classify things does not entail that any kind of distinction is possible.

Furthermore, how we conceive things is embodied within a language and is inherited by those who learn that language. Far from individually constructing the world, we acquire those constructions which (although socially developed) are possible because of certain features of reality which make them possible. It is not that there are multiple realities. Rather are there different ways in which reality is conceived, and those differences may well reflect different practical interests and different traditions. Social constructivists in the sense of paradigm B are rarely found at 30,000 feet. Of course, not any social group has conceptualized the world in the same way as aeronautical engineers and scientists. But the possibility of so conceptualizing it is not itself a social construction. It is to do with certain conditions prevailing independently of our wishing them so. There are *discoveries* in mathematics (and those discoveries made air flight possible) as well as constructions.

That, it might be conceded, is true of the physical world, although that would be a big concession. One might, therefore, concede that there is a science of the physical world, but (so the argument goes) not one of the personal and social worlds. Our language of emotions, rights, intentions, attitudes, institutions would seem to be a social construction in a more thoroughgoing sense. Unlike the case of physical objects, there would seem to be no reality 'out there' independently of our creating it. Moreover, those creations are constantly reconstructed in the interactions between individuals. The moral words we use, appraisals we make, attributions of responsibility, descriptions we give of motives and emotions have a history which, so it would seem, are located in particular social and cultural traditions, and evolve through interactions between people within those traditions and between the traditions themselves. These constantly reconstructed ways of interpreting people and of relating to them, which have no reference outside the 'hermeneutic dialectic process' itself, cannot be true or false, objective or subjective as those terms are understood within paradigm A.

Again, however, the conclusions do not follow from the premises. Those premises are that the ways in which we describe, appraise or attribute responsibility within the personal and social sphere are themselves social constructs and that the 'reality' is somehow created and recreated through the very act of construction. Hence,

what it *means* to be a person (e.g. 'made to the image and likeness of God') is construed within particular groups and traditions. There is no *real* person independent of those constructions against which that account might be compared. There cannot, therefore, be a *true* account.

However, one needs to attend to the intelligibility of making such a claim. The very possibility of the social interactions, through which social reality is construed, depends on a shared understanding (howsoever vague and general) of what it is to be a person, namely, a centre of consciousness capable of intentional action, rational behaviour, emotional response and potential for assuming some level of responsibility. It is true that the conceptual framework through which we think about 'persons' could have been different; the way, for example, in which we differentiate the emotions could have (as they no doubt do in other traditions) highlighted some features rather than others. There is no 'a priori' limit to the number of ways in which we might have conceptualized the social life. But that is not to say that there are *no* limits to how it might have been organized. The distinctions we make depend on relatively stark features of recognizable human behaviour.

Just as the social construction of the physical world depends on a real world, independent of that construction and constraining what construction is possible, so the social construction of the personal and social world presupposes the independent existence of objects (persons) which can be described in terms of consciousness, rationality, intentionality, responsibility and feeling. The very 'negotiation' of meanings can be conducted only within a frame work of shared meanings, which meanings (in their most general state) are not open to negotiation. That is how the world is, independently of my construing it, and how it must be if I am to enter into negotiation with others.

Such a view reintroduces the unavoidable concepts of 'truth' and 'objectivity', albeit not in the sense of the naive realism which is attributed to paradigm A. By 'naive realism' I mean some sort of picture theory of truth in which the world as we conceive it is mirrored in the language through which we give an account of it. There is a one-to-one relation between the objects in the world and the nouns and pronouns which pick out those objects, between the

nature of those objects and the descriptors within the language. It is the mistake of those who criticize paradigm A that they attribute such a correspondence theory of truth to any position other than that found in paradigm B. It is wrongly concluded that, since 'naïve realism' is unacceptable, one is obliged to adopt paradigm B in which the notion of 'reality' is dispensed with along with 'naïve realism'.

Bridges (1999) demonstrates the poverty of such a move. The concept of truth, as indeed the concept of reality, is both too complex and too indispensable to be so easily dismissed. My argument has been that, in the ways in which both physical and social realities are conceptualized, the very possibility of the negotiation of meanings presupposed the existence of things (including 'person things'). These things must have certain distinguishing features which make possible our different constructions of the world. It is always possible to refuse a construction imposed upon one, not from bloody-mindedness, not from lack of interest, but from the fact that such a construction is not possible – given that reality (physical and personal) is what it is.

Given that there are 'persons', objectively speaking, existing independently of my constructing them as persons, and given that there are 'societies' of 'persons', again existing in relatively stable conditions, then there seems to be no 'a priori' reason why such persons and societies cannot be studied as 'objects' and to some extent 'added up', and generalized about. Indeed, it is that which policy-makers are often anxious to see as they deliberate about this policy to adopt or that intervention to sanction.

However, those who emphasize the distinction between quantitative and qualitative research are right in demanding caution in the extension of quantification to certain aspects of personal and social reality. How we describe that reality depends very much on the purpose of the description. It may, for certain purposes, be sensible to talk of X number of people being angry at what happened or having understood the nature of the problem. But for other purposes, the nature of that anger or that understanding, reflecting the different ways in which the people conceived the situation, would be wrongly perceived as the same kind of thing in each case. Surveys which tot up similar responses to the same question might in fact give a distorted picture of how the different people really felt about or understood a situation. And this becomes

even more insidious where children's understandings, knowledge and attitudes are given numerical scores or grades, and these then compared with others' scores or grades, as though it is the 'same thing' being spoken about. For some strange reason, this problem is rarely acknowledged, and thus, under the urge to quantify, we reduce to an arithmetical unit the complexity of children's struggle 'to make sense' or to understand.

False dualisms and political control

Behind the criticism of quantitative research lies an understandable suspicion of those who sponsor research and use its results in the interest of management. It is worth pointing out that educational arrangements are increasingly organized to serve economic and social interests as these are conceived by political leaders and that, in pursuing these ends, such leaders ask us to manage schools in the light of what research concludes to be the most 'effective' way of achieving them. It is equally true and worth pointing out that such research, in ignoring the complex transactions which take place between teacher and learner and which cannot be captured in the 'management and means/end' language of that research, distorts those educational transactions, and 'disempowers' and 'disenfranchises' (Guba and Lincoln's words) the teachers. It is as though the 'managers', aloof from the education process, seek general solutions to generalized conceptions of the problem, and then, in the light of the evidence, tell the teachers what to do. The result lies in a failure to recognize the peculiarities and complexity of the specific context, the ways in which the situation must be understood from the perspective of the participant, and the denial of professional responsibility to the teacher.

The acceptance of paradigm B, on the other hand, in denying the intelligibility of such an understanding of research (the clear distinction between researcher and researched disappears in the 'negotiation' of meanings which takes place in the 'marketplace' of ideas), is seen to liberate the teacher from this management control. Each context is created through the 'hermeneutic dialectic process', as consensus is reached about an understanding of the situation.

However, the shift to a paradigm where 'reality' (or the 'multiple realities') is (or are) totally created or constructed through the negotiation of meanings leaves the teacher vulnerable to a different sort of control. There are strong and weak negotiators, those practised in the art and skill, and those who are not. There is as much danger of the 'reconstructed realities' reflecting the dominance of those in powerful negotiating positions as there is of the researchers in paradigm A serving the interests of the educational managers. The links between knowledge, on the one hand, and power and control, on the other, are equally strong within both paradigms, albeit the nature of the connection is different. But this problem arises because of the severance of knowledge and understanding from some notion of reality independent of our construction of it. The one guarantee of freedom is that there are constraints on our construction of reality, namely, reality itself, and that it is always possible to challenge others' ideas and 'constructions' in the light of what is the case.

Summary: mistaken opposition

My argument is that the opposition (not the distinction) between quantitative and qualitative research is mistaken. The 'naive realism' attributed to those who espouse the more quantitative methodology is not justified. How we conceive the world could be different and, indeed, is different from social group to social group. Such 'social constructions' are constantly reconstructed as new experiences force us to reshape how we understand things. Hence, the need for that interpretive and hermeneutic tradition in which we seek to understand the world from the perspective of the participants, or to understand a set of ideas from within the evolving tradition of which they are part.

However, such differences in how we understand reality are possible because there are stable and enduring features of reality, independent of us, which make such distinctions possible. And this applies not simply to the physical world but also to the social and personal. There are features of what it is to be a person which enable generalizations to be made and 'quantities' to be added or subtracted. Most persons have predictable emotions and capacities

which make it possible for certain purposes to consider them the same from person to person – and thus open to quantification. The qualitative investigation can clear the ground for the quantitative. As Campbell, the aforementioned pioneer of evidence-based medicine wrote:

Qualitative knowledge is the test and building block of quantitative knowing. This is not to say that such common sense naturalistic observation is objective, dependable or unbiased. But it is not all naïve. It is the only route to knowledge – noisy, fallible, and biased though it may be. (Campbell, 1979, p. 186)

Equally, the quantitative can be suggestive of differences to be explored in a more interpretive mode, as when the number of exclusions (quantitative research) provokes a more qualitative investigation of the reasons why.

I started this chapter by referring to the way in which educational research often falls into two competing, philosophical camps. One embraces a scientific model for understanding educational practice; the other emphasizes that human beings cannot be the objects of science and that research must focus upon the 'subjective meanings' of the learners.

However, attention to the many ways in which research is conducted reveals a more complicated picture. In some respects, people are the 'object of science', that is, of generalizations and causal explanations. In other respects, however, they escape such explanations through interpreting the world in their own personal ways. Yet again, such personal interpretations draw on traditions, on public ways of understanding the world, on social customs and practices. Different methods get at these different explanations. Understanding human beings, and thus researching into what they do and how they behave, calls upon many different methods, each making complex assumptions about what it means to explain behaviours and personal and social activities.

The dominance of any one methodological approach to educational research gives priority (in some cases exclusive priority) to certain kinds of explanation, and thus to certain assumptions about human beings. Indeed, one could argue that some 'theory of human nature'

lies behind any particular approach to educational research. By 'theory' I mean that cluster of beliefs and values which underpin our understanding of things and which become the premises upon which those explanations hang. However, other researchers might make very different presuppositions, and therefore adopt different methods.

The next two chapters aim to illustrate the philosophical presuppositions of these methodological differences.