SCIENCE AND EDUCATIONAL RESEARCH

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Abstract.

At present the most powerful and influential groups in education see the solution to matters of educational concern as mainly falling within the province of an educational research which is fundamentally scientific.

This thesis sets out to examine whether this assumption can be substantiated and, in the possible scenario that it cannot, to look at an alternative form of educational research.

It begins with the philosophical arguments which support the view that educational research, where it is empirical, should be mainly scientific and continues by looking at what contemporary educational researchers have said about the nature of educational research. The role philosophy of education might take in this context is also examined. The thesis continues by looking at the
prescribed methodology of educational research and examines the philosophical assumptions of such a methodology. It continues by looking at the major assumption of scientific endeavour which is that it is nomological.

The conclusions drawn from the foregoing are that, for various philosophical reasons, the notion that educational research can be founded on scientific method and applied through a process parallel to engineering is fallacious and needs to be reviewed.

A review of the philosophical situation with regard to understanding human beings as would be necessary to understanding them in an educational context is undertaken in the fourth chapter. This marks the beginnings of an alternative, non-scientific, framework for educational research. A case is made for the thesis that individual actions are understood properly against a background of information which includes beliefs, intentions and historical circumstances. Consideration is then given as to how this might be put in such a way as to be of practical use in the deliberation of how to tackle educational issues. The final chapter outlines how a possible substantive piece of educational research might look.
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Prologue.

My reasons for writing the present thesis stem from a fear, which I hope to justify in the first three chapters, that educational research, as presently conceived, makes certain fallacious assumptions about the appropriateness of methods developed within the natural sciences to matters of educational concern. Since such methods are the basis of educational policy their use in education have far reaching implications for all concerned. In the thesis I therefore call into question the use of scientific methodology in educational research and attempt to provide an alternative. Since, however, I appreciate that the use of scientific techniques of enquiry cannot simply be dismissed I have devoted a considerable amount of the thesis to a critical review. This exercise has been, in retrospect, useful in a way not anticipated at the outset, for, in uncovering some of the weaknesses inherent in the attempt to base educational research on scientific principles it has been possible to see more clearly what the alternatives might
look like. It is one of my hopes therefore that the critique which forms the first part of the thesis is also the basis for substantial progress in the second part which looks at an alternative method.

With the above comments in mind I have started the thesis with a chapter which provides an educational context to the whole. It focuses on the issues by looking at the debate between Paul Hirst and D.J.O'Connor. D.J.O'Connor's arguments are particularly pertinent because he argues strongly for the use of scientific methods in educational research and policy.

Having opened the topic in this way a critique of scientific method in educational research would seem to be the next appropriate step. It is, however, deferred to chapters two and three. The reason for this is the prior need to look at philosophy of education and its possible role in providing such a critique and in its relation to educational practice. My reluctance to proceed with the critique at this point stems from an uneasiness about the role and importance of philosophy of education, as it has been perceived, and an attempt to set out some of the reasons why I consider the discipline to be pivotal with respect to other disciplines within what might be more widely described as educational research. Insofar as I argue that philosophy of education has direct implications for the practice of educational research I hope that the thesis itself - which has practical conclusions - is an exemplar of what I mean.
I delay a look at science for a second, and equally important, reason. This is to establish that educational researchers themselves either openly or covertly assume that the methods developed within the natural sciences are not only an adequate but also an appropriate basis for the empirical side of educational research. This is to avoid the valid criticism that the whole thesis is based on what is normally referred to in philosophical circles as an 'Aunt Sally'. I feel that, for the reasons given in the first two chapters, no such criticism can be made.

The second chapter opens what I have here called the critique by looking at the recommendations of educational researchers. The recommendations looked at are all concerned with the methodology of empirical educational research. The books used were taken at random from various libraries, the only factor common to each being that the title and introduction indicated that the work was devoted to an explanation of the methodological techniques of empirical educational research. It is surprising, again in retrospect, how similar these recommendations were, especially since one or two were written twenty or so years before the others.

The third chapter continues the critique by looking in some detail at the major assumption of science which is that it is nomological. The various ways in which laws connecting antecedent conditions and their alleged effects might be formulated are considered and
the conclusion is reached that whichever permutation is utilised, there remain insurmountable difficulties. Failure to come to terms with these difficulties constitutes a considerable lack of openness on the part of those educational researchers who espouse scientific method, a lack of openness which does not conform to the spirit of true enquiry which, minimally, must include the ability to be critically aware of the assumptions one makes and the readiness to adapt to new insights.

It is hoped that the thesis exemplifies these qualities since between chapters four and six it attempts to lay the foundations for an alternative research methodology which avoids the fallacies of scientism. The groundwork for this is found in the fourth chapter where the central question concerns how we come to an understanding of each others actions and thoughts. The conclusion reached is that such an understanding is not a private action but is the outcome of the way we construe the world and each other - a way which we take together. History, convention and intersubjective agreements are thus all partly responsible for what we say about each other and consequently enter into our concepts, our language and, ultimately, how we understand what others are doing, thinking or feeling. At the same time we all experience the world in different ways; our own preferences, perspectives, emotions and perceptions giving us a unique view of how things are. This uniqueness, together with our common history, give rise
to a radically different basis to educational research to the atomistic one accepted by science. It contains, firstly, a common historical framework and secondly, a number of unique perspectives on the human events contained within that history which give rise to a plurality of possible accounts. The thesis argues that any research which deals with human affairs must accommodate this. Chapter five attempts to deal with the problem by proposing that a narrative which draws upon various individual accounts is the starting point for one possible form of educational research. Chapter seven attempts to further resolve the problem by incorporating the possibility, in actual research, of allowing the individuals whose initial accounts had given rise to a narrative to discuss with each other and with a group referred to as the 'panel' both their own perspective on events and how or why accounts differed or varied. This, seemingly democratic process, does not enter the research for undisclosed political reasons but, as is argued, from an epistemological concern for objectivity. The fundamentals of this concern are dealt with in the fourth and fifth chapters.

Engineering, as a method of applying science to human affairs, is rejected in chapters one and two. Chapter six thus looks at the possible alternatives. Various forms of deliberation are looked at and the use of a 'calculus' of pure practical reason is rejected. In its place I have suggested a form of deliberation based
very much on the actual processes of practical reasoning which operate within the daily process of human affairs.

The final chapter is an attempt to give an illustration of how a piece of non-scientific educational research might be both formulated and carried out. The chapter is intentionally practical. The methods outlined, while not the only possible methods, are methods which might be adopted by educational researchers without much alteration. In other words, the final chapter is a substantive proposal for educational research. At the same time it is the outcome of a philosophical thesis. This reiterates and highlights the thesis stated earlier that philosophy of education has a major role to play in contributing to the practice of educational research as well as its theory. In arriving at this I have had to begin with what I perceived to be a major flaw in educational practice, examine its claims and then move through a more detached philosophical consideration of the ramifications and assumptions of such claims and then return to practical suggestions which avoid the flaws encountered within the process and yet meet the requirements of educational research. I hope that this, as a method of philosophy of education, as much as the actual proposals in the final chapter, will make a lasting and fit contribution both to those engaged in educational research and practice and those whose lives are enriched as a result.
1.0 **Educational theory.**

This, the first chapter, is an introduction to the field which I regard as the problematic and therefore, as indicated in the prologue, focuses on what others have said about educational research, its methodology, its relation to theory and the role philosophy of education might take in this context. The problem is not a new one for, whilst not thinking specifically of education, it was Aristotle who first questioned the use of science in enquiries concerning human affairs.

Aristotle thought that deliberation was appropriate in certain circumstances and inappropriate in others. In this thesis I wish to argue that this fundamental distinction is overlooked in what passes for educational
theory and its associated research programme, both in the
de facto sense that research departments are based on the
sciences and the de jure sense that even those like Paul
Hirst, who oppose the positivist prescriptions of
philosophers such as D.J.O'Connor, tacitly assume some
reliance on the sciences as foundation disciplines. Since
little of substance has been written on educational
theory, apart from a long running debate between the two
above mentioned philosophers, I will make them the
starting point in a discussion which will examine the
main issues and problems raised by the notion of an
educational theory together with its research programme.

Before I do, however, I wish to look a little more
closely at what Aristotle had in mind when he spoke of
the inappropriateness of deliberation to certain things,
for it is in his comments that we may find the basis, as
I have indicated, of a critique of the de facto notion of
educational theory and its associated research programme
which has become orthodox. The reason, which will become
clearer, is that what is received as orthodox in
educational research is precisely what Aristotle warns
against. What therefore was it that Aristotle thought
wrong about doing research in areas like education with
methods appropriate to the sciences? He says:¹
Surely nobody deliberates about what is eternal, such as the order of the universe or the incommensurability of the diagonal with the side of a square; nor about eternal regular processes whether they have a necessary or a natural or some other kind of cause...for none of these results could be effected by our agency...Nobody deliberates about the past either, but only about a possibility in the future.

The sphere about which one deliberates is a sphere which is concerned with actions and with the future, in other words, with what is under our control. It is not concerned with what is outside that control which includes: 'those branches of knowledge that have precise rules of their own'\(^2\). In the category of 'those branches' Aristotle includes science which he describes as follows:\(^3\)

We will assume that all we know cannot be otherwise than it is...therefore the object of scientific knowledge (episteme) is of necessity...it is eternal, because everything that is of necessity in the unqualified sense is eternal. It (Scientific Knowledge) proceeds either by induction or by deduction. Induction leads us to first principles and universals, while deduction starts from universals...Thus scientific knowledge is a demonstrative state (and) a person has scientific knowledge when his belief is conditioned in a certain way, and the first principles are known to him...

Science, then, for Aristotle, finds its proper objects in the sphere of invariable objects, an investigation of which gives rise to 'scientific knowledge' or 'episteme', while deliberation finds its proper objects in human actions and the future and does not give rise to 'episteme' but practical wisdom or 'phronesis'. At the same time scientific knowledge is knowledge of universal principles or laws and is not
concerned with prediction and control of the future of human actions, this being the task of human deliberation.

Since this is not a thesis on Aristotle, I will not labour the point. The question, however, which he raises, cannot be ignored by anyone seriously concerned with educational theory and its research methodology, since it is obviously within the province of deliberation with its reliance on 'phronesis' rather than science and its associated methodologies.

Even a cursory glance, however, at the structure of educational research programmes will reveal the absence of prudential wisdom in its method of enquiry and its almost complete reliance on science. Further, science is asked to produce predictions about the future of human events and not merely content itself with the formulation of universal principles. In the educational system we therefore either have an excellent means of understanding and controlling our schools, teachers, classroom behaviour, examination results and so on or we do not. If we have, and even scientific evidence points to the fact that we have not, then most certainly Aristotle was mistaken. I, however, agree with Aristotle's insight into the division of research methods and therefore intend to show that Human Science, as commonly understood, i.e., as covering those sciences which use methodologies developed within the tradition of the physical sciences, is not merely an inadequate tool for dealing with pressing educational problems, but is completely inappropriate. I
argue that there are far better means at our disposal and those means I spell out in the final chapters. The first chapter acts as an introduction to the main debate. It outlines educational thought concerning educational theory and research as it developed over recent years under the following headings:

1.10 Educational Theory and the practice of research.
1.11 The debate between D.J. O'Connor and Paul Hirst.
1.12 The contribution of Philosophy of Education.

1.20 The Technico-Rational Model
1.21 The Process Model

1.10  **Educational Theory and the Practice of Research.**

One of the characteristics of the debate over educational theory and the type of research most appropriate to it, is its lack of consensus. This, perhaps, stems from the various groups involved in the debate, and the vested interests they have. Very generally, those engaged in the human sciences see themselves as supplying the hard data upon which educational practice is grounded via an intermediate or applied science such as management. On this view teachers are aided in the classroom by having a background stock of knowledge supplied by the sciences. Given that they
are adequately trained as managers, they may apply that knowledge in the expectancy that whatever objectives they have defined for themselves, they will be reasonably confident in their achievement. Against this is a variety of stances which see the teacher as the main source of information either through reflection on their own activities or through active engagement in research. In the philosophical world the champions of the scientific viewpoint have been philosophers such as D.J.O'Connor, T.Moore and Brenda Cohen. O'Connor, for example, says 'Education, like medicine and engineering, is a set of practical activities and we understand better how to carry them out if we understand the natural laws that apply to the material with which we have to work.' He admits that practice comes first, 'but its theoretical justification has to wait for the scientific development that can explain its success.' The wait, apparently, is to be relatively brief, for he adds that 'the development of a scientific psychology has put us in the position where we no longer have to rely on practice to suggest theory.' The same line is taken by Terry Moore who says that, 'Educational theory...is a field in which all the main disciplines of educational study may be used to support practical recommendations.' Brenda Cohen takes the argument a little further by 'rubbishing' the alternatives to science and Paul Hirst as the alternativist's champion. She says,
By deliberately cutting off education from anything resembling scientific standards of truth or validity, Hirst is, in fact, returning the subject to its traditional status as a field where all may propound their ideas with as complete a freedom and as much imagination as in the past.

Remarks such as these are probably responsible for, and explanatory of, William Guy's comment that 'One of the most surprising features of the literature concerned with educational theory is the scant reference one finds to theories which are implicit in the classroom teacher's actual practice.' Today, however, such is not the case and various alternatives to the scientific-cum-managerial model espoused by the positivists have been put forward. These are prominent in the work of Castell and Freeman, Carr, Stenhouse, Elliott, Schon and Hirst. I will say more about these alternatives, some of which are referred to as 'Action-Research', in 1.21 and so I will not comment at this point on either its proposals or its relation to my own conclusions. Instead I will concentrate, as indicated, on the crucial debate between O'Connor and Hirst which, in many ways, exemplifies the major disagreements between the two main 'camps' which might be broadly characterized as positivist and non-positivist or scientific and holistic.

1.11 The Hirst/O'Connor debate.

I have chosen the debate between Professors Hirst and O'Connor for an extended exegesis both because it has been influential in polarizing thought over educational
theory and because it raises most of the questions which I am attempting to answer in the thesis as a whole. I will begin with a fairly detailed account of O'Connor's arguments as put forward in his paper 'The Nature of Educational Theory'. I will follow these with Hirst's replies both from the same journal and from a chapter entitled 'Educational Theory' written some eleven years later.

O'Connor's main aim, it seems, is to clear up some of the 'muddle' and 'twaddle' which is talked about educational theory. 'The study of education', he says, 'is analogous to economics' but, 'differs from' it in that while, 'it draws its factual basis from scientific disciplines, it does not have a clearly marked and well defined subject matter.' This, however, is seen as a temporary problem for, 'we have to wait upon the relevant developments in the appropriate sciences to sort it out. This raises the question of what the appropriate sciences are and O'Connor gives psychology, economics, sociology and human biology as 'the most obvious ones'. Economics, again, provides the closest analogy because, like economics, 'education aims at optimizing the efficient use of scarce resources'. Indeed, continuing the theme of efficiency, 'we might regard the educational system of a given society as the product of social engineering whose construction is guided by the currently accepted concept of human welfare and made possible by knowledge of the sciences which make it
possible (sic) to realize this ideal. O'Connor sees two aims of educational theory: 'the explanation of the workings of the educational processes and the system it operates in' and 'their improvement in the light of our knowledge of these workings...' 22

From this point on the paper takes a different turn. Firstly it tries to restrict the notion of theory to one with two conditions, and secondly it attempts to show why Hirst is wrong about his notion of theory and about the way in which values enter theory. I shall not dwell on the 'arguments' brought for restricting the term 'theory' to its positivist sense for the 'arguments' simply and dogmatically state the criteria to be accepted. The two conditions are of more interest because O'Connor cannot see why educational theory should (i) want more or (ii) need more. The two conditions which, given (i) and (ii), would appear to be sufficient for a 'genuine theory' are that, 'it should be (a) explanatory and (b) refutable.' 23 Two important features of explanation are noted and, says O'Connor, 'If educational theories did no more than conform to these standards...they would indeed be a kind of scientific theory'. 24 These important 'facts' about explanations are that, '(i) No true explanation can be given to one who lacks the necessary background knowledge' and, '(ii) An explanation is a conclusion arrived at by inference and so must conform to the requirements of any valid inference.' 25 The second of these is further explained
as one in which we know the 'premisses are true' and that
the 'inference is a valid one.' This, if I am not
mistaken, is more or less, what is meant by the words
'nomological' and 'deductive' and is straightforwardly
'Hempelian'.

Having thus outlined his view of the role of
educational theory, O'Connor turns his attentions to
Hirst's 'serious objections' to it. The first objection,
that educational theories are radically different from
scientific theories in that they concern themselves with
guiding practical activities, is neatly avoided by
O'Connor who states his absolute agreement. Applied
sciences, he says, do just this - like engineering they
apply the knowledge of the pure sciences and use the
principles gained 'as a guidance system'. The
disagreement is located in the 'additions' Hirst would
make to the two 'criteria for a genuine theory' that
O'Connor has isolated. What more Hirst would add,
O'Connor suggests, is that the 'theory (shall) determine
precisely what shall and what shall not be done...in
education.' Further, because of the nature of the
addition, 'Hirst wants to import value components into
the theory itself.' This seems to be the main
stumbling block for O'Connor who sees the system guided
from the "outside" by such factors as 'the currently
accepted concept of human welfare'. Hirst is
criticized on two grounds, first for thinking that in
aiming at educational goals we require a 'logic of moral
reasoning', and secondly for assuming that anything of a factual nature can follow from such moral reasoning. The first argument is backed up by the valid suggestion that there is no such thing as a worked out moral logic and the onus is on Hirst to show that there is or could be. The second argument challenges Hirst to demonstrate the 'logical relations between...statements of value and statements of fact'.

In concluding, O'Connor makes two bold claims: first that 'we have no good reason to suppose that a purely empirical basis for educational theory would be inadequate', and secondly that the value of the 'methods of science...is that they rest on the belief that reason (that is, tested methods of assessing and evaluating evidence) is our only guide in problem solving'.

In the course of this thesis I shall challenge the second of these claims head on: for the first part of the thesis is concerned solely with such claims. The first claim is the one Hirst feels most compelled to meet. He begins with what he perceives to be their apparent agreement over the concern educational theory has with 'improving and guiding practice', but says that 'His account of the guidance the theory offers is that of the technical means the sciences can provide for realizing ends coming from outside the theory - from society at large and its 'currently accepted concept of human welfare. To my mind (however)', Hirst continues,
'the theory is itself concerned with determining ends as well as the means of education, the answers to all questions about what ought to be done, moral as well as technical.' In support of this contention Hirst questions O'Connor's, 'fear of having a theory whose logic we cannot at present satisfactorily elucidate' and the consequences this has for a theory which has as its proper object the reasons and values of practitioners. The consequence, for Hirst, is that whatever empirical basis or empirical theory O'Connor is proposing, it will be, 'quite inadequate for practical judgements'. Besides, he adds, science developed quite independently of a fully worked out 'logic of scientific explanation': 'indeed ...it developed in spite of gross confusion.'

On O'Connor's point about the relation of values to facts, Hirst brings forward a number of significant features. First the dissociation is, 'contrary to their relationship in the actual conduct of educational debate'. Secondly, 'the means often involve activities that must be assessed not merely as efficient means but also in moral terms as ends in their own right.' Thirdly, they, the means, 'can be constitutive of certain ends and so on.' Fourthly that, 'society's notions of general welfare are far too general to enable those in education to derive detailed principles from them'. Fifthly and finally, 'the conflict of values...creates endless problems on which even individual teachers must make value judgements for themselves.'
I make no personal comment on the debate at this point except to say that I broadly agree with Hirst's position. I do not think the argument will be resolved, however, while the Human Sciences themselves remain unchallenged within educational theory. That, and a proposed scheme to replace the positivist element in research, is the substance of the present thesis. As far as values are concerned in this thesis, they will form the basis upon which educational research is undertaken in all but a few cases. They are thus, on the view I will adumbrate, not merely part of Educational Theory, they are central to it.

1.12 The Contributions Of Philosophy of Education.

The debate, as I have outlined it, is unresolved today. It is unresolved in the sense that educational institutions carry on as if there had been no debate. With the possible exception of the 'Centre for Applied Research in Education' in East Anglia (in this country) most schools of education involved in research rely on the technico-rational model which utilizes the methods developed within the natural sciences on the assumption that these are going to provide the data which will improve practice. Correlated with this is the growth of various sciences dealing with efficiency and management.
These 'applied sciences' perform the necessary function (on the model's assumptions) of transforming the hard data into rational procedures which, they hope, will bring about an educational revolution. It is my opinion that this is an unlikely dream because of the dubious status of the human sciences. The debate will, I think, only be finally resolved when it has been conclusively shown that the human sciences are fakes: empty replicas of natural science which can neither produce the general laws they need nor even assume that they are there to be discovered. I have devoted Chapters Two and Three to this task.

The task I have set myself involves a mode of philosophy which has not been too prominent in recent philosophy of education. In the two chapters just mentioned which question the status of human science I am performing a critique with Kantian underpinnings: the question I am asking is, 'How are these sciences possible?' In the last chapter however, where I am concerned with putting forward an alternative methodology, my philosophical 'model' is not quite so clear. Indeed, the question of which role philosophy takes in making substantive proposals is not one which has received much attention. This lack is unfortunate in an applied philosophy such as philosophy of education. I will take up this matter again after I have looked at some of the claims philosophers of education have made
about their role in education and in its research. Castell and Freeman divide these into two broad categories. The first is conceptual analysis and the second is the justification of social practices. This division is readily supported by statements by the role descriptions of Philosophy of Education offered by the philosophers themselves. My interest is to see whether these roles are adequate to the task of resolving the debate which is fundamental to educational research: is the scientific model correct, and if not how do we go about constructing a new one?

R.S. Peters, for example, says that 'What distinguishes the philosopher is the type of second order questions he asks..."What do you mean?" and "How do you know ?"' and that this involves him in a 'search for criteria (which) is the kernel of philosophical inquiry.' In so doing, 'they make explicit the conceptual schemes which...beliefs and standards presuppose; they examine their consistency and search for criteria for their justification.' Ultimately, the 'philosopher's task...is to apply analyses of concepts and theories of justification...' with a view to clarifying the scientific or moral discourses which form, for the philosopher, the first order of enquiry. Much the same explanation is given in the 'Logic of Education' produced by both R.S. Peters and Paul Hirst, but here just what is involved is made a little clearer, for example, 'What
we do is to examine the use of words in order to see what principle or principles govern their use... (and) ...in attempting to make explicit the rules... (we) get clearer about our concepts.' And this, they add, is a preliminary to answering other philosophical questions. Glenn Langford adds his support saying that, 'Philosophy as a second order discipline... concerns itself not with the subject matter of first order disciplines, but with the medium in which they are conducted; that is, with the discourse peculiar to them and the procedures implicit in them.'

Not every one agrees however. A.O'Hear, for example, says that while, 'The tendency has been to say that it is not the philosophers job to lay down prescriptions, but rather to adopt a second order stance... philosophy of education (should be) concerned with educational aims.' This feeling of frustration is reiterated by John White who says, in the introduction to his book on the aims of education that while 'until recently it has been analytically oriented... (and while) ... it has been interested in aims... (it has been interested) almost as much in how the concept of an aim is to be understood as in substantive questions of what aims should be.' The reason for this is that, 'philosophers of education... (have) been chary of saying what they think aims ought to be because they have felt this kind of question lies outside their discipline:
philosophy does not prescribe what ought to happen; its job is to clarify, in a second order way' the concepts, arguments and assumptions embedded in theories, in this context educational theories.' Exactly, in fact as Paul Hirst and Terry Moore respectively say it should be: 'Philosophy, I shall take it, is above all concerned with clarification of...concepts 'and 'It is not the philosopher's job to set the educational aims of a society, but he may show how the various ways in which value judgements may be supported.'

The contribution which conceptual analysis and the justification of social practices make is looked at in greater depth in a paper by Richard Peters and John White. I will look at each in turn together with Peters' remarks in his book 'The Concept of Motivation' where the recommended techniques are applied.

In a section entitled 'Philosophical Research not geared to Social Science and Psychology' the authors argue that since 'educational research' is concerned to provide 'new knowledge which is...relevant to initiating people into what is thought worthwhile' it 'cannot be restricted to empirical enquiries.' Curriculum design, for example, raises questions fundamental to ethics such as why some activities are preferable to others. There have, therefore, to be,'good reasons for having some things on the curriculum' otherwise there is 'no rational basis for deciding on priorities.'
Educational practices, such as that of punishment, need, the authors argue, to be looked at philosophically 'before the relevance of psychological research into the effects of punishment can be shown.'

Ethics thus has a central role in educational research - but this is not the only contribution philosophy makes. Various examples are given of psychological theories - such as those of Freud and Piaget - which have crept into educational thinking without much prior thought about the relevance of such theories to specifically educational contexts. Similarly there are 'countless questions' which cannot be simply answered empirically: 'What is a school subject?' and 'What degree of "specialization" should there be at secondary school?' are just two examples.

The authors then move on to procedures such as activity methods, learning by experience, discovery methods, indoctrination and so on which are brought in or out of educational practice in the wake of a new report or slogan without reflection as to what they mean or indeed whether they are educational. Further to this there are, 'embedded in these procedures...principles which prescribe how children should be treated' but which are nonetheless unexamined principles, principles for which no justification has been given. Here, just as with the justification of the principles which govern education provision and distribution, there is a need for philosophical contribution.
Where educational research is geared to the sciences, the authors argue that 'the philosopher might be able to help the social scientist in the clarification of his aims.' More radically they might ask whether the social sciences 'can be theoretical sciences, in the sense that physics is...'. Such radical work is instanced by D.W. Hamlyn's 'dismissal of theories of perception in psychology', a dismissal the authors feel might 'be generalized to cover all social and psychological phenomena' on the grounds that 'the phenomena of human behaviour requires logically disparate types of concepts for their description and identification, and hence for their explanation.' The reason given is that the concepts describing human actions cannot be reduced to 'mere bodily movements': they cannot, therefore be adequately dealt with by a general theory. Less radically philosophers can point to the flaws in experiments - point out, for example, that intelligence tests may in fact measure a lack of interest in doing the sorts of things intelligence tests demand. Or, again, less radically still, they might point out that what appears to be an empirical discovery is no more than a conceptual truth derived from the meaning of the words used to describe the experiment and its findings. An example given of the latter is Hamlyn's criticism on the 'figure ground hypothesis': 'we would not call anything an object unless it could be distinguished from everything else that forms its background.'
Richard Peters applies these types of methods to psychological work on motivation in his book on that subject. Here, his main line of attack is from the perspective of ordinary language and its concept of motivation as compared to the 'technical' use of the word in various psychological theories. Space prohibits a lengthy exegesis but some of his concluding comments are interesting in that they throw light on the way he sees conceptual analysis as a critical tool.

My interest in the type of philosophy used in the above stems, as I have indicated, from concern over its critical bite. Peters claims, on 'logical' grounds, that highly generalized theories are impossible. But it is worth asking just what these logical grounds amount to. He says they derive from the distinctions we make in ordinary language from which we see that action is rule governed and conventional. 'The job', therefore, of the human scientist is consequently, 'to exhibit the structure of goals and conventions in unfamiliar societies.' Leaving aside the complication of Quine's injunctions of doing this at all well and leaving also on the side the rather strange remark that the sciences are concerned with 'unfamiliar societies', let us look at what is being assumed. Logic, as it is here used, is ambiguous between that which refers to something like deductive laws and that by which 'family resemblances' are seen to exist between words or their uses. Since, Peters does not mean the first and since the second is
said to be contingent on history or culture, nothing which needs to worry the scientist is said. The scientist is free to go on looking for better descriptions of human actions.

I conclude from this that conceptual analysis is an inadequate tool for Peters' purposes. Its clarificatory role, whether in a familiar or an unfamiliar society, amounts to anthropological philology and a syntagmatic one at that. This has a fatal consequence as Charles Taylor, Paul Feyerabend and Richard Rorty all admit: science may, in the future replace our 'common sense' concepts. Those who resist include Norman Malcolm and I put my own arguments favouring his views in 3.8. For the moment I am merely concerned to spell out the problem. It is: science can claim to be able to offer better explanations of human behaviour because it can (in the sense that it is not logically impossible) reduce our present, contingent and conventional descriptions of ourselves to more fundamental descriptions which are not conventional nor are they 'intentional' or purposive. My own arguments concerning this problem agree with Peters about the 'intentionality' of action descriptions and in the fact that they are contingent on our historical practices (the argument for which forms the first part of chapter four). My departure is that the statement that our languages contain these conventional elements is not threatening to science for the reasons I have stated. A critique of the
sciences must show also that while such descriptions are contingent they are irreducible to non intentional descriptions. Consequently while I accept that the scientist may produce descriptions of ourselves which are not like those we have I reject the possibility of their identification with our intentional descriptions which leaves a referential mismatch. I also reject the possibility of semantic synonomy between intentional and non-intentional descriptions which leaves truth conditions intact. The argument thus concedes to science the possibility of alternative descriptions but denies to it the very means - i.e. translation between the two - which would render it possible to talk of the same thing. In the absence of identifiable instances of the same event: actively or neurologically described, the scientific enterprise runs into a seemingly insuperable problem for, it cannot talk - across the intentional / non-intentional divide - of the same event again. This is so because it is axiomatic to the scientific enterprise that (i) correlations are made between independent variables which are identifiable as the same in an all possible worlds scenario and (ii) that the variables concerned are seen to correlate again and again. Where science cannot talk of the same event it cannot talk at all.

I move now to a consideration of the scientist's view of what should happen in educational research. Since there is a growing 'movement' towards involving teachers
in research I will divide this section into two halves. The research described in the first, which most closely resembles O'Connor's 'science', is fairly typical of the type of research carried out in both Britain and the United States. I refer to this approach as the technico-rational model and distinguish it from what Stenhouse and others refer to as the process model. Both models, I argue, even in their most modern forms, are 'positivist': that is they rely ultimately on the methods established by the natural sciences and as a consequence fall within the unification of science programme which, I believe, is the real heart of positivism. The following two sections provide an introduction to a more philosophical consideration in the following two chapters.

1.20 The Technico-Rational Model.

The technico-rational model is characterized by the testing of formulated hypotheses which are based on a collection of data. Supported hypotheses are perceived as being useful in educational settings because of the way they can be used to underwrite policy and thus support technologically construed managerial decisions which seeks to manipulate behaviour in ways thought desirable. This 'interest in behaviour modification in education', has, according to Schwieso and Hastings, seen, over 'recent years...an increase of interest.' As evidence
they cite the greater use of behaviour modification techniques 'within special and ordinary schools and through the publication of a spate of books urging the use of such procedures by teachers.' 51 My object in this section is not to make any particularly telling philosophical comments on that model but merely to indicate the sort of direction this form of scientific thinking has taken and is still taking.

I begin with some remarks about Professor Hazel Francis' inaugural lecture at the London Institute of Education not because she represents the tradition, but because she says she does not, as indeed is indicated by the catch phrase of the address - 'Winds of Change'. I find this interesting because, as I hope to show, its contents are well within the tradition and not, as is surely intended by the obvious rhetorical ploys in the title, well away from them. In fact we have to look no further than the second paragraph to find her warning against 'undervaluing psychology as a means of providing an information base from which policy makers may act' 52 and, further, advocating that it (psychology) can 'also be the basis for evaluating tentative theoretical models of defined real-world domains.' 53 What is even more telling are the scientists with whom she allies herself. She quotes approvingly, for example, work by Pask 54 who 'used the term conversation for learning mechanism' and argued that learning mechanisms (conversations) consisted of at least two levels of
processing, the first of which includes 'responding to feedback from ...effects' while the second requires 'action...on processes...to alter them in the light of feedback from the effects of such alteration'.\textsuperscript{55} This, adds Francis, extends, 'research methods and thinking well beyond those of behaviourist and experimental cognitive psychology.'\textsuperscript{56} Perhaps I am peculiarly blind to the meanings of words, but if, by some mischance, I am not, then I seem to discern, in Pask's work, as retold by Francis, the ghost of Skinner clothed deceptively in the form of a computer.

Professor Francis's own work is not so clothed: it is, for all its rhetoric, openly scientific. She says, for example, that after making 'observations (which) included examples of children with some explicit knowledge of letter-sound correspondences...' she made a 'hypothesis (which) suggests probabilistic and analogic strategies on coming to know about English spelling'.\textsuperscript{57} Although she does not spell out, perhaps advisedly - given the very 'radical' views she is putting forward - what is to be done with a hypothesis, it is a fair, a probabilistic, guess that it will be put to the test.

Another psychologist well known in educational circles is David Child. One merit of Child's book\textsuperscript{58} is that it does not attempt to fudge the issues: it is downright abusive about the 'anecdotes' of children and teachers concerning their own experiences in the classroom. It is straightforwardly and unapologetically
positivist. I quote from the section entitled Educational Psychology:\textsuperscript{59}

We cannot rely on our independent observations alone. When we observe children in class or at play, it is deceptively easy to draw conclusions based on isolated incidents and to make generalizations about all children from these incidents. This is called anecdotal evidence. It is sometimes helpful as a starting point for more systematic observations or as confirmation of a general principle, but anecdotes cannot serve as the sole criterion for making decisions about children's education. Instead psychologists try to formulate generalizations based on representative groups of people...or in animals where...the findings can be validly transferred to human situations.

The only problem he sees in this - including his use of the word 'valid' in the case of transferring generalized animal behaviour to humans - is how, 'to convert a generalization into a form which makes it useful in individual cases.'\textsuperscript{60}

This lack of critical awareness amongst educational psychologists is not helped by some philosophers who heap esteem on science. Moore, for example,\textsuperscript{61} says that while philosophers can show 'how value judgements may be supported' the 'assumptions made about children will be primarily the province of science.'\textsuperscript{62} Consequently he sees the need, for Educational Theory, of 'specialists like Piaget, Freud, Kohlberg and Bernstein, to give (sic) accurate up-to-date knowledge of what children are really like.' Not just that, however, for, we are also told that in order to make 'assumptions about the effectiveness of methods we need help from psychologists,(and) learning theories like
those of Skinner and Bruner...63 The lack of awareness shown in these comments of the different or even conflicting accounts of how children 'really are' is indicative enough of the inadequacy of the account. Even if Moore's comments were to be taken seriously we would still need some way of telling, from among the differing accounts of children, which best approximated to their 'reality'. I wonder if Moore would consider this a matter for science or common sense to decide?

1.21 The Process Model.

The work on "Action-Research" by Lawrence Stenhouse, John Elliott and others is often considered as an alternative to the scientific or technico-rational method of research. In this section I will review some of what these authors have had to say on the subject and offer a few comments of my own. In accord with the introductory nature of this chapter, I will refrain from specific philosophical difficulties which I leave to other chapters.

Since Stenhouse's work is considered an 'alternative' to the positivist approach it comes as something of a surprise to read what he says. In a sustained section entitled 'Towards a Research Model'64 his main thesis is remarkably close to the scientific model just discussed. The main difference being the way in which hypotheses are arrived at: through
teachers' own reflections on their practice. Apart from this however we find that, 'research must aspire to situational verifiability' which involves teachers in 'mounting a verification procedure in his own situation.' Ignoring a previous observation to the effect that teachers and pupils often perceive the situation in differing ways he adds that, 'data drawn from such studies...are useful (as) hard data. They are rooted in real situations and allow of a high degree of verisimilitude.' When talking of the data itself he is hardly, if at all, distinguishable from the positivist. For example he says of the 'measurement results of the Humanities evaluation' that 'the most robust results were correlated shifts on the Manchester Reading Test, The Mill Hill Vocabulary Test and measures of pupil self esteem.' In the next sentence he argues that,'the hypothesis that the three variables correlate' is explored in a 'wider setting.' Now, not so unexpectedly, we find him explaining the causes of change in schools in a thoroughly scientific manner: 'changes will take place as a function both of the experimental treatment and of the contextual treatment which the school is offering at the same time.' Worried by the variables which might influence and thereby render invalid the test he advocates the use of a 'battery which, placed alongside tests orientated on a particular programme, give an indication of the contextual variables in any one setting. Well-standardized tests
exist which would be strong candidates for inclusion in such a battery.' In concluding, he remarks that (the above) is an attempt to, 'integrate action and evaluation research into a unified research model.' I leave my comments on the evaluation side of this unity to chapter two.

What then of action-research: the first stage in Stenhouse's programme? He says that, 'the idea is that of an educational science in which each classroom is a laboratory, each teacher a member of the scientific community.' Each teacher will have 'a research stance': 'a disposition to examine one's own practice critically and systematically.' However, research cannot take place unless the classroom is 'open'. It needs to be so in the sense that the teacher can both negotiate his role and progressively work towards a redefinition of it - as researcher. The necessity of this derives from the fact that the 'teacher needs to teach that definition of himself to the pupils...he makes it clear that the reason he is playing the role of researcher is to improve his teaching and make things better for' his pupils.

Thoughts such as these are fairly representative of those interested in action-research. In their interim report on the Ford Teaching Project, for example, Elliott and Adelman state that,
The central conviction behind much of our work with teachers has been the importance of helping them to develop greater autonomy and control over their performance in classrooms by reflecting on the consequences, both intended and unintended, of their actions.

However, as with Stenhouse, the links with straightforward positivist research are espoused in the later stages of research. Teachers make and reflect upon their practices but,\textsuperscript{74}

(while) action-research aims both to contribute to an understanding and solution of the practical problems faced by teachers in the classroom situation (it also aims to add) to the development of a theory of teaching.

Teachers' reflections, they add, more revealingly, 'contribute to the development of social science theory'\textsuperscript{75}: it is there that the 'theory of teaching' is developed.

Talk of 'theories' brings us back to the debate between O'Connor and Hirst and to the heart of the controversy. While we have seen that Hirst is openly opposed to an unashamedly scientific approach, a glance at his own notions of what good theory should be reveals an ambivalence not unlike that in Hazel Francis. While positivism is not openly espoused it is latent in his later work\textsuperscript{76} and especially in his concern with the development of 'practical principles'\textsuperscript{77} derived from teachers' 'operational educational theories'.\textsuperscript{78} Unlike Stenhouse and Elliott he is coy about what to do with 'operational theories'. He says, for example, at one point that 'educational theory...draws of course, on all the theoretical knowledge available in the social
A little later, however, he says, 'it is the job of such disciplines as psychology, sociology and philosophy...to provide a context of ever more rationally defensible beliefs and values for the development and practical testing of practical principles.'

The coyness is perhaps due to the unemphatic place given to the sciences: what, for example, does 'context' mean? At the same time Hirst does seem to be saying that the theoretical knowledge 'available in the sciences' has the last say in rationally defending practice. Why else does he use the word 'testing' in the context of the rational defense of practical principles? It is worth noting, in passing, that Hirst had earlier argued as follows: 'Explanation in terms of beliefs and values, of reasons as well as causes, seems to me to be logically necessary (for educational theory) and explanations of this kind do not, to my mind, fall within the pattern of explanation in the sciences...'

One cannot but be left wondering, given the intentional nature of human actions, exactly what role science would play for Hirst in providing a rationally defensible practice.

I should say, in conclusion, that I do think that there is a way of providing a rational framework for educational practices, but I do not think it to be the way of O'Connor, Stenhouse or Hirst. What that way is I leave to the final chapters. Before that I provide what I hope are some philosophical reasons for entertaining a degree of scepticism over the whole project of
understanding human beings scientifically.
Chapter One: Footnotes.


(2) ibid, p118.

(3) ibid, p207.


(5) ibid, p109.

(6) ibid, p109.


(19) O'Connor, op cit, p97.

(20) ibid, p98.

(21) ibid, p98.

(22) ibid, p98.

(23) ibid, p102.

(24) ibid, p104.

(25) ibid, p103.

(26) ibid, p104.

(27) ibid, p104.

(28) ibid, p106.

(29) ibid, p106.

(30) ibid, p98.

(31) ibid, p108.


(33) ibid, p109.

(34) Although Hirst does express doubts about the possibility of science providing adequate explanations of human behaviour. He says, for example, that 'because it (education) involves deliberately planned activities, education is only characterizable by attending to the way those involved in the institution conceive of what they are doing (and) if the delineation of education as an institution requires an understanding of human purposes, which I do not feel are reducible to an understanding of what is observable, we can say right away that the study of education must involve more than a study of the relevant sciences.' [op cit p112]

(35) ibid, p113.

(36) ibid, p114.

(37) ibid, p115ff.

(38) Hirst is relying here, as he admits in his footnotes, on Hugh Sackett's paper: "Curriculum Planning: Taking a Means to an End" in 'the Philosophy of
Education' (Ed. Peters) Oxford, 1973. Sockett lists five ways of taking the means-end relationship: 1 - as contingent; 2 - as logically necessary; 3 - as logically constitutive in the sense that means are part of the end; 4 - as logically constitutive in the sense that means instantiate the end and 5 - as logically limiting. (some ends are precluded)

(39) Castell and Freedman, op cit, pages 9 and 11.


(44) J.White, 'Aims of education restated', RKP, 1982. page x.

(45) P.Hirst, "Philosophy and curriculum planning" in 'Knowledge and the curriculum', RKP, 1974.

(46) Moore, op cit, p15.


(49) ibid, p6.


(51) ibid, p81. (My emphasis)


(53) ibid, p11.

(54) ibid, p12. (Pask G. "Conversational techniques in the study and practice of Education" and "Styles and Strategies of Learning" both in the 'British Journal of Educational Psychology', 1976.)

(55) ibid, p12.
(56) ibid, p13.

(57) ibid, p21.


(59) ibid, p3.

(60) idid, p3.

(61) Moore, op cit, p15.

(62) ibid, p15.

(63) ibid, p15.

(64) Stenhouse, op cit.

(65) ibid, p136.

(66) ibid, p137.

(67) ibid, p138.

(68) ibid, p138.

(69) ibid, p140.

(70) ibid, p142.

(71) ibid, p156.

(72) ibid, p155.


(74) ibid, p1.

(75) ibid, p2.


(77) ibid, p20.

(78) ibid, p18.

(79) ibid, p5.

(80) ibid, p19.

(81) Hirst, op cit, p112.
Chapter Two.

Experimental and Evaluational Research in Education.

2.0

In the first chapter the psychologist Child characterized the scientist's work as the 'formulation of generalizations.' These 'generalizations', or 'principles', as D.J.O'Connor calls them, may then be used as 'systems guidance' instruments in educational policy implementation. In my view these two authors represent a tradition of scientism which conflates 'phronesis' with 'episteme'. In order to show that this is a misguided and possibly fruitless enterprise I intend to look in more detail at what the scientists do when they formulate 'generalizations' and attempt to 'guide systems'. The first of these activities is normally referred to as 'experimental research' while the second
as 'evaluational research'. Since, however, both employ the same basic methodology I shall concentrate on that.

Our first task is thus to discover what scientists involved in educational research say about their methods. I should say also, at this point, that I am concentrating on methods which are described as scientific and do not wish that anything I say is seen to apply to other forms - many of them valid - of educational research. In order to find out what scientists mean by experimental method I have selected a number of textbooks on the subject.¹ These, while chosen randomly, show a remarkable uniformity in what they prescribe showing both that they are fairly representative and also how strong the scientific tradition is.

2.1

I will begin with a comment by De Groot on the way in which applied research is located in the overall scheme and then pass on to look at the detail of the experimental situation and its evaluating procedures in order to see more closely what sort of research is carried out, what it hopes to gain for those to be educated and possibly identify some areas in which there may be room for philosophical assent or dissent.

In his work on methodology Adrian De Groot explains the relationship between applied Human Science
and its theoretical background as follows: \(^2\) A theory together with its ramifications, insofar as these have been empirically worked out and tested, may be designated as the then available nomological network... Such a network may naturally be at different stages of actual realization. Ideally, it would provide 'complete' coverage of the area of reality with, preferably, nothing but positive confirmation outcomes. Such completeness is said to have been attained when, 'the theory is confirmed as such by a forum of co-scientists as a system of laws.' He then explains just what the nomological network of a theory comprises: a 'theoretical model,' he says, which has 'purely deductive consequences...; derived hypotheses and predictions...and "evidence", (which is) the factual empirical outcome of investigative procedures.' It is the lower level of scientific research - the 'evidence' and the investigative procedures which produce it, together with the predictions made on the basis of it to which we will confine our attention in the present chapter.

De Groot puts experimental research in its theoretical context. But what are the 'investigative procedures' which form its basis? Most of the literature on this subject is in considerable agreement, and that agreement has changed little over a twenty year period. Best, \(^3\) for example, writing in 1959, says of an experiment that it:
involves the comparison of the effects of a particular treatment with that of a different treatment or of no treatment (in which) the experimental group is exposed to the influence of the factor under consideration; (while) the control group is not. Observations are then made to determine what difference appears...in the experimental as contrasted with the control group.

In 1983 Mark Abrahamson⁴ writes,

In most cases (of experimental research) there is an experimental group corresponding with each independent variable that is to be manipulated ...subjects...are 'exposed' to the independent variable, (while)...in the control group they are treated identically except that they are not exposed to the independent variable.

On the previous page he gives us an example of the sort of result which this type of investigation has produced:

...it is commonplace to find that education, occupation and income are all interrelated. The question then becomes, which variable is independent (that is, cause) and which dependent (that is effect)?

I will deal with the notion of cause in greater depth in the next chapter. For the moment, and in order to avoid lengthy philosophical excursions, I will add one final view from Verma and Beard,⁵ who say:

In...experimental research, through manipulating an experimental variable, attempts are made to determine how and why a particular condition or event occurs. This manipulation is deliberate and systematic. So, for any experimental study, there has to be an independent variable that is manipulated by the researcher under highly controlled conditions.

I think we can say - without reviewing endless volumes - that scientists are fairly well agreed over what to do in an experiment. We can now, therefore, move on and look at some of their terms a little more closely. The variables are the most obvious candidates and of these, while many others, such as the extraneous
variables are mentioned, the two most important are the dependent and independent. Putting together what has been gathered from various sources, the main scheme which emerges might be summarized as follows: by excluding all conditions bar one, an observer may see any correlation there might be between this condition, referred to as the independent variable, and another referred to as the dependant variable. The effect, let it be stressed, is only observable where all other conditions are held constant. While this might summarise the main lines of agreement, it is, however, a little unfair. Firstly, because after Fisher's research scientists say they have found a way to measure the effects of multiple variables on a dependent variable using a technique referred to by Dennis Child\(^6\) - and not without a hint of admiration - as 'factor analysis'. Secondly, because research workers themselves are acutely aware of the problems inherent in keeping tight control over the variables. Ethical considerations are sometimes cited as a reason why perfect conditions cannot be met, but, as I argue later in the chapter, the problems may transcend ethics and point to a more obdurate, problematic subject.

From what has been said we can now divide up the area of experimental research and look at the following items. I include the notion of validation although not much has been said about it above. Its importance, however, is simply that its procedures 'validate' the
experimental findings. Insofar as this is the case they form an indispensable part of the experiment. I shall, of course, say more about what such procedures are in the relevant sections which are as follows:

2.20 What are variables?
2.21 The attempt to measure the variable.
2.22 Units.
2.23 Operational Definitions.

2.30 Validation. (Internal)
2.31 Validation. (External)
2.32 Validation. (Repeatability)

2.40 The notion of applied science: Engineering.
2.41 Problems with the analogue.

2.20 What are Variables?

I begin with a few more details concerning the variable before attempting to take a philosophical overview of the it. Best,⁷ writing over twenty years before Verma and Beard, describes variables in virtually the same way. He says that they,
are the conditions or characteristics that the experimenter manipulates, controls or observes. The independent variables are the conditions...that (the experimenter) manipulates... (while)... the dependent variables are the conditions that appear, disappear or change as the experimenter introduces, removes or changes the independent variables.

De Groot voices the same opinion, but adds\(^8\) 'that the variable (as operationally defined) must adequately represent the construct-as-intended...'. By this addition he brings in another factor which we will later examine. For the moment, let us be content to see what has been added. It is that the variable - which might be something like personality - must, in order to be quantified (although so much is not yet apparent) be given an operational definition, which must 'adequately represent'. This further complicates an already complex situation for, as Hoover points out, 'The variable "personality"...is reputed to have over four hundred definitions in the professional literature'\(^9\) the professional researcher having to adopt, without plumping, for one which is both quantifiable (a condition of the experiment) and is adequate (a condition of saying anything appropriate about its subject.)

2.21 The attempt to measure the variable.

The central feature of the experimental situation as we have outlined it above entails, for Abrahamson, the measurement of the effects of one variable, the independent, on another, the dependent. In order for this
measurement, which entails measures of central tendency (mean, median, mode) and dispersion (standard deviation, deciles and other percentages), to take place it is crucial that other possible variables be kept constant. Further, a scale must be adopted which, 'implies the choice of a particular mathematical model (in which)...the phenomena of the outside world, the real world, are represented in analogue.'

This 'analogue' which Abrahamson regards as, a map of the real object system' seems to be a model based on, or closely related to the vector analysis of forces in physics. This insists that if two equal forces act on a point in opposite directions the point remains where it is. Increase one force, or alter its direction, and the point moves. Where it moves may be directly plotted mathematically without a 'real' experiment ever taking place. The reason is that the point's movement varies with the two forces in an entirely predictable way. The reason for the extreme predictability in variation in these cases is due to a number of factors. The first is that the forces acting in the same circumstances always act in the same way. The second is that the amount of force exerted is completely quantifiable. In both cases the resultant is predictable from the measured quantities.

The question is whether the conditions which must be satisfied in order to obtain such precision obtain, even approximately, in educational experiments. In order
to gain some insight we must firstly enquire which conditions must be satisfied for it to be the case. Before we can answer this, however, we must ask just what it is that has to be satisfied. According to Abrahamson, it is the mathematical analogue of the 'real object system': the mathematical abstraction representing the statistical dependency of one variable on another. If we return to our vector example the equivalent would be the statement of real forces acting on a point in quantified mathematical terms. The forces, for example might be represented as 140 kilogrammes and 95 kilogrammes while the direction as a number of degrees. In this way a real system of forces (such as those acting on the span of a bridge) may be exactly replicated analogically.

The conditions of this replication must involve, firstly, the nature of the material (which must be such that it acts in a constant way) and secondly that the variable must be redescribable in a way which allows for its exact quantification. Given these two conditions the interaction between two or more variables may be replicated analogically in mathematical terms. This is supported by both De Groot, who says of measuring that it, 'is assigning numbers to objects on the strength of certain empirical observations.'\textsuperscript{13} and Hoover for whom it is the, 'counting the units of a thing.'\textsuperscript{14} In either case, whether 'assigning numbers' or 'counting units', there is the presupposition that whatever is the object of assignation it is the sort of object which takes
quantificational adjectives without provoking serious semantic doubts. In Human Science the objects of quantification are always either humans or human related and thus when Abrahamson says that,\textsuperscript{15} 'The calculation and interpretation of specific tests of significance and association are contingent upon the level of measurement of the variables in question' he necessarily presupposes that the object, the person, or group are, without stretching our semantic sensibilities, the proper objects of measurement. If he does not he admits that the 'calculation and interpretation' of such events is impossible.

2.22 Units.

I move now to a more direct consideration of just what is entailed by the necessity of quantification. In the attempt to make a mathematical analogue of real world systems two things seem necessary and both are readily admitted by scientists. The first, as we have just mentioned, is that of maintaining some form of semantic equivalence. This raises questions concerning the possibility of appropriate operational definitions. The second concerns the more fundamental question about the attribution of numerical adjectives to some nouns. This I simply call the problem of units and it is to this which I move first.

De Groot\textsuperscript{16} says of measurement that,'it has
become equivalent to mapping into an objective scale...it is assigning numbers to objects on the strength of certain empirical observations.' In order for this to come about there must be 'operational definitions' for the variables in question and units into which a scale is divided. In certain areas, of course, these cause no particular problems; those in which the objects themselves come in discrete units, for example: the number of people unemployed, the number of deaths, the number of children, etc. Each of these takes a number unproblematically as an adjective. Units for this type are unproblematic as well since they are the object of what is counted. The fact that these can so readily be quantified means also that operational definitions for them are also generally unproblematic. Such is not the case with much that forms the object of study in the educational world however since they are neither in readily operisationable form nor are they readily quantifiable. These are separate issues and I shall deal with them separately.

Firstly those not obviously quantifiable. In this group I include all those variables which do not readily take a number as a predicate. For example: one personality, three character, six intelligence, two satisfied and so on. In order, however, to meet the criteria set out by Mark Abrahamson$^{17}$, for whom as already noted 'The calculation and interpretation of specific tests' are contingent upon the level of
measurement' such variables as these must be treated in some way so that they do take a number predicate. The first problem then is how to make them more amenable: the problem of units.

In Physics, as in the other natural sciences, units are arbitrarily defined. Their arbitrariness does not matter: we use a metre as well as a yard and there are rules which govern their translation. It is also usual for the quantities themselves to be conventional. This sounds strange until it is remembered that units such as a kilogramme, a pound, a foot, etc all have no other meaning than the one we give to them and define for them. Let us look at the yard.¹⁸

We need to ask what being a unit presupposes. Certainly, something's being a yard long does not presuppose its being a stick or a piece of metal - for a yard can be defined and reproduced as either. It would seem therefore that straightforward material things are not necessary conditions of being a yard: it can be a particular wavelength. Detection of wavelengths however does seem to presuppose, for us, some more material things: oscilloscopes and the like. In a sense we could say that being a yard presupposes a universe with size: something we can measure, define and reproduce.

Then again, if we said a yard was such and such a length - defining it, say, as the length of a piece of wood - and then proceeded to give directions for building a house with a different length piece of wood, or wood of
differing lengths - calling them all yards, we should simply reject the notion of a unit and give up the building.

Here then we have a number of notions all of which seem to be important to being a yard. The first is that it is reproducible as the same length in some 'substance'. The second is that it must perform a function in a wider system - such as building a house - and be, in the context of that system, adequate to the job.

The project envisaged by a human science demands that its variables are quantified in such a way that they meet the two criteria outlined above: their units must be reproducible and must function in a wider system. The question is whether human science can claim this. Typically the variables are described as some form of action, intention or state: lazy, motivated to learn or disadvantaged for example. These or variables like them are tested to see whether there is a significant relationship between them expressed by their repeated correlation in the experiment. High correlations are expressed as high percentages or decimals approaching 1.

Now, reverting to our vectors, if it is found that a force of one hundred pounds pulls a weight a distance of ten yards against an opposing force of fifty pounds, I can verify this by repeating the experiment as much as I like. If the variation in distance pulled is 1/100th of a yard, I explain this by the slight variation
in conditions (temperature affecting friction etc) and I can express this variation as a percentage over a given number of pulls. In the above experiment, if the deviation of 1/100 was constant, I can say that it was 0.01. I might want to add that, given the minor changes in temperature, this was of no or little significance. Further, if I wish, I can subject the experiment to a 'validating' procedure by applying the two criteria outlined above. I can, that is, 'reproduce' the forces in a different scale and substance and try again. Providing I change my scale in a consistent way - e.g. tonnes for pounds and steel rope for twine - I can expect the mathematical relationships expressed to remain relatively constant. This would also be the case in a wider but less predictable system where a team of horses were expected to pull a tractor out of some mud. In principle, the forces - and the possibility - could be calculated before the horses ever start to pull.

Whether the 'paradigm' holds across intentional, social or psychological states, of course, is the crux of the present problem. For Abrahamson\textsuperscript{19} it must do so. The 'must' implied here however needs qualification: it 'must' if the mathematical analogue of human interaction is to be 'mapped'. If is pivotal however, for if the paradigm does not cross over then no such analogue is possible. The question thus becomes: 'Do ordinary purposive and intentional descriptions of human actions and states take descriptions which are
transferable to other 'substances' and are the interactions observed between them such that they will be transferable to and useful in other systems?'

One simple answer is that they obviously do not: that is why human scientists ask for such intention-alistic descriptions to be 'operationalized'. This raises further questions of synonomy which I shall look at in the next section. For the moment, let us deny the scientist the luxury of translation. The scenario the scientist is faced with is the measurement of certain aspects of the person or society without recourse to more obviously quantifiable 'equivalents'. This denies him or her the possibility of atomistic tendencies such as analysing a term such as personality into its 'constituents' such as extrovert - which is again 'analysed' as a willingness to lead groups, not minding what others think and so on. Notice that the last group are much more amenable to numerical qualification than the original. It is the original 'holistic' description which presents the problem, for while a willingness to lead is transferable across people, a personality is not: at least the idea causes some resistance. Why is this so - if a person is extrovert why is his personality not 'the same' as another extrovert? The answer seems obvious enough: extroverts vary in their extroversions. But, if this is so, two things seem to follow: (i) analysis, even in terms of 'typical' tendencies, seems hollow and (ii) transference of the original description is simply
wrong - there being simply too many 'types' of behaviour associated with each.

What then of units? The conclusion is that while it is feasible to say that a particular person takes the lead in eight out of ten situations and is therefore likely to take the lead in other situations it is not possible to quantify in a similar way for many of the ways we have of describing people. We cannot, for example, without semantic abuse assign numerical values straightforwardly to terms such as personality, character, courage, virtue and so on. This is also the case for social descriptions such as volatile, harmonious and so on. This is why there is a need for operational definitions. That need is indicative enough of the force of my point. What then of such 'definitions'?

2.23 Operational Definitions.

Having outlined some of the problems concerning the use of units we are now in a better position to see just how that problem leads into the field of 'operationalized' - or 'transferred' definitions.

It should be recalled that for Abrahamson it is a necessary condition of measuring the effect of one or more variable on another that they be quantified. I have suggested that this entails some sort of unit, it also presupposes that these are the same across substances and useful in some system. I have tried to show that for some
central descriptions of ourselves neither condition is met: personality, for example, cannot pass in an unqualified way across individuals nor is it useful to say, per impossible, that people with the 'same' personality will behave in predictably similar ways in the future. What is now left to show is that the attempt to make the project possible - by 'operationalizing' such descriptions - is nothing more than changing the topic: that is why some have said that intelligence tests measure what intelligence tests measure. Obviously the crux of this argument hinges on a semantic point. Before I go into that however let us see what researchers say on the topic. Best indicates that he sees it as a tool for making 'vague' variables observable. De Groot, in his more thorough exposition, expands this to spell out in fair detail exactly what this entails. I quote at length to give the full flavour of his thought:\[20\] whenever a concept or construct is to be used in an empirical investigation, a minimum of empirical specification is needed...this boundary line must be marked clearly enough to enable the investigator to discriminate - objectively, adequately and with sufficient reliability between A and non A; e.g. between boys and girls, intelligent and non intelligent children, social groups and collections of people not to be included in the construct group, democratic and non democratic forms of government etc. Frequently a graded scale will be drawn up to 'measure' intelligence quotient, price index etc. To these ends are needed one or more empirical specification statements providing an objective instruction on how to proceed in given empirical cases, so as to effect the distinction between A and non A, or between different scale values...the instruction then specifies the operations to be carried out...to determine the...'value' of the variable. Thus in a psychological investigation, for instance, the concept 'intelligence' is empirically specified by the set of instructions for the
operations of administering and scoring test X, calculating the IQ, and possibly classing the subject under 'high' or 'low' intelligence ...such a set of instructions defines the concept. A definition on this basis is called an operational definition.

There are a number of important aspects to this process. The philosopher has to keep in mind that while the end result of the experiment is supposed to relate two variables what has in fact been measured are their operationalized translations. In order to bring this out more clearly consider the actual operationalization of the word 'alienation' as carried out by Seemen. Abrahamson sees it as, 'a good example' of Factor Analysis. In the first step Seeman divides the concept into four 'subconcepts' viz:

1 Isolation from others.
2 Powerlessness in the face of world events.
3 Normlessness, or nonadherence to conventional procedures.
4 Meaninglessness, a sense of confusion and uncertainty.

Following this initial division of alienation a number of further steps are needed before it is observable in discrete items however. In the next step, Seeman says, ...it is necessary to develop items which will operationally define each of the subconcepts...the degree to which an item indicates the underlying dimension is expressed by a factor loading. (This) expresses the degree to which a factor, or dimension, explains the variance in that item. The final step...involves developing a composite index for each dimension.
These 'final steps' have been 'worked out' by Zellor, Neal and Groat,\textsuperscript{23} whose analysis of the 'normlessness' dimension is indicated by the positive replies given by candidates to the following statements. (Listed under Item).

<table>
<thead>
<tr>
<th>Item</th>
<th>Factor loading</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. In order to get elected ...a candidate must make promises he does not intend to keep.</td>
<td>0.63</td>
</tr>
<tr>
<td>2. Those running our government must hush up many things if they wish to stay in power.</td>
<td>0.51</td>
</tr>
<tr>
<td>3. Having 'pull' is more important than ability in getting a government job.</td>
<td>0.49</td>
</tr>
<tr>
<td>4. Those elected...have to serve special interests as well as the public's interest.</td>
<td>0.32</td>
</tr>
</tbody>
</table>

Now it is not my intention to get entangled in the problems of factor analysis itself. The point I wish to stress is the way in which a word, alienation, is taken, divided into four 'subconcepts', and then 'operationalised' into 'items' which are both observable and quantifiable. Further, they are 'weighted' against each other; number one, for example, carrying twice the 'weight' of number four.

We began by noting that equivalence of meaning must be preserved if the test or experiment is to say something about the 'real object system' which it measures in pre-operationalised and post-operationalised form if it is to conclude with as much confidence as J.Asher does when speaking of intelligence tests:\textsuperscript{24} 'Intelligence is what all valid intelligence tests measure.' The notion of validity hangs on equivalence of
meaning. Alienation therefore must mean in part that people are cynical about the way in which others get government jobs, it must mean that those who are not cynical about how government jobs are got (given its 'factor loading') are, to that degree, not alienated. In fact the complete meaning of 'alienation' must be exhausted in a complete list of the items constitutive of the four dimensions of it. Yet isolation and powerlessness need have nothing to do with alienation, whatever their operationalised 'items'. Irish monks and St. Augustine were 'isolated' as are Faroe islanders and astronauts. Yet Irish monks were not alienated, Faroe islanders not cynics, astronauts not without power and St Augustine not without norms. On the other hand this may be unfair; perhaps those who are alienated need all the subconcepts and items to qualify. So, for example, since monks may be isolated but not without norms they need not be alienated. Who then are alienated? The answer can only be: 'Those who satisfy the whole list of conditions.' They must be Cynical, Normless, Isolated and Powerless people who also find life meaningless. If one were to be discovered however, and this point is more serious, would they be described as alienated? I think not; seriously depressed perhaps but not necessarily alienated. A king may be alienated from his people by not caring for them, yet not lack power or friends. A worker, according to Marx, may be alienated yet not lack power, norms or comrades. Man may be alienated from himself, or God and meet none of the conditions laid
down. So, at least in one case, it seems that the condition of semantic equivalence is not at all met. The definition is merely stipulative and no guide as to who are alienated, nor to how they would act. Consequently the test is invalid.

Is this an isolated case? In order to make a more general point something else needs to be said about intelligence, personality, self esteem, character and so on to show that there are other reasons why these sorts of terms cannot, in principle be 'operationalised' in a way which preserves meaning. In order to look for this we need to outline two types of theories of meaning. We need not, for our purposes, show that one or the other is better. Two widely held theories are (a) that meaning is given by truth conditions which are verification transcendent and (b) that meaning is given in use. The first stresses context independence, the second, context dependence. On the grounds of the latter it is easy to see why a term cannot be operationalised in the required way. It lacks both context, use and, consequently, sense.

This seems to be the problem with alienation: the word takes a meaning in a context such as 'Black South Africans are alienated from their political rights by a repressive white regime.' (Yet they are not 'isolated', 'lack norms' nor do they find life meaningless.) Without such a context, words hang in a vacuum, no sense being attributable to them. The same is true of 'intelligence'. We say, 'He used his intelligence to solve the problem'
without inferring that 'Intelligence' is a thing, that it has quantifiable characteristics or that it is independent of contextual factors. And here, there is also a further problem: the reification of the object. This problem raises pertinent questions in the other theory of meaning mentioned: for according to this theory the word 'intelligence', to be meaningful, must be true in terms of its truth conditions. If, however, it is true that intelligence is what is measured by intelligence tests then intelligence is an ability - the ability to perform well at intelligence tests. What this shows in terms of truth conditions is just what the cynics say: intelligence tests measure what intelligence tests measure. If, on the other hand, intelligence is a structural feature of the neurological system then the ability to perform in intelligence tests must be shown to have the same truth conditions as that structural element. This identification between performances and states will take up a considerable part of the next chapter so I will postpone comment on it at this point.

There are, however, two points which need to be stressed in conclusion. The first is that the quantification of variables requires, because of intransigent features of many ordinary descriptions, a translation into descriptions which are more amenable to taking number predicates. Consequently whatever is measured, in the end, is not exactly what was of interest in the first place. Secondly, if what is measured is to
'correspond' to what there is in the 'real world system' it must be shown to be so by rigorous semantic argument and not, as is the case with the term alienation, merely concocted to allow the 'experiment' to take place.

2.30 Validation Procedures: Internal.

If there were no problems with the measurement of variables we might have been able to conclude that an experiment could be carried out. Even if this were so, however, scientists stipulate a further demand on the testing procedure. This extra element is usually referred to as validity testing. Normally there are three forms of it.

I start with the first: Internal Validity. This, according to Best\textsuperscript{25}, is achieved when, 'the factors that have actually been modified actually have a systematic effect...and...when all extraneous factors are excluded.' In other words when it has been concluded that it is the independent variable that systematically affects the dependent, and not some other cause. I want to ask a simple sounding question concerning the possibility of altering the independent variable while all other 'extraneous' variables which might otherwise affect the dependent are excluded. I shall argue that it cannot be isolated from its context in the way required and that therefore internal validity is at most an unobtainable goal limited to all but the simplest
experiments. The reason for this is to be found in the nature of the variables themselves. We are not dealing with inanimate objects but human beings. Variables include people's personality, environment, political views, class, intelligence, self-esteem, ability to cope, teaching technique, classroom efficiency, race, character, gender, self-view, gender and so on. The experiment demands that one of these, or a combination, if we believe in factor analysis, is isolated from other possibly influential variables so that it alone accounts for a change which is systematically observed in the dependent. This brings up the problem of repeatability, which we will examine later. There are, I contend, problems related to the nature of the 'extraneous' itself, for everything which may be called historical context might arguably be included. The problem then can be stated like this: to what degree is it possible to exclude factors 'external' to variables under experiment so that we are in a position to state categorically that they are not operating as extraneous factors? A few examples should suffice to show the enormity of the problem. If, for example, what is being looked at are the effects of a particular type of teaching method on children's attitudes or learning, how are such factors as particular teachers personalities, the social and hereditary background of the teaching group, the weather, the previous relationships and experiences of the class and so on, to be held either
constant or in such a state that they have no 'effect'? In this type of situation the real rift between the human and natural sciences is at its most clear. The physicist does not need to worry about the feelings of molecules towards the tin can that contains them: they hit it all the same. Human 'molecules', however, may take certain 'extraneous' parts of their context as relevant and react to it in a quite unpredictable way. 'Unpredictable', however, not in a sense which may be allowed for by making 'technical' adjustments but in another, far more difficult sense; a sense in which it is always undecidable what aspects of a context will, on any given occasion, be relevant to what people do. A child might succeed in a class using a particular teaching method because he liked the teacher. 'Liking a teacher' thus becomes an extraneous factor. What must the researcher do to ensure that the observed effects are systematically related to the teaching method? In this case, it seems, he must control the likes and dislikes of the children under observation. To do this he must first find out why they did well. Then, if he finds that one did so 'because I liked the teacher' he must exclude the child. There is something logically fishy about this however, for only after 'doing well' is the child isolated. In order to counter this the researcher must exclude all children from a proposed experiment who are going to be influenced by factors other than the teaching method. These might include whether they will have a cold, whether they are
going to like the teacher and so on. In other words a condition of excluding extraneous variables and ensuring internal validity of an experiment is that prior to the test situation the extraneous variables are identified and excluded. A further condition, which involves the procedure by which such extraneous variables are identified, makes the situation more complicated, however, for in order to know what affected the children in the experiment, the experiment must have taken place. A presupposition of internal validity is thus either foreknowledge, which is, at best, difficult to obtain, or time reversal which is, at present, impossible.

2.31 External Validity.

Having established internal validity, the scientist must, Best insists, 'also determine whether the systematic relationships that have been identified, isolated and measured can be generalized - used to predict relationships outside the experimental setting. The extent to which this goal is attained is a measure of the external validity.' If we assume that some degree of correlation has been found and that an experiment is internally valid we are further exhorted to test the validity 'outside the experimental setting.' A simple example of this sort of procedure in an educational context would be one in which a particular teaching method had been found to produce high levels of
achievement and was subsequently tested in more schools to see its more general application. External validity in this context would be satisfied if the higher levels of achievement were reached in the other schools. In this way the method can be used to predict future performance to within certain limits. It will be noted that we have now moved away from a strict experimental situation to one in which all the parameters have been loosened. Gone are the controls on external factors, gone too the careful manipulation of the variables. Thus, to an extent, no real comparison is possible. The 'outside' does not, or need not, resemble the experimental situation at all as is implied by the notion of 'generality' envisaged. Validity in this context is therefore I suggest a very weak term carrying little, if any, weight at all and doing very little work as far as the experiment is concerned. Any teacher knows, for example, that one method may work with one class and not another. Spread evenly across different classes, therefore, one can expect a certain amount of improvement in some classes, none at all in others and a backward step in others. One might wish to translate these findings mathematically and calculate some increase or decrease against some standard, but I am not sure what it would show. Scientists themselves have voiced these doubts as well as others. De Groot cites personnel selection as an example. He says, 27 'If it is attempted to solve this problem (of selecting suitable personnel)
by advance personnel selection, a valid predictor must be found, e.g., an aptitude test.' The problem however is with the 'criterion of validity'. De Groot gives an example of the problem. Briefly, it involves an independent test to validate the accuracy of Carbon 14 dating. The independent criterion is the age as given by historical experts who have agreed upon an exact date for a particular object. The question thus becomes how well the C14 method will agree. It is supposed that it agrees, but, De Groot asks, with what has it agreed? It cannot be assumed that the historians who set the criterion are correct. Indeed, as is in fact the case, the accepted criterion today is the C14 test. What 'validates' on one occasion may, at some later date, be the subject of validation. This reversal, De Groot reports, is a common phenomenon as in the case of intelligence and neuroticism tests. In the same way the outcome of a prediction needs an external test. In the case of personnel selection, perhaps, these would be the report of colleagues, managers and so on. However, the criteria they set would parallel that of the historical experts, their ability to judge being limited to a certain amount of time. There is a sense therefore in which external validity is self-validating or self-invalidating since both standards of validity internal to it and external to it are no better and no worse than each other. There is therefore no 'independent' standard of external validity.
2.32 Repeatability as a condition of validity.

De Groot, says that 'empirically, one can determine the accuracy of a measurement only by repeating it a number of times.'

This brings problems of its own however: mostly turning on the problematical way, absent in the physical sciences, in which experiments can be said to have been repeated at all. The notion of repeatable, for example, entails the sameness of that which is to be repeated. Similarly the repeats must occur in the same conditions. In either case the human scientist faces philosophical problems. The first concerns the identity criteria by which two or more human events are said to be the same. The second, points at the possibility of historical conditions being the same. To take the first. Actions, even those recorded under experimental conditions, are governed by criteria which are either not available to the scientist or part of the 'environment' in which it is performed. In neither case (in the former I am thinking of intentions which, in the absence of a means of checking, are always liable to some degree of interpretation) is all the necessary data unambiguously observable.

In the second place actions which have identical physical movements may be quite different given the indefinite ways in which contexts and intentions enter into their descriptions. If the scientist observes A at
time t doing x, and again at time t_i, how does he know whether the conditions are the same? The quick answer is that he does not know unless they are either specified or controlled. But by what criteria, other than an arbitrary one can he specify, from the total historical context, which particular circumstances are relevant to A's act? By what manipulation does he control the historical circumstances? Does the observer presuppose a knowledge of which historical conditions are pertinent for each individual in the experiment, and if not how are the conditions to be held constant known to be conditions? In the absence of such knowledge or such control how are conditions supposed to be held constant?

These points are not meant to be conclusive: nor will they be taken to be. I will not continue with them at this juncture, however, because they receive greater attention in the following chapter. The points made there, however, may be seen as pertinent to this aspect of validation.

2.40

Engineering as a paradigm of applied human science.

In the final section of the present chapter I hope to indicate some of the difficulties which are inherent in the second aspect of the scientistic enterprise which hopes that some of the 'knowledge' gained through
experimentation is useful in 'guiding systems' - or practice as we might prefer. While this aspect might be treated by itself it is interesting to note that it is very similar indeed to 'external validation' in the sense that what is presumed to have been gained epistemologically from the experiment is actuated in other contexts. The main difference is merely the context of testing. In engineering the 'tests' have presumably been completed satisfactorily: what is left is to control the environment on the basis of the findings.

Since I think it would be a nonsense to use a word like 'engineering' and then talk of some way of systematically 'mapping' experimental findings onto a real world which bore no relation to the systems developed within the natural applications of 'engineering', I will assume that these are what is intended by authors such as O'Connor and Popper. Because of this I shall start (2.41) by a consideration of a purely, I hope, physical type of engineering problem. I shall then, as with my use of vector analysis, above, attempt to see whether such a system is useful or applicable in the human contexts one would find in educational situations. Before that, let us remind ourselves, briefly, of what O'Connor recommends.

O'Connor's sentiments and expectations of engineering (see 1.11) comes close to that expressed by R.J. Smith, quoted from Asher's work on educational research.29
Education's purposes are comparable, particularly in research functions, to other applied fields such as engineering. According to Smith 'Engineering is the professional art of applying science to the efficient conversion of natural resources to the benefit of man.' Educational research is the professional art of applying science to the efficient use of man's inner resources as well as for his educational benefit.

Asher suggests, for example, that, 'there is no reason why a satisfactory scientific background to education should not enable us to bring about those educational outcomes which are accepted as desirable in a given community.' An optimism which he thinks is supported by such factors as the **undoubted fact** that 'the growing points of medical knowledge lie largely in pure science...'. There are no factors, apparently, to give rise to any suspicion that their might be reasons why this analogue might prove inappropriate. On the contrary, the idea is grasped as a solution - by O'Connor - to an inefficient use of resources for since, 'the practice of education may be compared with the practice of medicine or engineering...(and)...engineering consists almost entirely of the use of scientific knowledge in determining efficient means to agreed ends.' So educational ends, set by the community are most appropriately and efficiently achieved by applied educational theory conceived as science. I argue in the next section that the analogy does not hold and the optimism is misplaced.
Let us look at the notion of engineering contained in O'Connor's account of applied science. Imagine the following situation. A crane has to lift a 40 tonne piece of concrete and place it at the bottom of a deep hole to act as the foundation for a tower. The crane can lift 80 tonnes vertically, but when the 'arm' is at 45 degrees, it can only lift 25 tonnes. The piece of concrete we have needs to be lowered into a hole twenty yards from the crane. The question an engineer is faced with is whether the crane will topple as the arm is lowered, a question which entails calculating the degree to which the arm needs to be lowered in order to drop the concrete twenty yards away. Without performing the calculation, let us briefly follow the steps required. Firstly there are two parameters, (a) maximum vertical lift (i.e 0 degrees) = 80 tonnes, (b) Maximum lift at 45 degrees = 25 tonnes. Somewhere between these two is the maximum angle for a weight of 40 tonnes. The question is the angle demanded by twenty yards. This can easily be worked out knowing the height of the crane, the top of which follows an arc as it moves out. Let us say that it comes to 30 degrees. The next question is therefore what is the maximum lift at 30 degrees. This can be calculated using a graph in which the x axis records the angle, while the y axis records the maximum weight.
The graph shows the approximate points between 0 degrees of crane 'outswing' and 80 degrees at which the crane will overbalance. It shows that when the crane swings out to 30 degrees the maximum weight it will carry is in the region of 32 tonnes. Thus it will not carry 40 tonnes at the required angle.

In performing this small exercise - which is not important in itself - I hope to gather together some of the principles of mechanical engineering. Firstly, the whole idea of transporting a piece of concrete to a hole twenty yards away using a crane was 'mathemcized'. In order to do this we needed certain pieces of information about the crane: its height, and its weight carrying characteristics. We also needed to know what was being asked of the crane, and then we were able to see if this came within the limits of the cranes possibilities. We found that it did not and make the practical conclusion that this crane will not do the job. In a practical situation what we have shown will be enough information to let an engineer know that he needs to order a larger crane.
O'Connor sees the relationship between scientific research and educational practice in terms of engineering. Let us therefore see what this entails, (a) in our example and (b) in the human context.

(a) I shall ask what are the necessary conditions which enable the engineer to conclude that he needs a larger crane. Put more simply, how does he know that the crane will fall over if it carries 40 tonnes to a distance of twenty yards from the crane. One answer, is that he knows because he has just confirmed our calculations. What therefore, must be the case, (i) in order for mathematicization of a system to occur and (ii) for the analogue to act as a predictor in a real system? Taking (i) first, let us consider some situations in which it would not — in order to see what differential criteria are involved. To begin, let us imagine that the system of weights for cranes was not that for concrete. Twenty tonnes for a crane was not equal to twenty tonnes of concrete nor is there any way of directly translating one system into the other because the weight of concrete lumps was decided by a throwing contest by giants. The ones which the giants threw furthest were the lightest, and those they could not throw far, the heaviest. There were, however, different sized giants and this has no effect on the weighting system, even though some can throw much further than others. Given this, our engineer could not make a mathematical analogue of the problem. This could be characterized as a problem of
variable units. Secondly we might enquire about the material out of which the crane was constructed. Suppose that because of a completely unknown property of the material, its breaking point varied unsystematically so that on one day it could support virtually any weight, while on another, it snapped quite easily. Again, our engineer would be at a loss. This could be characterized as the problem of variable material. Suppose, lastly, that a general law which normally held was given to sudden an unexplained lapses. Take the law, for example, of angular momentum. As a mass moves further from its pivot its potential angular momentum increases:

\[
\begin{array}{c}
\text{pivot} \\
\text{0} \quad 10 \quad 20 \quad 30 \text{ feet} \\
\text{\_\_\_} \quad \text{xgm} \quad \text{xgm} \quad \text{xgm}
\end{array}
\]

So that, for example, at 10 feet, \( x \) grammes has a potential angular momentum of 20 units. At 20 feet this increases to 40 units and at 30 feet to 80 units. This law is presupposed by the mathematics we used earlier. But, in our imaginary situation, it holds according to a random feature which we cannot explain. Here again, the engineer's attempts to mathematicize are thwarted. This could be characterized as the problem of undependable laws. In asking for circumstances under which a mathematical analogue could not be made we have provided conditions necessary to its holding. Briefly, invariable units, predictable material and dependable laws.

We shall, I think, touch on all three of these 'unpredictable' aspects in the next chapter and so I
will not labour the consequences they have for human science at this juncture. I shall, instead, content myself with a few, perhaps, introductory thoughts on their implications for the use of engineering as a means of policy implementation.

(b) Since O'Connor sees engineering as the paradigm method of implementing educational policies those methods must in some sense parallel our example of the crane, and, further, they must have similar necessary conditions. It will be my contention that these conditions cannot be met and that therefore the notion of engineering, even if there were no problem with the sciences which he mentions, is inadequate to the task he sets for it: the formulation of practical policies and an efficient means of establishing them.

We could leave the onus on those wishing to support this view to provide grounds on which it is acceptable to utilize the idea of engineering to human situations. There are, however, a number of points to be made before this. Firstly any defence must entail a dehumanized anthropology since it must consist in showing that humans act in ways which are invariable to a degree which allows for specifiable and quantifiable units (we have already questioned these) to be allocated. Yet this seems to fly in the face of experience - it being commonly accepted, for example, that toleration to pain varies considerably; factors affecting motivation vary
from individual to individual; what counts as an achievement for one need not be for another and so on. All this points to the absence of any parallel means of allocating units between the human and the natural sciences. This is because of the problem of variable units. The same applies to the material, which unlike the steel of the crane is not given to precise description whether in terms of flexibility, breaking point or structural characteristics. When our imaginary engineer calculated the weight the crane would carry, it was not part of his assumptions that it might not. It either, because of our knowledge of steel, will, or will not carry a certain weight. A hint of bivalency informs our expectations of the natural world. No such hint is apparent in our expectations of each other, however. Such assertions as, 'He can lift 160 kilogrammes but he cannot lift 180 kilogrammes' are usually 'softened' by comments such as, 'Well, not in his present form' or, 'At least not until he puts his mind to it'. The 'material' of the human being simply does not have the analogous nature it must have in order to make the notion of engineering plausible. Least of all when it comes to the laws which must be assumed in order for mathematization to be carried out. Since most human scientists argue that the basis of research is empirical, it is for them to bring forward one law of human behaviour under which another action may be subsumed and hence explained. The
empiricist demand has not been met by empiricists and consequently the third condition of engineering in not met. Nor is it ever likely to be met, for such a law must hold counterfactually. While this condition holds, any general statement concerning human behaviour which purports to have the status of a law must prove itself to hold, given the circumstances, in any possible world. That any law holds in any possible world is a metaphysical thesis. Of the scientists who have tried to show that the laws of physics hold throughout the universe only Einstein seems to have come close to a theory in which they do. Yet even he agrees that in the 'singularity' it would be quite impossible to give a definite answer to the question as to whether the laws of physics still hold.

Far from the physics of the singularity, however, lawlike human behaviour seems to give out in rather more mundane situations such as sensory deprivation, extreme pain, anxiety, stress, love, fear and so on. In case the rejoinder is that these cases can be cited, proves the opposite case, let me add that just where, on the scale, people go mad, give in, commit suicide, fall head over heels, run away or take arms and fight against a sea of troubles cannot be stated. Nor, as O'Connor wrongly assumes, can they be predicted, for a condition of prediction is that certain features remain constant and it is just those features which resemble the weights
assigned by a nation of giants quantifying concrete in a throwing competition.
Chapter Two: Footnotes.

(1) The common theme is that they are all introductory texts to research methods in the Social/Human/Educational Sciences.

(2) A.D.De Groot, 'Methodology', Hague 1969. p82.


(7) Best, op cit, p27.


(10) Abrahamson, op cit, p55.

(11) ibid, p55.

(12) ibid, p56.

(13) De Groot, op cit, p212.


(18) This is not to say that their will be no disagreement over the numbers involved and unemployment is a good example. This discrepancy however comes at a later stage where the ways of counting, the interpretation of statistical information and disagreements over operationalized definitions arise.


(20) De Groot, op cit, p84-5.

(21) Of Factor Analysis, De Groot says: (p308) 'the factor analyst's hope is mostly that they (emerging
variables) can be seen as representing constructs of a causal or fundamentally structural nature'. Best (p92) explains that, 'factor analysis is used for dealing with more than one variable and is associated with R.A. Fisher whose concept of achieving pre-experimental equations of conditions...and his concepts of analysis of variance and...covariance...made possible the study of complex interactions through factorial designs in which the influence of more than one independent variable upon more than one dependent variable could be observed.' See also Ando, Fisher and Simon. 'Essays on the structure of social science models', MIT, 1963.


(25) Best, op cit, p92.

(26) ibid, p93.


(28) ibid, p246.

(29) Asher, op cit, p7.

(30) ibid, p7.

(31) See 1.11

(32) The 'Singularity' is a term of art used by cosmologists to refer to the theoretical single object which pre-existed the big bang. The problem it poses for physics concerns whether or not the 'laws' of physics operating in the present universe were the same as those in the singularity.
3.0 The Attempt to Establish Nomological Laws.

The Positivist typically claims to establish connections of a causal type which constantly conjoin a set of antecedent conditions with another set of events which, when experimental data has confirmed the constancy of the conjunctions, are said to be effects of the former. When experimental findings are such that a firm relation between the antecedent set and the set said to be the effect is established then their relationship is said to be governed by a law. The verification (or falsification) of such laws (framed initially as hypotheses) are enough (following Hume) to satisfy the positivist in as far as causality is concerned. (Though attempts are made to fit newly discovered laws with other ones to form a theory of events which is explanatory of
Although I have outlined the methodological types generally recommended for research in education I have not, as yet, said much about the possibility of validating laws of human behaviour which the positivist expects to discover. Since this is a very large topic in itself, and since it is implicitly central to the thesis we are opposing (the thesis which D.J.O'Connor put forward as outlined in chapter one) I have devoted the third chapter to an examination, far from adequate, of the issues raised by the notion that one can subsume human activity under laws. My main concerns will not be with the notion of law itself - for I am going along with the positivists in their acceptance of Hume's formulation outlined in the first paragraph - but with the possibility of formulating a hypothesis which when tested under the experimental conditions prescribed in Chapter Two has a chance of verifying a law.

The central theme of the present chapter is how might a (hypothetical) educational scientist defend the notion of a hypothesis which contains the assumption that there is a causal relationship between two sets of human events? I shall assume that whatever sentence formulates the hypothesis, the hypothesis is testable and that a condition of testability is that some observable event(s)/change(s) in events occur. It will also be seen that on the whole I shall deal with arguments which would normally fall within the province of psychology. This
restriction is purely (again) one of space. I do not think that sociology or economics escape the kind of arguments I shall be putting forward - the multiplicity of individuals with which they are concerned tend to exacerbate rather than ameliorate the difficulties. This is especially true where the 'unity of science' programme attempts to 'reduce' the social sciences to the psychological - and these to the physical.

The chapter attempts to present arguments to show that whatever defence the hypothetical educational scientist might make that these are either fraught with difficulties which need to be met or that they are unsustainable. As stated earlier, the positivist views sets of non-accidentally conjoined events as constituting laws. These laws are causal in nature and are said to obtain between discrete, contingently related sets of events. This appears to give the scientist four options in setting out his hypothesis. He might attempt to formulate it in terms of (i) Mental events causing mental events (ii) Mental events causing physical events (iii) Physical events causing mental events and (iv) Physical events causing physical events.

In order to keep the chapter as brief and to the point as much as possible I shall not discuss what is meant by event or whether the categories 'mental' and 'physical' are the most appropriate. This is simply pragmatic because of the potential proliferation of problems inherent in these areas. Nor shall I discuss (i)
and (iii): (i) because it has not played a significant part in educational research and in areas where it has (as, for example where one thought causes another and leads to a chain of thoughts) it is uncontroversial; (iii) since it is generally agreed that physical things such as drugs, do causally effect mental events and that they do so in various ways from LSD hallucinations to concussion after a fall. I freely admit that there are borderline cases such as the connection between lead poisoning and academic performance or a lack of vitamins and concentration. Research in these areas seems to be possible although rendered difficult because of the intentional features encapsulated in the mental effects.

Even omitting these, the remaining two relationships generate a surprisingly wide variety of problems and positions. I shall try therefore to tie down the number of possible moves open to the advocate of positive educational research in as simple a way as possible. Basically there are two broad options: the phenomenalist and the materialist. The phenomenalist typically tries to keep the 'intentional' features of the mental and separates actions from behaviour or movements. The materialist typically attempts to do away with intentionality (I use the term in the tradition following Brentano, Chisholm, Quine and others) and reduce actions to movements. It does not follow, however, that the phenomenalist is anti-positivist or that the materialist is pro-positivist; indeed A.J.Ayer, a phenomenalist is
also a positivist while the materialists McGinn and Davidson are against the possibility of anything resembling positive psychology - and that would include educational research along those lines. Other materialists however, especially the behaviourists such as Hull, Watson, Pavlov and Skinner do believe in the usefulness of positive science as an aid to various social and psychological problems as do the structuralists such as Marx, Dennett, Harré, Chomsky, Piaget and Fodor.

Space also demands that I narrow my focus as far as possible on that form of science which I outlined in the second chapter. For various reasons this excludes the Realists ¹ who oppose positivism in following Hume's idea of causality and do not formulate hypotheses in quite the same way preferring to talk in terms of 'analogues', 'models', 'necessities', 'structures', 'mechanisms' and the like.

Restricting myself thus to the type of methodologies put forward in the previous chapter the field is still enormously broad. Consequently I have tried to keep to the more 'classical' moves our hypothetical educational scientist might make if he is to sustain and defend the type of research he recommends. As in the second chapter I shall assign section numbers and provide a brief indication of the broad movement of the chapter through a summary of their contents. These are as follows:
3.1 The general scheme espoused by positivists.
3.2 A.J. Ayer's attempt to provide grounds for the possibility of a Human Science from a phenomenalist's point of view.
3.3 The rejection of the possibility of psychology from a materialist's point of view. Argument 1 - reasons are causes but no laws can be deduced from mental phenomena.
3.4 Davidson's argument against the possibility of psychology on the grounds that mental and physical predicates are such they would not allow for the formation of psycho-physical laws.
3.5 Conclusions.
3.6 Reductionist Strategies: 1.
   The possibility of an all embracing extensionalist logic in which 'intentional' descriptions are translated into extensional equivalents.
3.7 Reductionist strategies: 2.
   'Molar' behaviourism in which the mind (and with it intentional states) is denied and actions are reduced to physical movements. I include Rylean 'logical behaviourism' as a variant from the more standard forms.
3.8 Reductionist strategies: 3.
   The Central State Identity Thesis.
In accord with the general scheme of the chapter, 3.2 to 3.5 deal with theories which allow for a causal relationship between the mental and the physical while 3.6 to 3.8 deal with theories which allow for a causal relationship between the physical and the physical. Before looking at Ayer's arguments that the mental can cause human actions and that laws can be derived from this (in principle) I want to make a brief comment on the place this sort of work has in the overall positivist scheme. This is an important comment in the context of O'Connor's remarks about a 'theory' of education in that 'theory' - at this level - is, for the positivist, derived from a consideration of a number of established laws. The importance of this, as I see it, is that educational science is still in the position (from the positivist's own perspective) of still having to establish its first law and, in consequence, cannot, on its own terms talk of educational theory.

3.1 The Positivist Scheme.

C.G Hempel\(^2\) says of explanations that they, ...

...may be conceived...as deductive arguments whose conclusion is the explanandum sentence, E, and whose premiss-set, the explanans, consists of general laws, \(L_1, L_2, \ldots, L_r\) and of other statements, \(C_1, C_2, \ldots, C_k\), which make assertions about particular facts. The form of such arguments, which thus constitute one type of scientific explanation, can be represented by the following schema:
Explanatory accounts of this kind will be called explanations by deductive subsumption under general laws, or deductive-nomological explanations. The laws invoked in a scientific explanation will also be called covering laws for the explanandum phenomenon, and the explanatory argument will be said to subsume the explanation under those laws.

Here is a rather famous statement of the method of the physical sciences. There will not be universal assent to this view, but it is commonly accepted as a paradigm of research methodology in 'normal' science. Its ancestry takes us back to Aristotle who, in his analysis of science (quoted in the introduction to chapter one) tells us that scientific knowledge involves us in Induction 'which introduces us to first principles and to universals' and to deduction which 'starts from universals.' As indicated, I feel that it is important to see what follows in the context of the whole scheme of positive science, if only to see that for the positivist nothing of educational significance has so far been explained in the sense in which 'explanation' is equated with an 'explanandum' sentence.

The first alternative which the educational scientist might resort to in this context is that mental events cause physical events such as human actions and that from the observation of these it is, in principle, possible to discover the laws under which they are subsumed. A.J. Ayer presents a clear defence of this view.
3.2 A Phenomenalist defence of Human Science.

A.J. Ayer's paper offers a clearly stated and unapologetic argument attempting to show that general laws of human behaviour are at least a possibility and as such allow for the possibility of the deductive-nomological scheme as a method of explaining that behaviour. We shall therefore cast a critical eye over this defence with a view to seeing whether it really holds up.

Ayer starts with the assumption that the methodology developed in the history of the physical sciences is appropriate to the study of human beings. Indeed, he asserts that science of either (any) type is to do with the discovery of empirical regularities which then form covering laws through the application of inductive techniques. Events, including human actions, which can be demonstrably shown to be subsumed under those laws are thereby said to be explained.

Even if we leave aside the spurious assumptions (see footnote 5) built into such a view we still have grounds for questioning the possibility of such a scheme.

Ayer's own argument is that given a Humean view of causality, actions are related to their antecedent conditions, (which he isolates as motives) in much the same way that events are related to effects in the natural world. Assuming therefore, what is already
controversial, that motives and the actions they are said to bring about can be independently and correctly described, he argues that the fact of a motive caused the fact of an action. His peculiar use of 'fact' is apparently necessary he says because on an orthodox reading of Hume the argument fails. Leaving aside the obscure question of how something which is not a state of affairs can be said to be in any sense the cause of something the argument still leaves much unanswered.

First Ayer uses the only available criterion he has to identify the efficient motive, namely the overt behaviour. This surely is unwarranted not only because of the philosophical assumptions in making such a link but also because of the necessity of accepting that behaviour is an adequate criterion of the motive. There are good reasons for rejecting the second of these. First it certainly has not been shown that the overt behaviour is a correct indication of the motive. Second, motives often have no outward counterpart at all as when I am afraid to do what I am motivated to do or when I lack a sufficient skill to perform. Third the meaning of a piece of behaviour is given, not only by the 'inner' cause but by the outward circumstances of its performance: I may have a motive to do something but when I do that precise thing I am told that I have done something quite different. An example might be when a chess novice wishes to mate and moves his Queen only to find that he has lost his Queen. Fourth, human nature being what it is, we often act in a
contrary fashion: we do not always put on a coat to keep warm so 'wanting to keep warm' (one of Ayer's examples) is not a sufficient motive for putting on a coat. We often act 'in spite of ourselves' and so our actions, while identifiable themselves are not always adequate to identify our motives. Ayer's argument, if it is to hold up, must show however not only the action to which a motive gives rise (say putting on a coat) but must specify how this motive is independently identified. If this is in behavioural terms, as Ayer suggests it should be, it cannot be in terms of the action it allegedly brings about. But, if I want to keep warm, and this brings about my putting on a coat, what other behaviour is manifest upon which I correctly identify my motive? Perhaps Ayer is thinking of my shivering or my grumbling about the temperature. If so how do we legitimately deduce my motive 'want to be warm' from my behaviour without making other assumptions about human beings such as when they shiver they want to be warm etc? One only has to look at human actions 'out of context' (i.e. without any knowledge of what happens before or after) to see how obstinately difficult it is to correctly ascribe motives, intentions or whatever one likes to the person one observes.

Unperturbed, Ayer presses on to give a rough indication of how laws may be established given such regularities as may be observed between independently identified motive-events and action-effects. His
suggestion I paraphrase as follows: A motive is sufficient for the performance of an action if, and only if the actor holds that that action (call it A) is the only method for bringing about a desired state of affairs S where there is no other state of affairs Si - Sn desired in preference to S. Ayer makes little claim for this however for he admits (i) that listing all the necessary conditions for the performance of A is problematical, (ii) that no examples of strictly regular and lawlike actions can be found and (iii) that even the humble hypothesis put, which is merely intended to show that the actor will always perform A, contains a stipulation which in practice cannot be met. (Which is that there is usually a list of alternative actions ai - an which provide the actor with alternative means of achieving S. (The choice is made non arbitrary on the account by adding further conditions, which, while making the process even more impractical, nevertheless do not hinder the principle.) We might feel as if, given all these admissions as riders that what is left is a fairly harmless piece of rhetoric on the art of the possible - for in the end that is all Ayer claims for it. He declares that, for all its practical difficulties it allows for the possibility of a human science based on the same principles as those outlined by Hempel - and that that is all that needs to be shown. Ayer may indeed be right: but since we all continue to act in spite of ourselves and since we all do daft things and take
the wrong course of action we may rest assured that what may indeed remain a perpetual possibility does so only in a world of speculation. Speculation, however, is hardly the hard rock upon which our hypothetical educational scientist is likely to find solutions to pressing and very practical problems.

3.3 Reasons, Causes and Laws.

The educational scientist might take a second route together with partners who also argue that reasons are the causes of actions, thinking that they might provide a more satisfactory basis for the laws he seeks. If he does so, however, he will find that for one of two reasons his presumed allies turn out to be foes. In either case he will find himself being drawn towards a materialist thesis where what appeared to be reasons or motives in the fully intentionalistic sense Ayer uses them turn out to be nothing more than physics under a different description. This, at least as far as Colin McGinn and Donald Davidson are concerned, is not the beginnings of psychology but its very death knell. McGinn and Davidson, however, come to the same conclusions but for different reasons.

McGinn sets out to show that reasons can be explanations of actions but, unlike Ayer, is forced to the conclusion that this does not mean that they can be
used to form the basis of a human science.

Actions might have intentions, he asks, but does this require that they are necessary? We can, as we know, pour Vodka down the drain, thinking it to be water, and perform an unintentional action but this does not entail that the action was unintentional under one of its descriptions. He argues then that actions are intentional under some description. Intentions are then analysed as a 'reason-for' which is further construed as a combination of a desire (comparable to Davidsons 'pro attitude'\(^8\)) and a belief. The action itself being the result of a piece of practical reasoning on the part of the agent in which the means (which must be believable - as possible) are worked out by which the end (the desire) is best attained.

The nub of the argument is reached in the second section where the 'intentional' features of actions - including reasons - are said to be 'rational causes'\(^9\) but that they (reasons) cannot causally explain actions.\(^10\) The reason given is that the concept of causation is extensional and hence true under all its descriptions while that of explanation is not, it being intensional and true in virtue of some descriptions only. For a reason to be fully causally explanatory both cause and explanation would have to be extensional. As he explains\(^11\)
...just as events are explained by other events only under some of their descriptions, so they instantiate laws only as described in certain ways...What is notable here is that reasons explain actions under descriptions which do not bring them under laws. Nevertheless, they must fall under laws as described in some vocabulary or other...by elimination it seems that the only kind of law this could be would be a physical law, they must be physically describable, i.e., they must be physical events.

This leaves our educational scientist in something of a quandary for, if he wishes to work with the ordinary descriptions we give of ourselves and continue to carry out research in the vocabulary of the teacher, parent and child then he is being told that this will lead to a dead end as far as his project to uncover laws is concerned. Whether intentional talk or description is indeed irreducible to extensional logic I discuss in 3.6. Perhaps, however, he sees hope in preserving his science by following the inexorable path towards physicalism. If he does so, however, McGinn has this warning:

...the move to antecedent causally operative brain states as the explanatory correlates of reasons requires a radical redescription of the event to be explained... since these are quite new descriptions to which action descriptions are irreducible (vide infra), such a move is open to the charge of simply changing the subject.

This, as might be expected, is not universally accepted and I explore the possibilities of taking this route under section 3.8.

There does, however, seem to be one further possible option open before the educational scientist is forced to either examine the compatibility of intentionality with extensional descriptions or the possible option of going for doing away with the
troublesome intentional features altogether. This is to return to the 'rationality' of the subject and attempt to salvage some universal features at this level. This is indeed the path taken by Hempel (despite Ayer's reservations about alternative strategies) in 'Rational Action'\textsuperscript{13}. Here Hempel tries to subsume individual desires (together with the necessary beliefs) under a law by the additional condition that the agent, at the time of acting be in a rational frame of mind. This results in some such formulation as: Any so and so having such and such a desire and the belief that they have the means to achieve that desire will, if they are rational, do so and so. Does the addition of the condition, 'if they are rational' help the researcher avoid the pitfalls inherent in Ayer's account? I think not and for a number of reasons. First we are not rational all the time and Hempel is trying to account for all, not some human behaviour. Second, philosophers, who have a professional interest in saying just what rationality is have come to no consensus over the matter. Third because, in an extraordinary way, the end result of such a procedure would be that a so called 'law' of human activity would covertly prescribe what rationality meant.

One way out would be to empirically discover how human beings actually come to make decisions and then build the findings into the hypothesis. There is a great
irony (at least, as far as Donald Davidson is concerned) in this for it was his (empirical) work into just such a theory that led him to give up psychology as an empirical science. He recounts that the research he was engaged in attempted to overcome simplistic notions of rationality by 'preferencing' desires in a weighted system. The result, 'Decision Theory', attempted to systematically predict which action would be taken given probable preferences. He notes that apart from its inability to do so, some interesting philosophical questions arose around the area of preference. One of the most interesting was that in practice people's preferences changed in 'irrational' ways. They changed, for example, in different contexts: a wager to win being a higher probability after a series of wins (whereas, in fact, the odds remain the same), and so on. To make the 'theory' more reliable, extra information would have to be added, for example, which individuals tended to bet more and more after a series of wins, and which would leave with their winnings. What does this show? Davidson's conclusions are that the more information we have about an individual's desires and beliefs, the more we are able to predict his actions, but not those of others. The moral: 14
...no single action can prove that a disposition like a desire or a belief exists; desires and beliefs, however short lived, cannot be momentary, which is why we typically learn so much from knowing about the desires and beliefs of an agent. This is the point, I suggest, where general knowledge of how persistent various preferences and beliefs are apt to be, and what causes them to grow, alter and decay...Hempel set out to show that reason explanations do not differ in their general logical character from explanation in physics or elsewhere. My reflections reinforce this view. On one point I am not persuaded, however: the laws that are implicit in reason explanation seem to me to concern only individuals - they are generalizations embedded in attributions of attitudes, beliefs, and traits.

If our educational researcher is really determined to carry on with this line of thought and attempt a formulation which is true of all individuals then further to a study of how individuals make decisions he must add all the conditions which explain how and why individuals vary. Whether this is possible, in Ayer's sense, I do not know but it seems on the surface to be the sort of undertaking which could never be achieved in practice and consequently of no value to pressing educational problems.

3.4 Davidson and the possibility of Psycho-Physical Laws.

If such a course of action seems too impracticable the researcher has few options left. Materialism, in one of its forms, is a possibility, but, if the researcher is determined to retain intentional features in his hypotheses and insist that laws can somehow be made of them then he is faced with the problem of cross categorial laws. If he does then he might wish to
consider Davidson's arguments\textsuperscript{15} for the coherence of three principles. To hold out for the possibility of some form of human science along these lines he would have to support the first two of the principles and deny the third. It is the third which raises the problem of cross categorial laws.

Davidson's principles, each of which he regards as true, are: (i) 'that some mental events interact causally with physical events'; (ii) 'that where there is causality, there must be a law' and (iii) 'there are no strict deterministic laws on the basis of which mental events can be predicted and explained'. Not wishing to entertain a mind/body dualism, Davidson admits that (i) and (ii) jointly require some identity thesis and some form of materialism, both of which he is prepared to defend. We will consider the identity thesis later (3.7) as it might well be a 'last resort' for our determined researcher.

For the moment we will imagine that he wishes to retain the intentional features normally attributed to the mental and consequently commit himself to the possibility of psycho-physical laws denied in proposition (iii). Needless to say the nub of the argument for the coherence of the three principles turns on whether there are strict psychophysical laws under which mental events, described as mental, may be subsumed. This, it might be recalled, is what Colin McGinn denied because of the incompatibility of the intentionality of 'explanation'.
with the extensionality of 'cause'. Davidson's argument gives ground to this point.

Davidson's argument is presented in his article entitled 'Mental Events'. I will attempt to show that Davidson is correct to reject the notion of such laws. If this position can be maintained then I see no alternative for the educational scientist but to give up the idea of formulating a hypothesis in which the mental is predicated of events listed under antecedent conditions and replace them by physical events. This raises problems of its own but for now I will concentrate on the case in hand.

Davidson begins by stating that 'nomological statements bring together predicates that are made for each other'. As an example of this he argues that certain predicates such as 'grue', which is a predicate of emeralds meaning green if examined before t and blue if examined after t, are unsuitable nomological candidates because we cannot form the lawlike statement 'All emeralds are grue' from its combination with emeralds. The argument, therefore, is that mental and physical predicates are nomologically unsuited. Firstly, however, we must see just what Davidson means by lawlike and then ask just what it is that makes grue-like predicates unsuitable, when in combination with physical subjects, for the formation of lawlike statements.

Of the lawlike he says that they,'are general statements that support counterfactual and subjunctive
claims, and are supported by their instances.'

Support by their instances is illustrated by 'All emeralds are green'. Green is therefore taken, in combination with other conditions, as inductive support for something being an emerald. This is not true of grueness, which is both green and blue at different times. It therefore does not support the induction 'X is an emerald' as an instance. The example, Davidson admits, is too strong for the case in hand, for, having observed that a certain mental event is followed by a physical event, we often make rough generalizations about what to expect in the future. We thus use mental predicates as rough inductive evidence or support for underlying regularities. This is not true of grue-like predicates which offer no such support. Thus there is what we ordinarily refer to as a rule of thumb. But, thinks Davidson, there is an important distinction to be made between rules of thumb. There are those generalizations whose instances give us reason to think that by the addition of further conditions and provisos stated in the same vocabulary we approach a more exact law. Davidson contrasts this, 'homonomic' generalization with 'heteronomic' generalization in which when instantiated 'give us reason to believe there is a precise law at work, but one which can be stated only by shifting to a different vocabulary'.

Physics is mostly homonomic whereas biology, geology and meterology are mainly heteronomic. The reason is that physics forms a closed
theory whereas the others utilize theories other than their own to express whatever laws they may have. Plate tectonics, for example, while descriptively remaining at the level of large movements of the earth's crust simultaneously explains such movements in the language of physics.

From this point the argument starts to take grip on the possibility of compatibility between intentional predicates with those of physical predicates in the formation of lawlike statements. Davidson argues, I think quite correctly, that physical concepts such as length and laws dependent on the idea of length themselves depend upon certain other elements within the same theory holding constant. One may paraphrase his point by asking what is presupposed by measuring a length. There must be, Davidson insists, a law of transitivity which asserts that x is longer than y for all conditions of x and y. For example, x is not shorter than y when shortened by its approach to the speed of light, for y is not in the same condition. But being in the same condition involves a further set of assumptions, taken from the theory, about the effects of velocity on length as well as the effects at high velocity of the effects of the time taken to measure length where length is defined in terms of an amount of time taken to travel from one end to another. (A time, which Einstein showed, varies with velocity.) Put in Wittgensteinian fashion, it is a rule that one meter is a rule and that rule is held in place in a
complicated web of concepts and agreements which constitute a form of life. There is no length simpliciter.

It is a thought similar to this that prompts Davidson to say that 'the whole set of axioms, laws or postulates for the measurement of length is partly constitutive of the idea of macroscopic, rigid, physical objects.' But it is just this 'background', hinted at in 2.22, which holds concepts in particular sciences together allowing the possibility of measurement, weighting and quantification in general which precludes heteronomic translation into an intentional language because, like 'grue', the latter has its own peculiar (in the sense of specificity) 'background' against which its concepts are held in place.

To clarify the point, and I feel it to be an essential one in the argument, let us consider an example intended to bring out the 'grueness' of the intentional. To make the point as strong as possible I will choose an intentional concept near to the borders of non intentional, reflexive behaviour. Take the intention of exclaiming that I am in pain. Pain, considered as an experience, we might wish to correlate with some form of physical state; an electric shock for example. Now we might want, as scientists to measure the amount of pain required to make a person exclaim that he was indeed in pain. Too much of it in fact! To 'weight' the electrical input, or calibrate it, if that sounds more
scientific, a necessary condition would be to ask the subjects taking part to report when, for example, they first feel pain and when they cannot stand any more pain. Call these 1 and 10 respectively. Now, given that the electrical charge is defined in terms of a physical theory similar to the one outlined for length: a theory, for example, allows one to say unequivocally that the same amount of electrical charge was delivered to a certain number of people and given that the avowal responses are part of intentional behaviour, we may ask of this concrete example, exactly what is grue-like about the avowals which make them unsuitable as instances of a general, albeit heteronomic law?

To answer this we need to return to Davidson's point about the interdependence of theory and instance. Electrical charges are rated against a vast background of theory which ranges from a theory of ionisation in chemistry to conductivity and resistance in physics. In our experiment we may assume that similar humans have similar skins and therefore similar resistance and conductivity. We may assume therefore that whatever is the cause of their pain experience, it is, electrically speaking, the same. However, and we do not need an experiment to bear this out, the avowals 1 to 10 will not be the same across subjects. What counts as an instance in electrical terms is not what counts as an instance of the same thing in experiential terms. Notwithstanding, we can stipulate that people who are given the same charge
of electricity experience the same thing, even though they do not agree in their avowals. This agreed, what can be profitably made of the avowals themselves? We might wish to say that some people are mistaken about the amount of pain they experience, in which case their avowals are wrong. This begs the question, however, and holds out the spectre of everyone being consistently wrong about what they say about their experiences. Since this position is untenable we must assume that there are individual differences either in pain experience or in the willingness to avow. The possibility of formulating a lawlike statement relating electrical imput and a certain avowal thus depends upon our ability to decide just how, or by what criteria, experience of pain and its expression varies. This brings in Davidson's 'other theory': the theory we have about ourselves, our intentions, beliefs, desires and so on.

To illustrate the problem, and I believe it to be an insurmountable one, consider what is involved. Consider, for example, the contents of a questionnaire designed to eliminate individual differences. It might include questions such as: Do you feel inferior? The intention being to discover which subjects might not say that they feel pain until it is rather intense and resist saying that they cannot stand more until past the point at which they would normally stop. A consideration of this one question alone will suffice to show that the project is not feasible: the 'inferior' person might well
'give up' all the more quickly! A further test is therefore needed to discriminate between people with inferiority complexes who are trying to prove that they haven't and those who have them and don't mind admitting it. But what about someone who has an inferiority complex, admits it, usually acts in a timid way and yet is secretly trying to overcome it: the test being a good opportunity (since there is direct comparison with 'normal people') to prove otherwise. Tests proliferate on an exponential curve of which Malthus would have been proud and this shows that attempts to standardize tests on intentional behaviour are subject to an infinite regress.

No correlation between electrical input and the intentional expression of pain could be made without a further condition which is that the reputed relation is already quantified. This, however, is precisely what cannot be had for the necessary correlation presupposes a standard test by which other tests are validated. Such a standard would constitute the knowledge acquired through a series of tests: it would be the end product of inductive investigation. It cannot, therefore, without circularity be used as one of the initial requirements. Therefore no inductive support can be gathered for a psycho-physical law and a law with no instances is no law.

3.50 Conclusions.
The options open to the determined positivist who still insists that educational research can be carried out along the lines prescribed by his science are now few indeed. In fact, if the arguments we have explored on his behalf prove to be correct then he cannot formulate a testable, practical hypothesis in which the antecedent conditions retain the intentional features attributed to them in ordinary speech. He is thus forced to reformulate the antecedent conditions in physical or behavioural language. If he admits this then there are still options open to him all requiring some form of reductionism. There is, of course, one great drawback with proceeding along these lines and that is that the physical causes of behaviour are not those talked about by those engaged in the practice of education, nor are they immediately observable. This puts the scientist at a great disadvantage if his work is to remain relevant to educational research simply because of its practical nature and the intentional language in which such practice is couched.

These considerations, the researcher might reply, are indeed practical difficulties, but they do not constitute insurmountable problems. The reason might be the belief that the language of physics employed can be translated into an intentionalist vocabulary and hence both scientific and beneficial to practical problems. It might be recalled, however, that Colin McGinn's argument
denies the possibility of a science of psychology on precisely this point. Ordinary explanations of human actions are explanations only under particular descriptions and hence fall under an intensional logic while the physical descriptions of causes in which laws are formulated are true under all descriptions and fall under an extensionalist logic. To defeat this argument our educationalist must show that intentional descriptions are redescribable in such a way that they are semantically equivalent (that no change of meaning has occurred) and that their truth values have not been altered. To see if this argument can be sustained I will look at the history of the argument and then at a more recent attempt to get around it which is most notable for its failure.

3.6 **Reductionist Strategies 1: Extensionality.**

Historically, intentionality has always been a problem for the 'logical positivists' for it seemed to threaten their twin theses of extensional logic and atomicity. Indeed, the early Wittgenstein thought the matter so crucial that he gave considerable attention to it in the Tractatus and attempted to 'write out' the word 'belief' from his vaunted logical calculus altogether. In this he was supported by both Russell and Carnap and it is not without irony that Wittgenstein became, eventually, the great defender of intentionality: not
least, because of the evident failure, in his own eyes, of the arguments he had used to eliminate it. I will recount part of that argument as an introduction to what I have to say on the subject.

Russell puts the problem presented by the intentional - which he refers to as a 'propositional attitude' - for the thesis of extensionality as follows. There are, he says, two 'principles' of extensionality:

The first part of the principle of extensionality says that all functions of propositions are truth functions, i.e. that, given any statement which contains as a part a proposition p, its truth-value is unchanged if we substitute for p any other proposition q having the same truth value as p...The second part...states that this is always the case, i.e. that, in any statement about a propositional function, any formally equivalent function may be substituted without changing the truth-value of the statement.

The problem caused by intentionality is that...

...the thesis of extensionality is not true of propositions asserting propositional attitudes. If A believes p, and p is true, it does not follow that A believes all true propositions; nor, if p is false, does it follow that A believes all false propositions. Again: A may believe that there are featherless bipeds that are not human beings, without believing that there are human beings who are not human beings.

thus,

those who maintain the thesis of extensionality have to find some way of dealing with propositional attitudes.

The thesis of atomicity is stated by Wittgenstein in the Tractatus as:

Every statement about complexes can be analysed into a statement about their constituent parts, and into those propositions which completely describe their complexes.'
Russell again supplies the problem posed by intentional descriptions25...

...in 'A believes p', p is complex; therefore, if Wittgenstein's principle is true (it) must be analysed into a statement about the complex p together with propositions describing p. Put more loosely, this means that p as a unit does not enter into 'A believes that p', but only its constituents enter in.

But, as Russell points out, there are two ps: the first is the proposition p while the second is the object of a propositional attitude. The first is subject to the principle of extensionality while the second is not: it may have a different truth function.

The problem this presents for our scientist is that since the principle of extensionality is a presupposition of subsumption he cannot claim to have provided a reduction of intentional language without first showing that truth conditions of the whole sentence have been preserved. But, as Russell points out, the preservation of truth conditions is exactly the problem: the part proposition 'believes that p' has not, or need not have, the same truth value as 'that p'. This shows that mental descriptions are not formally equivalent to state descriptions: the 'complex' to which he refers contains both.

I have provided some historical background not only to highlight the problem, but also to show how central the argument is in the history of the attempt to formulate a logic which is suited to both science and to the 'anomalous' mental. In the thirties supporters of the
so called 'unity of science' programme were optimistic about the resolution of the problem. Carnap, for example thought that, 'for every given (non-extensional) language Si, an extensional language Sii may be constructed such that Si may be translated into Sii.'

It is just this however, which has proved so intractable that modern exponents of the unity of science programme have given it up. To see why I will restate what the thesis of extensionality demands as put in a more modern, and available, form and then look at just why translations between it and sentences containing 'propositional attitudes' does not work.

Margolis summarises what it is for a language to be extensional:

1. in its sentences the substitution of codesignative expressions does not alter the truth value of the resultant sentences when compared to the original;
2. for its compound and complex sentences, truth values are a function only of the truth values of its constituent clauses;
3. for those clauses the substitution criterion is satisfied.

Sentences containing intentional clauses such as 'Tom believes that Cicero denounced Catiline' fail to satisfy the first two conditions although they satisfy the third. Such constructions (typically containing verbs such as believes, wishes, urges and fears) Quine refers to as 'opaque'. Of such 'opaque' constructions Quine says that they are those in which one 'cannot in general supplant a singular term by a codesignative term...without disturbing the truth value of the containing sentence.' The problem for a science requiring
the reduction of intentional descriptions to extensional ones is obvious: unless some way of 'translation' is found which preserves extensionality then no reduction is possible. This is therefore what Quine, following in the footsteps of Carnap, attempts to do, believing with his predecessor and mentor that the disposal of 'opaque' constructions would forward the march of science. He does this in a rather paradoxical manner, for instead of showing how intentional sentences can be adequately translated into extensional ones he argues, adamantly, that they cannot. 'Even', he says, 'indirect quotation, for all its tameness in comparison with other idioms of propositional attitude, and for all its concern with overt speech behaviour, seems insusceptible to general reduction to behavioural terms; the best we can do with it is to switch to direct quotation, and this adds information.'\(^{30}\) From this he concludes, 'If we are limning the true and ultimate structure of reality, the canonical scheme for us is the austere scheme that knows no quotation but direct quotation and no propositional attitudes but only physical constitution and behaviour of organisms.'\(^{31}\) This conclusion, however, does not follow. Nothing relating to the 'true and ultimate structure of reality' is entailed by the opacity of certain verbal constructions. What is entailed is the defeat of the extensionalist thesis which is that intentional idioms are reducible to extensional ones and
this is enough for my case against this form of reductionism.

Educational Science, if that is an appropriate description, has, traditionally, followed Quine's advice and gone ahead with a reductionist programme regardless of philosophical difficulties. In the final section of this chapter I will show why reductionism - whether in the traditional guise of behaviourism or in the more recent garb of materialism - offers no way forward to the educational researcher. I begin, for historical reasons with what is now generally felt to be an inadequate form of research: behaviourism. Because of its long history and the infamy of some of its findings - I refer to that of Cyril Burt - I will not tie myself closely to the work of any one behaviourist but concentrate on the general themes to be found in their writings.

3.7 Reductionism 2: Behaviourism.

The second reductionist strategy our hypothetical researcher might try to make is to take Quine's option and drop all talk of intentional idioms altogether. This, quite familiar, path is usually characterized as ontological behaviourism. Its premisses are well entrenched in all of the methodologies discussed in the second chapter and it is possibly the most potent form of positivism. In a sense it 'resolves' the problem of reductionism by ignoring it. That is, while Quine and
others admit that no translation is possible between 'ordinary' language and the language of movements, the programme proceeds regardless. This does not of course either vindicate it or allow it to avoid the general anti-reductionist arguments. I believe that the ontological behaviourist, whether he likes it or not, still has to justify his ontological stance and this entails meeting the arguments brought for the ineliminability of the intentional.

This having been said the problem posed by ontological behaviourism has to be met from a different angle. This is perhaps best formulated in the form of the question, 'How well does the non-intentional language of the behaviourist cope with the creature with which it deals?' This broadens the scope of the enquiry for the notion of 'coping' is, for the behaviourist, something entirely to be understood in the context of science. This aspect of behaviourism has been referred to as methodological behaviourism. Although there is some irony in the fact that ontological behaviourism is the belief that intentional phenomena are nothing more than idiomatic conveniences (the choice to ignore arguments favouring the importance of the intentional hardly constituting hard hitting reasons on which to base that decision) it is with its methodological counterpart we must deal. What, it must be asked, does a non-intentional, fully extensional 'thing' language look like and what philosophical problems arise from it and the
project of methodological behaviourism in general? The first of these questions is answered by the fact that classically it has tended to replace the whole range of intentional verbs associated with our desires or reasons for action with 'response' and the intentional content of the desire with 'stimulus'. Causal relationships, on this view, are expected to be discovered particular 'stimulus conditions' and particular 'responses'. The answer to the second arises in the context of the attempt to carry out the experiments themselves. Methodological problems, that is, arise within the context of the attempt to work within such a restricted language and thus the problems become inextricably linked.

The philosophical problems they give rise to fall into two main categories. The first concerns what I shall call the 'externality' of causation. The second concerns the so called neutrality of the data language. Of the first I shall make three points. Unlike the Central State theorist or his functionalist counterpart the behaviourist has to explain behaviour as effects ('responses') to external causal antecedents. This raises a number of thorny issues. One of them is analogous to the problem of dualism in that it is not at all transparent how an independent environment can be causally efficacious with regard to other systems (living ones) with which it is not (except in some particular circumstances) literally in contact. The behaviourist cannot resort to some intermediate factor - such as a
mind — since this is what is explicitly denied. If the behaviourist is arguing that some form of causal relationship exists between two physical entities, one a 'stimulus' and the other a brain, then the causation concerned is presumably both physical and is restricted to the impact of millions of photons on the retina — photons which act as a 'stimulus'.

This seems to me to be an intolerable state of affairs and one which can only be resolved by the admission that something more than bare physical relations is necessitated by the notion of 'taking something as a stimulus'. Certainly the mere impacting of photons alone will not do the work of explaining either the notions of being stimulated or responding to or by something. Indeed the very idea of seeing something as a something given only photons, retinas and nerves gives rise to insurmoutable problems explored by the later Wittgenstein in the latter part of his 'Investigations'.

Put simply these criticisms amount to the fact that something's being seen as a something (a duck, rabbit or stimulus) presupposes an interpreting mind in order that it is seen as that something at all. Behaviourism, rather cleverly, has attempted to avoid this by experimenting (typically) with stimuli which have psycho-somatic effects on their subjects: food, for example, causing salivation. What they do not so readily admit to is that most stimuli in the human environment are not related to
us in quite such an intimate way. I 'take' the various
signs on a motorway to 'mean' something and whatever that
something is, affects my driving. I might, for example,
stop or look in my mirror. How might the behaviourist
explain this phenomenon without (again) recourse to some
central organizing principle in me which 'understands'
both that the sign was to be taken as a sign and that I
understood what it meant? This point will be taken up
again in the last section.

Another, parallel, argument stems from the
apparently creative ways animals and humans have of
'responding' to the stimulus. This may not be quite true
in the very simple, but also very unnatural, case where a
rat has been 'conditioned' to follow a particular path
through a maze to find food. It is however true - as
Kohler showed - that monkeys used an extraordinarily wide
variety of ways of obtaining their goal. Kohler's
comments on his findings (that some 'organizational'
principle is involved) are echoed in Daniel Dennett's
question as to how, non-intentionally, the behaviourist
specifies just what it is that is learned.\footnote{32 How, that
is, does the behaviourist say that the rat performs
certain movements which are related to certain stimuli
without the use of such terms as 'believes that', 'knows
that', 'remembers that' and so on? It seems that the
behaviourist is committed to the unconvincing thesis that}
the rat's skeleton moves appropriately - a thesis which, incidentally, is easily questioned by asking how people with muscular disorders ever obtain the 'goal' they 'aim for'. The thesis is 'unconvincing' simply because what constitutes 'appropriateness' of movement can only be defined in terms of aims, goals or purposes and yet these all have intentional connotations. Intentionality, therefore, is not actually dispensed with: it is a presupposition of behaviourism.

The second set of questions revolve around the language - supposedly topic neutral and physical - in which such behaviour is couched. A familiar but powerful criticism is that behaviourists simply cannot account for actions which are given significance by social convention. Signing a cheque, for example. The reason is that any particular movement is meaningless unless the rules which conventionally govern its interpretation are given. It follows both that actions, properly described are irreducible to movements and also that the movements observed by behaviourists are (whatever they might claim) are totally opaque to them. A corollary of this is that one and the same action might be performed by an almost infinite (given human invention) number of movements. An example which comes to mind are the movements made in making a bid at an auction. Some of them are agreed by a majority but some are defined by as few as two people -
one of whom is the auctioneer. The behaviourist not only has problems explaining these sorts of movements but also explaining away actions which are properly characterized by their motive or purpose. I might, for example, want to buy a particular painting at an auction. The method by which I secure it is covered by a rule known only to me and the auctioneer and agreed between us - I have a reason for wanting the painting, an agreed action (putting out my tongue) for securing it and the reason I have explains, in these circumstances, my action. For the behaviourist, however, I do not have reasons or wants, only responses. My response (putting out my tongue), which, for the behaviourist is merely a movement, is not only opaque but no purpose can be derived from it alone which gives it any sense. Indeed, it might be the case that I hate the auctioneer, put my tongue out at him and, unknown to me my great aunt buys me the painting which the auctioneer then gives me. The behaviourist simply cannot deal with the whole gamut of human actions because they only make sense given our purposes, reasons, conventions and the like. In fact, the behaviourist quickly finds himself in a circle in this regard, for, in order to be able to say what movements relate to what stimuli, he must revert to conventional and intentional signifiers. Without that, as Dummett argues, the whole science (so called), of behaviourism is indeterminate. There is, however, a further, and equally interesting
problem facing the determined behaviourist at this point. How does the behaviourist account for what is going on while he is making inferences about the way various movements are related to various stimuli? Given the very obscure movements taking place in the auction room the ready answer cannot be supplied by reference to any form of conditioning – the situation is completely unique. Taken one stage further this raises the serious question as to just how the behaviourist explains – within his own framework – the very complex set of movements involved in his doing science and what stimulus is there for it? Surely the behaviourist cannot, in all authenticity, state that the stimulus for research is the need (drive?) to understand the nature of human actions – for what kind of stimulus is that if not an intentional one? It therefore seems to me that the whole project presupposes the intentionality of the subject – who might be the behaviourist – and then proceeds, systematically, to ignore it. Quine’s methods seem central to all behaviourist enterprises: first discover the very central core of philosophical problems that stand in the way of human science and then, in the interests of science carry on as if they did not exist. If behaviourists would entertain the intentionalistic notion of 'bad faith' they might discover that they suffer from it. Luckily for them they do not.
One way of avoiding 'bad faith' is to show that when intentional terms - including the notion of mind as anything over and above the body - are 'analysed' they turn out to mere bodily dispositions. For support in this 'analytical behaviourism' our researcher might turn to Gilbert Ryle. Ryle's famous thesis is that Descartes' 'Ghost' is exorcized when it is realized that it is simply a 'category mistake' to view the mind as something over and above the machine which it supposedly inhabits. The mind is a reification of machine parts, and mental ascriptions are dispositions to behave in certain ways. Ryle says summarily: 33, '..."my mind" does not stand for another organ. It signifies my ability and proneness to do certain things...'.

This 'logical behaviourism' might well appeal to our scientist since it essentially dismisses the mind leaving only the physical performance of actions. Ryle's 'analysis' is surely wrong however. Thinking, for example, cannot be simply spirited away as some form of internalized saying - however many centuries this might have taken to develop - for the concept of thinking is used to distinguish what is said from what is merely thought. Even Pooh knew the difference between just standing and standing and thinking.

There are three forms of argument against Ryle's position. The first two question the correctness of his analysis while the third raises serious ontological questions about just what it is which develops the
capacity to internalize its sayings and, indeed, just what it is that is subject to proneness and which is disposed. The 'analytic' type fall into two 'categories'. The first is that it is perfectly permissible, linguistically, to reply to a question concerning what one is doing with answers such as 'Just thinking', 'Just resting' or, indeed, 'Nothing'. Secondly, there are a whole variety of mental states or events which either do not have or need not have any overt behavioural manifestation. Thoughts, for example, need never be expressed. A pointed silence can speak a thousand words. Moreover it is simply not possible to express all of our thoughts either because of the time it would take or because many of our thoughts are half formed, vague or even, inexpressible. Finally the emphasis Ryle - and behaviourism generally - puts on physical movement brings up the problem of solipsism. It does so in a form in which there is a disparity between first person assertions and third person accounts. Wittgenstein, when he said that the inner stood in need of outward criteria, was referring to the plain fact that we ultimately rely on some observable phenomena if we wish to try and interpret a person's mind. He was not saying either that 'outward criteria' were sufficient conditions for that interpretation, nor was he committing himself to behaviourism. He was pointing out that in the case of other people we have only the observable evidence of their actions to go on in asking what is on their mind.
This, however, is not the case in the first person. If I experience pain I do not mean by it that I am behaving in a particular fashion. I mean that I have an experience within me which, qua experience, is not publically available. Ryle's thesis ultimately cannot allow for the translation of private experience into public performance. For him it is what I do and am prone to do which arbitrates upon what my 'internal' experiences are. Thus the 'outward' becomes criterial in a non-defeasible way and consequently defines what the mind is. The behaviour of others, however, is rendered completely opaque by this since the thoughts, desires and projects of individuals which, in ordinary life, explain and interpret those actions are explicitly denied that role. Where behaviour becomes the criterial arbiter of action then action becomes indeterminate.

The third argument is broadly ontological. Descartes, for all his mistakes, at least allowed the cogito to have a place. Ryle's thesis, however, does not seem to allow for a 'centre of activity' which co-ordinates any idiosyncratic proclivities the person (if that is not another 'category mistake') might have. This notion - that thoughts and other mental goings on - are properties of something is central to Ryle's critics. Among these, and interestingly, is D.M. Armstrong. I say interestingly because Armstrong finds the lack of a central controlling factor a serious flaw in Ryle's philosophy of mind. At the same time, however, Armstrong
does not want to bring back the 'ghost'. Thus while his main criticism of the thesis is that dispositions entail some state which is disposed such a state is a material state: a state of the brain. This idea has, in a historical sense, taken over from behaviourism and for the very good reason that central controlling states do seem to be required by the very complex nature of our behaviour. There is a sense therefore that more recent work in the philosophy of mind has taken note of Gestaltist criticisms of behaviourism but which has, at the same time, been wary of the problems posed by ontic dualism. The result has been the attempt to keep the mental but only as an epiphenomenon of the physical. The mind and mental events, it is argued, are identical with states of the brain. This line of thought might provide educational research with the nomological base it requires. I shall argue, however, that it will not.


The Central State Identity Thesis, as it has become known, might be attractive to an educational researcher because of its philosophical respectability. This is not to say, of course, that it has no problems: it does and I shall be at pains to point them out later. Its initial attraction is in its apparent ability to talk of both minds and brains and at the same time explain the causal relationship between mental events and action. It
does this, as I have mentioned, by arguing that mental events are identical with physical events. Thus the mental is ultimately neurological or chemical in nature and perfectly well able to cause other physical events such as actions. Before exploring this intriguing thesis I should comment that there are various forms of it - forms, incidentally, which have evolved in response to philosophical pressure. To ease matters I shall deal with two forms only. The first, weaker form, argues that the tangible internal goings on - such as pains and sensations - are in fact identical with brain states while the second, stronger form, argues also that such things as thoughts and the mind itself are identical with brain states. From a historical perspective it is the latter which predominates.

Both forms of the thesis are held by J.J.C. Smart - the stronger form coming as he and Armstrong became more confident. I will begin then with Smart's account of identity in both forms and then concentrate on the problems posed by the stronger, since this is the sort which would interest the educationalist. Smart says that the identity in question must be 'interest free' and 'strict' in the sense that 'Sensations are nothing over and above brain states'. The strong version goes further and talks not only of sensations but of thoughts: 'I wish to elucidate thoughts as inner processes and to keep my thesis compatible with the physicalist viewpoint by identifying inner processes with brain processes.' The
sense of strictness here is the kind expressed in Leibnitz's Law or principle of indiscernibles. On this principle any discernible difference is enough to disallow identity.

If this thesis can be sustained then, perhaps one might have to concede to our hypothetical defender that there is a road for science to follow. I think, however, that there are a number of reasons why the thesis cannot be sustained and that (on a pragmatic level) even if it could it is a thesis which could not (even on its own terms) be of any possible use to educationalists. I begin with reasons why I don't think that the thesis is sustainable. The first is a reason stemming from Wittgenstein's comments on the seeming impossibility of squaring the temporal and spatial characteristics of the physical with the lack of these in the mental. Since the physicalist thesis states that the identity in question is contingent it should therefore be open to the scrutiny of an empirical test.

Norman Malcolm argues that such a test is impossible. He supposes that Smart has to accept, as a necessary condition of identity, that \( x \) is only strictly identical with \( y \) if and only if \( x \) occurred at the same time and place as \( y \). He argues that this condition cannot be satisfied empirically: 'indeed,' he argues, 'it does not even make sense to set up a test for it.' The reason, he claims, is that even though we may have determined that a certain process occurred at the same time as a
particular thought occurred, we could not make the further test of whether my thought occurred inside my skull. At the same place, in fact. The reason: 'that no one has any notion of what it would mean to test for the occurrence of the thought inside my skull independently of testing for a brain process.' This objection, as far as I can see, stands. Smart's thesis holds the correlation to be a contingent one; hence the tests have to be independent. Therefore an independent test for the location of a thought does seem to be entailed by an empirical proof of the thesis.

What if, however, it is the case, as Rorty and Feyerabend suppose, that ordinary language is contingent and changes with scientific discovery. It may be true now that no one now 'has any notion' of how to make a test and it might be a conceptual confusion now to talk of the location of mental events - but if language is contingent and changes with science - it would seem to be open to Smart to defend his position by arguing that this situation might change. Malcolm would then have to argue either for some reason why this language now has to be taken as having some superior status vis-a-vis scientific advance or, that there is some language transcendent reason for thinking the notion 'unintelligible'. Malcolm's answer, in part, is that the word 'thought' is part of our language now and it is that we are debating. Any future change in conceptual schemes therefore would not affect this, particular, historically
bound, dispute. Another dispute, in another language, would simply be a different dispute. In this language, Malcolm argues the Wittgensteinian point that thoughts are only the thoughts they are against a practice. He gives an example of my thinking of a milk bottle. The idea of a milk bottle derives from its role in a practice of drinking, delivering and milking: social phenomena which cannot be reduced to anything 'inside the skull'. This, he suggests, does not square with Smart's thesis in that the brain state is a sufficient condition of the thought while a necessary condition of the thought is a social practice. A defender of the identity theory and scientific materialism, if he wanted to be consistent, would be forced to reduce social practices to physical states and while this is possssible for milk bottles it is difficult, for perhaps language transcendent reasons, to hold that 'the rule that milk bottles will not be collected unless they are placed outside the door is a configuration of particles.' I think that the reduction of such a rule to a physical thing language is an intractable problem for Smart and for the central state identity thesis in general because the identity of (a supposed) brain state which was 'identical' with a belief state with an intentional object such as a social rule would indeed entail the identification of physical states. Rules, however, are conventions, and are therefore indefinitely replaceable. A condition of asking what rule is enshrined in a brain state would be a
question concerning what rule people had agreed to or believed to hold and this creates an inescapable circle in which rules and the like could never, themselves, be identified as physical states without recourse to a belief. I also think that the notion that it might be possible, at some future date, to locate thoughts in space and time to be adequately answered, as it is by Malcolm, by suggesting that this would simply change the topic. If such 'thoughts' were talked about as occurring here or there they would be a different sort of thing to the thoughts we talk about today. To insist that they are the same seems to beg the whole issue of identity.

There are two further reasons, as I have indicated, why the central state identity thesis is unlikely to help an educational researcher solve his difficulties. The first of these is that even if it were to be shown to be true no translation rules would be available between the language of physical states and mental events which would enable the researcher to report his findings in ordinary language. The second is that in the event of this failure any possible science would not be one which could deal with educational problems. It would be irrelevant.

The first question concerns the possibility of translating neuro-talk into ordinary language. I am assuming that identity is central to establishing common reference so that, for example, in intentional language (C1) 'My reason for doing x was...' has the same
reference as 'X.Y.Z' (call it N1), which is its neuro-talk equivalent. (Equivalence here is equivalence in truth conditions and not semantic synonymy.) My claim is that even if the identity thesis is true there is no possibility of making such a translation and hence no means of making any possible findings known to educationalists in terms which would be useful to them. The argument rests on a requirement, which is a condition of translation, which cannot be met. This requirement is an independent criterion which serves to establish that the reference of C1 is identical with reference of N1.

To see why this condition cannot be met, let us consider Smart's supposed analogue: that an electrical discharge is identical with a flash of lightning. The flash is a perceptual phenomenon, while the discharge is a 'state' description of it. The flash derives from our ordinary talk of things, while the discharge from scientific theories about how things are. How is translation possible? How, that is, do we know that they both refer to the same thing? The answer lies in some form of human agreement: the scientist points to a flash of lightning and affirms that it is that object which he describes as a discharge. The 'man in the street' points to the lightning and asks the scientist to explain what it is. They agree at the start that what they are talking about is the same thing. Where such talk is not possible, as in cases of 'radical translation', Quine's thesis argues that what translation there is, is at best
indeterminate. It is indeterminate because certain, unverified assumptions about another language and its use, are built in to the translation. (Assumptions such as 'They are are not like us in having interests a,b,c etc) Translation, put roughly, becomes more determinate as the ability of the language users to communicate increases. Indeterminacy increases as the number of unverified assumptions about the languages increases.

Now, if we return to Smart's example, we can see why the analogy between lightning/discharge and sensation/brain state breaks down. It is simply this. The identity of the lightning/discharge is established by the possibility of the scientist and man in the street coming to an agreement over what thing constitutes the reference of their different descriptions. Similarly, Gavagai would be more determinately translated if the situation demanded less guess work and more communication. In the case of our translation however, C1 is known to all, because it is by definition, ordinary language, whereas N1 is known by a few. The question resolves itself into whether a determinate translation can be made between C1 and N1 users through their ability to come to an agreement over the object which constitutes the object of their respective talk. In the lightning example the scientist was able to do this because the object was available to both to see. In Quine's example, the rabbit was available to both also. This is not just coincidence, for a condition of our ability to communicate is that we
talk about the same things. This is the main import of Wittgenstein's insistence on the need for 'outward criteria'. But it is just this which is unavailable in the proposed C1 - N1 translation. There is no possibility of agreement because there is no one, observable entity to agree about.

Perhaps, a detractor might say, this is so, but it does not serve to show that no translation is possible. Such translation can be carried out at the level of theory and identity established by intertheoretic rules. Smart's flash, for example, could be made part of a common sense theory which told a story about thunder-storms and bright light combined with loud bangs in the sky and rain. It might include information about people being killed by such flashes. The scientist's theory includes work on electricity, how it jumps across gaps in a circuit when the voltage is high enough, that the colour of this discharge is blue and so on. Then, without any agreement, and nothing common to point to, it could be deduced, independently of the two theories that they were about the same thing: namely electrical discharge. Here again, however, the possibility of translation seems to depend upon common descriptions in both theories. The theories described, for example, both contain the description blue, (although in one case this would be expressed as a wavelength) they could both, presumably contain the description 'flash', they could both tell stories about the harmful effects
such discharges have on human beings and, I suspect, that it is upon these intertheoretic descriptions that a deduction to the effect that they were about the same thing is made possible. Such intertheoretic descriptions however, do not occur between C\(_1\) and N\(_1\). We do not describe our reasons, nor, indeed, our sensations in terms which include neurone firing rates and N\(_1\), presumably, knows nothing of our experience of their firing or of our reasons for acting. In other words a condition which seems, at least, to be necessary in the making of intertheoretic rules is once more absent in the C\(_1\) / N\(_1\) case because there is no common ground in which to start. In consequence, anything that might be said by our researcher in N\(_1\) would be of no possible importance to education which is conducted in C\(_1\).

In saying this we have come full circle in our investigations regarding the possibility of a scientific base for educational research. The Identity Thesis provides, perhaps, the most likely way forward but it is flawed as far as educational research is concerned because it would speak a language unknown to and irrelevant to education. In any case the proponents of the thesis are attempting to solve rather intractable problems which arise within the philosophy of mind and consequently with the realms of possibility — it has never been their intention — to my knowledge, that such a theory carry the weight of actual research. That, it seems to me, is the main criterion with which
educational research should concern itself - that it is pragmatically useful in actually improving the educational opportunities of any population. It is to this task which I now turn.
Chapter Three: Footnotes.

(1) A fairly typical account of the realist position is outlined by Roy Bhaskar in his book, 'The possibility of Naturalism', Harvester Press, 1979, pages 13 and 15. I include these two quotations to bring out the emphasis placed on (a) tendencies and (b) metaphor but not on laws or causal connexions.

On the transcendental realist system a sequence A, B is necessary if and only if there is a natural mechanism M such that when stimulated by A, B tends to be produced.

Typically, then, the construction of an explanation for, that is the production of the knowledge of the mechanism of the production of, some identified phenomenon will involve the building of a model, utilizing such cognitive materials and operating under the control of something like a logic of analogy and metaphor, of a mechanism, which if it were to exist and act in the postulated way would account for the phenomena in question.


(5) Problems with the assumption that events can be said to be the same include the fact that, in the natural sciences, events are said to be the same if they have the same antecedent conditions and have, given the same circumstances, the same effects. In these conditions the same events may be seen to recur as many times as one wishes to observe them. If these occurances are found to be the result of non accidental associations, they may, if suitably universal, be described as laws. Nothing like this has ever been approximated by human action/events for at least four reasons: One, because the condition of 'same antecedent condition' cannot be met. Two, because we lack an adequate ontology of events which would bring variously described action/events under the same description. Three, because changing historical circumstances prevent the repeat of the same action/event even supposing they were possible (the circumstances being the same is a condition of repeatability). Four, because even if the same action/event in the same circumstances were to take place there would be no reason to assume or expect the same effect since the people effected by the action/event might not react in the same
way each time. This last condition of success in repeatability could only be met in circumstances where not only were the same acts performed many times in the same historical circumstances,(which is impossible since history does not, strictly, repeat itself) but in which the future actions of other 'affected' individuals would remain the same.

(6) Ayer, op cit.


(10) ibid, p27.

(11) ibid, p40.

(12) ibid, p32.


(14) Davidson, op cit, p274.


(16) ibid, p207ff.

(17) ibid, p218.

(18) ibid, p217.

(19) ibid, p219.

(20) ibid, p221.

(21) Disregarding the famous, or infamous, experiments along these lines which pretended to examine a patient's attitude to authority. While interesting, in that they tend to support the "anomolosness" of the intentional, they are not what I have in mind, which is a straightforward report of pain.


(23) ibid, p247.


(29) ibid, p151.

(30) ibid, p220.

(31) ibid, p221.

(32) D. Dennett, 'Content and Consciousness', RKP, 1986. p34.


(34) These examples are given by Jenny Teichman in 'The Mind and the Soul', RKP, 1974. p51.


(37) J. J. C Smart, "Materialism" in 'The Mind Brain Identity theory', p165. The 'physicalist' viewpoint Smart has in mind is stated in the same volume ("The Nature of Mind" by D. M. Armstrong, p67) as the thesis that, 'We must try to work out an account of the nature of mind which is compatible with the view that man is nothing but physico-chemical mechanism.'

(38) N. Malcolm, 'Scientific Materialism and the Identity Theory', in Borst (Ed.). p175.


(40) P. Feyerabend, 'Materialism and the mind-body problem', in Borst (Ed.). p142ff.

(41) Malcolm, op cit, p178.

(42) Identity theorists do not claim that this
translation is possible either. It is only required where the theory has a practical application as it would in educational research.

(43) J.J.C. Smart, 'Sensations and brain processes' in Borst (Ed.). p56.

Chapter Four.

Preliminary thoughts on an Alternative Educational Research Methodology.

4.0

To a large extent I have devoted the last two chapters to the questions raised by O'Connor's scientistic position. The position I want to revert to now is the one outlined by Aristotle at the outset of the thesis which is basically that the methods of the natural sciences are inappropriate to both understanding and predicting human behaviour and that more appropriate methods involve 'phronesis'.

That having been said I want to make it clear that what I have to say on the matter by no means follows directly from Aristotle but rather that it is inspired by Aristotle. Chapters five, six and seven explore and expound this inspiration and consequently constitute what
might be referred to as the second part of the thesis. That leaves the rather awkward question concerning the place, in the thesis, of the present chapter. I see it as 'mezzanine' in that it both comes between two parts and undergirds the one that follows. Insofar as it relates to what has already been argued, it relies on the irreducibility of the intentional and on the 'anomalousness' of the mental. Insofar as it undergirds what is to come, it attempts to 'unpack' some of what we mean by human understanding along what I shall call 'objectivist' lines.

I have taken it as read that understanding oneself and others in particular circumstances is a condition of taking a practical decision. Taking a practical decision is the subject of chapter six and I will not discuss it here. What necessitates the present chapter, in my view, is the fundamental split in contemporary thought over what is presupposed by understanding oneself and others. Some, who I refer to as 'subjectivists', take it that such understanding is something like a matter of introspection which is then 'projected' empathetically at or 'on to' others who are then 'understood'. Others, who I refer to as 'objectivists', take it that part of what is entailed by understanding ourselves is that we already understand others and that this understanding comes from being brought up as part of an historical community. The difference might be put in sharper perspective by saying
that the subjectivist projects private meanings on to the world - including the human aspect of it - while the objectivist comes to an understanding of the world by being a public part of it. In the first case meaning is subjective, in the second it is what I shall call 'historically mediated'. That having been said, I use the terms objectivism and subjectivism to illustrate opposing ends of a spectrum of philosophical positions. I am less concerned with individual philosophers, inside or outside the philosophy of education, who might veer more to one side than to the other, than with the fundamental difference such 'leanings' would have on actual research methodology. I will spell out the implications that acceptance of a broadly objectivist position, which I argue for, lead to at the end of this chapter. (4.70)

What these implications look like in practice I leave to the following three chapters.

I shall argue, as stated, for an objectivist understanding, i.e of one in which our understanding is 'mediated' by the historical context in which we find ourselves. I shall then, in chapter five, go on to suggest that such an understanding as is required for educational research is best, or most appropriately, 'encapsulated' by a narrative which then becomes the subject of an ongoing historical process in which understanding is hopefully reached between people and resolved. Since the outcome of such a process is a practical recommendation and the process itself is a
dialogue between various groups of people with an interest in education, there is, in my thesis, no need for such a thing as an educational theory. I also argue, in the final chapter, that the type of research involved entails a breakdown in the distinction between research - conceived as one discrete activity - and policy making - conceived as another.

To get to this point, however, I want to show firstly that the case for objectivism (which is broadly the thesis that human actions and feelings are only understood against a publicly defined historical background) is stronger than that for subjectivism (4.30 - 4.40) and secondly that objectivism applies to virtually all behaviour which might be encountered in an educational context even though some types of behaviour are so complex that they appear to set a limit to the our intersubjective understanding of them (4.50f). The present chapter provides the groundwork for this and provides, to a limited extent, some reasons for utilizing narrative as a possible basis since it is only through such a medium that the requirement of including historical contexts is made possible. Explicit arguments for this together with its implications and problems, are examined in chapter five.

4.01 Chapter Contents.

I shall briefly indicate the major sections of
the chapter and then begin by briefly reflecting on two pertinent comments by David Wiggins.

4.02 David Wiggins on Science and Aristotle.

4.10 Gadamer and Dilthey: Objectivism and Subjectivism.

4.20 Brentano and Intentionality.
4.21 The Intentional and the Historical.

4.30 The Case for Objectivism: Hamlyn and Wittgenstein.

4.41 Conclusions.

4.50 Putting Objectivism into practice.
4.51 Self Deception and Lying.
4.52 Reflex Behaviour.
4.53 Pain and pain related behaviour.
4.54 Emotions and their expression.

4.60 'Frameworks' of understanding.

4.70 Conclusions.
4.02 Some Reflections on Science and Aristotle.

While Paul Feyerabend has no doubts about what to do with scientists\(^1\), one of Aristotle's expositors, David Wiggins, provides an indication of both what is wrong with the scientists' projects\(^2\) and the indicators which Aristotle provides for any attempt to proceed\(^3\):

I entertain the unfriendly suspicion that those who feel they must seek more...want a scientific theory of rationality not so much from a passion for science, even where there can be no science, but because they hope and desire, by some conceptual alchemy, to turn such a theory into a regulative or normative discipline, or into a system of rules by which to spare themselves some of the agony of thinking and the torment of feeling and understanding that is actually involved in reasoned deliberation.

But, nevertheless:

...if there is no prospect of an ordinary scientific or simply empirical theory of all action and deliberation as such, then the thing we should look for may be precisely what Aristotle provides - namely, a conceptual framework which we can apply to particular cases, which articulates the reciprocal relations of an agent's concerns and his perceptions of how things objectively are in the world; and a schema of description which relates the complex ideal the agent tries in the process of living his life to make real to the form that the agent impresses, both by way of opportunity and by way of limitation, upon that ideal.

By 'conceptual framework', Wiggins is referring to Aristotle's work on choice, judgement, perception,\(^4\) 'situational appreciation,'\(^5\) and so on. These, together, form a group of concepts which play an important role in 'phronesis' or reasoned deliberation which, in turn, is the alternative he provides for science. I will follow Aristotle - or Wiggins - no
further, however, for while they provide pointers, the pointers themselves do not spell out what must in the end be a viable alternative to the naturalists' attempts at methodology.

4.10 Gadamer and Dilthey: Objectivism and Subjectivism.

I begin my positive thesis therefore with a question: what, to use Charles Taylor's phrase, are 'self interpreting animals'? I begin here because whatever we decide to do in the future, we do it on the basis of some form of understanding of ourselves in the present. There is controversy over this, however, as Hans-Georg Gadamer points out: 6

Self reflection and autobiography - Dilthey's starting points - are not primary and are not an adequate basis for the hermeneutical problem, because through them history is made private...In fact history does not belong to us, but we belong to it. Long before we understand ourselves through the process of self examination, we understand ourselves in a self-evident way in the family, society, and state in which we live. The focus of subjectivity is a distorting mirror. The self-awareness of the individual is only a flickering in the closed circuit of historical life.

Before setting out on the actual arguments for and against objectivism I hope to add a little clarity by briefly sketching out what I intend to do. History is a central theme because, as I argue in the early sections, the meaning we ascribe to things is not private but one derived from the way we have been taught. Meaning, I shall say, is therefore mediated by history - it is infiltrated by how others see things. This has a bearing
on understanding and on the way we construe empathy. Dilthey, Collingwood and Ayer are all subjectivist about the meaning of empathy - they see it as something coming from ourselves, as something essentially private. I therefore discuss the correctness of this view by opposing what I have called the subjectivist with the objectivist represented by Hamlyn and Wittgenstein. There is therefore a section on the 'private language argument'. My aim in this is to establish Gadamer's point that our understanding of the world and, more especially, of ourselves and others is historically mediated rather than having its origins in some a-historical, private perspective in which understanding is to be found, ultimately within our own self-consciousness - our subjectivity.

I begin with Gadamer's insight that self interpretation is dependent upon historical understanding: that the self is only understood in the context of its historical circumstances. The psychological school of introspectionism, dismissed by the behaviourists is here displaced by that against which our behaviour gains significance. The reversal is from the inward subjectivity of 'empathy' theory to the outward objectivity of circumstance. On this view, consequently, and ironically, understanding of the self is a matter of historical research. This is of crucial significance for the direction of the present thesis for if verstehen is construed on a subjectivist model then
educational research will, in all probability, turn out to be a species of 'self reflection and autobiography'; a view, incidentally, not too far removed from that adumbrated by the proponents of 'Action-Research'. If, however, it is construed objectively, 'self' understanding becomes refocused so that the self understands itself in that in which it 'comports itself proximally and for the most part': in its historical circumstances. I shall argue in this chapter that the objectivist is correct (4.30 - 40) and draw conclusions for educational research (4.70) having examined the scope of objectivism (4.50f) within an educational context.

4.20 Brentano and Intentionality.

The consequence of the arguments in Chapter Three regarding the irreducibility of the intentional is that a redescription of 'propositional attitudes' preserving the same truth conditions can only be carried out amongst synonomous descriptions which keep the intentional meaning of the original. This has profound implications for any inquiry into human action or mental states. It is profound because explanations of such must be given in terms which do not omit the intentional character. Intentionality may be said to have two aspects and is ambiguous to that extent: it may refer to what people intend to do or it might refer to the scholastic sense of intentionality, reintroduced by Brentano, as a mark of
the mental. Both senses are central in what I have to say concerning the way we understand ourselves now. The first sense is important because questions about what people intend to do are central to questions concerning both human actions and practical reason. The second is important for understanding the way in which features of 'intentional objects' are ascribed.

Intentionality in the second sense is the sense used in the present section. It is so here because I am not, for the moment, concerned with what people do but how what they do is described: this is central to our understanding of the human world. I will follow Brentano's use of intentionality, if only to pin it down a little. As a mark of the mental, Brentano used the word to distinguish the mental from the physical. 7 It had, he argued, two characteristics: firstly that verbs describing activities of the mind such as beliefs, desires and wants characteristically take an object which need not exist. 8 Secondly that such mental ascriptions have a propositional content. This content we may equate with the content of a propositional attitude: it is that which is expressed by x in wanting x, believing x, meaning x, desiring x, intending x, perceiving x and so on. It is this feature of the intentional which allows us to disambiguate 'opaque' reference when pointing, for example, is insufficient. An example might illustrate my meaning more clearly. A person temporarily loses his ability to speak and, wanting to point something out,
points at the centre of the landscape before him. How do we go about discovering what it is that he is pointing at? The answer is by discovering what he wants. We might, in the circumstances, make a list of a number of possibilities and then ask him questions so that he might nod or shake his head. As time passes and the person becomes evidently more frustrated at our inability to guess 'what he means' he gradually recovers enough to tell us that he was pointing at the beauty of the sunset which had now gone. Intentionality is illuminated in the example because we were denied one of the keys to understanding others: knowing what they desired. In this case, knowing that he desired to point to the sunset. The example also serves to illustrate the way in which ascriptions work. The world, including the human part of it, takes any number of different descriptions and in understanding it we need to know what governs the selection of these descriptions. The example showed that a desire underwrote an appropriate description: it did so because it was the desire which gave sense to the pointing gesture. This also tells us something about the way we describe actions, for actions, like a gesture, are ambiguous when we have merely behaviour to go on. This element in intentionality is central to what Quine has written on indeterminacy - what is needed, in order to know whether gavagai and rabbit are synonyms, is not that two people have the same stimulus or indeed that they act the same way, but what their beliefs are. The same
sort of point was made by Davidson, mentioned in the last chapter, that the poor fit between the physical and mental was that the actions or behaviour of an individual could only be understood with any degree of determinacy through an adequate knowledge of his beliefs. 10

4.21 The Intentional and the Historical.

If, however, understanding the situation is a matter of interpreting its intentional features it is not those features themselves which concern us here but their conditions. What is it, that is, which enables us to say, for example, that when I knew what it was he wanted to point out I could see it as well? Obviously this is a complicated question, so I shall try to break it down. One seemingly fruitful way of doing this is to rephrase the question to read: How does A know that the object specified by the content of his own description is the same as the object construed by B's understanding of that description? How do we know, that is, that two descriptions succeed in referring to the same intentional object?

According to Hamlyn and Wittgenstein two conditions of agreement in judgements are, firstly, that there must be an object independent of both observers that makes it possible to point at anything at all and secondly, that there must be some similarity in the physical perceptual systems of the observers. This
condition is supported by the feeling of unease we might have when asked whether a human and an insect—with quite different physiological sense systems—see the same thing. These conditions do not seem particularly controversial, but neither are they jointly sufficient. There must be a further element, at least, which mediates our self understanding and this, I believe, is history. History, by which I mean that whole complex of culture, circumstance, language and beliefs in which an individual is 'embedded', serves to disambiguate what is seen and enables two or more people to 'see' the same thing. It is not, as is supposed by the holders of the causal theory of perception, a 'given'.

4.30 The Case for Objectivism: Hamlyn and Wittgenstein.

I come then to Hamlyn's argument that a public standard is indeed a presupposition of an agreement in judgements.

Hamlyn's case, which I paraphrase very briefly, is as follows: he asks how it is possible that we can claim objectivity in our assertions regarding colour words? Certainly it cannot be based on private experience because if it were, there 'could be no case for thinking that these were judgements concerning colour, since there could be no common understanding of what colour is and a fortiori no real concept of colour.' What therefore
makes what seems to be a private experience a public one is the fact that the object of perception is public and that we are directed to it (or as Brentano would have said - it becomes an intentional object) by a common framework of concepts. These concepts have public criteria and to understand the criterion is to understand 'what constitutes the conditions in which the concept is properly given application', conditions which can be understood by all. Concepts such as red are only examples of a very wide range of concepts and which together provide a conceptual scheme with which we understand the world. The conceptual scheme is based on agreement but is not just conventional for it is embedded in what Wittgenstein called a 'form of life'. This is the background against which our colour judgements and all other judgements are made: it renders both communication about the same things possible and it also makes objectivity possible. In a later article dealing with knowledge about ourselves, Hamlyn adds a further important point about agreement. Here he says that, 'self knowledge is only possible for one who has stood to others in the kind of relation that makes agreement possible...for only by so standing is it possible for him to know what it is to do so.' Objectivity, therefore, about both the world and ourselves, entails standing in a form of life in which intersubjective agreement as to the correct application of concepts is understood. We are,
according to this version of objectivism, essentially social animals and our 'interpretation' is derivative of our understanding of the social context into which we were born.

It is not important, I feel, at this point to argue with the details of the case which Hamlyn presents. What I do want to stress is the form of the objectivist case and the way it links in with what I have said about understanding. The form is Kantian in the sense that it asks how understanding is possible but unlike Kant it locates the 'rules' in society and not in a rational faculty. If Hamlyn is correct then understanding the situation will be a form of submersion in that situation - it will be an investigation of the rules which society has defined which cover our ascriptions. It will also affect our deliberations about that society for they will be embedded in the intentional network of the society itself and not be correct or incorrect by any other standards than those which agreements underwrite. Here it is important to see what I am not saying: I am not saying that our deliberations or our projects which result from them will be restricted by society, but that the very thoughts, desires, intentions and reasons we have are constituted by that society as desires, reasons etc. Research will therefore not be able to stand outside society as the positive sciences had hoped: it will be from within. Needless to say this is of vital importance
if a research scheme is to emerge in the later chapters of this thesis.

The subjectivist need not accept Hamlyn's account of course. Consequently it needs to be supported. To do this it is necessary to go back to Wittgenstein's later work - for it is upon that which Hamlyn's thesis hangs. In Wittgensteinian terms, what is at the centre of the dispute is 'privacy': whether, that is, one can say, without some external standard, that something is the same as something else.

Wittgenstein's main writings on privacy are contained in the Philosophical Investigations, between paragraphs 243 and 315 and since most of his points are made through the use of rhetorical questions I shall try, after quoting them, to bring out their argumentative force.

The discussion begins in 243 where Wittgenstein asks, 'But could we also imagine a language in which a person could write down or give vocal expression to his inner experiences - his feelings, moods, and the rest - for his private use?' This question leads to more: 'How do words refer to sensations?'17, 'In what sense are my sensations private?'18 and 'How do I use words to stand for sensations?'19 The answer, at first simply stated is that, 'When one says "He gave a name to his sensation" one forgets that a great deal of stage setting in the language is presupposed if the mere act of naming is to
The force of the argument supporting this is presented in the next paragraph in which we are invited to imagine someone who writes the letter 'S' in a diary on each day of its occurrence. The question is whether a definition of the sign can be given. Certainly a public one cannot - for the sensation is private. So Wittgenstein suggests the possibility of giving an ostensive definition to oneself, by 'pointing' to it inwardly, or speaking, or writing the sign down while, 'I concentrate my attention on the sensation.' But what is this ceremony for,' he asks, if by the words, 'I impress it on myself' I mean that the 'process brings it about that I remember the connexion right in the future'. The argument thus leads to the solipsistic conclusion that, since, 'I have no criterion of correctness' (apart from my own impression of rightness) then, 'whatever is going to seem right to me is right. And that only means that we can't talk about right'.

The force of this argument, for the subjectivist's position is reinforced by the remark that, 'The essential thing about private experience is really not that each person possesses his own exemplar, but that nobody knows whether other people also have this or something else.' The argument is thus two pronged. The first prong reduces the advocate of private sensations to solipsism through his inability to provide an independent criterion for matching sensations to
marks, sounds or whatever, so that one 'can't talk' of the 'right' use of a sensation word. The second prong, which links with the question of language acquisition, is that in the absence of any outward criterion for the signification of a sensation nobody else has access to its reference. Jointly then the subjectivist position is shown to lead to both solipsism and an inability to communicate. This is finally illustrated in the well known passage about the beetle in the box,\textsuperscript{26} which does not need repeating. It's importance, however, lies in the fact that it does not matter what is in the box for, without anything public to point to, anything or indeed nothing could be said to be a beetle. Applied to sensations, the same conclusion is reached: self referring expressions which point 'inwardly' and have no 'outward' manifestation may be said to be about the same sensation and even agreed to be so, but like the beetles, their reference remains indeterminate. It is the indeterminacy in referential propositions which leaves the private linguist without decidable conditions of truth and this is fatal because it leaves him with insufficient conditions of understanding another person. Put another way; the subjectivist's arguments lead toward solipsism while they ignore the necessity of some form of intersubjectively verifiable public criteria. It is for the subjectivist, as represented by A.J. Ayer, to answer this question and it is to that which I now turn.
Ayer, in a paper entitled, 'Can there be a private language', attempts to refute the objectivism of Wittgenstein and present the subjectivist alternative. In it Ayer makes four distinct points. The first questions the need for an independent 'test for determining that a sign is being used correctly'. Testing must end somewhere and in the end must rest 'on the testimony of my senses' and on my ability to recognize the same again. No process of 'checking', in other words, can establish anything unless some acts of recognition are taken as valid in themselves. This does not, however, meet Wittgenstein's point. The 'idle ceremony' of impressing myself of the connexion between words may well establish what sensations I am going to call the same - and this may presuppose some recognitional capacity which validates for us that they are the same. Wittgenstein's point, however, concerns the conditions of language, one of which is undoubtedly a recognitional capacity, but the force of 'anything can count as right' is only clearly seen in the context of the need to communicate about the same things. That need presupposes a criterion available to the senses of a number of people so that they have a public object of reference and not a private one which is indeterminate. Ayer's point therefore only amounts to a comment about a necessary condition of language - what he ignores is that
what counts as the same is not given to our recognitional capacity simpliciter for what counts as the same presupposes a way of classifying which reflects our interests in classification. In recognizing the same again we implicitly presuppose some intentional feature therefore which is not contained in the sensation. The intentionality of the 'given' objects of perception is grounded in a framework which necessarily transcends our private experience of them. The 'necessity' of such transcendence derives not from our inability to recognize objects but from the fact that we need to agree on what aspect of an object we refer to when we refer. This in turn presupposes a language which matches different intentional contents to different referential intentions. The reason, as I argued in chapter three, is that intentionality is incompatible with extensionality and reference is an intentional term. Its intensional content therefore is only disambiguated by reference to a criterion independent of the privacy of experience and available to others. Hence recognizing the same presupposes a public framework against which what counts as the same is given. This framework is conventional and cannot be part of a sensation.

The same argument applies, with minor amendment, to two of the other points Ayer makes. The first of these is that a hypothetical Robinson Crusoe could name the objects he sees while the second is that a sign does not presuppose, as Wittgenstein assumes, that one must be
able to 'observe the object that it signifies, or at least observe something with which this object is naturally associated'. The most a Robinson could do would be to make a noise in particular 'stimulus conditions' - naming is a convention which presupposes a language which contains the necessary conventions for being able to do it. Similarly with signs: a sign such as '$' says nothing unless a convention for its interpretation exists: then and only then does it act as a sign. A sign which has an indeterminate reference such as one which is unobservable is inadequate as a sign: for what is the point of a sign which points undecidably there. It is as if one possessed a compass that always pointed north in a world in which north moved so that no one ever knew where it was. The compass would be useless for setting a direction - for that presupposes both that the compass points north and that north stays in the same place. Staying in the same place, however, is something fixed by convention. It is certainly not the case that relative to the sun that north is in the same place. What fixes its place is therefore a context which excludes certain other features of the universe bearing on the issue. Such contexts are presupposed by signs of different sorts and it is this presupposition of context which entails their lack of privacy.

4.41 Conclusions.
I conclude that Ayer and the subjectivists cannot sustain their case in the light of Wittgenstein's work on privacy. Fundamental to this is the fact that in order to talk of the same thing there must be something commonly available to observers - hence public - about which agreement in judgments may be reached. The very act of agreement brings with it a certain contingency which transcends the 'natural' but includes what Wittgenstein refers to as a form of life. This includes the context which is itself inextricably historical.

There are two implications of this for educational research. The first is that in order to say just what is happening in any set of human events it is necessary to understand a considerable amount of personal and historical 'background' - a condition of which is an ability to understand that background oneself. Secondly that because there is no 'primary' or 'privileged' position from which to view events, anyone engaged in the interpretation of human actions, as an educational researcher would be, would be forced to take account of the meaning or significance ordinarily given to those events by the people involved in them. Here, of course, a certain degree of privilege might be allowed to the agent whose actions are the products of his intentions - but the main point remains - that even in this case the understanding an agent has of his own actions is historically mediated. The first of these, I shall argue, entails a lengthy description of the events under
consideration which take account of differing and sometimes conflicting points of view. The second breaks down the barriers between researcher and researched in the sense that one's own view of events is always open to further scrutiny and opens up the possibility of dialogue as an important, if not, fundamental, aspect of human enquiry.

4.50 **Putting Objectivism into practice.**

Human life and experience cover a vast area only part of which is relevant to educationalists. In this section I wish to look at the ways in which objectivism might be used to interpret some of the more intransigent features of our behaviour in order to show how wide a scope of understanding it allows for and hence how great its use might be in educational research. I will, however, not be dealing at this stage with substantive proposals but, I believe, that what emerges from the following provides the basis for the practical issues that follow. My main conclusion is, therefore, that objectivism provides the basis for such understanding as is possible at all and, as I have argued, provides a more suitable approach than subjectivism or science. I should at the outset make absolutely clear that I am not attempting to show how objectivism meets all possible cases: what I am saying is that there are areas of human life which are difficult to understand and that while
objectivism helps us to come to some conclusion as to what is actually going on there are also some forms of behaviour which may well remain obscure. I do not see this as a weakness of the thesis, however, but rather as a strength - for it seems to be a relatively straightforward fact about ourselves that sometimes we simply have no answer to questions about either what happened or why it happened. At such times we are quite simply, and properly, puzzled.

As an introduction let me recap a little. Objectivism, basically, is the stance that individuals are understood against the significance given to them or 'conferred' upon by a community of individuals whose life is, of necessity, temporal. Meaning is thus 'historical'. Subjectivism, on the other hand, attempts to ignore the historical and focuses on the way an individual understands the world he or she finds him or herself in. Given this it would seem that certain aspects of human existence escape 'historical' significance. Broadly speaking these include all those private goings on which everyone experiences. What is at issue here is thus a question which concerns the extent to which objectivism can claim validity in educational research. It is saying that there might be areas of human life which are so difficult and complex to understand and for which there are few, if any, public pointers, that objectivism gives out and cannot explain them. An example might aid my explanation. Any human action might be considered a mask
for another action. Indeed, there is a sense in which we all wear masks which vary with our social situations - at home, in front of the children, at school and so on. A very practical problem for objectivism is thus the simple one that the public criteria by which we ascribe actions are misleading. They do not, however, 'tell' us either that they are misleading or in which ways they are misleading. We might thus attempt to deal with several layers of being - the 'exterior' and the real goings on. Good literature has always done this - not only in exposing the masks people wear but in making us reflect on our own masks. Not attempting to look behind overt and public 'criteria' leads to what one might call naive objectivism: it is a form of objectivism which takes everything at face value. My argument in this section is that to be of practical use objectivism must not do this, it must, mainly by the utilization of context, attempt to see actions for what they are even though this might not agree with what agents say they are and so on. As part of the attempt to 'see' beyond overt and public actions we must look at various areas of human experience which are either beyond our visual grasp (the 'inward') or ambiguous for some other reason. In order to do this I will examine five different areas as examples not only of such difficult areas but also as examples of how objectivism might begin to tackle them. The areas are as follows:
(a) Unexpressed, private experiences which might include emotions such as grief, remorse and guilt or simple sensations such as twinges and tickles.

(b) Expressed, private experience such as the expression pain, the expression of anger and so on.

(c) Behaviour which is uncontrollable such as blinking in a sand storm, fleeing from danger or the simple knee jerk and feigned copies of them.

(d) Intentional behaviour in the sense outlined earlier; that is behaviour having some sort of reason, desire or belief behind it which gives it both meaning (to the individual performing it) and motivation.

(e) Behaviour which is defined by convention such as 'scoring a goal' or performing a curtsy.

Since objectivism relies on the existence of a public object some of these are problematic. Broadly speaking a chart could be drawn up with the least problematic at one end and the most problematic at the other. It will be immediately apparent that those actions which are (i) overt and (ii) defined precisely will be the least problematic while those which are (i) unavailable to the public and (ii) ill defined will be the most problematic. There will of course be many
combinations in between.

Since this section deals with practical limits I will concentrate on the problematic end of the scale. The reason for this is that since it is understanding we are seeking, those actions which are both public and well defined present no obvious problems. I will not, therefore, discuss (e). Intentional action, however, presents problems which arise in the form of some form of self deception or lies. Uncontrollable responses such as knee jerks or epileptic fits do not present many problems provided that they are recognized as such - that is they are not feigned. When they are, there is a problem with regard to their recognition. Categories (a) and (b) are the most problematic because, by their very nature, they contain either, in the case of (a) some aspect which is unavailable, or, in the case of (b), a difficult 'link' between overt behaviour and that which underlies it. A brief overview thus shows that at least some aspects of normal human life, areas such as lying, grieving and uncontrolled responses present practical problems for the objectivist and are of a type which concern educationalists.

In what follows I shall attempt to take some of the sting out of such problematic areas by showing that even the most problematic aspects of human experience can be understood providing a good deal else is known about both the individual and the historical context (which includes social expectancies, rules, 'mores' as well as
what is generally accepted as 'history'). I will discuss the areas in turn as follows:

4.51 Problems concerning uncontrollable behaviour.

4.52 Problems concerning the interpretation of overt expressions - such as cases where some sort of self deception or lying are involved.

4.53 Problems concerning the interpretation of 'inward' experience and its outward manifestation. I shall use pain and grief as contrasting examples.

4.51 Problems concerning uncontrollable movements.

I think that the first point to be made in this section is that the problem facing the objectivist is not whether there are innate mechanisms which give rise to uncontrollable behaviour but how to correctly characterize these where it is proper to do so and to distinguish them from behaviour which is simply a pretence. The reasons - in the context of understanding human behaviour - are of tremendous importance since we do not, and should not treat the two types of behaviour as the same: uncontrollable behaviour is mere movement, whereas feigned 'uncontrollable' behaviour is not only intentional action but is, or may be, an intention to deceive. The differential treatment these deserve is
fundamental in many areas of life but is seen most clearly in courts of law where decisions concerning the intentions of an agent are often taken as paramount. The fact that non-intentional behaviour does not carry imputations of blame and so on can also be an important motive for pretending that one's actions are actually uncontrollable. An example of this would be where a murder resulted from what appeared to be an epileptic fit. If this act were committed by a person known to suffer from epilepsy it would constitute an extremely difficult case. Disruptive behaviour in the classroom is sometimes of this type: the school has to decide whether the behaviour is intentional and fully voluntary or whether it stems from some involuntary inner mechanism. It needs to know this not only for reasons of educational research but also to determine what remedial steps should be taken. Often, as is well known, 'professional' psychologists are called upon - but the grounds upon which they base their decision are dubious if, that is, they rely on the methods of a human science discussed earlier.

For the objectivist as well there seems to be a problem - overt, public behaviour cannot be interpreted simply on the basis of the behaviour itself but must include some biographical material which is unavailable. The question is obviously complicated by the possibility of deception - in many law cases the crucial questions facing a jury concern neither behaviour nor intention but
the credibility of the person who says that the intention was lacking.

Not all interpretation, however, is a species of deference. I mentioned that a jury's main concern is often with credibility. This seems to be central to understanding others in areas where some doubt has been cast over the origin of action. In certain but seemingly few areas of human behaviour it seems incontrovertible that a reflex has occurred. Where there is no controversy there is no problem for the objectivist: the action is classified as reflexive. It is the grey area between the incontrovertibly reflexive and the honestly intentional, which is problematic. To a certain extent deference (in the sense that the epileptic is referred to the proper medical authorities) occurs in the former but not the latter.

If credibility is central to normal interpretation we are forced to ask just what is involved in it. Before this, however, it is necessary to indicate a few of the 'grey areas' of human life which call for this form of understanding. They range, I would think, from cases in which people have been subjected to indoctrination and torture to rather insignificant habits and 'quirks'. At the former, extreme end we interpret behaviour in the light of our knowledge of past history. A child, for example, who consistently cringes at the mere sight of a lifted hand might well have been the subject of physical abuse. The verification of such abuse
adds to our understanding of his behaviour and gives credibility to his explanations. This might be significant in understanding withdrawn or disruptive children: the explanations they give of their 'actions' might be found more, or less, credible in the light of their history. The same applies, but with decreasing explanatory value, with less serious behaviour such as flinching at the sound of a loud voice. The main point however, and the one I want to metaphorically underline, is the mere fact that in attempting to understand certain forms of behaviour we attempt to square the behaviour we observe both with explanations given and with the history of the individuals concerned. This having been said it is also by no means clear that finding something to be credible is any form of guarantee that the behaviour is either correctly explained or described. One reason for this is that individuals might have reasons for hiding their real intentions and consequently lie about themselves. This is the next problem for the objectivist.

4.52 Lying and Self Deception.

Finding someone's explanation to be credible appears to entail finding that his avowals are not, to the best of our knowledge, lies. There are, however, quite credible criminals who lie. The 'best', it might be suggested, are those who lie convincingly. A convincing lie, if what has just been said is itself credible, is
one in which lies are found to square with explanations and past history. The question thus shifts from whether avowals about actions square with the past to whether there is reason to doubt avowals in the first place. The problem for the objectivist is again not whether some overt behaviour has taken place, for this is not in doubt, but whether the behaviour is to be taken at face value or not. Not to do this, as I explained earlier, is to commit the fallacy of naive objectivism. The fallacy is in taking defeasible criteria as if they were non-defeasible. The credible liar is the most difficult case here because his lies about his present (or past) actions appear to be consistent with other aspects of his life. Given no further information than that everything about a person's life appears to be consistent would not in itself lead to the further question as to whether the person lied. The question about lies, especially in the case of the credible liar, arises ex hypothesi, not from any discrepancies in his life history but from some further reason which must concern motives.

If, however, it is the case that a speech act (a lie) is misinterpreted as another (telling the truth) because it is unnoticed on the grounds of consistency then this seems to indicate that too small a portion of a person's biography has been taken into account. The match between lie and life history is only possible given a particular portion of that history. Motivation, on the other hand, relates to major projects which might take
decades to unfold. Credible liars such as criminals with perfect alibis remain undetected until their larger, criminal, motives are exposed. In the light of these, lies become both credible and explanatory. Indeed, they become helpful biographical material in themselves.

Lying, even where it is difficult to detect, is not, consequently, an intractable problem for the objectivist. If it is a problem at all it is either because the lies are consistent with sections of biographical material or because larger sections are not taken into account. The main implications, however, like those of the previous section point towards a fuller understanding of the individual and his life history.

Self deception is unlike lying in that the agent is unaware of the mismatch between what is said or done and other biographical material. Lying is an intentional act whereas self deception is not, or does not seem to be. Awareness, in the case of the liar, is an awareness of the falsity of a piece of behaviour. It is difficult to find a parallel to this conscious intention in the case of self deception. This makes the matter much more thorny. It cannot, for example, be argued that self deception is exposed through either an understanding of past history or of projected futures. Self deception is necessarily beyond the conscious awareness of the individual concerned unless some verbal trickery is employed to suggest that the self knowingly deceives itself. In the absence of the 'other' self, however, it
is difficult to see clearly just what self deception involves. It is like successful lying in that someone is deceived and unlike it in that it is unintended. If it parallels anything it thus parallels unintended but nonetheless false assertion or avowal.

Ordinary usage gives some insight into the kind of falsehood at work here. People often use the phrase to denote someone who has misjudged his possibilities: an athlete, for example, who is reasonably able at a local level but who thinks himself capable of the Olympic Games. The kind of self deception involved is a misjudgement in terms of oneself and the world. The outcome is a false expectation about one's projects. It results in a set of 'unreasonable' actions. These actions are only 'unreasonable' in the light of the past as presented against the future; past athletic success at school leading to expectations of Olympic Gold. This archetypical self deception involves no problematical division of the self suggested by the notion of one self keeping something from another self-same-self. Instead it is a fairly unproblematical lack of accurate appraisal on the part of the individual. Put this way it should be fairly easy to see both how it fits in with our previous 'problems' and with the way around them. If self deception is an inability to formulate reasonable goals for oneself on the basis of past experience then understanding self deception will involve matching biographical material with real-world possibilities.
Mismatching by individuals will reveal - to them - the type and extent of self deception involved. The solution then to what seemed a completely subjective problem is in fact to be found in the observers rather than the observed for it would seem that the main problem of self deception includes a lack of awareness of the possibilities the world offers. There is here, consequently, a case not only for an adequate biography of the individual and the historical context in which they live out their lives but also for the considerable importance others have in forming reasonable expectations within that context.

The more problematic type of self deception presents a more thorny case. This is made more difficult by questions about self deception itself. I have argued that at its most problematic, self deception is unlike lying in that it is unintentional. While there is controversy over this - and I have admitted that there is - I do not see quite how the objectivist deals with what, for all intents and purposes is an unintentional 'action' or 'belief' except as a piece of unintentional action or belief. That is, objectivism, must regard 'actions' carried out on the basis of 'beliefs' which appear to originate in consciously intentional mental states as the same as involuntary actions: that is as pieces of behaviour which do not carry with them elements of blame, responsibility and so on. Children in schools who feel that they need do no more work for an 'A' level on the
basis of some rather poor GCSE results need their self
deception put right. Children who, in the light of their
previously poor academic performance, insist that they
need do no further work to obtain an 'A' level and who do
not consciously doubt in their ability appear to be
deceived in a more problematic way. One problem is to
differentiate between the two. The other is to treat the
second case as if the belief and the action (taking the
'A' level) were involuntary and not irresponsible
behaviour. Educational policy regarding the first is
basically pointing out the discrepancies between
achievement and prospects. Educational policy regarding
the second is of a totally different order and appears to
involve some form of therapy in which the mismatch
between perceived prospects and real prospects are
revealed. The first appears to require some form of
rational explanation, the second an unlocking of some
unknown psychological factor. It would thus seem that,
again in the second case at least, there are grounds for
referral outside the type of counselling to be found
within normal educational provision.

Thus, while objectivism is basically a theory
which explains how meaning is attributed to actions (or
other things and events) it is of use in areas such as
lying and different kinds of self deception in that it
pays attention not only to the 'markers' that defeasibly
identify actions as actions of a certain type but also,
in taking social and biographical material into account,
allows differentiation between types of action and behaviour to take place. These, as indicated in the previous paragraph, may then form the basis of future action.

4.53 The Interpretation of Expressed and Unexpressed Experience: Pain and Grief as examples.

In looking at the problems facing the objectivist over 'inner', and possibly unexpressed experience, a good place to begin seems to be with Wittgenstein's phenomenological approach to pain language. The central question, as I see it, is how we know that someone is in pain when we cannot experience that pain? To the subjectivist this is an unanswerable question, or, to be more generous, a question with an indeterminate answer. The notion of an 'external criterion' however, helps to grasp the objectivist's solution: it is, as we said earlier, a public, but nevertheless, defeasible criterion. Put simply, if I understand another as being in pain, I do so on my taking certain behavioural manifestations as characteristic of pain. Defeasibility enters the picture because of further understanding such as feigned behaviour and so on. This element, as Wittgenstein points out, is not a complicating feature in the case of animals - although it is certainly true that 'playing dead' is a defensive action utilized by some. The important link between Gadamer's assertion and Wittgenstein is that understanding that another is in
pain is mediated by certain 'characteristic' behaviours which are taken as indications that someone is in pain. This is the point at which history enters what is otherwise a private experience. History provides the means of interpreting the behaviour even though the sensation of pain is, one might say, a-historical. It was on this basis that I argued in the final section of chapter three that psychology cannot quantify, trans-individually, pain reactions: it cannot assume a universal reaction to given 'doses' of pain. Where it does, it simply begs the issue.

More important now, however, is the need to see just how history mediates our empathic understanding; for it is not simply that we transfer our subjective understanding to others. What intervenes is a learnt interpretation of the way we express ourselves and the way we take the actions of another. This intervention or mediation is what I have referred to as historical: it rests upon the arbitrary and contingent rules of interpretation which we become initiated into as we grow into an historical community. Stated as baldly as this, a number of important questions are raised. Firstly we might ask whether there are not some behavioural manifestations which are not contingent upon historically derived rules. Secondly we might ask whether there is something 'more fundamental' than interpretation to understanding - interpretations which are grounded in non contingent factors such as our 'nature' or our innate
mechanisms.

As for the first question there does seem to be a prima facie case for supposing that there are 'universal' forms of behaviour which develop 'out of' our physiological make up. As far as pain and its expression are concerned, I have no doubt that an excess of it results in fairly universal forms of behaviour. This, however, is not to contradict what was said in the third chapter - for our own 'threshold' - the point at which the experience vents itself in outward anguish is characteristically peculiar to the individual. This might be infuriating to the torturer and torture to the positivist but it need not present the objectivist with any particular problems. Indeed - given the autobiography of the individual and some insight into the historical circumstances within which the individual is living - it is possible that more information of explanatory value is available than would otherwise be the case. A simple example might suffice to underline this central point. In traditional African initiation rites the adolescent is expected to show no 'overt' reaction to the infliction of pain because the lack of reaction is taken to be a sign of maturity. The same effect (though different circumstance) is seen in the unwillingness of prisoners to give vital information under extreme forms of torture. In both examples the historical circumstance is at least as important as the biography of the individual in explaining the expression of pain. The same is true in
interpreting forms of behaviour which resemble the expression of pain where no pain is either inflicted or experienced. This is true in various societies where the overt expression of 'pain' is expected - as in certain African funeral rites - because the absence of such behaviour is taken as insulting to the dead and to relatives of the dead.

The conclusions which can be drawn from this are twofold. Firstly that, in extremis, pain will be expressed in behaviour which is virtually universal and that this expression will be recognized for what it is by any member of the human race. This expression of pain is as near as one can come to the a-historical and the non contingent. It - like the knee jerk - is involuntary. However and secondly, most pain is (thankfully) not of this order and is subject to the intervention or mediation of both the character of the individual concerned and the social circumstances in which pain is experienced. In a multi-cultural society it will be the second of these which the educator will encounter and so it will be to the individual and his culture that the educationalist will be best advised to look to understand what is happening.

Grief might be said to resemble pain in that it is an internal experience (albeit an emotion) which may or may not take an overt expression. It is unlike pain in at least two ways: it is not as easily identified as the internal experience it is, as pain, and it does not seem to
have an extreme form of expression which is universally identifiable. It shares with pain the necessity of understanding both historical and individual circumstances. This is shown by contrasting the expectancies in Irish culture - where 'keening' is a socially acceptable and, indeed, expected way of showing grief - and English culture where the 'stiff upper lip' stifles any 'show of emotion'.

One result of the fact that grief itself is not readily identifiable to the individual experiencing it is that it is often others who interpret their behaviour for them in the light of their historical circumstances. This is an interesting observation when it is put against the fact that grief is expressed differently at different times by the same individuals. Certain forms of behaviour, that is, cannot be said to be expressions of grief unless much more is known about the individual. What more needs to be known, is of course, rather indeterminate, and, in any case related to particular circumstances. This rules out two factors: (i) the possibility of drawing up a defeasible set of rules which, when appropriately applied, reveal that someone is suffering from grief and (ii) setting down the criteria which define grief behaviour.

This lack of precision need not deter the objectivist - it is perhaps merely an admission of the complexity of the human subject - for it points towards a more complete understanding of both the individual and
the historical circumstances of their lives before anything of value can be said about human events and their significance. This appears to require something more extensive than the 'atomic' bits of data beloved of the positivist. It also seems to require a complete reappraisal of the methods of both observation and evaluation. I will suggest in the following two chapters that the first of these requires a narrative and that the second a dialogue. For the moment, however, I will confine myself to a more embryonic concept - that of a framework of understanding.

4.60 Frameworks of Understanding.

A framework - which includes the general context of actions, the intentions which are self ascribed, the admitted desires and so on, together with what MacIntyre refers to as the historical context - is merely a way of mapping out some features of the human environment which are pertinent to understanding another's activity. As a framework it is more something to be worked within rather than some calculating machine for deciphering other people. That, however, is not to say that it lacks importance or that it is somehow inadequate. Adequacy is relative to a task and the task here is merely setting the scene. It is important insofar as it is capable of dealing with the following problem. Let us say that a person says that they have a pain at their heart. How are
we to understand what they mean? What they mean is more easily seen when more contextual information is added. If, for example, I supply four pieces of extra information about each individual, the meaning of the single statement becomes disambiguated in a way which renders understanding possible. The four pieces of information are: (i) I am depressed, (ii) I suffer from angina, (iii) I have just eaten my dinner too quickly and (iv) I have been stabbed.

Unfortunately the human condition is far more complex. If, following up the example of grief, I become drunk after my child has been run over in a traffic accident, can I conclude that my drunkenness is an avoidance of grief or an expression of it? The answer is, I think, indeterminate. What makes it less determinate is a fuller picture of the person. In this 'fuller picture' it will be relevant that the person is normally drunk in the evenings, has never been drunk before, is of a quiet disposition and not given to public 'displays', is introverted and normally unable to express himself to others and so on. Indeed, even the person who is, shall we say, drunk, may not be able to state categorically that it was because of his child's death or that it was his way of expressing grief. These observations point to a level of interpretation in understanding others which is limited, ultimately, by a certain indeterminacy. This, however, in a non-positivistic context, is not necessarily either a bad thing nor a hindrance: it is
part and parcel of what it is to be human and to live with and alongside others.

The example of grief is again instructive. Grief might be expressed by repression but its actual expression might be in a long series of angry fits. A person who does not repress grief might weep each night, another, however, might get drunk and after that into a 'punch up'. The point here is that while there are fairly unambiguous criteria for 'punch ups', 'getting drunk', 'weeping' and 'bottling it up', there are no direct ways of saying or telling which of these, if any, are the results of grief. However, Wittgenstein, Davidson and MacIntyre are at one in providing a 'framework' in which understanding that someone's behaviour results from grief and not from a myriad of other causes or reasons is found. It is within these frameworks, I believe, that the actual behaviour of individuals becomes understandable. I quote from each to show what I have in mind. Wittgenstein says,\textsuperscript{34} for example, that

\begin{quote}
what determines our judgement, our concepts and reactions, is not what one man is doing now, an individual action, but the whole hurly-burly of human actions, the background against which we see any action.
\end{quote}

In similar fashion Davidson writes,\textsuperscript{35}

\begin{quote}
There is no assigning beliefs to a person by one on the basis of his verbal behaviour, his choices, or other local signs no matter how plain and evident, for we make sense of particular beliefs only as they cohere with other beliefs, with preferences, with intentions, hopes, fears, expectations and the rest.
\end{quote}
Finally, MacIntyre, who says,\textsuperscript{36}

Once we have understood its importance the claim that the concept of an action is secondary to that of an intelligible action will appear less bizarre and so too will the claim that 'an' action, while of the highest practical importance, is always a potentially misleading abstraction. An action is a moment in a possible or an actual history or in a number of such histories. The notion of history is as fundamental a notion as the notion of an action. Each requires the other.

4.70 \textbf{Conclusions.}

In this chapter I have attempted to make out a case for an objectivist, as opposed to a subjectivist, approach to understanding human behaviour. I have done so because I believe that a non-scientistic approach has to philosophically appraise its epistemological base. The two stark options which I have presented are admittedly idealized but I believe that the substantive points which form the conclusions remain. These are firstly, that the world of human actions is largely constituted by our agreements about them. Secondly, that even though human actions are typically intentional (although some may be characterized in conventional terms as well) they nevertheless have an accessible public aspect which is the basis of our judgements about the behaviour of others. Thirdly, that since it is within the historical context that agreements are made it is towards history that we must direct our attention when seeking an understanding of human behaviour. Finally, that in all but the most elementary cases human behaviour is
understood within the biographical context that both personal history and social history provide.

The implications for research into educational matters is not, at this stage, immediately apparent — nor is it meant to be. If anything can be said it is that whatever educational research might be like, it requires some method of providing an adequate basis for understanding the phenomena with which it deals and that it (the research itself) is a corporate part of the constitutive historical context in which it takes place. I have made some embryonic moves towards 'unpacking' the implications of the first of these in talking about 'frameworks of understanding'. I shall elaborate this in more detail in the next chapter. The second is an inescapable context in which everything else is embedded. To say this is not only to say that research partly constitutes its objects by virtue of its being a part of history itself but also that it needs to be reflexively aware of its constitutive role. This partly entails the non-reliance on so called experts since the very contingency of what we say about ourselves and others is dependent upon the corporate agreements we have made.

In sum; educational research requires a data base which allows for a considerable amount of historical and biographical material. It is itself part of history and needs to be aware of that and of the context which it itself imposes on the objects it studies. What this amounts to in practice is the very important role it
gives to actors' self descriptions or to the descriptions given by others well placed to comment. It consequently is in opposition to that school of social theory which seeks to give a more adequate description and explanation of events by subsuming them under some category or other which derives not from the actors themselves but from some technically derived theory thought to cover all societies and actions. The reason for this is that in the absence of any foreseeable nomological framework such a theory would be baseless. It is also is in opposition to any educational theory which places too high an emphasis on any particular body or group as having privileged status. What follows is therefore not to be seen as an attempt to form the basis of a social theory but an attempt to deal with social and individual problems in the absence of such a theory.
Footnotes: Chapter Four.

(1) P. Feyerabend, 'Science in a Free Society', Verso, 1978. He says, for example, 'Conceited and intimidating scholars, covered with honorary degrees, university chairs, presidencies of scientific societies are tripped up by a lawyer who has the talent to look through the most impressive piece of jargon and to expose the uncertainty, indefiniteness, the monumental ignorance behind the most dazzling display of omniscience: science is not beyond the shrewdness of the human race. I suggest that this shrewdness be applied to all important social matters which are now in the hands of experts.' p98.


(3) Ibid, p237.

(4) Ibid, p237.

(5) Ibid, p236.


(7) Jenny Teichman makes an interesting point to the effect that some mental descriptions such as suffering do not take objects while some conventional descriptions such as owing and promising do. ('The Mind and the Soul' RKP p94) The point however seems to be related to the intimate connection between mental ascriptions such as 'desiring x' and the action which it disambiguates: conventional actions are 'rule covered' but still take an intentional description because they are the performatives of intentional states; the action which results from some desire, belief etc. It is because of this that normative acts characteristically take two descriptions: the intentional and the conventional. It is also an interesting philosophical point as to whether the intentional is a necessary condition of the ascription of the conventional description. Is the ascription 'married', for example, properly ascribed to a couple who, for say medical reasons, do not know what they are doing? Certainly this distinction is made in law for a murder is not a murder without the intention to kill.

(8) F. Brentano, 'The True and the Evident', RKP, 1966. Here he says, '... by an object of thought I meant what it is that the thought is about, whether or not there is anything outside the mind corresponding to the thought'. p77.

p29f.

(10) D. Davidson, "On the very idea of a conceptual scheme" in 'Inquiries into Truth and Interpretation', Clarendon, 1984, where he says '...a man's speech cannot be interpreted except by someone who knows a good deal about what the speaker believes (and intends and wants)...' p195.


(12) ibid, p100.

(13) ibid, p101.

(14) ibid, p103.


(17) ibid, para 244.

(18) ibid, para 246.

(19) ibid, para 256.

(20) ibid, para 257.

(21) ibid, para 258.

(22) ibid, para 258a.

(23) ibid, para 258b.

(24) ibid, para 258c.

(25) ibid, para 272.

(26) ibid, para 293.


(28) ibid, p256.

(29) ibid, p257.

(30) ibid, p259.

(31) ibid, p258.

(32) ibid, p258.
(33) I admit that this is by no means an adequate account. However, as in other cases, to do the subject justice another thesis is required.


(35) Davidson, 'Mental Events', op cit, p221.

(36) A. McIntyre, 'After Virtue', Duckworth, 1981. p199.
5.0 Narrative as a Data Base.

Although the main focus of this chapter will deal with narratives I want to use the opportunity of an introduction to 'tie up' what I have already said and what I am about to say. It might appear that the intention of this thesis is negative or destructive. This is not the case although I can see a number of reasons why it might look that way. The first is that I have been rather sceptical about the fruitfulness of following a scientistic line of thought. The second, which will become more obvious, is that I am not going to attempt to provide an alternative theory. The reason for my scepticism over the first of these is, as I have shown,
that it is is fraught with philosophical difficulties. I could say that the second follows from the first if, by 'theory', one meant a nomological framework built upon the observations and experiments of scientific investigation. In this case one would not expect me to provide such a theory. If, however, the word 'theory' is taken in a wider sense - perhaps the sense used by Paul Hirst, John Elliott and others - one might reasonably expect the remainder of the thesis to provide the basis of some alternative. This is not to be - partly because the word theory does have the scientistic overtones D.J. O'Connor imputes to it - and partly because I am not sure what to make of the notion of untestable, unfalsifiable theories. I accept that my critics will accuse me of using the word in a positivist sense (and also that science itself is essentially positivist) but I see nothing but confusion arising from using words with scientistic overtones for work which is meant to to be in contrast to science.

The contrast I wish to make is nothing more than the contrast Aristotle makes between Science and Practical Wisdom. Theoria belongs to the former, Phronesis to the latter. It is the application of Science to Education in this sense which I have rejected. Phronesis, which I loosely interpret as the activity of mind intent on resolving some practical problem, does not involve a theory developed through science (which is O'Connor's notion) but an understanding of what might be
called the human geography in which the problem presents itself and ways of resolving the problem given that geography. In educational terms, therefore, what is needed is not a theory but a means of achieving certain ends. Educational 'theory', on this model, is not a species of reflection but of rational action. What follows is therefore more a method for resolving educational problems than a set of proposals for understanding what is going on in education. The reason for the latter is that it already exists - or rather - various opinions as to what is going on exist. The problem is not so much what is going on as how to change what is going on to what we would like to go on. The question, at root, is therefore both practical and ethical.

Having said that, I am now going to virtually contradict myself by saying that the contents of the present chapter deal almost exclusively with what is going on. This needs some explanation. Firstly, as Aristotle pointed out, any practical judgements will involve an assessment of the situation in which action is to be taken. The 'situation' is 'what is going on'. Secondly, the attempt to say what is going on raises philosophical difficulties of its own. The first of these is illustrated by the sports writer whose descriptions of a football match are invariably disputed from supporters of either side. The second, more thorny issue, is the question concerning what settles the apparently
conflicting descriptions. This is, in brief, the type of question I attempted to address in the previous chapter. I hope that now its relevance will be seen because the answer given is not that there is one trans-subjective answer to be given, but that what there is is partly (I am not advocating an Hegelian Idealism) constituted by the presuppositions and agreements over the way in which what there is is described. This I have called, because of its ongoing nature, historical.

There will be, if the points I have made about the objectivist position and the sports writer hold, two distinct reasons why descriptions of what goes on vary. The first has its roots in historical agreements and the second in interpretations and misinterpretations of intentions, reasons, purposes, conventions and so on. If I am right the second of these presupposes the first. To avoid confusion I should point out that the first - constitutive factors - do not correspond in any way to either cultures or societies although they might. Social reality is constituted differently within the same language and culture (depending on the definition of these). Illustrations of this abound in the areas of political ideology and religion. Marxists and capitalists give completely differing accounts of what is going on in society as do theists and atheists. At a less extreme level managers often have a different perspective on events to workers. Needless to say these differing views will affect what is said about what is happening in
education. They are, however, different in kind (although the distinction becomes very blurred at certain points) from descriptions which differ because of an inadequate understanding of someone's intentions or the rules which govern a social practice.

To recap a little before proceeding with the main arguments; I have said that I am concerned more with deliberating about what to do in education than with theories about it and that such deliberation involves an understanding of the situation within which one is working. This, however, presents an initial problem for there are (for at least two reasons) differing accounts of situations. To help with this problem I have argued that from an objectivist position the only arbiter available in deciding between accounts is the historical community whose contingent decisions constitute the very meaning of correctness and correct application. This provides the answer to the question, 'What is the criterion by which correct descriptions are said to be correct?' If, therefore, the attempt to come to a practical solution presupposes an understanding of the situation, but descriptions of situations are complex matters, it would seem that some vehicle with the capacity of taking on board such complexity is required. This, I suggest, is a narrative. I suggest it not as a necessary condition of practical reason for in everyday affairs our memory acts in its place. I suggest it as a pragmatic equivalent of memory where the affairs to be dealt with
are of such an order of complexity that memory alone is inadequate and, in a social context such as education, too liable to individual bias. There is a sense therefore in which what I have to say goes against two main trends in educational 'theory' - the first being atomistic and scientistic and the second, perhaps more suprisingly, the efforts of those individuals in the past who have attempted to write an educational theory out of their own experience. There is, however, a more fundamental reason why individual bias is to be avoided and that is the reason provided in the preceeding chapter: communities constitute meaning and therefore are the arbiters on any individual view of what is happening in education or any other social field. What the community says about education is therefore of first order importance in describing educational situations. This alone, however, does not either provide an argument for a narrative as a data base for educational research or solve all the problems which arise if one were to be written. What it does seem to suggest is that nothing of any great social or educational import is going to be said without fairly large imput from the historical community in whose history education takes place. This point I hope to make central in my main substantive conclusions in chapter seven. For the moment I move to arguments which do seem to point towards a narrative as an adequate form of data base. I will then tackle some of the benefits and problems which such a basis raises. To
simplify matters I will begin with a chapter breakdown.

Thus far I have focused on the more general concerns which link it with both the previous work and future chapters.

I now want to narrow in on the theme of the present chapter itself. In order to do this I have included a brief introduction which explains why I consider the rest of the chapter important. The chapter is therefore divided up as follows:

5.01 An introduction.

5.10 Narratives as possible vehicles for contextual information.

5.20 The usefulness of narratives in isolating causes or giving explanations.

5.30 The need for Pragmatics.

5.40 Objectivity.

5.50 Conclusions.

5.01 Introduction.

Before I can focus on the topic of this chapter
it needs to be given a place in the thesis as a whole. As
is indicated in the title, the narrative is regarded as a
data base: it is the place where the basic information is
recorded. Most of this chapter will deal with either why
a narrative is a suitable vehicle for this record, how
narratives might be used in giving explanations and some
of the problems arising around the general area of their
objectivity.

   A data base, which, it will be argued normally
involves a collection of contextual material, usually
comes about as a result of some identified problem. What
generates this 'problematic' may be various. Having
collected information which is pertinent to the problem
and its solution, most educational research will then
involve some form of reasoning process in which the end
point will be some policy intended to resolve the
problematic. Briefly then, the present chapter will deal
with the collection of data while Chapter Six will deal
with the reasoning process.

   The arguments contained in the present chapter
are based on the assumption that 'collecting the
information' is no simple matter and that, mainly as a
consequence, a considerable amount of contextual material
is required. It is, in a sense, a secondary argument that
narratives are appropriate means of recording this
information, as a person with an extraordinary memory
could perform the same function. There is no suggestion,
therefore, that educational research requires, as a
condition, a written narrative nor that large volumes of context are required in all educational research.

Before I continue I will give three reasons why contextual material will be involved in most cases. Firstly, simple human actions such as digging a garden, bullying, disrupting a lesson or being assertive are not as easily identified as it would seem. The same applies to what people say - what words mean (in general as well as in the context of human actions) - is dependent largely on context. Secondly, where an educational researcher is trying to do more than just describe what is happening, that is, where he is attempting to give a cause or an explanation, he will again find the need for a certain amount of contextual material. Thirdly, when a researcher is faced, as one would be in educational research, with attempting to give an objective account, it would not be long before the researcher would realise that a number of different accounts of the same events become possible. This may be because of various perlocutionary influences on the people giving information or it might be more to do with ideological preference (see 5.40f). In either case the attempt to become more objective would involve other points of view.

This brief introduction allows me to explain the role various parts of the chapter play in attempting to deal with these issues. 5.10 looks at arguments for the importance of context in understanding what is going on. 5.20 looks at how a large amount of contextual material
is helpful in isolating causes or giving explanatory accounts. 5.30 looks at the reasons why semiotics (in particular pragmatics) might be helpful in reading a text correctly and 5.40 looks at some of the problems of moving towards objectivity.

5.10 The importance of context in understanding human events and the possible use of narratives as a record

One of the more persuasive arguments for the importance of context in understanding human actions and therefore as the basis for understanding and describing situations is that individual actions can only be fully understood or made intelligible when placed within an adequate biography. This was the central point I was trying to make at the end of the last chapter. We need, to be succinct, a way of making tangible those intangibles which together constitute the framework which enables us to understand others and ourselves. In order to make the point we need to narrow the focus of attention from the broad perspective taken in chapter four to the narrower one in which the focus becomes what individuals are doing and the criteria by which the way their actions are described are decided.

Despite MacIntyre's comment that, 'analytical philosophy...(has a)...tendency to think atomistically about human action and to analyse complex actions and
transactions in terms of simple components. Analytical philosophers have indicated the importance of context for a fuller understanding generally and for the understanding of what we say and do in particular. (I am here subsuming speech under action) Strawson, for example says,

To know the meaning (or meanings) of a sentence...is to be at least partially equipped to understand how any serious utterance of it by a particular speaker in a particular context is to be taken...(and)... sentence meaning alone, without help from the context of utterance, will rarely reveal just at what points the general concepts which figure in a proposition are there conceived as attaching to the world...

Similarly Frege stresses that where the context is not known, (if, for example, an utterance is spoken on stage)

it must still always be asked, about what is presented in the form of an indicative sentence, whether it really contains an assertion. And this question must be answered in the negative if the requisite seriousness is lacking.

H.P. Grice, in attempting to give an adequate account of how successful communication was possible, supports these general remarks by suggesting that, "for x to have meant anything, not merely must it have been uttered with the intention of inducing a certain belief but also the utterer must have intended an 'audience' to recognize the intention behind the utterance.' This he updated in 1971 to read, 'U intends to produce in A effect E by means of A's recognition of that intention'. In so doing he builds into his account the features felt to be necessary by Strawson and Frege. This analysis has been extended by John Searle in his work on speech acts, itself an extension of J.L. Austin's work, so that the
conditions presupposed by successful performance are themselves brought into the open. Of particular interest are the 'preparatory' conditions which involve both utterers' beliefs, intentions, expectancies and hearers' understanding of these.

These philosophers provide grounds for the thought that any 'data base' which is to provide the basis for human research must be of a form which is such that the conditions necessary for making actions understandable, or, 'intelligible' as MacIntyre puts it, are met. That is, they must include contextual material which goes far beyond the utterance, whatever its intention and include hearers' expectancies, interpretations, the illocutionary and perlocutionary 'forces' at work in the speech context and so on. These conditions may only be met within a longer tract in which the intentions of the agents together with contextual information surrounding their utterances including the beliefs and expectancies which the audience/hearer must have in order that communication is successful are contained. The argument, put another way, is to be found in MacIntyre's 'After Virtue.' Here MacIntyre argues that in order to discover which, among various descriptions such as, 'digging', 'taking exercise', 'preparing for winter', or 'pleasing his wife', is most apt, it is necessary to see individual actions in their setting. In the example, the descriptions all relate to a man digging in his garden in which there are two possible settings:
'a particular type of household-cum-garden setting with the particular narrative history of that setting...or ...the narrative history of marriage.' He concludes from this that whether he was primarily digging the garden or pleasing his wife can only be told from the narrative setting in which his action is an episode: 'We cannot...characterise behaviour independently of intentions, and we cannot characterise intentions independently of the settings which make those intentions intelligible both to agents themselves and to others.'

A possibly stronger argument, though obviously related, is that only when we are in possession of a considerable amount of information about an agent's circumstances, intentions, reasons, wants, conventions, emotions and so on, can we even begin to understand and therefore correctly describe the actions of an individual or group. Put at its strongest the argument amounts to the assertion that actions cannot even be properly identified (put under a single, appropriate, intentional description) outside a narrative context. Indeed, this seems to be what MacIntyre seems to be pushing towards.

My argument, stated baldly, is that it is largely indeterminate what a person is doing even when they are doing something rather simple such as 'digging' - to use MacIntyre's example - without some further information. To determine what they are doing, MacIntyre argues, is to 'place' their action in a 'particular
narrative history, because, 'behaviour cannot be characterized independently of intentions and we cannot characterize intentions independently of the settings which make those intentions intelligible both to agents and to others.'

5.20 **Explanations and the isolation of Causes.**

I have said earlier that where a researcher wants to do more than merely describe a situation but go on to explain events or provide causes for them then he will be aided by contextual information. If this is so then what I have to say about the way in which causes are sought in ordinary situations (that is non-nomological causes which we refer to in everyday speech) will support the argument that educational research requires a large data base composed mainly of contextual material. At the same time the following section goes some way to giving an educational researcher some 'food for thought' about how explanations and so on are gained.

In the light of the fact that the search for causes and explanations can only be carried out in the context of narratives (and I do not mean that these need to be written) it seems pertinent to explore this area in some detail. I will not touch on the more obvious applications to education at this point because these will be made clear in the final chapter. What will be said about causation is not new: what might be of
significance is the hitherto neglected possibility of using a narrative to record the contexts from which explanations are gained and causes isolated. In chapter seven I will try to incorporate this aspect into a research scheme which might be of use in attempting to isolate causes such as those of truancy and racism in schools. For the moment, however, I will focus, with the aid of philosophers who have given special and significant attention to this area, on causality. What follows is therefore somewhat expository and deals, on the whole, with published material essentially concerned with the causes of physical events. I feel however, and I hope that it will become reasonably plain, that what is discussed has relevance to the explanation of actions.

I shall begin with a fairly straightforward account of what Hart and Honore say on the matter of causal identification and then go on to Mackie's view before putting the main conclusions together in terms of narrative context.

Hart and Honore begin their analysis by looking at an example in which, in context A, a lighted cigarette is seen as the cause of a factory fire while in context B the presence of oxygen is seen as the cause. They comment that 'The general laws which we may need to demonstrate the causal connexion in these cases will not tell us that in one case oxygen can be cited and in the other not.' This has the consequence that in deciding the cause, some further principle which guides our
identification needs to be looked for. They stress the need to distinguish cause and conditions because causes cannot be simply equated with necessary conditions; 'for the contrast of cause with mere conditions is an inseparable feature of all causal thinking'\(^{17}\) and is related more to 'the context of the enquiry, who asks the questions and why.' Their reasoning is that in the context of an insurance claim the mere presence of a lighted cigarette in a factory full of inflammables is enough to identify it as the cause (context A) while in context B where they may still be a lighted cigarette in a factory in which, because of its extremely volatile contents oxygen is excluded, it is the presence of oxygen, not the cigarette which is singled out.

In this way Hume's analysis of cause as the observed 'constant conjunction' of pairs of events in nature \(^{18}\) which has passed down as the logical doctrine 'that every singular causal statement implies, by its very meaning, a general proposition asserting a universal connexion...'\(^{19}\) does not apply in Human History, Law or ordinary discourse which normally includes persistent states, failure of events and omission of human beings to act. Not only is it too simple, but they add, as Mill has pointed out, there seldom is such a relation between a consequent and the single antecedent. Indeed, it is usually a sum of conditions. This raises the question of how antecedent causes are isolated and Mill himself is unclear on this.
Unlike Hume then, who thought of a cause as both necessary and sufficient, Mill thought of it as sufficient only, with the addition that such conditions as were said to be sufficient were joint complex sets. And, as well as seeing a difference between scientific and other causal contexts, Mill allows that the same event may have different causes on different occasions. Following this lead, the authors look again at the ordinary concept. They note the interesting fact that we do as a matter of fact agree in our judgements as to causes in the ordinary sense, which again seems to indicate a principle of choice at work. This 'principle' they isolate, from an examination of their examples, as,\textsuperscript{20} 'something which interferes with or in the course of events which would normally take place' and which need not be an event or sequence of events for...\textsuperscript{21} 'it is common to speak of static conditions or negative events as causes.' Thus, 'what is normally the case' assumes significance as does our general knowledge: for it is this which decides what is and what is not normal. Of this 'general knowledge', they add that, 'it is knowledge of the familiar 'way' to produce, by manipulating things, certain types of changes which do nor occur without our intervention'.\textsuperscript{22} They therefore conclude, in agreement with Gasking\textsuperscript{23} that the causal nexus is more like a recipe in which generalized causes are not those which would be attributed through attention to invariable sequences. Summing up\textsuperscript{24} they say,
The line between cause and mere condition is drawn by common sense on principles which vary in a subtle and complex way, both with the type of causal question at issue and the circumstances in which these causal questions arise.

Warning against Collingwood's thesis that such principles are attempts at control, they remark that:

perhaps the only general observation...is that in distinguishing between causes and conditions two contrasts are of prime importance. These are contrasts between what is abnormal and what is normal...and between a free deliberate human action and all other conditions.

About these final comments they make a number of further interesting remarks. Firstly, concerning the normal-abnormal distinction they say that the distinction itself can only be made in a context and usually reflects our practical interests. Of the Voluntary-Involuntary distinction they add that because involuntary acts such as a reflex resulting in breaking a glass are not usually regarded as causes, the need to know the conditions under which actions were taken is central to the assignation of a cause. Sometimes, therefore, it is necessary to go backwards through involuntary acts to discover a voluntary one before the cause is known. In the example above, for instance, while a lighted cigarette might have caused the fire, it is no longer regarded as the cause if it was also found that it was deliberately put there.

In effect, in the typical case...when we ask for the cause, we are asking that some abnormal lapse from routine be rendered intelligible by being exhibited as an instance of certain other normalities, namely, those general connexions which characterize experience and are formulated in broad and general terms.
Thus a lighted cigarette is the cause of a factory fire in a factory where fire is not usually present and if it was not put there intentionally. In a factory in which oxygen is normally excluded owing to the inflammatory nature of its contents, it is the presence of oxygen which is the cause unless it was intentionally put there. Necessary conditions become causes in certain contexts. However, what is finally said to be the cause is a matter of full investigation involving knowledge of normal and abnormal conditions, the interests of the investigation, lapses, absences, intentions of human agents and probably quite a lot more. Even the examples given become more difficult when, for example, we discover that someone threw petrol on the cigarette, so displacing the original cause on the grounds that the fire was thus assured. What Hart and Honore do admirably is twofold. Firstly, they show the complexity involved in ascribing causes in ordinary situations and secondly, give some indication of how such causes are finally decided upon. These insights help to show the value of narratives in causal ascription for, without actually saying as much, what Hart and Honore are saying is needed in order to identify a cause, is a great deal of background information: information which, I have argued, is best put in narrative form. I now move to Mackie's work which, while taking a slightly different and more formal approach to causation, buttresses up the necessity of contextual information in the identification of causes.
and therefore provides more reasons for supposing that objectivity about causes presupposes knowing a good deal about their setting.

Like Hart and Honore, Mackie also takes an example as his starting point. He says, 29

Suppose that a fire has broken out in a certain house, but has been extinguished before the house has been completely destroyed. Experts investigate the cause of the fire, and they conclude that it was caused by an electrical short circuit at a certain place.

Then he asks what the force is of their statement that the short circuit caused this fire? Firstly, he argues, it is not a necessary condition because something else could have caused the fire. Neither, however, is it a sufficient condition for other factors had to obtain: such as the existence of nearby inflammable material. Yet, while the identified cause was neither necessary nor sufficient, there were, nevertheless a set of conditions which together were jointly sufficient for the fire including negative ones such as the absence of a sprinkler which are also not necessary in themselves. Of all the jointly sufficient conditions the short circuit, while not being necessary for the reasons already given was, as Mackie puts it, an 'indispensable part'.

Summarizing his short, but insightful analysis he says, 30

In this case then, the so called cause is, and is known to be, an insufficient, but necessary part of a condition which is itself unnecessary but sufficient for the result.

He terms this type of condition an INUS condition. Now, Hart and Honore, I believe help to explain this analysis
in providing what we might call a rationale for it. Using their principles the short circuit is isolated against other possible antecedent conditions because, in the normal run of things there was no fire and that some abnormality, such as the short circuit, is a candidate to explain its recurrence. Further, in the assumed absence of any intentional act, which would displace the short circuit as the cause, and, in the further absence of any other abnormal conditions obtaining (such as a dropped cigarette) the short circuit is the only relevant condition to fill the bill denoted by Mackie's 'indispensable part'. There are, indeed, other indispensable parts such as neglected wiring, and while Mackie's analysis does not go far enough to explain why they are excluded, Hart and Honore at least give some indication why they may not be so; perhaps the wires had been in a poor state for years and thus were not to be described under 'abnormal'. A similar comment could be said for the person normally responsible for maintenance, for, as was rightly pointed out, intentional acts of neglect displace other possible candidates. We might wish to say something like the fact that the wires had not given trouble for years, acts in mitigation of intentional neglect. What this shows, however, more than either analysis, and is the point I should like to stress, is the thoroughgoing nature of the type of enquiry necessary to ascertain causes and the part played in the enquiry by both human judgement (for example, about the mitigating
circumstances) and the interests the enquirer has in coming to a particular conclusion about the cause.

Mitigation is extremely relevant to this example because it brings with it conditions not directly related to the enquiry in hand, such as whether the person responsible for the wiring, was in good health, was normally responsible and so on. The questions asked on the other hand, presuppose not only interests but also further presuppositions about what sort of answer is permissible. These considerations are considered by Rescher\textsuperscript{31} where he considers some of the implications and assumptions which go along with questioning. They are helpful here because they further help to illuminate why a particular answer is given to the question about what was the cause. This is made clearer when we see that, for example, the questioner already has an idea as what would count as an appropriate answer\textsuperscript{32}, which is an interesting comment in the light of the dispute over the nature of causes in general for a convinced Hempelian, a Mill, a Hume and a Mackie who would come with different assumptions as to what would count as an answer. So too, one would expect, would an arsonist, a firemen, an insurance agent and the man responsible for the upkeep of the house. And since, as Rescher points out\textsuperscript{33}, 'An explicit answer to a question is one that repeats the substance of the question itself; (e.g. What colour is it? The colour is red.)', one could expect to find different answers 'repeating' differing questions. There is yet a
further complication with questions which are ambiguous, such as the question, 'Why do owls hoot?' which may be variously answered (a) because they don't chirp, (b) because they protect their territory that way or (c) because of the dimensions of their beak and so on. This is so since each question has a presupposition which 'is a thesis (or proposition) that is entailed by each and every one of its admissible answers' and these 'reflect precommitments and...constitute what we bring to the very posing of our questions...'. Again, 'Questions are always projected on the basis of the cognitive "state of the art", relative to an existing body of putative knowledge'. Since this is so, it is easy to see why what constitutes the 'indispensable part' in an INUS condition, is going to vary widely according to factors which outstrip the 'normalcy' conditions of Hart and Honore. It is because they belong to another aspect of the enquiry concerned far less directly with discovering the cause but with preconceived presuppositions which underwrite the 'field' of possible candidates and which therefore constitute possibly separate classes of 'admissible answers'.

Causes are one form of explanation where human beings are concerned. I have attempted to give reasons for the belief that their identification is related to a number of circumstantial conditions. This conclusion may be broadened to include explanations, if there are any, which are non-causal. If the work of theorists like Hart,
Honore, Mackie and Rescher reveals anything, it shows how, given different circumstances, interests, perceived intentions of actors and so on, a researcher might identify a cause as a sufficient condition at one time and an insufficient condition at another. The very attempt, however, at isolating causes seems, because of its complexity, to involve us in the construction of a narrative in which various conditions are seen to play a part. Difficult as it might sound, it also seems to be a conclusion of this analysis that the identification of one thing or event as the cause and explanations in general, may be as much a product of ideological and other bias as anything which might actually be found. I hope to incorporate this insight into my research scheme in a positive way by allowing, in fact encouraging, differing viewpoints in as many places as possible.

There are a number of reasons why I think work such as we have considered is relevant to educational research. Firstly let us take a contemporary example - the crash of the Boeing at Lochaber and compare it to another physical event such as a school fire; although I am not saying that the analysis applies only to cases where the initial problem is to discover whether the cause is human or physical. Initial research in both cases would attempt to isolate the cause as human or physical. If physical, the enquiry passes over to the physical sciences. Pilot error and arson, however, as possible causes, would take the type of research required
out of the physical sciences. In this case our account of causation and context becomes pertinent. In the case of the pilot, the enquiry might discover that pilots in general worked too long and that this impaired their ability to take quick, appropriate and evasive action. In the case of arson an educational researcher might isolate a number of disaffected pupils as the cause and then go on to look at reasons why they were disaffected.

A second example brings out another aspect of the use of narratives both in providing descriptions and explanations. This is where a pupil complains of being the subject of bullying. On further investigation it is found that the person involved in 'bullying' describes his behaviour in terms of 'playing rough'. Obviously an investigation concerned with bullying in schools is going to have to look at the descriptions of actions used here and the way they enter into the investigation. The pupil who is the subject of bullying would give grounds for an investigation into, and, perhaps, explanation of, bullying. The pupil involved in 'bullying' would attempt to argue that the investigation was concerned with 'rough play' and that distinctions need to be drawn. Distinctions, however, are partly made in an attempt to defend or make a case. In this case the bully defends his behaviour by changing the language of the bullied. This type of interplay between language, action and locutionary factors is at the centre of 5.30. It is also further argument for the inclusion of context.
5.30 Understanding Narratives: The Need for Pragmatics.

It might be helpful to preface this section with a number of comments which are mostly reflections on some of the arguments put forward in the previous three chapters. This will, I hope, put into context what I now want to say and also explain why it needs to be said before I can even begin to discuss the issue of objectivity.

In the third chapter one of the points made was that intentional behaviour could not easily be subsumed under laws. One reason for this is that unlike such forces as gravity intentional behaviour is extremely varied and difficult to predict. In the next chapter the difficulty of dealing with the intentional was further brought out and a case was made for understanding others within a framework. In the present chapter I have tried to argue that this understanding is best 'captured' within a narrative. However, when narratives are examined it starts to become clear that not only do they vary in content as perceptions of events vary but that attempting to use them to isolate causes or provide explanations is fraught with other problems. Some of these deal with the ideological underpinnings of biography, while others deal with the ways in which questions influence answers and so on. In other words the very attempt to deal with one set of problems has given rise to quite another set. I want
to say, however, that I do not think that the new set of problems is an insurmountable obstacle, rather that they call for a different approach. This approach falls mainly within what has been referred to in linguistics as 'pragmatics'. This, of course, would change the whole emphasis of research into human behaviour away from the observational type discussed in chapter two towards a detailed biographical type - resembling case history - but which incorporates the tools appropriate not only to understanding others ('empathy' etc) but tools appropriate to understanding the medium in which such information as there is is recorded. In my view this is a narrative and therefore the appropriate tools are linguistic.

Having put the section into context, I will now proceed to say why I think semiotics and pragmatics in particular is necessary to understanding a narrative and, indeed, to writing one. Earlier in the chapter I argued that in order to say what was happening it was necessary to obtain an overall view of the situation. What I did not say, at that point, was that part of the overall situation or context are the locutionary factors at play. This is obviously a very complex area but I think that its implications for educational research are fairly straightforward. Firstly, most, if not all educational research is an attempt to answer a question. This question, which consequently contextualises the research, needs to be recognised and taken into account at the
outset, since, as I will show, questions prescribe limits to possible or admissible answers. Secondly, many of the agents whose descriptions eventually become part of the narrative will have given them as answers to a question. What they say will therefore need to be looked at in a more general context which includes the speech act in which they are involved and (which is part of the same thing) at the way in which they perceive the audience. A Head, for example, whose school is threatened with closure because of low academic results - as happened recently at Kingsdale School in Southwark - has a completely different perception of the audience (the readers of the Independent newspaper, the Times Educational Supplement and so on) than, say, the parents of a pupil leaving with very low GCSE results.

Educational research, of course, does not need to correct what is said, what it needs to do is to understand more fully the influences such factors have on the agents involved and incorporate these into the narrative itself. There appear to be two different activities involved in this process. The first concerns philosophers and the work they do which helps the educational researcher and there is the work the researcher does in incorporating such information in the narrative. I will look at some work which has been done in the former area and then attempt to see the implications it might have for the researcher.

Before beginning, I think we need to recap a
little, if only to identify the problem or, perhaps, more importantly, to show that there is a problem and the type of problem it is. In the account given by Hart and Honore, causal identification has a large subjective component. This refers to perceptions that (a) conditions were normal and that (b) agents were, indeed, acting voluntarily. I call these 'subjective' because they cannot be directly obtained from the situation: they are, largely, the way we construe the situation. This can be seen more easily if we accept firstly, that what counts as normal is a relative judgement and secondly, that what counts as voluntary carries with it metaphysical underpinnings, the rejection of which might entail a different interpretation of what constitutes antecedent conditions. The same subjective elements are to be seen in Mackie's account: the cause is not sufficient and not, by itself, necessary. A short circuit, to repeat his example, cannot, without other conditions being met, cause a fire. On the other hand other things could start a fire. When, therefore, we identify something as a cause we are making a choice. We firstly set parameters around a situation and then select from among various possible candidates something which we call the cause. Both selections, the situation and the identification, have a subjective element. Using Mackie's example, again, we could, for example, widen the situational context and identify the electrician who renewed the wiring as the cause, or, to take the example further, we could identify
the householder who chose an unqualified electrician (because he was cheap) as the cause - or was it the need to save money?

The question, then, is how does further information about the interrogative context aid us in either the identification of causes or in writing the narrative. The work of Rescher and Putnam are relevant here as they highlight this very point. Rescher's comments illuminate the problem because they tend to show that the very questions we ask in an enquiry about causes preclude certain, otherwise possible, answers. This, I think is fairly easily shown if we add to my last, extended, example, three questioners. The first, is an inspector from the electricians' guild. He is trying to eliminate unqualified electricians from carrying out work by making a report which is intended to show the damage they cause. His question is, 'who put in the wiring and was he qualified to do so?' Would it be illogical or somehow empirically wrong for the inspector to identify our 'electrician' as the cause of the fire? I think not. The second question comes from Mackie's expert who, as we have seen, identifies the cause as the short circuit. The third comes from the wife who has always suspected that her husband is careless about house maintenance: she asks if her husband checked on the qualifications of the electrician and whether he regularly looked at the wiring to see if it was deteriorating. Would it be empirically wrong or logically absurd if she came to the conclusion that
her husband caused the fire through neglect? 

Hilary Putnam argues a similar point, but in the wider context of questions seeking explanations as answers. He quotes an interesting example from the ethnographer Garfinkel. He cites three examples of questions where an explanation is expected. I will paraphrase them first. In the first a professor is found naked in a girls' dormitory and the explanation given as to why he was there is that he could not leave without exceeding the speed of light. The explanation is justified in terms of the covering law. In the second a square peg does not go through a round hole with the same dimensions. The explanation given is that by computation we find that trajectories that take the peg through the square hole will not take it through the round hole. (The covering law comes from physics.) While in the third a famous bank robber is asked why he robs banks by (a) a priest and (b) a robber.

Putnam comments that while the first and second satisfy the Deductive-Nomological model of explanations, they are terrible explanations. They are terrible explanations, he argues, because in the first example we knew this explanation could be given before asking; what we wanted was another explanation. 'Why' questions, he argues, presuppose certain interests. Because we wanted to know, not what physically stopped the professor leaving, which should have been obvious, but why he was there at all, the explanation violates background interests. In the case of
the second example we don't know or even want to know the mathematics of trajectories that can be given, because the answer can be given in terms of one simple mathematical fact. This explanation therefore violates methodological interests. In the third example he quotes Garfinkel's remark that 'Why' questions always presuppose a 'space of relevant alternatives'. The questions are relative to assumptions about the questioner: priests want to know why people are driven to rob, the robber wants to know the reason for robbing banks as opposed to, say, large houses which are less well guarded etc. So, reinforcing Rescher's points, questions and the answers given are interest-relative and, without some prior understanding, agreement or assumptions, the 'space of relevant alternatives', when applied to questions about causes, may throw up a whole host of possible candidates to fill Mackie's 'indispensible part'.

Putnam's own contribution to a form of rationalization, given in the context of a reply to Quine on indeterminate translation, is the submission that we must assume some familiar explanation to our own. In other words, the wider court of appeal to which we apply in differently identified causes is that of a consensus amongst people who share, or who have agreed, prior to the investigation, to share a common view.

One aid to solving some of the difficulties outlined involves, as I have suggested, work in the area of pragmatics. What is needed, in other words, is a much
better understanding of the ways in which the language we use to record our actions and the ways we have of questioning and answering each other function. I include, in the footnotes, a list of work along these lines which appears to be helpful. I also include (in a short appendix) an example of my own, of the way in which such work could be used in the analysis of behaviour in an educational setting.

The problem I attempt to deal with there looks at the relationship between questions and answers and the various factors which influence their relationship. I have suggested a method which utilises a notion of expected response. Questions which elicit a response which is appropriate to it are said to be convergent. Degrees of convergence can be gauged by the rhetorical relationships involved. The example is intended to show two things. Firstly, how a linguistic approach can go some way to resolving questions concerning explanations and causes and secondly, how this method can be applied to educational situations.

As a consequence, most of what I have to say about the help philosophers of language can be to educational research is implicit in the appendix. More explicitly, however, there seem to be a number of significant points which may be picked out. Firstly, a working knowledge of the ways in which speech and actions are contextualized by locutionary factors is going to be an almost necessary requirement for the person making the narrative.
Secondly, that while the answers given should not be altered, the locutionary context should be spelled out so that what is said can be seen in the light of those factors. The main implication is therefore that any researcher engaged in the collection of a narrative will have to have a certain level of expertise not only in interviewing but also in attempting to obtain information - including that about the speech act - which makes answers differ. This brings us neatly to the next section which deals with objectivity, for if, as is usual, accounts and explanations do differ, we also need to be aware of what makes them differ and have some possible solution to the question, 'which, if any, and on what grounds, are differing narratives true?'

5.40 Narratives and Objectivity.

I come now to one of the central questions which will concern those engaged in educational research: how can we ensure that narratives are objective. It will be apparent by now that this is an extremely complex question not only because of the difficulties raised in the latter part of chapter four but also because of the various ways narratives can be both written and read. What follows therefore is going to be far from definitive - it will only begin to address the issues.

Before I begin let us review some of the ways which may make narratives differ. (a) - (e) come from
what has been said, (f) - (g) because of what will be said while (h) simply stands to reason.

(a) Differing historical perspectives which may include both cultural or ideological perspectives. (4.10-41)
(b) The perspective a group - such as an interest group might have - as opposed to that which an individual might have. (5.20)
(c) Difficulties which were discussed in the latter parts of chapter four which arise from the 'opacity' of intentions (or, for example, the ambiguity of convention.) (4.50-4)
(d) Actual disagreements about what happened which might have arisen because differing narrative contexts have been assumed. (5.20)
(e) Differing locutionary contexts. (5.30)
(f) Disagreements as to what constitutes an objective account of events. (5.41)
(g) The way in which historical events are put together. (5.42)
(h) Biased accounts.

In this section I shall be concerned with (f) and (g). This, obviously, is not because I think that these are the only two factors which effect objectivity, but because they have not yet been dealt with. It should be stressed, therefore, that (a) - (h) all enter into the
question of objectivity. What will be seen, I hope, is that in looking at (f) and (g) all the others fall into place. The reason is that when a sequence of events has to be written out an understanding of ideology, culture, intentions, conventions, audience-relativity, speech acts and differing views has to be taken into account.

In looking at this area I shall use the work of historians. This is mainly because historians, by the nature of their work, have been engaged in writing out a sequence of events. I am not suggesting for one moment that they are in any sense a paradigm for the type of research I outline in chapter seven. To do so would ignore the importance I have placed on discovering actual agents intentions, actual social conventions, actual disagreements and so on all of which would be unavailable to all but contemporary historians. I shall attempt to show the importance of agency, locutionary factors and so on in what I have to say by underlining them in the context of what I have to say about (f) and (g). I hope that this will underline their importance without going over the same ground again.

I will begin with (f), which is the objectivity of historical events. My main argument will be that any narrator, historian or other, who thinks that one can simply provide a sequenced set of events or chronicle is sadly mistaken. The mistake is that it ignores the importance of most of the factors I have listed as (a) - (e). I shall refer, where necessary, to this as naive
historiography. I shall then move on to a second section which centres on (g) where the opposite appears to be the case. This is where history has become - because of the insurmountable problems inherent in naive historiography - a fiction: an author's imposition of a story where the events which occur play little or no role at all. This I shall refer to as fictive historiography. I refer to the first section as 5.41 and to the second as 5.42. The attempt to find a middle path is outlined in 5.43 which is then followed by my main conclusions regarding objectivity in 5.60.

5.41 Naive Historiography

History has been traditionally divided into two (broad) parts or stages. The first is the collection - or examination - of a chronicle of events while the second is an explanation of the events. My argument is that this simple division cannot be sustained. The reason, already, indicated is that the questions asked by the narrators and their contextual setting, together with the perceptions those narrators have of possible or admissible explanations is already at work in the construction of the narrative and its content. This influences the work of historians - or contemporary researchers - in two distinct ways. Firstly, in the case of the modern historian who is engaged in writing history, it influences the way that historian perceives
his or her own work. In the case of modern historians attempting to work on historical texts the influence is more subtle. It is subtle to the extent that the (modern) historian must examine the interrogative contexts of the (ancient) historian with whom he or she is dealing. I shall take an example of the latter to lead into a discussion of the objectivity of contemporary narratives. The example comes from Caesar's 'Gallic Wars'. In it Caesar 'describes' the Gauls as, 'extremely superstitious; and so any person who is suffering from serious diseases, as well as those who are exposed to the perils of battle, offer or vow to offer, human sacrifices, for the performance of which they employ Druids.' He continues, 'Some tribes have colossal images made of wickerwork, the limbs of which they fill with living men; they are then set on fire, and the victims burnt to death.'

I have enclosed the word 'describes' in speech marks to indicate the speech act which it implies: an objective account. Yet, if one looks at the interrogative context of the narrative, one finds that its audience is Rome and that its intention is justificatory not descriptive per se. It is a justification - to detractors at home - for the Gallic War, for the conquest of Europe. It operates as a justification by trying to show the miserable state of the barbarians and the benefits that could be gained by Roman control. Put, however, in its interrogative context,
what might be taken as objective narrative becomes mere propaganda: its truth value an indeterminate quantity. Now, returning to what I said above, if the method espoused by naive historians is correct and events are to be left as Caesar described them (which is an assumption) then it will appear to later generations that what Caesar said, was said as if he were acting as a mere chronicler of events - which, qua politician, he was not.

Morton White, who would, on the criteria I have provided be a naive historian states his case as follows. Firstly there is a distinction to be made between the narrative, which he explains as a conjunction of non-explanatory empirical statements, and history, which is the explanation of the narrative.

Secondly, that the main problem for the historian is how to decide from among competing narratives of the same events, which is the better. After discussing and dismissing their selection on either subjective grounds such as interest and morality or on essentialist grounds such as the 'spirit' of the times he formulates his own ideas. These he describes as broadly relativistic in that they allow the choice of events to be made with reference to the aims of the historian. Further on he suggests that in the process of writing history the historian must both select facts in accord with his aim and 'colligate' them. By this latter feature he appears to be indicating a principle by which the collected events are organized - a
principle which he regards as resting on a value judgement that will sometimes be relative to differing standards of importance. Returning to his original question, he then suggests that both the principle of organisation and the selection of facts are subject to a judgement of worth by professional peers.

The problems with this approach are twofold. Firstly, even though recourse is made to other historians, there is the presumption that a set of events can be collected independently and before an explanation of them is given. Secondly, what are seen as individual events are organized after their collection. The first of these seems to suffer from what I have referred to as naivety. The second relies on a circularity, the basic assumptions of which are hidden. The circularity, using the Caesar example, is to be found as follows. If it is assumed that Caesar was describing Britain then the account is descriptive. If it is assumed that the account was ideologically motivated then it is justificatory. Either way the chronicle must make an assumption. The circularity comes in when it is asked just how the historian knows which assumption is true. In order to do this Caesar's intentions need to be understood before anything that Caesar wrote can be used in a reconstruction of the times. This places the narrative as the outcome of sequencing events. But, and this is the crux, if our earlier arguments concerning the necessity
of a narrative context in order to 'place' such things as intentions, then that context is a presupposition of sequencing events. The narrative underwrites the chronicle - not as naive historiography would have it - the other way round.

5.42 Fictive Historiography

If the naive historian's approach is fraught with the problems we have outlined, so is its opponents'. The opponents view is that given the problems of settling which events actually took place (and here I suppose such things as intentionality count as mental events) and given the problem of sequencing them it is better to regard history as a form of literature. At its extreme, history is a story.

An example of this approach is seen in Hayden White's book 'Metahistory' where he classifies the work of Michelet, Ranke, Tocqueville and Burckhardt in terms of literary styles or story forms. He identifies them respectively as Romance, Comedy, Tragedy and Satire. Similarly in 'Fables of Identity', Northrop Frye identifies 'mythic' forms in the work of Hegel, Marx, Nietzsche, Spengler, Toynbee and Sartre. These two philosophers, however, differ in their attitude to this. Northrop Frye takes what might be called a realist view of narrative when he says of the historian that he,
works inductively, collecting his facts and trying to avoid any informing patterns except those that he sees, or is honestly convinced he sees, in the facts themselves.

For Hayden White, however, Frye is wrong to castigate historians for their 'mythic' style and even more wrong to say that this deadens their sensitivity to 'found data' for in Hayden White's view:

...histories gain their explanatory effect by their success in making stories out of mere chronicles... (for they are) 'emplottements': the encodation of the facts contained in the chronicle as components of specific kinds of plot-structures, in precisely the way that Frye has suggested is the case with 'fiction' in general.

The dilemma, then, for 'fictive historiography' may be summarized as follows: for Frye, historical explanations are plausible when they succeed in discovering the story implicitly contained in historical events; while for Hayden White they are, 'made into a story (by) all the techniques that we would normally expect to find in a novel or play.'

Ankersmit takes an overview of this debate and argues against Frye's position (which is also that of Collingwood). This he characterizes as a 'picture theory' similar to that in the Tractatus. He argues that it has 'a built in tendency to confuse things which should be kept apart.' This tendency, he says, consists in the assumption that the 'translation rules' by which the object world is mapped into the narrative are provided by the social sciences and that this confers an unwarranted epistemological privilege on the methods of those sciences. A more fundamental objection, he
thinks, is that the 'translation rules', which are
earlier likened to those used in cartography, 'will never
be more than arbitrary selection rules...for the past is
by no means like a machine...nor is (it) like a
landscape: the "historical landscape" is not given to the
historian; (for) he has to construct it.' 51

The second of these arguments seems straight-
forwardly convincing. It seems convincing for the reason
that Hayden White gives 52 in response to Benveniste’s
conclusion that, 53 'The events seem to tell
themselves'. He remarks: 54

But real events should not speak, should not tell
themselves. Real events should simply be; they can
perfectly well serve as the referents of a discourse,
can be spoken about, but they should not pose as the
tellers of a narrative.

The compelling aspect of this argument, apart from its
simplicity, is the apparent absurdity in its denial, for
we are dealing with a literary form when we deal with
recorded history and not with events which 'speak for
themselves'.

These sentiments are echoed by Louis Mink 55 .
He comments that while it might be a presupposition of
common sense, 'that historical actuality itself has a
narrative form, which the historian does not invent but
discovers, or attempts to discover', 56 it is mistaken.
The mistake lies in the further assumption, which is the
belief in the possibility of a 'Universal History' 57
which is the claim that, 'the ensemble of human events
belongs to a single story (with) a single central subject
or theme in the unfolding of the plot of history.\textsuperscript{58} The narrative, however, for Mink, because it does not reflect the story history tells, cannot itself be either true or false. The reason Mink gives is that,\textsuperscript{59}

the same event, under the same description or different descriptions, may belong to different stories... its particular significance will vary with its place in these different... narratives. But just as 'evidence' does not dictate which story is to be constructed, so it does not bear on the preference of one story to another. When it comes to the narrative treatment of an ensemble of interrelationships, we credit the imagination of the sensibility or the insight of the individual historian. This must be so, since there are no rules for the construction of a narrative...(and)... so narrative form in history, as in fiction, is an artifice, the product of individual imagination.

The situation, and conclusions, outlined here bear remarkable resemblance to both Hayden White's insistence that, 'historical sequences...be emplotted in a number of different ways so as to provide different interpretations of those events and to endow them with different meanings'\textsuperscript{60} and to McIntyre's comments on the importance of narrative context to descriptions quoted earlier. The main problem, as I see it, is in the fictive historian's use of 'imagination', 'myth', 'story', 'emplottment' and so on, all of which point to idealism.

5.43 Can Objectivity be preserved?

Firstly I will restate the problem as I see it. Morton White, Collingwood and Frye do not see the problem because they 'cut into' it at an arbitrary point. This point is located where events are seen as events without
the requirement of a further narrative in which to embed them. That this point is arbitrary is only seen when its circularity is exposed: intentions and actions are only understood in contexts and these are understood only in terms of earlier historical circumstances. Where, however, does the regress into the past stop? This question, I think, is analogous to the question about the proper context of an enquiry into causes. There is no 'right' place to stop - there is only a question to be answered. Naive historiography is committed to an unsubstantiated assumption at some point and it is this point which is arbitrary.

Mink and Hayden White admit this but do not see it as a problem: that there is literally no place to stop the 'emplottments' or the 'mapping' of events by the imagination is the way history is. It is a story. If they do stop then it is for reasons of literary style or imaginative flair, reasons which are, it seems to me, equally arbitrary.

The main difference between the two groups over where to stop is not that one is less arbitrary than the other but that the second group does not mind being arbitrary: their world is pluralistic and the choice between Homer and Marx a matter of taste. Yet it cannot be just a matter of taste: there are reasons for supposing that 'Bright eyed Athena' never came among mortals as an eagle which are different in type from
reasons which question the notion of historical necessity.

If we have grounds for supposing this then these grounds are not to be found in the nature of things but in what we find intelligible. We do not, as a matter of fact, really believe in Homer's gods but this does not hinder our enjoyment of Homer. At the same time I have reservations about the actuality of historical necessity. My reasons for being sceptical of the existence of the latter are, however, based on various historical factors which can be intersubjectively verified which have not pertained as Marx, on one reading, might have predicted. My reasons for being sceptical of a visitation of Athena, however, are not based on historical events - after all, no one denies that there were eagles in ancient Greece - my reasons are that I find the idea unintelligible. The problem, which will now become evident, is that I also find reasons - described earlier - to be sceptical of the findings of a positivist human science. The difference, however, is that I can give reasons which are intersubjectively understood for my scepticism - reasons which are ultimately grounded in a commonly accepted 'intelligibility'. There is a problem remaining - although it is only a problem which exists within a transient historical community - and that is that we do not agree as to what is intelligible. Yet, there is no further criterion by which to judge intelligibility as
far as I can see, other than by as complete an understanding as can be acquired by a diligent gathering of various kinds of information about the circumstances which are the focus of our attention.

This breaks out of the impasse not by grounding it in some unsupported assumption nor by losing sight of events altogether. It does so by a recognition of the limits of our ability to understand. It stops the search for Caesar's intentions from disappearing into Roman or British studies by admitting the indeterminacy of the search but affirming what seems to make the most sense. In the same way, contemporary narratives - such as the one I will outline in the final chapter - are limited by the attempt to make as much intelligible sense out of what people say and do, as is possible. It is in the attempt to make sense that agreement will be reached or not reached, as the case may be, and so what is necessary is not agreement but the possibility of agreement and this, I shall argue, is wrung out of a dialogue between the historical agents themselves and those who wish to understand them. It is my thesis, therefore, that the incorporation of many and various points of view, dialogue between them and an attempt between individuals - possibly with differing points of view - to come to an agreed and intelligible understanding of events, is the best we can do to be as objective as possible about a set of historical events which are recorded in a narrative.
There are four main conclusions I wish to draw from the discussion in chapters four and five. The first two are concerned with the role of the narrative, the third with the use of the narrative and the fourth with its objectivity.

The first conclusion deals with the narrative itself. While it is tempting to say that a narrative is somehow a necessary part of educational research it will have been obvious that I have not seen it in this light. All my arguments point to its pragmatic use as a research tool. The reasons why I think that it makes pragmatic good sense to use a narrative as opposed to a more atomistic basis are as follows. Firstly for the reasons aduced in 5.10 which are, broadly, that a wider understanding of context is required if we wish to say what, if any, actions are taking place. This was also central to the arguments about the difficulty of saying what was taking place in the second part of chapter four. The conclusion is also supported by the role the historical community takes in deciding on appropriate descriptions - a narrative which consists in what people say is happening, is as near as we can get to the judgements upon which agreements are based. Secondly, non-nomological causes (agent causation etc) and explanations dealing with human events normally require a considerable amount of background material before they
begin to make sense. Thus the arguments put forward for the usefulness of narratives in order to discover causes and provide explanations is also an argument for the use of narratives as part of the investigation. Thirdly the arguments put in 5.30 and appendix A suggest that the speech acts which contextualise actions and descriptions are themselves an important part of understanding what is done or said. Indeed, I have argued that much valuable work can be done in understanding actions by understanding the locutionary factors which encapsulate them. Finally, the need for objectivity involves, almost as a necessary condition, a number of human perspectives on events. (I am not here attempting to define objectivity although I would subscribe to some such view as that put forward by D.W.Hamlyn in his paper on objectivity.61) As outlined at the beginning of section 5.40, I feel that there are a large number of factors which influence the ways in which events are perceived and all, or as many as is practically possible, need to be taken into account. I have also given reasons why I think that certain points of view (Athena appearing as an eagle) are less acceptable than others. The upshot, however, is that again we are forced to take in a large amount of context if objectivity is to be sought.

The second conclusion, mentioned above, is that narratives, if used in educational research, provide an excellent basis for the discovery of causes and in the provision of explanations. I do not need to repeat the
arguments of chapter three to say that cause, as used here, refers to an efficient condition which is not nomological in the ordinary sense in which they are used.

Thirdly, as I have tried to show in section 5.30 and appendix A, much can be gained by the application of various branches of semiotics to the interplay of locutionary factors within texts which aid in the ultimate understanding of the events they record.

Fourthly, while I have not spelled out exactly what is meant by objectivity, (see the note above and footnote 56) I have suggested that a more objective viewpoint is reached if we take into account various points of view. I have also suggested, with McIntyre, that viewpoints which make intelligible sense are of more value than those which do not. This needs some elaboration although, of course, it would be impossible to do the subject justice in a thesis in which objectivity is not the central theme. Basically the rationale for what I have suggested in 5.40 is grounded in the arguments of chapter four. That is they are grounded in Wittgenstein's arguments about agreements in well founded judgements as the basis for objectivity. The objectivist, it seems to me, is committed to at least two positions. The first is that the grounds for judgement are found in the 'public' domain: that which is intersubjectively verifiable. The second is that while truth itself is not negotiable our descriptions of it and our explanations of it are. Objectivity is thus reached
through a process of negotiation and this process needs to take in various viewpoints in order to allow for those factors which account for the differences in judgements. Ultimately, of course, there may well be no agreement. The arguments I have put forward allow for this in that they only require a pragmatic conclusion. This might sound less than exciting for those engaged in 'pure' philosophy but in education, as in other political spheres where policies are required to remedy real situations, pragmatic agreements might be all there is.

The last of these points was suggested at the end of section 5.40 and will be taken up again in the final chapter where it plays a central role. That role, which involves the participation of the historical community in helping to write its own biography can be seen both as an attempt (from 5.40) to make the narrative objective and as a direct consequence of the constitutive role the community has in making its own history (4.30).

Before I come to the practical and substantive conclusions concerning the role of the narrative in educational research there is one further topic to be discussed. If, as Aristotle suggests, we use practical reason to solve practical problems then not only will it be necessary to have an overview of the situation about which we are reasoning (which was the point of the narrative) but we will need to understand the nature of reasoning itself. Thus, in the next chapter, I address the problem of whether or not there are rules of
practical reason and whether actions — which Aristotle thought to be the outcomes of such reason — can be deduced from any given set of premises.
Chapter Five: Footnotes.

(1) A. MacIntyre, 'After Virtue' Duckworth, 1981. p190.


(7) see page 59 of 'Speech Acts'

(8) MacIntyre, op cit, p190.

(9) ibid, p192.

(10) ibid, p192.

(11) ibid, p192.

(12) I believe this way of putting the matter derives from Jacques Derrida.

(13) McIntyre, op cit, p192.

(14) ibid, p192.


(16) ibid, p10.

(17) ibid, p11.

(18) ibid, p13.

(19) ibid, p13.

(20) ibid, p27.

(21) ibid, p28.

(22) ibid, p29.

(23) (See D. Gasking, 'Causation as Recipes', Mind, 1955)
xciv. p479)
(24) Hart and Honore. op cit, p30-1.
(25) ibid, p32-8.
(26) ibid, p38-41.
(27) ibid, p40-1.
(29) ibid, p15.
(30) ibid, p16.
(32) ibid, p131.
(33) ibid, p132.
(34) ibid, p133.
(35) ibid, p133.
(36) ibid, p141.
(37) 'The Independent Newspaper', Saturday 11th February 1989. (The same incident was reported in the 'Times Educational Supplement.)
(38) See Rescher above.
(40) I include, here, a list of work on similar lines:


I. and W. Kummer, 'logic of action and practical arguments' (in Van Dijk).

(41) Appendix A


(46) ibid, p52-3.


(50) Ankersmit, op cit, p94.

(51) ibid, p86.


(53) Quoted by White, p3.

(54) ibid, p3.

(55) Louis Mink, 'Narrative form as a cognitive instrument' in Canary and Kozicki.

(56) ibid, p134.

(57) ibid, p136.

(58) ibid, p137.

(59) ibid, p145.

(60) H. White. op cit, p48.

(61) D.W. Hamlyn, "Objectivity" in 'Reason' (Ed. R.F. Dearden, R.S. Peters and P.H. Hirst). Published by RKP, p96. Here Hamlyn says, '...intersubjective agreement acts as a kind of linking-point between truth and understanding. For understanding what we mean presupposes agreement on the application of our terms at certain points, and agreement equally constitutes the criterion of the concept of truth. Thus intersubjectivity provides the basis of objectivity, although it would be impossible to argue that intersubjectivity and objectivity are the same.' Given this notion of objectivity, I think that it will be fairly clear why I perceive the need to bring various 'narratives' together if only to provide the possibility of objectivity.
Chapter Six

6.0 Practical Reason: its use in Education.

I introduced the topic of the thesis, in the first chapter, by way of a discussion between Paul Hirst and D.J.O'Connor. In that chapter I was concerned to bring out the scientism of O'Connor and contrast it with Aristotle's comments on what was the proper province of science. We have now discussed what might be called the fact finding element in the process and I have argued that the scientific method is inappropriate. I now want to move on to the second stage in an applied science - which is its application. O'Connor's arguments on this were that the application of educational research resembled engineering and he compared education to medicine in this respect. I have argued in the second
chapter that the concept of engineering in the context of applying the findings of educational research is also inappropriate. I thus wish to return to Aristotle for help. He suggests that the appropriate method of thinking out what to do in human affairs is to deliberate about it and having deliberated, act. Indeed, Aristotle considers action to be the end result of deliberation.

This chapter thus follows on from the fifth in that it looks at how educationalists might apply the notion of practical reason – or deliberation – to their research. I argue that the use of various forms of logical reasoning – usually referred to as deductivism – are inadequate to the job in hand and suggest that there are 'ordinary' sorts of practical reasoning already in public use which are far more suited and able to solve practical problems. I do not claim, however, that such forms of practical reasoning are infallible. Rather, like the narrative, they take into account a great deal of relevant information and in various ways manage to take much of this information into account before a policy decision is made. The 'rationale' of the chapter is thus to provide a working basis upon which educational problems recorded in the narrative might be resolved. In the final chapter I will attempt to incorporate both chapters five and six into a substantive method for use in some educational research. The contents of the present chapter break down as follows:
6.10 Deductivism.

6.20 Some Problems with Deductivism

6.30 Practical Reason and Educational Research.

6.40 Conclusions.

6.10 Deductivism

I have included this section, not out of a pure interest in the theoretical intricacies of practical reason, but because there is a real sense in which solutions to practical problems may result from the pursuit of a formalised system. If this were to be the case then educationalists could utilise this in order to formulate policies which were directly deduced from a given set of premises. Having said this I must state that I feel that practical reason in everyday affairs seems far too complex a business to be reduced to a simple formula. Indeed, given what has been said about the complexities of merely describing and understanding human affairs, I feel that practical reasoners would spend their time more profitably looking at this complexity and seeing how we in fact reason in a complex world. However, I think it only fair to look at some of the more theoretical work done on practical reason,
perhaps because it may solve some of the complexities by reducing them or simplifying them. Having said that, it must also be said that I cannot do the whole area justice and thus confine myself to the work of Roderick Chisholm and a number of his critics as a fine example of the work going on in the field of practical reason.

To begin (rather at the deep end) I will cite one of Chisholm's examples which illustrates, for him, how practical reason can be formalised:¹

(A) (1) p occurs  
(2) p requires that S perform A  
therefore (3) S has a duty to perform A

(B) (1) q occurs  
(2) q requires that S perform an act incompatible with his performing A  
therefore (3) S has a duty not to perform A

(C) (1) r, as well as q, occurs  
(2) r and p does not require that S perform A  
therefore (3) The requirement, imposed by p, that S perform A has been overridden.

Of this argument form he says it is both a 'significant part of our practical reasoning' and 'a valid logical argument.' Von Wright, on the other hand, thinks that this is oversimplistic and that a fourth mode of modal logic: the deontic mode or mode of obligation,² needs to
be developed to handle complex human interactions. A mode he thinks would form the basis of, 'a schema of explanation...comparable...in the human sciences to that of the deductive nomological explanation in the natural sciences.'

Anscombe remarks that she is 'out of sympathy' with the 'assimilation of practical reasoning to a trivial move of speculative reasoning'. In similar vein Raz comments that he, 'is not convinced by (the) implicit claim that the principles of the logic of requirement...go a long way towards solving the main problems of practical reason'. These 'main problems', as he explains, are to do with how we, in ordinary life, normally go about making decisions, weighing alternatives, resolving conflicts and so on. The question therefore (which somehow transcends Chisholm's treatment) is to what extent can a deductive account of reasoning do justice to the vagaries of day to day decisions about what to do. Since Chisholm thinks that it forms a significant part of our 'exhorting, justifying and excusing' let us see how well his treatment stands up to its claims. The central concepts Chisholm uses are those of duty, requirement, fittingness and overriding. Each of these, with the exception of fittingness, is mentioned in his example. Consequently I shall discuss these before attempting to deal with the wider question. Requirement is central and so I will make a start with it. In his example requirement entails a duty, indeed,
this relationship is the missing 'fittingness'. Examples of requirement, he tells us, include such things as: 'making a promise requires keeping the promise; wronging a person requires compensating the person; virtue (if Kant is right) requires being rewarded' and so on.

Further to this he formulates a general principle which 'tells us that the relation of requirement is like the relations of logic: if it holds between any two states of affairs, then it holds necessarily between those states of affairs'. From this and an existential principle asserting the actual existence of such states of affairs between which 'R' (requirement) relations hold, a further five general principles are derived. These principles spell out when states of affairs are or are not logically compatible, are disjunctive or conjunctive. The conclusion, if we are permitted to jump over the detail which includes five axioms for right practical or moral reasoning, is that, 'a valid practical argument, with premises referring to requirement, and a conclusion referring to what one ought to do, would have the form: a's occurring requires me to do b; a has occurred; and nothing has occurred to override this requirement; therefore I ought to do a'. A 'valid practical argument', incidentally, will countenance no alternative, for, 'if, as often happens, two people agree on all the facts and accept the same principles of morality and yet find themselves in disagreement...then at least one of the people has failed to see that...some requirement has
been overridden'.

Anscombe, Raz and Watkins make different, if related points, about the use of requirement. I shall briefly state what these are and then add a few observations of my own. Anscombe's comments concern the logical status of the relation expressed by Chisholm's 'aRb' where R stands for 'requires'. In the example, the relation is that between an event and a duty; but what event, asks Anscombe, requires mercy as an obligation? Raz makes the point more vividly by asking why we should not act with malice even though malice might be wrong in itself? The point may be widened to question Chisholm's own examples: what 'necessity' holds between promising and keeping a promise or between wronging and compensating? The only necessity there is, even in Raz's case, is a conventional one: not one which is 'like logic'. Watkins asks just that when he says, 'but which logical relations is it (the requirement relation) like?' Not, he adds, 'the relation of logical implication; for that obeys the...principle of augmentation...for it is a conspicuous feature of Chisholm's requirement relation that...it can easily happen that p requires q but p&r does not.' We are left then with a spurious use of the word logic which is brought in, one suspects, to cover the informality of convention with a respectable face of formality. Indeed, Chisholm's 'logic' presupposes a set of agreements which hold universally. This thought is belied by Chisholm's
quotation of Samuel Clarke\textsuperscript{13} when he says, 'We may say 
\textbf{with} Samuel Clarke, "that our duties are a function of the eternal relations of fitness that hold among things."' One is tempted to add \textbf{with} Gilbert and Sullivan, 'Let the punishment fit the crime': the problem, of course, is that what punishment fits which crime is unspecified. Fittingness, which expresses the relation, is only made specific within particular cultures and times.

We can now see why Chisholm's optimistic conclusion, that moral or other disagreement is essentially settlable, is wrong. The claim that 'at least one person...has failed to see that some requirement has been overridden' ignores or overlooks the culture dependence of overriding. It does this for two reasons. The first, already mentioned, is its contingency; the second, its intentionality. Chisholm presupposes a truth functional logic with all the extensionality which that requires. Unfortunately the resolution of moral disagreement through practical reason involves intentionality and that presupposes intensional logic. Whatever states of affairs Chisholm has in mind when he talks of relevancy, when they are human states they are 'under a description' and not substitutable salva veritate as he supposes. Consequently, disagreement may go deeper than the mere failure on the part of one person to see that overriding has taken place: there may well be a failure to see that the same event has taken
place. Two solutions seem possible as a solution to this lacuna: neither helpful to Chisholm. The first is that all cultures adopt the same stance to every practical issue and included in that is a weighting procedure for 'overriding'. The second is that an extensional logic is developed for human actions so that for whatever x is a fitting desert for whatever y (given that what is fitting is agreed upon), x and y remain identifiably identical x's and y's in all possible worlds and narratives.

Two further and somewhat surprising things about Chisholm's account are that it says little or nothing about what we usually mean by practical reason and, partly as a consequence, omits completely any mention of the 'springs of action'. Commenting on the first of these, Raz\textsuperscript{14} spells out what he feels a more adequate account must include:\textsuperscript{15}

A comprehensive theory may well include a set of technical concepts...but to show that they fulfil their function one must, firstly, analyse the logic of ordinary discourse on matters involving conflict of reasons and, secondly, show that the proposed conceptual framework can be used for the same purposes.

This seems good, common sense, but, to revert to my initial question, can such a theory cope with the complexities of ordinary life and remain within the domain of logic. What I have to say next points to the fact that it cannot.
I move on then to more general problems with a broadly deductive approach to practical reason. Anscombe puts one of the main problems with Chisholm's account quite caustically. She offers a practical syllogism: 'Nicotine is a deadly poison, what's in this bottle is nicotine' and draws the 'wrong' conclusion - 'so I'll drink it.' I say 'caustic' because it reveals a complete inadequacy in Chisholm's work which is that it has no account of agency or intentionality. One presupposition of making the correct 'deduction' in a syllogism is knowing what the agent's intentions are: in this case, suicide. Without that the formal system is vacuous. A further point, related to my comments on narrativity, is made by David Milligan who shows how important the context is for understanding an agent's reasons. The context, he says, presupposed by most logicians working on practical reason is one in which there is 'a clearly established purpose and only one means of achieving it.' This context however is very unusual, even in simple cases, it being far more usual for the agent to have to make a choice between alternative means or, since ends are appropriate to a decision, between alternative ends. He provides an example which could easily find parallels in an educational context:

Suppose someone is faced with choosing among candidates in an election...He might, in such a
situation, be fairly sure about what he wants the
government or committee to achieve, and thus all he
has to decide is which of the candidates seems most
likely the achievement of that end. On the other hand
he might not be so sure, and yet feel he has an
obligation to vote. Before he deliberates he might be
uncertain about what the government or committee
should be trying to do, and part of his deliberation
would involve deciding about just that. Indeed it
could be argued that no reasonable decision about
voting could be made until that question was settled.

Another example brings Milligan\textsuperscript{20} very close to a
comment by David Wiggins (see below) referring to the
same problem:

...before setting out on a drive we have no definite
plan. We first decide where we want to go, and in
making that decision we may not have any definite
purpose, such as finding some rare plant. All we know
is that we want an enjoyable outing and that many
different ones would be enjoyable.

Wiggins, who calls such examples 'non-technical', says of
them:\textsuperscript{21}

that I shall have an extremely vague description of
something I want - a good life, a satisfying
profession, an interesting holiday, an amusing
evening - and the problem is not to see what will be
causally efficacious in bringing this about but to
see what really qualifies as an adequate and
practically realizable specification of what would
satisfy this want. Deliberation is still zetesis, a
search, but it is not primarily a search for means.

Adding that there will be a coming and going between the
end and the means until both an adequate specification of
the end and practically realizable means are found.

The main point of these examples is twofold.
Firstly they bring out some of those elements of day to
day practical reasoning which make it so different from
the 'technical' account. The second, more philosophically
important point is that it is difficult to see how a
formal deductive account could be given of them.
David Milligan offers a number of reasons why, even for simpler cases, it cannot. His first point is that even where an agent seems to have a definite end and only has to calculate the means, the situation is still less simple than it appears. The reason is that 'at every stage of the reasoning other factors than the tendency to achieve the one given end may be relevant'. The bridge builder, for example, might have very specific designs, very definite ideas about how the bridge might look and so on, but other, possibly suppressed, factors are relevant: what will be its effect on the ecology; is it, after all, too small for the expected volume of traffic; is it worth the extra expense as opposed to a smaller bridge higher up the river and so on. To this a deductivist might reply by suggesting the addition of all the other factors as augmentations of the original premiss. This, however, presupposes that 'all the wants can be known or fixed in advance', which cannot be the case: consider, for example, that after the bridge is built it is found to have serious consequences on the local ecology and attracts far more traffic than expected. These results were not known before the bridge was built and cannot therefore be built into the premiss. Milligan admits that extra premisses might be built in as the deliberation proceeds and that this might preserve its deductive claim. However, in a further example, (in which someone is forced to give up his original end and hence the whole project because of a moral reason),
Milligan challenges the deductivist to show how such a non-deliberative preference could be built into the set of premisses. He suggests that they cannot, arguing that deductivism cannot, in the end, cope with deliberation which involves choice, ordering of preferences or coming to a decision about what one wants in a particular situation.

An appeal to decision theory is also dismissed as a possible answer on the grounds that it 'has little or nothing to say about preferences' and is only applicable where the outcomes of actions can be reasonably well predicted. Still less, he argues, can it cope with a choice such as that faced by a school leaver for, just as in the case mentioned by Wiggins, deliberation is not merely of means but of ends in a dialogical sort of way in which little is decided until some specification is settled on.

I shall focus on two reasons, both compelling, which Milligan gives for his suspicion that deductivism cannot cope with choice and decision. The first focuses on the addition of a further premiss. Using an example of a syllogism in which the first, true, premiss is, 'Medicine is a good career for John because it is well paid' he shows that the addition of a false premiss such as, 'All careers which are well-paid are good careers for John' renders, as valid, the conclusion that medicine is a good career for John. On the other hand, the addition of a true premiss such as, 'Some careers which are
well-paid are good careers for John' renders the argument invalid. Further still, the addition of a more likely premiss such as 'Careers which are well-paid are likely to be good careers for John' only justifies the conclusion that medicine is likely to be a good career for John - which is not the stated conclusion. Summing up, Milligan suggests that no additional premiss which tries to turn an argument into a deductive form will reveal the real structure and in many cases simply change the argument. 25

The second argument focuses on the deductivist's account of mistakes in argument: mistakes which can only arise because the agent has inadequate premisses or reasons incorrectly. It has obvious force and consequently I shall quote it in full. 26

Suppose two social workers...have to decide whether a mentally handicapped child should be admitted to an institution. They agree about what should be done. Each might claim that they had made a responsible decision for which they could be held to account: they had reasons which justified the decision and which would enable them to criticise the alternative decision. Each might claim that, given the facts, their conclusion was inevitable. But they could criticise each other's reasoning - agreeing about the facts and not about what followed from them.

What would be the grounds of the criticism and at what point in their decision making do they make the choices necessary for there to be responsibility? To try and settle their differences they go through the stages by which the decision was reached. They agree about the extent of the handicap, the effects on the different members of the child's family of having him at home or in the institution, the facilities and the quality of the caring at the institution, the effect of each alternative on the child's happiness and on his development. They agree that both the child's interests and the family's interests are important. Their disagreement lies in their assessment of the ways in which the interests of the child, the parents and the other children are
affected and of the relative importance of the interests of each.

The challenge, insurmountable, as far as I can see, to the deductivist is how to give an account of this piece of reasoning: reasoning which is all the more important here because it typifies the sort of reasoning which is at the centre of attempts to solve educational problems.

I think these arguments show that no formal, structural or logical, account can be given of practical reason. This poses a problem for anyone given the task of trying to say something substantive with regard to it. However, I do not think this is an insurmountable problem: logic is not the only possible solution and it was always an unlikely candidate for dealing with Kant's 'mother wit'. The problem we are left with is whether a non-technical approach to practical reason either fares any better or, more significantly from a pragmatic point of view, has anything of value to say about better deliberation.

I believe that Donald Davidson was right to point to beliefs and desires as the key to understanding both other people and ourselves. The point is reinforced by Anscombe's 'poison' example: no conclusion can be drawn from a practical syllogism unless something is known of the beliefs, intentions or state of mind of the agent. Anthony Kenny makes the same point whilst trying to persuade us of the logic of satisfaction: he says, 'the defeasibility of practical reasoning comes about because of satisfactoriness being - like explanation - a relative
notion: something is not satisfactory simpliciter, but satisfactory relative to a given set of wants.'\textsuperscript{28} Milligan fastens on to the importance of this and makes 'feature wants' central to his explanation of deliberation.\textsuperscript{29} He lists five conditions of such wants: they have an object, the object has a specific feature, the agent has some 'pro-attitude' to the feature, the agent will try to bring about a state of affairs which has the feature and the state of affairs must non-incidentally contain the feature. Deliberation is thus, for him, a rational yet non-deductive method by which the agent sets about obtaining what he wants. I will utilize this using Anscombe's example in order to see how it works: the 'nicotine' has the 'feature', 'poison'; the agent wants to commit suicide; nicotine has the 'feature' required of the object, namely, it creates the state of affairs in which death is the outcome, and so it is not incidental that the agent drinks the poison. So far so good; but does it work with Milligan's 'Social Worker' example?

Let us, for the moment, unpack some of the complicating features of the social worker example. The first thing to be noted is the obscurity of the feature wanted. Indeed, it is a specification, to use Wiggins' expression, of the desired feature which is required. The two social workers have to come to some conclusion about what eventual state of affairs will best specify the desired feature of the want. What that feature is,
however, is not, as it were, given by the want: wants do not contain directions for actions - just as Wittgenstein's rules do not contain their applications. In this example, however, we are not even sure what it is which is wanted; except the totally vague want which is wanting to do the best - not even for whom is obvious. The common expression for this is wanting to do the best in the circumstances. 'In the circumstances' there are a number of people for whom the social workers want to do the best: the child, his parents and his friends. There are also many other factors; factors which, I believe, are not merely a backdrop to deliberation, but are powerful influences on the decision itself. These 'circumstantial' factors - which are by no means determinative - include various groups, or 'interested parties'. In Appendix B30 I explore, in greater detail, how these 'parties' might effect the decisions of agents. There I identify, as the most significant, three particular groups: those to whom a justification must be given, those concerned and those held in esteem. Although I will not go into any detail here one of the main points which I think needs to be made - and which links back to the previous chapter - is that such contextualizing factors affect the narrative in which actions are taken. Practical decisions made on the basis of personal benefit, for example, may be different from those made on the basis of the moral consequences. However, I think enough has been said to provide a pointer to further
considerations. Before that, and by way of introduction, I offer a brief summary.

I mention, firstly, a number of 'negative' conclusions which are helpful in the sense that they keep in perspective the problems to avoid. These are that attempts such as Chisholm's to reduce and formalise practical syllogisms are too simplistic for use in 'real life' situations and that practical reasoning is not a class of theoretical reasoning which, in any case, says little or nothing about what we usually mean by practical reason.

The 'positive' conclusions, upon which I believe we can build, are as follows. Firstly, what we normally mean by practical reason includes such factors as agents' intentions, beliefs 'pro-attitudes', relevant 'objects' of such attitudes and understanding of circumstances and so on. Secondly, that there is often a complex dialectic between means and ends which sometimes brings about either a different specification of the end or an alteration of means during the process. Thirdly, the type of reasoning employed in practical reason is not typically logical (according to the canons of logic books) and is more of the type referred to by Kant as 'mother wit' and, further, as the 'social worker' example shows, is liable to differing weightings according to the emphasis given to various interests, interest groups and moral considerations. Finally that, while no 'standard case' of practical reasoning exists, the best examples
are to be found in pieces of actual practical reasoning where cases are put, weighed, contrasted, argued for and so on. It is to this that I now turn.

6.30 **Practical Reason and Educational Research.**

The key question that now arises is thus how do we find a more adequate method of practical reason which is of use in educational research. The question brings the thesis back to its origins outlined at the very beginning of the first chapter. These, it will be recalled, stem from Aristotle's warning against the use of scientific methods to either predict or determine our future. The correct methodology for Aristotle, where human affairs were concerned, was Phronesis - Practical Reason. I have taken some time to outline my reasons why a formalized, reductive form of practical reason - such as that proposed by philosophers such as Chisholm - is inadequate. Practical reason, even in the simpler cases, such as whether to catch a bus or a train to work, is often much more complex than it appears: are there any roadworks, will the 8.43 be cancelled again, will I be able to get a seat, will I make my appointment at 10 o'clock, wasn't there something about a train strike on the news last night? Whatever the decision in human affairs **something has to be done** because I have to get there somehow!

Having to get there somehow is a rather essential
question when faced with the legal obligation placed on schools and education authorities to implement the Education Reform Act! In this context the quest for a 'more adequate' form of practical reason becomes at once both more urgent and more perplexing. Some ends, it seems, have been specified - assessments for pupils at 7, 11, 13 and 16 years. Specified attainment targets for each level. The transfer of major educational responsibilities from the government and local authorities to schools, parents, governors and teachers. A timescale by which various parts of the Reform Act are to be implemented and various working bodies to aid and prescribe that implementation. It seems as if ends have been specified and that only means need to be deliberated about. This however is not the case. It appears that while there are to be attainment targets for all core and foundation subjects these need not be the same as the national criteria which prescribe the standards expected for GCSE grades. A number of interesting scenarios arise from this. One possibility is that pupils will have to take two examinations, another is that attainment targets will replace GCSE, another is that pupils taking four or five GCSE subjects which are not part of the national curriculum will have to spend less time on GCSE work than on the compulsory component. Needless to say the timetabling problems are daunting but, and which is worse, they are subject to variables which are not prescribed by the act. In other words, ends are not
specified - many of them will simply be decided by individual timetablers and individual schools.

If it were possible to use the practical reasoning involved in the implementation of the national curriculum and then use this as an exemplar of other pieces of reasoning in public policy matters, then that paradigm might have been useful. However, as is patently the case, it is not a particularly helpful example. The conclusions of 6.20 help us to see why. Firstly, the reasoning process has not taken into account the intentions, beliefs and attitudes of those most affected. Secondly, it has not asked those most involved with the practical outcomes of the policies to be involved in the period of policy formation. Thirdly, it appears to ignore the enormous complexity of such radical policies and the effects they might have. Fourthly, its implementation is not merely the clearcut technical matter which phrases such as 'phased introduction' lead us to believe. Fifthly, its carefully constructed terminology supporting its case is often no more than a clever use of rhetoric which actually distracts from possibly important educational considerations. Finally, as suggested, while it appears to have specified ends these are not at all clear and need to be clarified if chaos is not to ensue.

These comments, however, do bring out some of the issues that are central to practical reasoning relevant to the formation of public policies. They highlight the need to look at alternative policies. They highlight the
need to expose rhetoric as against reasoned argument. They highlight the need to consult, at various stages, those who are involved in the actual implementation of the policy. They highlight the need for consensus and openness in important public policy and the need to identify practical problems arising from such a policy before they arise in practice. Of course, comments such as these presuppose a prior commitment to various democratic ideals such as equality of respect and personal autonomy which, while not constitutive elements of practical reason in general, do seem to be required as a matter of ethical propriety by the fact that they are fundamentally matters of public concern. They also help to prescribe some parameters around the notion of practical reason in public affairs even though such parameters might, in themselves, be unnecessary in all cases. Having said that, however, there does seem to be - from what has been said of the importance of historical practice - some justification for looking at the 'methodology' within a public debate. Not, however, as an exemplar for Educational Research - for it contains many weaknesses as well as strengths - but as an example of how we do, as a matter of fact, go about resolving major policy issues.

6.31 The Energy Debate and Practical Reason.

As stated, I look at the public debate over
various forms of energy not as an exemplar or paradigm for practical reason in educational contexts but rather as a means of exploring some of the intricacies of an actual rather than a ficticious debate. I make no apologies for not using the deliberations which have gone into the introduction of the national curriculum instead for the reasons outlined. I hope to show, however, both how the following discussion might be of use to a wide range of educational problems and how, ultimately, it might provide the basis for a more adequate basis for specific areas of educational research. At the same time I am conscious that the energy debate itself is not central to my thesis and will therefore make every attempt to keep the detail to a minimum whilst highlighting the main features of the deliberative process which is central to the thesis.

The issue which generates the need for deliberation concerns the necessity of providing a nation with energy and the alternative forms available. These are, in the main: coal, oil, gas, nuclear power or some new form such as wind or wave power. The largest context of the debate is not about ends or means but of the ultimate benefit to society. This then is where I make my beginning. The group we earlier termed those of concern are the members of society and their heirs, they also constitute the group to whom those responsible for making a decision are accountable. I will not reiterate the contextual factors outlined in appendix B except to say
that any decision must, in addition to these, be made within parameters already set within the society. These include land and property rights, conventions which exist within the tradition over rights of way, beauty spots and so on.

I shall argue that in the process of the debate the central characteristic which has emerged is the formation of defensible arguments supporting various courses of action which constitute the means. These are then examined together and weighed.

I begin with the end under discussion. It will be obvious that various answers can be given to the question as to what constitutes the greatest benefit to society. The particular answer given, it is interesting to note, plays an important role in the formation of the case. What sorts of answers can be given or, to revert to Wiggins' terminology, how is the end best specified? The most prominent ones which are given are broadly as follows: (a) that which is most cost efficient, (b) that which ensures the least environmental damage and a dependable source of energy, (c) that which ensures a lasting source of energy and provides work for our people and (d) that which utilises unlimited and free sources of energy. The features I have built into these specifications might be equated with Milligan's 'feature wants'. Debate over which of these different ends is most desirable may take place at several levels. Firstly, it might take place at the level of the case brought forward
to support it. I will look at how a case might be made below. Secondly, it might take place at a more philosophical level in which the various features mentioned are contrasted and evaluated.

6.32 The Public Use of Cases to Structure Debate.

As has been noted, the public debate has generated cases. These cases usually consist in a compilation of facts, various arguments designed both to support the case and bring out the weaknesses of other cases and so on. What then goes into making a case? We might choose the case for and against nuclear power as the best known in the energy debate. The proponent of the nuclear case might think that it supports (a); it is the most cost effective. It has a number of drawbacks however with respect to the features demanded by (b) to (d), so its case might take on a double aspect - the first presenting the positive need for it and the second showing the weaknesses of other cases which are met. Let us explore how it might respond to this.

Before we can do this it is well to remind ourselves that cases have a 'locutionary' context - here a justificatory one relative to a perceived audience. (A point which I tried to stress in my Caesar example in the previous chapter). In this instance we have identified the audience, the group to whom justification is owed, as the society at large. The notion that a case is not just
facts and reasons but is audience related and out to persuade is an important factor which I will return to at a later stage. It is in areas such as these, however, that the need to remain objective becomes paramount. This underlies my emphasis on objectivity in the previous chapter.

6.33 Competing Cases.

The case for a particular desired end may have a number of aspects and we have already sloughed off one. Other aspects might be classified broadly as the case for the particular end and the case against its competitors. Both cases often rely on a selection of facts seen as of relevance and both cases often seek to construct an argument around those facts. Initially, therefore, a selection procedure takes place: facts are chosen for their relevance to a case whether they are for or against. This selection presupposes a number of criteria. Firstly, criteria of relevance and secondly, criteria which identify 'pro-attitudes' in the audience. Relevance, in our example, might be determined by various other factors such as whether the fuel is readily available, cheap, dangerous and/or unlimited. They might include manpower and whether that is readily available and efficiently utilisable. They might include environmental issues such as whether the power station is unsightly or gives off a poisonous gas and they might
include such factors as the danger of the work place and so on. Indeed the list could be quite lengthy. Given all this, the facts which are relevant are chosen and, given that the audience has, or is perceived to have, certain pro-attitudes, the relevant facts are sorted to present the brightest picture. Justification, however, also demands defensive arguments which answer criticism. Here further facts are sorted to counter arguments likely to be brought against the case. These, at least in our example, will include research findings showing the almost negligible harm brought about by radiation, examples of low radiation readings and so on. At the same time, and for the same reason, the case will contain all the weaknesses it can find in the competing cases: acid rain, ruined moorland and estuaries, a high mortality rate amongst coal miners, unsightly pit waste, ugly oil rigs which are also a shipping hazard and so on. Again, facts are selected for a purpose and that purpose is determined both by the feature desired, the beliefs which give rise to it and the need to present the case to an audience.

The second feature of the case mentioned was its attempt to show how many of the desirable features of the competitors' case it could accommodate. Here again there is careful selection of factual information and an argument framed so as to show how this case fits all or as many of the bills as possible. Opposed to the environmentalists its power stations are few and far
between and safe; opposed to the proponents of wind and wave it leaves areas of natural beauty as they are; opposed to the coal mining lobby it is much safer and cleaner and so on. Much the same could be said about a further aspect of any case - which is the case it makes against its opponents' case - pointing out weaknesses, deficiencies and so on.

So far we have only dealt with what might be called the first stage of the deliberative process. The second and most vital stage follows on and is that in which the various cases are evaluated before a decision can be made. In order to facilitate an easier 'unpacking' of this stage I shall summarise what has been said thus far concerning the making and structure of a case. We began by assuming that some unquestioned end was accepted: this we stated as a general requirement concerning the energy necessary to the benefit of society. However, such 'ends', as we have seen, do not determine the means - indeed, it is this which gives rise to competing cases. Cases consisted of arguments, built around factual information, which did one of three things. They provided direct and positive arguments for the project, a defense against possible counter arguments, an attack on rival arguments and they attempted to show how they met, within a degree of reasonableness, the most desirable features of the other cases. Within these arguments we noted that facts were selected by various criteria related to the type of
argument (defensive, positive etc) and by reference to what were perceived to be pro-attitudes amongst those to whom the arguments were directed. Choice is then made upon all the available information which includes various cases and counter cases. The decision - which is hypothetical in the present example - is then made by weighing one case against another.

6.34 Case Evaluation and Weighting Procedures.

How then are cases weighted? I think that it is fair, because of various interests and values, to assume that individuals will have pre-formed beliefs about which ends best specify whatever end is most beneficial to society. Any weighting will therefore take the form of an interplay between beliefs already formed and the arguments presented. I shall not here comment on the beliefs formed and will mention them only as they arise in the context of the case presented.

In our example public deliberation is a form of thought based on the comparison of carefully constructed cases: it consists in a rational evaluation of the arguments presented. However, in the case as I have described it, cases themselves are neither rational nor fair. They are not rational (unless rationality is reduced to rhetoric) in the sense that they present only their own best case and they are not fair in the sense that no cognizance is taken of the value of possible
alternatives. This is why, if the deliberative process is to be rational and objective, it needs to take into account various cases and the rhetorical ploys each utilises. This aspect serves to underline my earlier statement that the example I am using is not a paradigm for any form of educational research but is useful insofar as it highlights structural elements - some of which are far from constructive or conducive to rational policy making. Weighting as a fundamental feature of evaluation is therefore central to rational policy formation.

It will be obvious that, at this point in time, I cannot use the present example to illustrate how evaluation has taken place - for it has not been resolved. What I have to say about evaluation is thus prescriptive rather than descriptive. Its rationale is based simply on the fact that it would seem to be good sense to look at various cases, weigh them against each other and then make what would be, by this point, an informed decision.

What then is weighting? Not wishing to fall into some form of essentialism I defer the answer to ordinary discourse where what is usually meant is the attribution of some form of value. Weights are normally allotted in the context of all the cases considered together. What we say about our methods of weighting often seem mundane. We say such things as, 'All things considered' or, 'I don't think A's argument was very good - he seemed to leave out
the fact that...' and, 'He was having us on', 'He might think that x has been shown to be safe but I don't believe him' and so on. Hidden below these blithe comments, however, are serious worries which a philosopher would classify as fallacious arguments, arguments with unsupported premisses, insufficient inductive evidence, and non sequiturs. And indeed, whether lay person or philosopher, these are the very ways in which arguments are evaluated and weights given. In the context of deliberation however, and even supposing that a philosopher had gone through all the arguments pointing out their inadequacies, I doubt whether a conclusion could be drawn from that alone. The reason is that all the cases will contain these elements (except in exceptional cases - but how many philosophical arguments at the highest level of abstraction are ever completely watertight?) and they will contain pejorative elements too, with little or no weight at all. Now it might be tempting to conclude from this that deliberation is merely a cover for opting or plumping but I do not think it is. The informality of the weighting system contains its own rationality and while it does not conform to the laws of propositional calculus it does contain its 'mother wit'. There are, after all, different arguments, different facts used in support, cases put and cases answered, justifications given and defenses made. There are also good reasons why some arguments are spurious, why some facts are not established, why some
statistics have alternative interpretations and by and large it can be seen whether any one case has a larger or smaller quantity of these than the others. It may well be that the inadequacies are fairly evenly spread but this points to the most important factor in deliberation: there is no simple formula, no calculus, no single rule which determines what the correct solution to a piece of practical reasoning is and because of this we are forced to rely on our experience of the past, our intuitions and our imagination, as well as our rational faculties to guide us. This, if I am not mistaken, is the very reason Aristotle marks it off from theoria; the theory of the invariable. Deliberation contrasts with it because it deals with possibilities and not certainties. The person described as having practical wisdom does not need to know the laws of logic or the discoveries of science for his deliberation is a product of his understanding in the widest sense of the term and that wisdom, as Kant rightly says, 'cannot be taught in school.'

6.40 Concluding Remarks.

There is a reason why chapters five and six have been divided into two chapters although they both deal with the deliberative process. Separating them, as I have done, may have given the false impression - which may find its origins in the legacy of scientism - that the collection of facts or data is one discrete activity
while the formulation of a policy is another. This, however, is not my view. It seems to me that, given an educational problem and given that a policy - in the form of a set of recommended actions - is to be the outcome of a piece of educational research, the 'collector of facts' is of necessity well aware both of the problem and of the possible actions which may be taken. The necessity derives from the notion of relevance, mentioned earlier, for facts are, almost literally, endless and so, consequently, are narratives. Thus, a requirement of the compilation of an adequate narrative upon which deliberation can take place is that the narrator - the collector - is a deliberator. Deliberation, at this early stage, takes the form of looking into the problem, thinking which circumstances may or may not be pertinent to it and keeping in mind the possible solutions which may be practicable. Fact finding, therefore, even in its most elementary form, has a context and this context is the problem and its possible solution. The division between chapter five and six is thus somewhat misleading, especially if it is taken to reinforce the scientific differentiation between research and policy. Educational research, if it is to take on board the complexities of human personality, society, history and so on, must work within a wide framework of practical reason and not just attempt to simplify it through science or some form of simplistic process of reasoning. At the same time it must recognise the relationship between the ends it endeavours
to achieve and the means it employs to achieve them. Policy cannot be hived off from research any more than research can be hived off from practice. Education is about people, and educational change is brought about in an educationally beneficial way when the people being educated, as well as the educators, are involved from the outset in the process of change. At the present time we have four distinct groups: the taught, the teachers, the researchers and the policy makers. Given this, the lack of dialogue between them, the all pervasive ethos of scientism and the absence a critical reflection on matters such as rhetoric, new policies, if successful, would seem to be more likely the outcome of chance than of reason. The final chapter is thus dedicated to fundamental reform so that reason, and not chance, is the dominant force in educational policy and research.
Chapter Six: Footnotes.


(5) J.Raz, "Reasons, requirements and Conflicts" in Korner (Ed.). p22.

(6) Chisholm, op cit, p3.

(7) ibid, p5.

(8) ibid, p15.

(9) ibid, p16.

(10) Anscombe, op cit, p21.

(11) Raz, op cit, p27.

(12) J.W.N.Watkins, "What Does Chisholm Require of Us?" in 'Practical Reason', (Ed. S.Korner). Watkins, following Popper, defines the relation as follows: 'if a conclusion q is logically implied by premisses p, then it is logically implied by any augmented premiss p&x, whatever x may be (and even if x is inconsistent with p).'

(13) Chisholm (op cit, p13) quotes Samuel Clarke from his 'Discourse upon Natural Religion' published in London in 1706.

(14) Raz, op cit, p23.

(15) The task of analysing the 'logic of ordinary discourse' has been pioneered, in the context of practical reason, by G.H.Von Wright. I include here some examples of work which has attempted to take into account the various complexities I have mentioned.

Von Wright G.H, 'Norm and Action', R.K.P, 1963. As an example of the width of his work I include the following quotation (p viii):

[Quotation from Von Wright]
We could say that formal logic, as we know it today, is essentially the logic of a static world. Its basic objects are possible states of affairs and their analysis by means of such categories as thing, property, and relation. There is no room for change in this world...Acts, however, are essentially connected with changes. A state which is not there may come into being as a result of human interference with the world; or a state which is there may be made to vanish. Action can also continue states of affairs which would otherwise disappear, or suppress states which would otherwise come into being. A necessary requirement of a logic of action is therefore a logic of change.

I also add a quotation in which the difficulties of such a task are accepted. I should add that on my own view, because of the 'embeddedness' of actions and their norms or rules in a narrative, the perceived difficulty will prove insurmountable within a truth functional logic. (p ix)

The conception of deontic logic as a logic of norm - propositions challenges the question, what it means to say of propositions, or of norms generally, that they exist. Wherein does the 'reality' of a norm lie? This is the ontological problem of norms...I find the problem extremely difficult, and do not feel at all satisfied with the details of my proposed solution to it. But I feel convinced that, if deontic logic is going to be anything more than an empty play with symbols, its principles will have to be justified on the basis of considerations pertaining to the ontological status of norms.

(16) Anscombe, op cit, p19.


(18) ibid, p48.

(19) ibid, p49.

A parallel example within an educational framework might be as follows: A school staff meets to decide what to do with a child who is a constant disruptive influence on a class. The decision reached will not only concern means, for there will be a prior consideration of which ends are most desirable. These might include the best interests of the class, in which case removal of the disruptive child might be the best means. On the other hand, it might not be in the best interests of the disruptive child psychologically to take him out of the one context in which he has established firm relationships.
(20) ibid, p49.


(22) Milligan, op cit, p51.

(23) ibid, p52.

(24) ibid, p54. See also Donald Davidson, who says, for example, in his essay, "Psychology as Philosophy", ('Essays on Actions and Events', Clarendon, 1982. p236) that he 'found it impossible to construct a formal (decision) theory that could explain (the results) and gave up my career as an experimental psychologist.'

(25) ibid, p58.

(26) ibid, p64/5.


(28) ibid, p76.


(30) Appendix B
Chapter Seven.

7.0 Substantive Proposals for Educational Research.

Introduction.

The thesis I have presented so far might, up to this point, be said to have three parts. The first, in chapters one and two, presents a possible mode of educational research which I have characterised as 'scientistic'. This mode consists in two discrete activities: (a) experimental research after the model of the positive natural sciences and (b) the application of the findings of such research to educational situations. The second of these - which I earlier referred to as a type of engineering - is probably, though not necessarily, the sort of activity envisaged by a science of management. The second part, elements of which were
found in chapters two, three and four, was concerned to show how, for one reason or another, this model of research and application does not work, or, as I should prefer to say, is inappropriate. The third part might be said to have two components. Of these, the first attempted, in chapters two, three and four, to see what hinders the scientistic programme while the second, in chapters four, five and six, attempted to find what one might consider 'building blocks' which avoid the problems of the scientific endeavour and provide, at the same time, a firm foundation for a more appropriate means of making educationally beneficial policy decisions.

This final chapter therefore constitutes the fourth part and attempts to create one viable, educationally beneficial alternative to what science has attempted to do. This, I consider fundamental to the thesis as a whole, for it has never been my intention to say that because scientism is an inadequate basis for education research that there is no adequate basis for educational research. Indeed, throughout the thesis I have kept in mind the possibility of providing a more appropriate basis for an actual research model taking into account the weaknesses exposed in scientism. In order to do this, I have divided the chapter into three sections. The first (7.1) is a reflection on scientism. The second (7.2) is a bringing together of the 'building blocks' and the third (7.3 - 7.5) is a proposal for a substantive model of educational research which
avoids the pitfalls of scientism and incorporates what seems to be necessary when dealing with human beings. The chapter breakdown is therefore as follows:

7.1 Reflections on the implications for the scientistic model such as those proposed by O'Connor.
7.11 Summary of the main conclusions concerning the scientistic model.

7.2 Introduction to the main substantive proposals.

7.3 The Research Proposal.

7.4 Collecting a Narrative.
7.41 The Range and Scope of Narratives.
7.42 The role of the narrator.
7.43 The role of the narrative in practical deliberation.

7.5 The Panel and its role in Practical Reasoning.

7.6 Concluding Remarks.

What I propose, it must be said, is not meant to be a standard model for all educational research, but rather an example of how educational research might be carried out in a way which is not scientific. Many questions of both a philosophical and a practical nature will not be answered. Indeed, this thesis generates many
philosophical problems (about the nature of democracy, the objectivity of narratives, the best mode of deliberation and so on) for quite a few more theses. It also can be tested in that it concludes with proposals which are intended to be practised and not just left on a library shelf. I begin with a short reflection which relates my concerns in the first chapter to the present. It concerns the scientific approach to educational research and policy as advocated so eloquently by D.J.O'Connor.

7.1 Reflections on the implications for the scientistic model such as those proposed by O'Connor.

The major implication for the type of scientism advocated by D.J.O'Connor is that in the light of my enquiry (if conceded) it will no longer be possible to presume that a human science - including the so called 'applied' branches, such as 'management science' - forms an explanatory background (in the case of the former), or a method of implementation (in the latter). A corollary of this is that the roles of those involved in scientistic research and management cease to have a function. It follows also that in the absence of any well established 'laws' or known causal chains which might have been the products of such a science, decision makers have nothing on which to base their decisions. Where once
a policy maker might have based a decision on a known causal connection between, say, truancy and poor employment prospects, now no such 'hard' information is available. If our arguments in chapter three, at least, hold good there is no possible way of experimentally verifying any such links, be they reductive laws stated in terms of an advanced physics or laws of a psycho-physical type. Nor, if our arguments in chapter two hold, is there a way of constructing the inductive basis for such laws.

As things stand at the moment, however, the 'technico-rational' paradigm is accepted - on the whole - and policy makers rely on such information. Because this is the case, information produced by methods outlined in chapter two is regarded as knowledge and is consequently seen as a legitimate basis for technological - or 'managerial' (in the human sphere) - application. We have, for the most part, and increasingly so in the present political climate, an overriding interest in technical control based on the findings of the human sciences and an unexamined belief in the efficacy of a managerial counterpart of the type of engineering developed, significantly by the Victorians, in the context of building bridges, railways and other industrial projects. Information in this paradigm is treated much as it might be by an engineer: given that $x$ is the breaking strain of metal $Y$, the supports of the bridge ought to be $z$ inches in diameter. Throughout the
first part of the thesis I was concerned to say, centrally, that the phrase 'given that x' cannot be assumed in human contexts. This, however, has the consequence of making the engineering model of applied human science redundant. The reason is that the manager-engineer must always begin her calculations with some given (See the crane example in Chapter Two where such assumptions had to be made.) Suppose, however, that it had been shown that children with a certain kind of personality and a certain home background always failed to get a G.C.S.E grade A then it should follow that a manager could use the information to help those children. He would have to start with what is given: children with characteristics s and t always fail Z. Policy would then be reached by considering which of the characteristics could be changed. On changing, say, the home background (with help from the welfare services!) so that it was not an s, she might then expect the child to pass Z - or at least have a better chance. But how is that kind of managerial function to operate? My answer - as outlined in chapter two - is that it cannot because the manager-cum-technocrat has no 'givens', in the form of hard scientific data, which might be 'applied'.

This radical difference, which is central to the human and natural sciences, gives reason to question the distinctive roles of researcher and policy maker which have historically accompanied the practice of the scientific investigation. If I am right, although it will
not form a central part of this thesis, these roles have mistakenly been carried over into attempts at human science and failed. They have failed at the practical level for the very reasons spelled out in the second chapter, that the researcher who provides the 'givens' is not actually producing anything like scientific givens and that the policy maker - attempting to apply these 'givens' - doubly fails; firstly because of the inadequacy of the 'given' and secondly because of the mistaken notion that management in education is an applied science just like engineering. A strong thesis which might be made is that the distinct roles of researcher and manager derive from an inadequate scientism and therefore ought to be dropped. I shall be content, at this point, to be wary of a hard distinction. My wariness, however, will become apparent in what I have to say later. The main difference being in the relationship between what is understood about the situation to be changed and the way it is to be changed. This intimacy is well understood by parents and teachers who want their children to change some aspect of their behaviour: it involves an understanding of the child because whatever change is sought is only brought to fruition by allowing the child to retain his identity and self respect throughout. What constitutes that self respect and identity has therefore to be known to the parent and teacher beforehand. Failure in this respect often leads to rebellion and, of course, failure to
change the child in the manner originally sought. The parallel to this in educational research - as I shall outline it - is in the appreciation the researchers have of the situation to be changed. Managers therefore, or policy makers generally, cannot act independently of that awareness. Where they do - and at the moment they try to - it will not be surprising to the parents, at least, if their policies lead in a direction which was unintended. I will incorporate the lesson in this into my proposals by beginning to break down the distinction between researcher and policy maker but also between public and researcher and between researcher and researched.

7.11 Summary of the main conclusions concerning the scientistic model.

I move now to a summary of what has been said so far. The points we have been making might be restated as follows.

1. The distinction between research as one activity and policy making as another may well be a rather unhelpful scientistic dualism which needs careful consideration in future.

2. Any model of policy making which presupposes some 'given', resulting from the enquiries of a supposed science of humanity and which is in that sense 'applied
science', is ungrounded. It is ungrounded for two reasons:

(a) It has no ground in the so called human sciences because they are not epistemologically sound and are not productive of knowledge about ourselves.

(b) In human affairs, policy presupposes an understanding of that into which the policy is to be implemented and cannot act independently of those methods by which such understanding is brought about. Policy makers, in other words, must be aware of the initial process of information gathering - in the process of what has been called research.

3. In the absence of a basis in 'given' information, the notion of 'applied' is inappropriate since it carries with it the concept of something - the given - which is to be applied.

4. The notion of 'applied', construed mechanistically is equally inadequate, for reasons which are independent of its ungroundedness. These, as illustrated, derive at root from the inability of the practitioner of applied science to mathematicize and quantify the objects of enquiry resulting in a fallacious analogue to the paradigm of engineering as applied in the non human world. (Or even, non animate world.)
Together these implications more or less render the scientistic model of educational research, policy and theory, both redundant and inappropriate. However, the present thesis is not nihilistic either in intent or in conclusion - for there are other ways of helping ourselves improve our educational system besides those of scientism.

7.2 Introduction to the main substantive proposals.

In broad outline any educational research will be generated by a concern - something is perceived to be not quite right, or, something appears to work so well that there appears to be a need to try to find out more about it. Educational research and policy decisions are therefore inexorably bound up to some view of educational practice or theory - it is, in other words, linked with aims and through aims to politics and ethics. Just what it is which generates educational research is not dealt with in this thesis because this is a matter of contingent, historical circumstance. However, without someone, somewhere, wanting to improve what goes on in education or the educational system, there would be no perceived need for research and change.

What then is involved in this process? Firstly, there must be the identification of a need - some
practice, for example, which obviously works well in one school and could be beneficial to others. This situation we might refer to as the targeted situation. Secondly, there must be some account of that situation. In our example this might entail going into the school, seeing what is going on and writing it down. It is at this second stage that the beliefs, desires, intentions and so on of the people involved (a dimension which I argued in chapter three were so difficult for human science to accommodate) may be taken into account. I have suggested in chapter five, that a narrative might be an appropriate means of doing this. Thirdly, there must be some form of consideration about the situation: how, for example, could the teaching methods employed be satisfactorily transferred to another school with a different intake of pupils and different staff. This involves the process of deliberation which was the subject of the previous chapter. In this context it involves both a situation and a policy - the situation is the good practice and the policy is its implementation in other schools.

The two things we have dismissed as possible methods in this type of enquiry - and which are central to the deductive-nomological scheme - are the possibility of discovering laws of human behaviour and the possibility of using logical deduction to arrive at policy statements. I have, however, provided some building blocks which can be used to build another structure. I do not argue that this 'structure' is the
only possible or even best possible means of arriving at educationally beneficial policies. What I do say is that because it takes into account the intentionality of actions and their context and attempts to work towards educationally preferable goals by a type of practical reason which is generally in use in day to day affairs rather than that which would otherwise be the province of expert logicians, it has a better chance of actually improving the education of people than other methods. Of course, since the thesis has direct consequences for actual research, this claim could be substantiated in practice. In this sense it meets one of Karl Popper's criteria for a theory, in that it is testable.

The following list summarises the main blocks with which I intend to build. Their inclusion also acts as an explanation, if it was not given, of work in earlier chapters. I mention the chapters where it seems appropriate.

1. The avoidance of some of the more blatant fallacies of scientism. (Chapters two and three.)

2. The importance of historical context in understanding a situation. (Chapter four.)

3. The intentional nature of human actions and the associated problems arising from attempts to make them compatible with extensionalism. (Chapter three.)
4. The 'embeddedness' of human actions in a contextual nexus the intelligibility of which presupposes the knowledge of a considerable amount of biographical material. (Chapters four and five.)

5. The pragmatic need for some such 'tool' (in research contexts) as a narrative to 'contain' the complexities implicit in the understanding of human actions. These 'complexities' include, minimally, intentions, intentionality, desires, beliefs, emotions, habits, innate reactions and historical context. (Chapter five)

6. The difficulties inherent in constructing and analysing a narrative whether concerned with pure, objective description or explanation.

7. The need for further work on the interrogative and rhetorical settings of narrative (and within narratives). I envisage that this type of work would build upon existing work both in pragmatics and speech act theory and attempt to contextualise it within political, ideological or more general historical settings.

8. The equal need for more work on the ways in which causes and explanations are identified, particularly with respect to the interests of the groups involved in identification.
9. The importance of the activity of deliberation in making practical decisions. (Chapter six.)

10. The impossibility of deliberating about human affairs in a purely deductive manner and the consequential need for human dialogue to be (a) aware of its own processes, contextual influences and constraints, and (b) reflectively creative in its attempts to resolve human issues.

7.3 The Research Proposal.

I will now state briefly the type of model I envisage for educational research and policy. It centres around two institutions - the 'narrator' and the 'panel', terms which I shall be expaining below. There is no sense in which it could be said to follow from the building blocks I have outlined; there are, I am sure, other ways of combining them.

In any enquiry there are at least four basic aspects to be considered. These are the perceived need for enquiry which is contextualised by some view of education and how it ought to be. In this sense, as both John White and Richard Peters have argued, ethics is central to education. Then there is the situation in which the 'targeted' educational activity takes place, whether it be mixed ability teaching, anti-racist teaching, the implementation of the national curriculum
or simply the behaviour of a particular pupil. Thirdly, there has to be some thought given to what is going on and how it might be going on. Lastly, there is the activity of implementing a set of actions intended to alter the situation in such a way as to make it more in accord with what was perceived in the first place.

Given this framework I will now show how the notions of narrative and practical reasoning can be made to work together to provide a viable method of not only doing research in education but also of tying it up closely with policy and its implementation. As I have already stated I have not touched the first issue mentioned above, which deals with how education might be, because this arises in real life situations and is related to how educational practice is perceived and it's aims. We must assume, however, that some educational practice has been isolated for some good educational reason. We must also assume that it has been isolated because it either needs remedial attention or requires further understanding or explanation so that it can be implemented elsewhere. I have argued in chapters four and five that human actions are such that an adequate account of them, which is required if something is to be done, can only be achieved in something broad and flexible enough to provide a whole picture of events. While I did not argue that this necessitated a narrative, I did argue that, for pragmatic reasons, a written narrative of events which recorded differing views, most closely
approximated what was required. A well written narrative thus provides a convenient description of the set of human events which constitute the subject of investigation. A narrator is thus also required.

The third of our basic parameters is that thought is given to the situation - thought which is directed toward some practical end. Practical reasoning about what to do within a living history requires that the narrative be read, the situation considered and possible courses of remedial, or other action be taken. It could be argued that one person could conceivably take on all these roles but - and it is a very important but - this would create unpropitious circumstances for both objectivity and explanation. The way around this, it seems to me, is that the roles of narrator and deliberator are not only separated but that during the process of deliberation the narrator herself has to defend or explain some or all of what has been written. The researcher becomes the researched. Deliberation is thus undertaken by either another person or, for reasons that again promote the greatest opportunity for objectivity, by a group. This group (which in very small scale or uncontroversial research might be one) I refer to as the panel. The word 'panel' seemed the least innocuous of the various choices available.

The role of the panel is to assimilate the situation, ponder it, argue between themselves what to do with it and provide a set of policies which when acted
upon put things right. In order to do this I imagine, but do not prescribe, that they will set up cases for and against various courses of action much as was discussed in chapter six. At the same time they might wish to either get a clearer picture of events or attempt to provide explanations for them in the sense of causes. In order to facilitate this, I suggest that they would be aided if they had the ability to talk both to the narrator and to some of the people described in the narrative. The end of the policy stage would come when the panel felt that the actions it proposed would change the situation with which they were concerned in accord with their brief (which takes it back to the original, motivating factor). The situation could be assessed by a similar process at a later date.

This then, in broad outline, is the process by which I conceive beneficial educational policies could be decided upon and implemented. I go on now to look at some of the problems this method might raise. I look firstly at the narrative and narrator (7.4) and secondly at the panel and the deliberative process (7.5). I shall then make a few concluding remarks which look ahead to the possibility of further work (7.6)

7.4 Collecting a Narrative.

Having a question to answer, the narrator is essentially free to put in the narrative whatever she
considers relevant. This demands deliberation at the outset, and since the deliberation derives from a policy-to-be-formulated, its parameters are set within that brief or desirable end. The narrator is therefore not merely a chronicler or one who passively collects facts; she exercises Aristotle's 'aesthetic' faculty in judging what is relevant, telling or of other significance from the otherwise infinite possibilities latent in the situation. It is this interplay mentioned earlier which is absent in the sciences which also causes a fracture in the distinction between researcher and decision maker, between scientist and technocrat. The exercise of that freedom is, however, obviously constrained both by the brief and the sensitivity of the narrator to discern something of significance in the situation-of-concern. To the extent that this 'discernment' is a necessary condition of the compilation of a relevant narrative which is ultimately also comprehensive the narrator must possess some degree of proven sensitivity in human affairs together with the necessary intelligence to utilize such information as is collected in such a way that it is organized into a readable story about the historical tract forming the object. At the same time I do not want to arbitrarily prescribe that narrative contents should consist only of records of contemporary historical conditions whether these be in the form of autobiographical statements or general social descriptions. Indeed I think that past
events as well as non contemporary biographical information and even photographs of damaged property, weaponry and so on might well contribute to the text as well and as importantly as those taken from the present.

The Collection of an Anti-Racist Narrative as an Example.

In this example I will imagine that a narrator has the brief to provide a narrative which will form the basis for the deliberation of an anti-racist policy to be implemented in schools.

The question which dominates the researcher and 'guides' her selection of material is the production of a policy which, when put into practice, will hopefully diminish racism in society, perhaps with the school as chief medium. 'Guide' is an unfortunate word to use in this context because she is given nothing to go by except her experience, her best intuitions and, probably the advice of friends and colleagues. Since it is a policy which is to be formulated, past policies might be of importance together with the effects they have had or were thought to have had. This might involve looking at other educational systems such as the American attempt at bussing. Writers, such as Chinua Achebe, N'Gugi Wa Thiongo, Leopold Sengor and V.S.Naipal together with biographical information about those who have taken part in an active struggle for the emancipation of a
subjugated people such as Gandhi, Martin Luther King, Robert Mugabe and Nelson Mandela might also provide material for the text insofar as they have thought deeply on the subject or have first hand experience of racism and its political manifestations. The insights into the nature of racism in its overt and violent form in countries where it is institutional rascism or its more benign paternalistic form practised by British, French and Portuguese administrators in pre-independent Africa, as well as certain forms of missionary work, can be derived from literary sources. Contemporary sources are unfortunately easy to come by, in the form of biographical material from British blacks in Toxteth and Brixton and from members of the various 'National Front' organizations which are increasingly influential with white working class youth who live in our inner cities. The experiences, successes and failures of the activists we have mentioned are also relevant to an overall picture apart from the intimate knowledge they have. Mahatma Gandhi and Martin Luther King, for example, believed and practised non violent protest and non co-operation. They were largely successful in the United States and India. President Mugabe and Nelson Mandela however, while holding the other two in high regard, do not believe passive resistance to be a viable policy in their context. The researcher might therefore be led to look at different historical contexts which are favourable to policies of various kinds before asking about the context
in which her policy is to be enacted.

In order to record the narrative in her own context the researcher must compile biographical data, as already indicated, from contemporary sources which must include a wider spectrum of participants than those mentioned above in order to get a perspective beyond any one group or individual. To this end she must collect autobiographical material from not merely Brixton black people or even the Brixton police, but other people in Brixton who are, as it were spectators and may provide a means of synthesizing the disparate accounts from more involved participants.

The general racism in society is a background against which the more specific issue of racism in schools is examined. Again autobiographies of pupils, teachers, social workers and parents give a fairly wide perspective on 'what goes on' at school. Presuppositions, especially of the perlocutions of reports, are of extreme importance in these areas, for each individual has reasons for saying more than 'just' what happened. The headmaster, for example, has the 'image' of his school to think about, the parents do not want their children 'tarnished', the pupils themselves might feel that exaggerated reports will help their cause and so on. At this initial stage of enquiry it is necessary only that the narrator records what is, to the best of her knowledge, a truthful account and one which she thinks accurately records the beliefs and observations of the
people - various though they may be - who have supplied avowed information in the first or third person.

7.41 **The Range and Scope of Narratives.**

Narratives may be used in a wide variety of educational contexts and the example above is not intended to place research within a large socio-political framework and that alone. Narratives might be compiled for quite isolated and local problems concerning, for example, a single school, class or child. A school, to illustrate the point, might wonder why one particular class is unmotivated, disruptive and difficult to teach. In this case the narrative's generativity derives from a desire to see a change for the better in that particular class; it derives from an educational desire to see happy, responsive children learning and enjoying the process. There is no reason why a parallel conjunctive narrative should not be written which concerns children in the same school with similar social backgrounds but who 'gel' well together and enjoy learning. With this brief the narrator will obviously want to record biographical material from both sets of children and teachers. The children themselves will produce accounts of behaviour and theories about that behaviour which vary one with another. This raises the philosophical issues I have already mentioned about the difficulties of adequately describing educational (and other) situations.
The same will be true of the teacher's accounts - one reason for this will be their differing experiences with the class, some, for example will be amazed at the problems others have and vice versa. The narrator needs also to be sensitive to other factors at work within differing social contexts such as a class. One of these will be peer group arrangement and the major peer influences exerted on the class as a whole. Another will be detailed case studies of individual children and home backgrounds together with parental attitudes to schooling and the child's reflection or non reflection of these.

In a similar way to the first example, 'extraneous' material in the form of books written by teachers or others qualified by experience to have some insight into these kinds of situation are of relevance and might be included in the narrative. Additions such as these add a dimension to what seems a local problem but which is in fact a common phenomenon in schools. Included in the narrative there might well be a comparison between schools with both similar and dissimilar problems. It is a well known phenomenon, for example, that two comprehensive schools in the same locality and with otherwise 'identical' intakes have completely different problems. One, for example, has good discipline, happy and hard working pupils while the other seems to suffer a blight of disruptive pupils, unmotivated classes and so on. Hence, besides a conjunctive narrative about differing classes there might well be another parallel
and conjunctive narrative about other schools in the same area. These will presumably include biographical material concerning the actual behaviour experienced and theoretical material from pupils, teachers and parents concerning the differences. One reason, for example, why parents choose particular comprehensives is based on their perception of these differences. These perceptions need to be recorded and possibly squared against actuality. The correlation between parents with positive attitudes to school and the general atmosphere of the school to which they send their children needs to be teased out.

7.42 The role of the narrator.

From the foregoing it will be apparent that the narrator's role is both wide and to a large extent uncircumscribed. It is, however, related to a question which comes from the problematic as perceived; from another desired end to the one which is presently perceived to be the outcome of current educational policy. This may range from isolated disruptive pupils in particular schools or it might, as I have mentioned, cover the attempt to instantiate new policies such as an anti-racist policy, an egalitarian one or something like a national attempt to raise the level of literacy. In performing the activity of collecting information relevant to whichever brief generates the research I do
not think that there are general principles which govern such a collection, determine criteria of relevancy or stipulate completeness, comprehensiveness or adequacy. I do think that the researcher-narrator does need to reflect seriously about these factors but unfortunately I cannot envisage any guidelines, other than intuitively obvious ones such as asking truants why they truant, where the brief asks about truancy. Consequently the narrator's 'role' is ill-defined, except where it concerns the production of a narrative. It is as ill-defined as the war correspondent's role and almost for the same reasons: the educational situation, like the war, begins to reveal its own secrets and these lead where they will. Determining or attempting to stipulate a precise role would, in these conditions, be counterproductive. To balance this there seems to be a consequential need for particularly sensitive people in the role. Such sensitivity parallels Aristotle's notion of the practically wise.

The narrator's role has a double aspect: the collection of information and the writing of the narrative. This, however, presupposes that a considerable amount of theorizing has gone on beforehand. Theorizing of this sort might include thought about such things as what is relevant, revealing or significant and about who should be interviewed, whose views provide an alternative perspective, what literature on the subject is available, what historical or contemporary figures have thought
deeply, taken part in or written about their experiences, and so on and so on. This, not inconsiderable aspect is not a temporally prior condition of writing, for it would probably develop as the narrative developed - but insofar as the narrative is something like a novel, albeit, an historical one, such theorizing is logically necessary to it. This input will involve the narrator's perceptions of the world and will therefore involve her own values and ideological preferences, however consciously restrained, and will give the narrative an idiosyncrasy which the panel (in their deliberations) must take into account.

This aspect of the narrator's role parallels the debate over value-freedom in orthodox discussions of human research models. I do not think that any discussion of that debate is either called for or necessary here, for I do not think that narrators can produce unintentional narratives. At the same time I do not think that narratives can be read as if they were unintentional objects and open to various compatible, yet incommensurable readings or 'misreadings', as is suggested in some work on narrativity and recent literary theory.

7.43 The role of the narrative in practical deliberation.

In this section I wish to bring together three strands in the thesis and explain how they work together
in the deliberative act. The first of these is the written narrative. The second concerns the context of the narrative and the third is the way these two enter into deliberation.

Our starting point assumes some panel of deliberators who are presented with the written narrative which, as indicated, is the result of some question which was generative to the research, and who are able to question the agents who are themselves quoted in the narrative. The narrative is a text and qua text is the work of an author whose intentionality is of supreme importance. This fact immediately raises philosophical problems which I have looked at in chapter four. In this thesis, questions concerning the intentionality of the text replace the rather outmoded discussion of value freedom. The question, as I phrased it, is not whether value free research is possible, because the notion of value freedom itself presupposes an ontology of events, but whether texts can be non-intentional and whether actions can be identified as actions of a certain sort independently of their narrative context. The characteristic of actions I argued for, is that they are not identifiable independently of context and therefore exhibit what I refer to as 'narrativity'.

The intentionality of the text will betray the narrator's identification of the three 'interested groups'. (These are briefly mentioned in 6.20 and more thoroughly in Appendix B) The 'interested parties' are
various groups in society who, for some reason or other, have an interest in the research or its findings. They include, as examples, groups who are concerned directly, such as those affected by the research or policies stemming from it, they also include those who have the final responsibility for educational policy and possibly a group or person who is held in some esteem, perhaps because of experience or previous research. A narrator, for example, who is thoroughly convinced that her work has to 'come up' to the standards set by the Human Sciences (presuming she believes in their validity) might see the professional group who represent those sciences as the group of esteem. Alternatively she might see the group to whom she is responsible, and to whom justification must ultimately be given, as the dominant group. In this case her perceptions of the reasons they accept as reasons, explanations or appropriate or adequate descriptions will influence the content and comprehensiveness of the text. If the group of concern is dominant, the narrative intentionality might be influenced by the knowledge that the panel of deliberators are at liberty to interview any or all of the agents who provide autobiographical material which she has recorded in the narrative. The fact of this twin interview does not guarantee objectivity but it ensures that the text, in intention, is accurate and it does so because both the narrator and the subjects who are the real historical agents whose 'voices' inform the text are themselves
liable to cross questioning from the panel. In this way the researcher becomes researched, the researched become researchers and narrator and narrated become co-participants in the deliberative process: they enter into deliberative dialogue with the panel and in so doing take part in policy making.

Research and Policy as two distinct elements collapse, within this schema, into research/policy which, while dependent on the narrative, is itself a single activity which is fundamentally one of dialogue. This central feature brings us the heart of the methodology which is proposed: the instantiation of practical reason through the dialogical relationship which exists between the 'panel', the narrator and the subjects of the text who become voices in the dialogue. What then may be said of the 'panel'?

From what has already been said, the panel are a group whose function it is to deliberate and come to a practical conclusion (i.e a specification of actions to be carried out) in response to the question which was generative to the project. There is no precedent for the make up of this 'panel', since no such body exists. Consequently, what I have to say is open to debate, reflection, criticism, revision and whatever else anyone else with an interest in it has to add. The closest analogue I can think of would be something like the way in which Lord Scarman conducted his enquiry into racialism. However the analogy breaks down at a number of
points which may be worth mentioning. Firstly, although agents who were perceived as 'interested parties' were interviewed, the narrativity of their descriptions and explanations was overlooked, with the consequence that a scientistic 'atomicity' pervaded the 'data' from which the report was ultimately drawn up. Secondly, while a report bearing some similarity to a narrative was drawn up, it came as a result of deliberation and not as a condition of it and it was viewed as if the narrative 'contained' its applications - i.e its policies. Thirdly, the compilation of the report and the onus of drawing conclusions from it was left to the intuitions of a person who was perceived to be (qua Judge) an expert in practical deliberation. It was consequently and fourthly, not an instantiation of dialogical deliberation - the main criticism of which is that the report which resulted was, from an intentionalist point of view, author dominated. Nothing equivalent to an isolation of the factors influencing that intentionality was thought pertinent and nothing equivalent to a cross examination of the report which is performed by the 'panel' and in which the narrator becomes the subject of research was thought substantially necessary. This all leaves the report open to frustrated criticism - police who think their case is inadequately represented and black people who feel aggrieved because certain police powers were not taken away but merely verbally condemned. Perhaps most significantly however, the report has not become
substantive policy. There would be no point to the research I propose if there were neither intention nor power to implement the policies advocated. The thesis therefore has not inconsiderable implications for the power structures which govern the implementation of policy. Indeed, while no overt political stance has been taken, the research presupposes - both in its intentional lack of social theory and in its democratic requirements - a form of liberal democracy. One of the major reasons for this is the implicit necessity of a shift in power away from both 'experts' and centralised government and towards those who are sometimes referred to (in figuratively 'flowery' language) as the 'grass roots'.

7.5 The Panel and its role in Practical Reasoning.

I have argued that it is a panel that deliberates about what to do in the circumstances. I have also said that the panel has a description of the 'problematic' in the form of a narrative and is at liberty to request the 'voices' in that narrative to appear before them to explain further what they said, why they acted as they did and so on. In this way I hope to have made room for the possibility of open and creative dialogue. What I have not said anything about, is who this panel is and how they are chosen or, indeed, who is qualified and by what, to chose. This is what I now intend to look at.

The first question to be answered, it seems to
me, is whether there are any groups who, by virtue of
some recognized expertise, have some sort of prior claim.
There is, as I intimated earlier, some precedent for
this approach as is indicated by the various reports
produced by teams of experts usually headed by someone
with publicly recognized abilities in relevant areas and
by whose name the report is generally remembered. Mary
Warnock is an obvious example here - having at least two
reports: the first on the integration of children with
special needs into main stream schools and the second on
the possibilities and moral constraints of genetic
engineering on the embryo. Lord Scarman I have mentioned
and others include Newsom, Swann, Brandt, Plowden and
Hargreaves. These might be regarded as the group of
esteem by virtue of their expertise but not either the
group to whom justification was owed or those who are of
concern. (See Appendix B for a clarification of these
terms). The single outstanding fact, so it seems, about
the deliberations of the panels headed up by experts in
this way is that the decision making procedure lacks the
conflict necessary for creative dialogue. This is not to
say that those interviewed, the barren women, the
disadvantaged and so on, were all in harmony with the
reports' findings. It is to say that because they were
not made part of the decision procedures whose outcome
was the report, their input made no vital impact on the
processes of the dialogue. The concern, one might say,
was taken away from those with the concern (to improve,
say, their own circumstances) and given to those with, at best, a professional concern.

Is this defensible? I have argued in the sixth chapter that the type of practical reason adequate to the task of solving practical syllogisms is not analogous to a calculus but must take into account the complexities of human affairs. I argued, further, that in at least some human debates opposing sides conducted their dialogue by means of constructed cases which contained positive and negative elements. These, when put against each other, create, although I have not expressed this in such strong terms previously, a conflict of ideas which, when argued out, engender a creative dialogue. It is creative because ideas which appear uncontroversial to one group are picked up and questioned by others with different perspectives. A case, for example, might be made out by some staff at a difficult inner city school that the solution to the problem is in enforcement of a strict 'law and order' policy. Others, however, might vehemently oppose this, suggesting that such a policy alienates the children, exacerbates the problem and leads ultimately to an upward spiral of anti-social behaviour countered by stricter 'laws'. The answer, they might suggest, is the staff-pupil relationships, constructive and relevant teaching methods and so on. The report system above takes this dialogue out of the hands of those to whom it is of vital concern and renders its concern lifeless by putting it into the hands of those for whom it is no concern.
My concern, as is becoming more obvious, is to make vital dialogue the centre of the deliberative process and this means involving those for whom it is of vital concern. There are a number of problems here, however. Firstly, it is possible that this group, while obviously at the centre of the problem, might, nevertheless have no real idea as to the solution of the problem. Secondly, it might be difficult to identify any such group. Which group, for example, is to be most affected by the introduction of economic awareness as a cross curricular 'subject'? Thirdly, there might be factors beyond the reach of those most concerned but which nevertheless effect the type of decision to be made. Fourthly, certain skills might be required which call for expertise. I will look at each of these in turn and as I do so it will become evident that the panel will require more than a particularly interested group for its functioning.

The first problem - that no real solution is to be found - corresponds to what we referred to earlier as a practical syllogism with unspecified ends. Debate, as in Wiggins' 'car journey' example, is not about how to achieve a certain, specified end, but about how to specify the end. This sort of question is typified by debates concerning what we want from an educational system: the ends are not already specified. The problem appears in different guises at different levels in the educational system: philosophers wonder about the
qualities of the 'educated man' and about the most appropriate means of achieving 'him'. Parents wonder what teachers are teaching and why it is so different from what it was in their day. Here, I believe, is where the Warnocks, Plowdens, Scarmans should reappear, not in the driving seat, but as people who have spent some time reflecting on the sorts of issues at stake. Brought back in this way, expertise can be helpful in deliberation without usurping it.

Bringing in people who have reflected on particular educational problems and who, therefore, bring with them a certain amount of expertise does not solve the second question however - the question as to who is most affected. The group who are most concerned or affected by the introduction of a new policy cannot be involved in dialogue if they cannot be identified. There are certain issues in education which do not seem to concern any one identifiable group but society in general by its effects. This leaves the question as to who the group of concern is, very open. The sorts of issues I have in mind are questions about truancy rates, vandalism and physical violence. Society 'thinks' it has a problem with these and yet 'thinks' also that they are someone else's problem. Yet whose problem they are is not at all obvious and nor is it obvious who is or should be concerned about them. Teachers and parents are candidates for those who are mostly concerned with such things but whether they are the only groups is contentious. A
teacher, for example, may find half her fifth form 
missing and is in that sense concerned. But that they are 
missing is something which goes far beyond the teachers 
competence to either control or explain. Many, for 
example, would put the blame for a rising tide of apathy 
in the upper forms of secondary schools at the feet of 
the government. In practice, unfortunately, blame is 
often allotted by ideological preference. Teachers are 
blamed by both press and media while teachers blame 
economic policy. Parents - who, after all, are 
responsible for rearing their children - seem to get off 
lightly. The solution, however, may be in leaving the 
issue about concern and concentrating on what might aid a 
constructive dialogue and replace the present idea of 
pushing the blame on whoever looks least attractive. We 
argued, after all, that the identification of causes - 
linked in everyday conversation with blame - can only be 
carried out within a wider context of accepted norms and 
questions. It is the acceptance of different frameworks 
of just these features which makes the 'game' possible. 
Constructively the cases ought to be made to come into 
conflict - not on the streets but in dialogue. Those who 
are of concern in issues with wider social settings must 
reflect that width and include teachers, parents and 
government representatives. Expertise, in these issues, 
might be gained from those most closely involved with 
young people at school leaving age such as careers 
officers, probation officers, employers, statisticians
familiar with unemployment figures and so on.

An example of the sort of problem I have in mind in the third of our areas, where certain elements of a possible answer lie beyond the reach of the immediately affected groups, might be the accountant who submits the cost of any proposals to the panel. This pragmatic but important area involves all those cases in which, for some reason or another, there are important factors outside the competence of those who are most concerned and which nevertheless need to be taken into account. Costing is one example. Others might include the implementation of certain teaching techniques, the architecture of schools, the setting up of a long term case study or the legal constraints to a recommended policy. Here, quite plainly, there are reasons why a panel might need specialist advice on certain, very specific, issues. Thus, while the basis of the panel should be those mostly concerned, the type of decision or its implications will sometimes necessitate other people who alone can make reliable estimates.

The last problem - where certain skills are required - could be construed to mean the same as the third: that outside expertise is needed on certain occasions. I do not have this in mind, however, but rather a more general set of skills which are intrinsic to the participation in dialogue per se. In some way questions in this area resemble those which ask whether the population of a democracy need some form of
democratic education. What, then does participation in
democratic dialogue presuppose? Firstly, and, I suppose,
foremost, is an epistemological requirement that the
participants understand both the narrative, the issues
and something about presenting and defending a case.
This, as I have argued, is not the acquisition of
numerous logical skills but rather an understanding of
what sorts of facts support particular conclusions, how
facts can be both for and against a case, what are mere
rhetorical moves, how arguments are put together and so
on. Further to this and, in a sense, underpinning the
whole project, is an ability to understand people - the
reasons people have for actions, the role emotions have
in taking decisions, the part beliefs play in forming
'pro-attitudes' and so on. Skilled inter-personal
understanding requires something than a mere thesis could
explain. It is consequently not something over which I
have spent time. There are, of course, philosophical
insights in this area which are helpful to understanding
the complexities of human actions: intentions, reasons,
motives, beliefs and so on. But an ability to use such
knowledge to understand others in real settings is not a
philosophical skill but something quite different.
Neither can it be assumed to exist within others who
qualify, for various reasons, to be members of the panel.
Indeed, it cannot be presupposed as a possession of
anyone qua their profession and/or background. Those who
seem to have it to a high degree are usually recognised
as such by their communities but do not thereby acquire
great fame or fortune. Those who have might include
certain poets, novelists or playwrights - such as
Shakespeare - although this is by no means a clear guide
since poets of great sensitivity such as Wordsworth were
reported to be almost unable to communicate with the very
folk they seemingly understood so well.

The outstanding question which remains is
whether anything substantive can be said of the
constitution of the panel. My answer is that there is,
but the substantive content will not, of itself, decide
whether specific persons or types of person will sit but
will provide guidelines by which appropriateness of
membership can be decided.

Without retreating into actual cases or examples,
in broad outline, what I have in mind is this. The
membership of any panel will largely be decided by what I
have referred to as 'the brief': what it is they are
given to do. It will also be determined by the size and
type of enquiry: whether, that is, it is a rather small
scale local enquiry such as the way in which a school
allocates its funds or a large scale one of national
importance such as the best means of implementing the
national curriculum. It will also depend on the complex
factors which enter into the creation of a dialogical
situation which is creative, on the identification, if
possible of the groups of concern and responsibility and
on whether or not certain types of expertise such as
accounting are necessary. My main claim, however, is that such considerations are, at best, only defeasible criteria for the selection of a panel. In practice the 'criteria' would probably only operate 'behind the scenes' in what would otherwise be a negotiated arrangement between those who, in the real world, felt that they had a right or a duty to take part.

7.6 Concluding Remarks.

It will have been obvious that despite my attempts in chapters four, five and six, to foresee problems of both a philosophical and practical nature, there remain large areas where more needs to be said and done. Some of them only arise in the context of the methodology outlined above. Others are of a more general nature. Of the first sort there is much more to be said on the nature of dialogue, of the ethical issues arising out of the constitution of the panel, of the actual role of the narrator and the relationship between her and the panel, of the legal issues arising from the panel's need to talk to those involved and of evaluation procedures which might again involve a panel. Of the second sort, there are, as I have indicated, large areas of work required in understanding narrativity, intentionality - particularly in relation to texts, rhetoric, semiotics and speech act theory in particular, ideology and the way
it influences descriptions and texts, the specification of ends, in education, and who is responsible for it and the relationship between the researchers, the researched and the policy makers. Both lists could be extended depending on what seemed most appropriate but many require a separate thesis by themselves and could not be summarised here. There is then much more that could be said but which could only be said in some future work. At the same time, the model which I have proposed, which is only one among many possible models, is a working model: it could, without much more theoretical work, be tried out. I have, to the extent that I have succeeded at all in providing a substantive conclusion to what is essentially a philosophical thesis, succeeded in one other aim, which is that philosophy of education has a vital function in educational research, underestimated I feel, by some, in that it can have practical implications as well as theoretical ones. I feel also, in this context, that I have attempted to carry out what Richard Peters was indicating when he wrote, in the introduction to 'Ethics and Education', that: 1

Assumptions about transmission also raise ... fundamental problems in philosophical psychology about the conceptual schemes employed by educational psychologists and the types of procedures by which their assumptions can be tested...which are particular problems in the philosophy of science.

Philosophy of Education may take the form of a second order enquiry but where it finds that conceptual confusion underlies the conceptual schemes employed by educational research, then it has first order
implications. The conceptual confusions we have located beneath the assumptions of scientistic research in education call for a radical restructuring of the actual process of research. My attempt to describe one such model may run into practical problems but its assumptions, at least, seem less questionable than those of a more scientistic bent and consequently more appropriate as a basis for educational research.
Epilogue.

In the prologue I outlined three aims in this thesis. The first of these was central to the thesis and concerned the place and role of scientific method in educational research. The second concerned the role philosophy of education might have in providing a critique of this, while the third was the intention to adapt philosophy of education in such a way that it had practical and substantive implications.

I did not work on the assumption that science might, if implemented, have damaging effects on educational policy; indeed chapter two set out to show both that scientific method was well established in educational research and that its concepts and assumptions were problematic. I also tried to show that this was also the case for an 'applied science' whose paradigm was something similar to that envisaged by engineers.
Part of the first chapter was concerned to show the importance that science had in educational thinking and educational philosophy in particular. There I outlined what some may consider the most important defence of the use of scientific method in education. At the same time it was a concern of mine that writers on the philosophy of education had not really provided an appropriate framework from which to both provide a critique of these practices but also propose a practical and substantive alternative. This, it seemed to me was vital, both for the practice of education and for philosophy of education. Thus I attempted to set down some of my views on the philosophy of education which would enable me to carry through the task set by the main aim of the thesis which was to provide not merely a critique, although that would have been a large enough job, but to provide a working philosophical framework in which a practical conclusion could be reached. The thesis could thus be be seen as an outworking of a rather Aristotelean piece of practical reason with practical proposals rather than actions as conclusions.

The first part of the thesis was thus concerned either with providing a philosophical framework within which to work or with a critique of the research practices which I felt could lead to damaging practical proposals. The third chapter became necessary as soon as it became clear in the second that the whole basis of the nomological enterprise rested on the possibility of
finding a way of expressing a lawful relationship between variables which were antecedent and consequent. As a result of this it became necessary, for reasons which I hope were sufficiently clear, to leave the philosophy of education as such and enter into the realms of what has been referred to as 'pure' philosophy.

As I have mentioned in the prologue, the thesis itself, in exploring the problems inherent in the scientific enterprise began, at the same time, to provide clues to a way forward. These came in two forms: positive and negative. The negative aspects appeared as pitfalls to be avoided and include such things as atomism and reductionism. The positive aspects, which sometimes appeared as the other side of the coin, appeared as points to be remembered and included. These include the importance of context for understanding, the significance of public criteria and agreement and the centrality of intentions, beliefs and purposes to understanding individual actions. All of these factors led to the idea that, at a practical level which would be necessary in a research scheme, some all-encompassing data base would be required which would be holistic as opposed to atomistic. A narrative seemed to be the most obvious candidate for such a basis.

Having got so far it was becoming apparent that any remnants of scientism in my own thinking would be radically challenged since the data base from which research started was so unlike anything in the sciences.
I therefore felt compelled to examine the narrative as a possible research tool. This gave rise to sections such as the use of a narrative in isolating causes and also the possibility of narrative objectivity. The second of these is a particularly difficult area philosophically since all the problems encountered by the proponents of atomism (the truth value of atomic propositions etc) and of extensionalism seemed compounded by an exponential growth of biographical and social information. The way ahead, it seemed, could not possibly involve the relation of variables for such variables as there were could no longer be taken in isolation from their context. At this point the polar opposite of the aims of positivism seem to have been reached.

One way out of this seeming impasse was to rely of the intuitions of the everyday reasoner. In daily life we all come to practical conclusions about what to do even though we have a mass of information which may be regarded as a set of premisses in a practical syllogism. Before I could take this form of thinking as a way forward however, I had to look at the possibility of formulating such a set of premisses in such a way that a practical conclusion could be reached deductively. This, as I hope I showed in chapter six, is not possible. The conclusion was thus that if such a large data base was to act as the basis of a piece of practical deliberation then a research scheme incorporating it would require a group of deliberators who would be set a practical
problem to solve. The narrative, and indeed the narrator, would thus become an integral part of the deliberative act which, unlike science, would attempt to reach practically feasible goals rather than 'the truth'. It is partly this philosophy which informs the seventh chapter in that it is agreement as to what to do which is in question not what may or may not be the case. I hope that this is not misunderstood as being the thesis that truth is too expensive a luxury for education. It is rather that our agreements are an important aspect on what we say is an objective view of how things are. What truth is in itself is quite another question and one to which a thesis such as this could not even begin to do justice.
Appendix A

Using Pragmatics.

What I wish to examine in the first appendix is the possibility of using some form of pragmatics to aid the researcher understand the situation recorded in the narrative. Briefly, this method will centre around the relationship between question and answer. The relationships I am interested in are basically those in which questions are answered in the way expected (convergence) and those which are not (divergence). An example of convergence is where a question asking for an explanation is answered by an explanation. An example of divergence is where a question asking for an explanation is answered by a justification (or excuse). The implications of this for education will be well understood by those who constantly deal with children who make excuses rather than give explanations of their
In order to examine the issue, I will provide a number of contexts in which questions might be asked and then look at whether convergence or divergence is to be expected. I will then give an example of how such a method might be used in educational research. I have given a letter to four different aspects of the context to help with clarity. There are, of course, far more aspects to the ones I have mentioned. It would be part of the pragmatics I envisage to work out details of influential contextual factors. The contexts are as follows:

(r) An unambiguous question seeking an explanation is asked.

(s) The person questioned perceives that an explanation is required.

(t) There is a high degree of agreement over 'fundamental' moral, political or ideological issues.

(u) The overall context is non-confrontational.

In contexts in which (r), (s), (t), and (u) are all met (which I characterise with a (+)) there would be every likelihood of a high degree of convergence. Where some or all are not met there will be higher degrees of
divergence. For the sake of simplicity I will call highly convergent contexts X and highly divergent contexts Y. To give an example of a highly convergent situation I will use the present ambulance workers strike. If a striking ambulance worker asks another striking ambulance worker why they are on strike there is a high degree of convergence because an unambiguous question seeking an explanation has been asked to someone who perceives it to be a request for an explanation and, because there is ideological and moral agreement in a non-confrontational context, an explanation is given. This might be symbolised as follows: X (+r),(+s),(+t),(+u). If, however, as often happens, there is a lack of trust or ideological difference between the questioner and the questioned, then we have a less convergent situation. This might occur, in the present example, where a member of the press questions a striking worker in a manner which indicates confrontation. The lack of shared ideology and the confrontational manner of the question leads towards a divergence between the type of answer asked for and the type given. In this case the type given will more probably be more in the form of a justification than an explanation. This might be symbolised as: Y (+r),(+s),(-t),(-u). There is, perhaps, another divergent context (call it Z) in which it is less determinate whether explanations or justifications are given as answers. These might include situations in which emotions such as anger 'interfere' in the relationship. This is
sometimes seen in strike situations (as at present in Germiston, Johannesbourg) where workers disagree with the actions of fellow workers as to the action taken which results in the total breakdown of dialogue. Symbolically, however, the situation appears to be the same as other situations which give rise to justifications. The main difference would seem to be in the degree of ideological disagreement or confrontation.

The three situations X, Y, and Z all presuppose knowledge about the questioner by the questioned. To these we may add a fourth context in which such knowledge does not exist. In these circumstances (u) is indeterminate and therefore most likely gives rise to an evasive answer. The ideological presuppositions of the questioner, however, might be guessed or deduced from the tone of voice or form of question given by the questioner. There seem to be at least two of these types of context: V and W. Since in both cases the confrontational aspect and the questioners intentions might be unclear (r), (s) and (u) are somewhat indeterminate. I have symbolised this with a question mark. What, therefore, distinguishes V and W is the perception the person questioned has concerning the ideological position of the questioner. Thus V might be symbolised as: (?r),(?s),(+t),(?u) and W as: (?r),(?s),(-t),(?u). Since there is the basis of trust in V there is also the possibility of some convergence. In W, however, no such basis exists and since the
confrontational aspect is also indeterminate the most likely outcome is a divergent evasiveness.

In the following example I hope to show the use to which this may be put in educational research by providing part of a narrative in which a certain pupil is suspected of stealing a pen. The narrative includes the pupils answers to questions put by various people who represent (by their roles, attitudes or relationships) examples of the contextual features outlined above. To avoid complication I have oversimplified the questions and answers in order to highlight the main points. The main idea here is to see to what extent such a technique might be useful in understanding the text. The text (which is 'idealised') is as follows:

Headteacher: I have good reasons to think that you have stolen a pen. What do you say?

Pupil: I don't know what your talking about.

Mother: I have a letter from your Headteacher saying that he has good reasons to suppose that you have stolen a pen. While I believe that the Headteacher would not accuse you of this without good grounds, I want to know your side of the story.
Pupil: I won't lie to you, I did steal the pen, but it was only because a teacher threatened to punish me if I didn't do the work properly and I couldn't find my other pen.

Pupil's friend: Did you really steal the pen?

Pupil: Yes, of course I did, but it was only old snobby's and he's got hundreds.

'Snobby': I'm going to get you after school! You stole my pen!

Pupil: I didn't steal your pen and my mates will make mincemeat out of you if you say I did!

Narrator: Did you steal the pen?

Pupil: Who are you?

Friendly Teacher: I heard that the Headteacher has accused you of stealing a pen. You're not like that are you?
Pupil: I only did it because that horrible teacher in the science department said he would belt me if I didn't finish my work. I was scared.

Given this text, the researcher is able to allocate, using the symbols above, positive and negative features of the context to the various aspects of it. The results might look something like this:

Headteacher: Y (+r), (+s), (-t), (-u).
Mother: X (+r), (+s), (+t), (+u).
Pupil's Friend: X (+r), (+s), (+t), (+u).
'Snobby': Z (+r), (+s), (-t), (-u).
Narrator: W (?r), (?s), (-t), (?u).
Friendly Teacher: X (+r), (+s), (+t), (+u).

A well worked out scheme would be one in which convergent (X) and divergent (Y) situations could be 'extracted' from the text. The advantage would be that it would be fairly clear where to expect differing types of answer. Interestingly (though of course this is not palpable proof) one of the outcomes of our examination of what is an oversimplified and somewhat unrealistic 'narrative' is that the context created by the narrator's question is highly divergent and therefore unlikely to reveal anything resembling an appropriate answer. The two contexts more likely to 'succeed' (in the sense that they
most nearly elicit the type of answer asked for) are created by the two friends, the pupil and the teacher, and the mother. This, if explored in greater depth than I have space for here, would, I am sure, reveal much more about the role of the narrator and the way the narrators questions might be framed, and, at the same time, about the importance of other biographical material - such as conversations with friends and the like.
Appendix B

In order to examine the context and the factors in it which play an important role in decision making I shall employ a device analogous to changing the scenery in a play. Firstly I shall outline some of the possible circumstances in which any public decision must be made. Then I shall change various elements in the circumstances in order to throw the predicament of the decision into relief and thereby identify, tentatively, some of the major influences in ordinary public decision making. In so doing I hope to cast some light not only on the way in which circumstances enter into the decision but also on the various groups involved. Those groups, or 'interested parties' as I refer to them are: those who are of concern, those to whom justification is owed and those whose opinion is, for one reason or another, held in esteem. These might be broadly characterised as follows: those of concern as the group amongst whom the action is
to be taken; those to whom justification is owed as those to whom one is responsible and those whose opinion is esteemed as those for whom there is respect in practical matters. It should not be thought that I am saying that these are or should be the only groups affecting the decision process only that they appear to be the most prominent.

I offer here six circumstantial factors which are likely to influence, in one way or another, the decisions which the social workers (mentioned in chapter six) have to make. I have underlined the word or words which seem to indicate, in each case, the type of circumstantial factors.

We may presume firstly that the social workers work within a brief and that this brief sets parameters around the circumstances with which they are dealing. Let us say, for example, that this brief comes from a head teacher, who, in liaison with social workers and others, forms the opinion that a certain child is not coping at school or at home because of what is perceived as a mental handicap. Having come to such a conclusion, and having gone through the appropriate legal channels, a certain welfare office is informed of its duty to resolve the problem in some satisfactory way. Now, if we consider these circumstances together with the question of appropriate criteria concerning 'those of concern', a number of significant circumstantial factors become evident. To begin with, the law, within any given
society, might prescribe who are deemed 'interested parties' and specify how they should be treated. Secondly, the social workers themselves work within pragmatic constraints such as time, effort and so on. Thirdly, there is 'informed opinion' as to what constitutes a mental handicap, what are the most appropriate remedies, who has rights over what and what the role of social workers is in cases such as these. We might include in this group professional psychologists, lawyers, social and psychological scientists, educationalists and whoever else, for whatever reason, has an authority which is invested by position. Fourthly, there are the physical options available; the number of institutional places, the number of home helps, the financial situation both of the family and the state and so on. Fifthly, there are those to whom the social workers are responsible and must justify their actions: the school, the parents, the 'law', colleagues and those who for other reasons have a professional interest. Finally, there must be some element of moral judgement which belongs to the social workers and to them alone; they, after all are ultimately responsible for making the decision and that duty carries with it a certain invested authority which cannot be overridden for 'academic' reasons.

I will now provide three variations in the background and speculate how, for each, the change might affect the social workers' beliefs about what constitutes
the most appropriate action. The first context is that of an underfunded welfare state, the people are suspicious of 'western' techniques and practices and the word family applies to a large group of relatives who accept responsibility for each other. The second is a society in which psychiatrists are opposed to institutionalisation as the appropriate means of caring for the mentally handicapped. The third is a society in which the rights of parents legally override any decision concerning their family whatsoever. My aim here, as stated above, is simply to see (hypothetically) how the three sets of circumstances might influence the way in which the social workers think and hence decide what to do with the child.

In the first example the social workers have no authority to make any decision, indeed, they have no role in the tradition. However, since the state finances a health service of sorts we may presume that they are obliged to intervene in what is otherwise none of their concern. Given their lack of authority and the hostility of the people the factor which is likely to influence their beliefs most is an avoidance of conflict. The overriding need not to offend the tradition isolates the group of concern as the extended family. The dominant reason for this might be related to the group whom the social workers perceive as the group to whom justification is due who would be the extended family. It might be argued, contrary to this, that regardless of what tradition might say society at large has the
ultimate responsibility for the welfare of its people. This view, if it were held, would raise the question as to how, given competing claims, any decision could be satisfactorily made. Whatever the result there do seem to be a number of significant factors at work. These may be put as follows:

(a) The group perceived as those to whom justification is owed dominates the identification of the group who are of concern. It would eradicate, for example, the moral claims of any institution which were at variance with the views of the extended family. The underlying reason for this dominance would be a fear that any decision made by the welfare state or its representatives would be deeply offensive to the rights of the family bestowed on them by tradition.

(b) A respect for what is perceived to be the tradition.

(c) A sensitive awareness of roles within the circumstances and the way that role is perceived by others.

In the second example we might hypothesize that a prestigious science has pronounced to a western audience, including the social workers, that institutionalisation is inappropriate. Again the fundamental factor influencing belief is likely to be the group the social
workers perceive as those to whom justification is due. This, however, need not determine their decision. Their decision, for example, will not be affected if, for some reason, they do not have any respect for such a science as psychiatry. However, those to whom they must justify themselves might well have a great deal of respect for it. Thus a dilemma is created where opinions differ. If, for example, the social workers believe strongly that institutionalisation is good they must weigh that belief against their need to justify themselves to authorities who are influenced by the pronouncements of the world of science. This weighing is further complicated by the fact that the reasons they give will have to be acceptable but are not those which are generally accepted. The dilemma thus takes on the form of a difference over what constitutes a good or acceptable reason. How might the dilemma effect the social workers decision? If they put the group whose opinions are (generally) esteemed above the group of concern because of a fear that they cannot justify their action, they sacrifice, for pragmatic reasons, their own opinion. If, on the other hand they choose to act in accord with their own beliefs, they relegate both the group of respected opinion and the group to whom justification is due below those of concern. Which hierarchy is chosen will rest on such factors as confidence to present alternative types of justification, the relative stress put on care for those concerned and those to whom justification is due and the
balance between them, respect for 'expert' opinion and much else including strength of character. What is more, connected with any disagreement over rationality, there is a difference as to what are taken to be facts. As we have described the situation the social workers do not accept one fact, alleged by psychiatric publication, that institutions are not appropriate methods for treating the mentally handicapped.

Although the situation is complicated I think that enough has been said to highlight two further factors:

(d) The importance of a hierarchy of groups, the exact order of which depends upon a multitude of other factors ranging from a respect for experts to a confidence to state personal views.

(e) The criteria which, for any individual, persuades them of what are and what are not to count as 'the facts'.

In the third example we have imagined that the rights of parents outweigh any other considerations. This has the effect of making the parents the group to whom justification is owed while leaving the group of concern open. It might also have the effect of making the group of 'experts' of greater importance than we have hitherto allowed them to have. The reason is that the social
workers might, in their attempt to justify what might be an unpopular decision to the parents, appeal to a 'higher' authority. The situation is different from the first two in that the situation is essentially less public. This allows for a wider interpretation of 'giving a justification' than would be the case in a more public context. How the social workers persuade the parents highlights another circumstantial factor (f). This would be the ethical dimension of decision making - in this case whether it is ever right for someone acting in an official capacity to take children away from their parents and how the social workers might justify this to themselves.

In conclusion it must be stated that the above is merely exploratory in nature. The important issues are not, in a sense, what circumstantial features play a part in decision making. They are, rather, that decisions cannot be understood fully unless circumstances are also understood. There is thus here a further argument for a narrative as the basis for educational research.
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