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 aforegoing 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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CONTENTS

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.

Chapter One.

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
incommensurabilty 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.'[4] He
admits that practice comes first, 'but its theoretical
justification has to wait for the scientific development
that can explain its success.'[5] 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.'[6] 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.'[7] Brenda Cohen
takes the argument a little further by 'rubbishing' the
alternatives to science and Paul Hirst as the
alternativist's champion. She says,[8]

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[9] 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,

[10]Carr, [11]Stenhouse, [12]Elliott, [13]Schon [14]and

Hirst.[15] 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'.[16] I will follow these with Hirst's
replies both from the same journal[17] and from a chapter
entitled 'Educational Theory' written some eleven years
later.[18]

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.'[19] This, however, is seen as a
temporary problem for, 'we have to wait upon the relevant
developments in the appropriate sciences'[20] 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'.[21] 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 <u>to be accepted</u>. 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.'[26] 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'.[27] 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.'[28] Further, because of the nature of the
addition, 'Hirst wants to import value components into
the theory itself.'[29] 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'.[30] 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'.[31]

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'[32], 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'.[33]

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.[34] 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.'[35] 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 practioners. The
consequence, for Hirst, is that whatever empirical basis
or empirical theory O'Connor is proposing, it will be,
'quite inadequate for practical judgements'.[36] Besides,
he adds, science developed quite independently of a fully
worked out 'logic of scientific explanation': 'indeed
...it developed in spite of gross confusion.'[37]

On O'Connor's point about the relation of values
to facts, Hirst brings forward a number of significant
features.[38] 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.[39] 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[40], 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[41], 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[42] 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[43], 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[44] 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[45] and Terry Moore[46] 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[47] 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.[48] 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.'[49] 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 non 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[50], 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.'[55] This,
adds Francis, extends, 'research methods and thinking
well beyond those of behaviourist and experimental
cognitive psychology.'[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 scientistic. 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'.[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[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:[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
> gereralizations 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.'[60]

This lack of critical awareness amongst

educational psychologists is not helped by some

philosophers who heap esteem on science. Moore, for

example,[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.'[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.'[65] 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.'[66] 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.'[67] 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 contextu al
variables in any one setting. **Well-standardized tests**

exist which would be strong candidates for inclusion in such a battery.'[68] In concluding, he remarks that (the above) is an attempt to,'integrate action and evaluation research into a unified research model.'[69] 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.'[70] Each teacher will have 'a research stance': 'a disposition to examine one's own practice critically and systematically.'[71] 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.[72]

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,[73]

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 straightfor-

ward positivist research are espoused in the later stages

of research. Teachers make and reflect upon their

practices but,[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'[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[76]and especially in his concern with the

development of 'practical principles'[77]derived from

teachers' 'operational educational theories'.[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

sciences'.[79] 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.'[80] 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...'[81] 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.

(1) Aristotle, 'Ethics', Translated by J.Thompson, Penguin, 1976. p206.

(2) ibid, p118.

(3) ibid, p207.

(4) D.J.O'Connor, 'An Introduction to the Philosophy of Education', RKP, 1957. p94.

(5) ibid, p109.

(6) ibid, p109.

(7) Terry Moore, "The Nature of Educational theory" in 'Theory and Practice of Curriculum Studies', RKP, 1978. p15.

(8) Brenda Cohen, 'Educational Thought: An Introduction', MacMillan, 1969. p23.

(9) William Guy, "Philsophy of Education as Meta-Theory" in 'Educational Philosophy and Theory' vol 9. Pergamon, 1978.

(10) S.De Castell and H.Freeman, "Education as a Socio-Practical Field: The Restructuring of Educational Theory." in 'Educational Philosophy and Theory' vol 10. Pergamon, 1978.

(11) W.Carr and S.Kemmis, 'Becoming Critical: Knowing through Action-Research', Deakin Univsity Press, 1983.

(12) L.Stenhouse, 'An Introduction to Curriculum Research and Development', Heinemann 1975.

(13) John Elliott, 'Innovation in Teaching and Action-Research', (An interim report on the Ford Teaching Project) 1974.

(14) D.Schon, 'The reflectice Practicioner: How Professionals Think in Practice', Temple Smith, London, 1983.

(15) P.Hirst (Ed), 'Educational Theory and its Foundation Disciplines', RKP, 1983.

(16) D.J.O'Connor, "The Nature of Educational Theory" in 'Proceedings of the Philosophy of Education Society of Great Britain' Vol.6, No. 1, 1972.

(17) P.Hirst, "The Nature of Educational Theory: a reply to D.J.O'Connor", in 'Proceedings of the Philosophy of Education Society of Great Britain' Vol.6, No.1, 1972.

(18) P.Hirst, "Educational Theory" in 'Education and its Foundation Disciplines', (Ed. Hirst.P), RKP, 1983.

(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.

(32) ibid, p109.

(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 deliniation 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 Sockett'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.

(40) R.S.Peters, 'Ethics and Education' Unwin, 1966. p15.

(41) R.S.Peters and P.Hirst 'The logic of education', RKP, 1970.

(42) G.Langford, 'Philosophy and Education', MacMillan, 1968. p26

(43) A.O'Hear, 'Education, Society and Human Nature', RKP 1981. p2.

(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.

(47) R.S.Peters and J.P.White, "The Philosophers contribution to Educational Research" in 'Educational Philosophy and Theory', Vol.1, Pergamon , 1969. p1ff.

(48) R.S.Peters, 'The Concept of Motivation', RKP, 1958.

(49) ibid, p6.

(50) S.S.Schwieso and N.J.Hastings, "Explanations and Practice: Behaviour Modification in Education" in 'Journal of Philosophy of Education' (Vol 20 No.1) Carfax, 1986. p81.

(51) ibid, p81. (My emphasis)

(52) H.Francis, 'Minds of their own', University of London, Institute of Education Publication, 1984. p10.

(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.

(58) D.Child, 'Psychology and the Teacher', Holt
Publications, 1981. p3.

(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.

(73) Elliott and Adelman, 'Innovation in teaching and
Action-research', Published by the Ford Teaching Project.
1974. p1.

(74) ibid, p1.

(75) ibid, p2.

(76) P.Hirst, "Educational Theory" in 'Educational Theory
and its Foundation Disciplines', RKP, 1983.

(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 aredescribed 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 Abrahamson[4] 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 ivestigation 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. Inasfar 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   <u>What are Variables?</u>

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[10], '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, [11]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 [12,]

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 fistly 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.'[13] and Hoover for whom
it is the, 'counting the units of a thing.'[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,[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[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[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 'operation-alized' - or 'transferred' deinitions.

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 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.[21] In the first step Seeman divides the
concept into four 'subconcepts' viz:[22]

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,[23] whose analysis of the 'normlessness'
dimension is indicated by the positive replies given by
candidates to the following statements. (Listed under
Item).

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

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:[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 powerless-
ness 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[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.'[26] 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 subequently 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   <u>Repeatability as a condition of validity.</u>

De Groot,[28] 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 there descriptions. If the scientist observes A at

time t doing x, and again at time ti, 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]
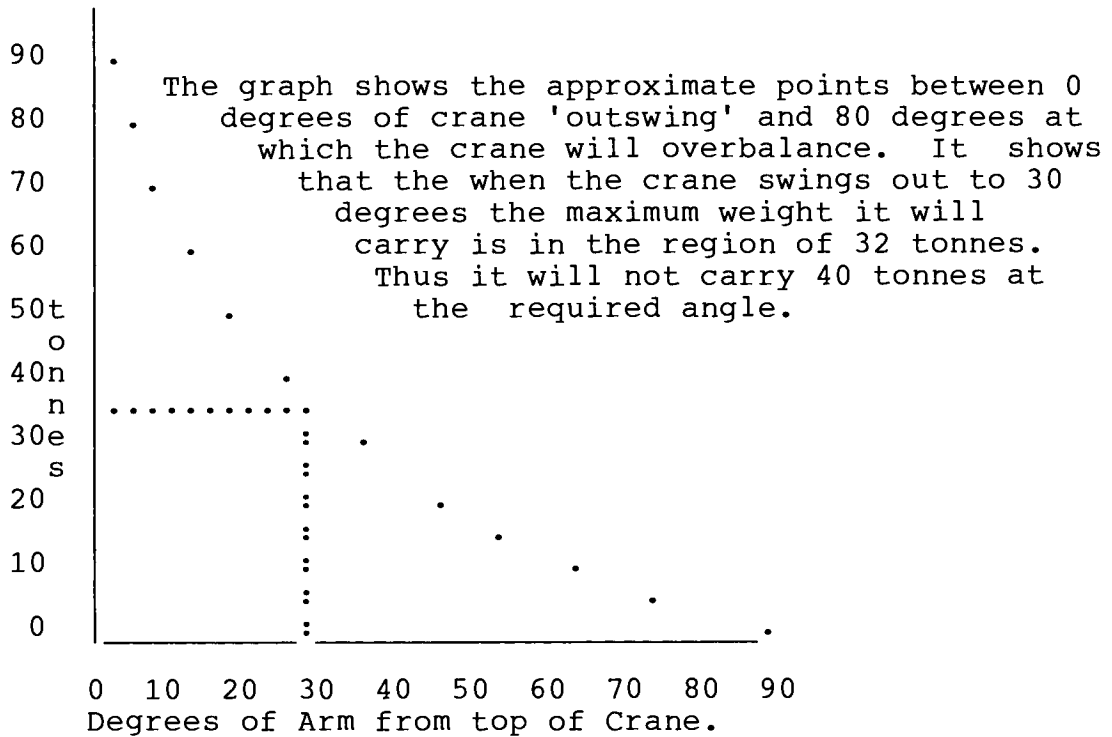
Education's purposes are comparable, particularly in research functions, to other applied fields such as engineering. According to Smith 'Engineering is the professional art of applying science to the efficient conversion of natural resources to the benefit of man.' Educational research is the professional art of applying science to the efficient use of man's inner resources as well as for his educational benefit.

Asher suggests, for example,[30] 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'[31] 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.

2.41   <u>Problems with the analogue.</u>

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) = 80tonnes, (b)Maximum lift at 45
degrees = 25tonnes. 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.

```
90   .
            The graph shows the approximate points between 0
80      .      degrees of crane 'outswing' and 80 degrees at
                which the crane will overbalance.  It  shows
70       .          that the when the crane swings out to 30
                    degrees the maximum weight it will
60         .          carry is in the region of 32 tonnes.
                      Thus it will not carry 40 tonnes at
50t          .            the   required angle.
  o
40n             .
  n   ...........
30e             :   .
  s             :
20              :     .
                :       .
10              :         .
                :           .
 0  |_____:_____.

    0  10  20  30  40  50  60  70  80  90
    Degrees of Arm from top of Crane.
```

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 'mathematicized'. 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:

```
    0       10      20      30 feet
     /\       |       |       |
    pivot    xgm     xgm     xgm
```
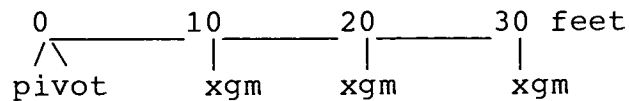
So that, for example, at 10 feet, x grammes has a
potential angular momentum of 20 units. At 20 feet this
increases to 40 units and at 30 feet to 80 units. This
law is presupposed by the mathematics we used earlier.
But, in our imaginary situation, it holds according to a
random feature which we cannot 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 mathematecization 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 posssible 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'[32]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.

(3) J.W.Best, 'Research in Education', Prentice Hall, 1959. p26.

(4) Mark Abrahamson, 'Social Research Methods', Prentice hall, 1983. p55.

(5) Verman and Beard, 'Educational Research', RKP, 1981. p67.

(6) D.Child, 'Psychology and the Teacher', Holt, 1981. p254.

(7) Best, op cit, p27.

(8) De Groot, op cit, p246.

(9) K.R.Hoover, 'The Elements of Social Scientific Thinking', St.Martins Press, 1976. p24.

(10) Abrahamson, op cit, p55.

(11) ibid, p55.

(12) ibid, p56.

(13) De Groot, op cit, p212.

(14) Hoover, op cit, p26.

(15) Abrahamson, op cit, p99.

(16) De Groot, op cit, p212.

(17) Abrahamson, op cit, p99.

(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.

(19) Abrahamson, op cit, p99.

(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...covarience...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.

(22) Seeman, "On the Meaning of Alienation", in the 'American Social Review', No. 24, 1959. p48.

(23) Zeller, Neal and Groat, "On the Reliability and Stability of Alienation Measures," in 'Social Forces' no. 58, 1980. p28.

(24) Asher, 'Educational Research and Evaluation Methods'. p100. (R.J.Smith 'Engineering as a career', NY, 1956)

(25) Best, op cit, p92.

(26) ibid, p93.

(27) De Groot, op cit, p249-51.

(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.

Chapter Three.

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

them.)

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 Stategies: 1.

The possibility of an all embracing extensionalist logic in which 'intentional' descriptions are translated into extensional equivalents.

3.7 Reductionist stategies: 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 stategies: 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,
> Li, Lii,....Lr and of other statements, Ci,
> Cii,....Ck, 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 :

```
Li,Lii,....,Lr
                  D-N ] Explanans sentences
Ci,Cii,.....Ck
_____
        E                Explanandum sentence
```

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.'[3] 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[4] 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 possibilty 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[6] 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
identifed 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[7] 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:[12]

    ...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'[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, infact, 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 impractable 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[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'.[16] 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'.[17]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.'[18]
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'.[19] 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.'[20] 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![21] 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 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 Stategies 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:[22]

> 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
> sustituted without changing the truth-value of the
> the statement.

The problem caused by intentionality is that [23]

> ...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 proposional
> attitudes.

The thesis of atomicity is stated by Wittgenstein in the

Tractatus as,[24]

> 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

descriptions[25]

> ...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 the problem. Carnap, for

example[26] 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[27] 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[28] '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[29] 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 extensionalilty 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 stategy 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 psyco-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.[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'.[34] 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 crieria,
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 commiting 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 [35] 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.

3.8 Reductionism 3: The Central State Identity Thesis.

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'.[36] The strong version[37] 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.[38] 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 occured 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[39] and
Feyerabend[40] 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 posssible 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.'[41] 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.[42] I am
assuming that identity is central to establishing common
reference so that, for example, in intentional langauge
(Cl) '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.[43] 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[44] 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 abilty 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 C1 and N1. We do not describe our reasons, nor, indeed, our sensations in terms which include neurone firing rates and N1, 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 C1 / N1 case because there is no common ground in which to start. In consequence, anything that might be said by our researcher in N1 would be of no possible importance to education which is conducted in C1.

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.

(2) C.G.Hempel, 'Philosophy of Natural Science', Prentice Hall, 1966. p51.

(3) Aristotle, 'Nichomachean Ethics.' (1139b-31) Penquin Books, Translated by J.Thompson, 1984. p207.

(4) A.J.Ayer, 'Man as the subject of science', Auguste Compte Memorial Lecture 6, Athlone Press, London, 1964.

(5) Problems with the assumptiom 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.

(7) Colin McGinn, "Action and its Explanation" in
'Philosophical Problems in Psychology', London, 1982.

(8) Donald Davidson, "Actions, Reasons and Causes" in
'The Philosophy of Action', (Ed. A.R.White), Oxford,
1977. p82.

(9) McGinn, op cit, p26.

(10) ibid, p27.

(11) ibid, p40.

(12) ibid, p32.

(13) A.J.Ayer, "Rational Action" in 'Proceedings and
addresses of the American Philosophical Association',
Antioch, 1962. Pages 5 - 24.

(14) Davidson, op cit, p274.

(15) D.Davidson, "Mental Events" in 'Essays on Actions
and Events', Clarendon, 1982. p208.

(16) ibid, p207ff.

(17) ibid, p218.

(18) ibid, p217.

(19) ibid, p219.

(20) ibid, p221.

(21) Disregarding the famous, or imfamous, 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.

(22) B.Russell, 'An Inquiry into meaning and truth',
Penguin, 1969. pages 244 and 246.

(23) ibid, p247.

(24) L.Wittgenstein, 'Tractatus Logico Philosophicus', Translated by Pears and McGuiness, RKP, 1961. para. 2.0201.

(25) Russell, op cit, p247.

(26) R.Carnap, 'The Logical Syntax of Language', Harcourt Brace, 1937. p245.

(27) J.Margolis, 'Philosophy of Psychology', Prentice - Hall, 1984. p13.

(28) W.V.O Quine, 'Word and Object', MIT Press, 1979. p214.

(29) ibid, p151.

(30) ibid, p220.

(31) ibid, p221.

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

(33) G.Ryle, 'The Concept of Mind', Penguin, 1976. p161.

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

(35) D.M.Armstrong, 'A Materialist Theory of Mind', RKP, 1968. p85f.

(36) J.J.C.Smart, "Sensations and Brain processes", in 'The Mind/Brain Identity theory', (Ed C.V Borst) MacMillan, 1979. p56.

(37) J.J.C Smart, "Materiarialism" 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.

(39) R.Rorty, 'Mind-Body Identity, Privacy and Categories', in Borst (Ed.). p187ff.

(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.

(44) W.V.O.Quine, 'Word and Object', MIT Press, 1979.
p27ff.

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. Inasfar as it relates to
what has already been argued, it relies on the
irreducibility of the intentional and on the
'anomalousness' of the mental. Inasfar 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.

4.40   The Case for Subjectivism: A.J.Ayer.

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 <u>Gadamer and Dilthey: Objectivism and Subjectivism.</u>

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[9]- 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[11] 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[12]: 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.'[13] 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'[14]- 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,[15]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 Wittensteinian 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 [16] 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

make sense'.[20] The force of the argument supporting

this is presented in the next paragraph[21]in which we

are invited to imagine someone who writes the letter 'S'

in a diary on each day of its occurrance. 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.'[22] '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'.[23] 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'.[24]

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.'[25] 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,[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.

4.40   <u>The case for Subjectivism: A.J.Ayer.</u>

Ayer, in a paper entitled, 'Can there be a
private language'[27], 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'.[28]
Testing must end somewhere and in the end must rest 'on
the testimony of my senses'[29] 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.[30] 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
<u>are</u> the same. Wittgenstein's point, however, concerns
the conditions of language, one of which <u>is</u> 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[31] 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'.[32] 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 them 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.[33] 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, responsibilty 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 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 is 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
cicumstances. 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 in 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,[34] for example, that

> 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.

In similar fashion Davidson writes,[35]

> 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.

Finally, MacIntyre, who says,[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   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 priviledged 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.

(2) D.Wiggins, "Deliberation and Practical Reason" in 'Essays on Arisototle's Ethics', (Ed Amelie Rorty), Univisity of California Press, 1980. p237.

(3) ibid, p237.

(4) ibid, p237.

(5) ibid, p236.

(6) Hans-Georg Gadamer, "The Historicity of Understanding" in 'Heidegger and Modern Philosophy' (Ed. Michael Murrey) Yale University Press, 1978.

(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 philosphical 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.

(9) W.V.O.Quine, 'Word and Object', MIT Press, 1979.

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.

(11) D.W.Hamlyn, "Objectivity" in 'Reason', (Ed Dearden, Hirst and Peters), RKP, 1975.

(12) ibid, p100.

(13) ibid, p101.

(14) ibid, p103.

(15) D.W.Hamlyn, "Self Knowledge" in 'Perception, Learning and the Self', RKP, 1983.

(16) L.Wittgenstein, 'Philosophical Investigations', translated by G.E.M.Anscombe, Blackwell, 1953.

(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.

(27) A.J.Ayer, "Can there be a private language?" in 'Wittgenstein', (Ed. George Pitcher) MacMillan, 1966.

(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.

(34) L.Wittgenstein, 'Zettel', translated by G.E.M.Anscombe, Blackwell, 1967. para 567.

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

(36) A.McIntyre, 'After Virtue', Duckworth, 1981. p199.

Chapter Five.

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   <u>Introduction.</u>

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 helful 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'[1] 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,[2]

> 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[3] 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,[4] 'for

x to have meant/nn 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,[5] '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.[6] Of particular interest are the 'preparatory' conditions[7] 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',[8] 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.'[9] 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.'[10]

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[11] - without some further information. To determine what they are doing, MacIntyre argues, is to 'place'[12] their action in a 'particular

narrative history'[13]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.'[14]

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[15] 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
[16] 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,[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...[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'.[22] They therefore conclude, in agreement with Gasking[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[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[25] the

distinction itself can only be made in a context and

usually reflects our practical interests. Of the

Voluntary-Involuntary distinction[26] 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 is also

found that it was deliberately put there.[27]

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[28] 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 unecessary 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 recurrance. 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[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[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[33], 'An
explicit answer to a question is one that repeats the
substance of the question itself; (eg 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'[34]and these
'reflect precommitments and...constitute what we bring to
the very posing of our questions...'.[35]Again,
'Questions are always projected on the basis of the
cognitive "state of the art", relative to an existing
body of putative knowledge'.[36] 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 Locherbie 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 futher 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[37] - 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[38]
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[39] 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 'indispensable 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,[40] a list of work along these lines which appears to be helpful. I also include (in a short appendix [41]) 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 explicity, 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   <u>Narratives and Objectivity.</u>

        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'.[42] 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.
[43]. 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
naivity. 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'[44]where he class ifies 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[45]

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[46]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[47]

> ...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 <u>contained</u> 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[48] takes an overview of this debate and

argues against Frye's position (which is also that of

Collingwood[49]). 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'[50] 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 straightforwardly 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 futher 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.'[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,[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'[60]and to McIntyre's comments on the importance

of narrative context to desciptions 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.

5.50   Conclusions.

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.

(2) P.F.Strawson, 'Philosophical logic' (Ed. P.F. Strawson) Oxford, 1967. p9.

(3) G.Frege, 'The Thought: A logical enquiry', Oxford, 1967. p19.

(4) H.P.Grice, 'Meaning', Oxford, 1967. p43.

(5) H.P.Grice. "Utterer's meaning, sentence meaning and word meaning" in 'The Philosophy of Language' (Ed. J.Searle) Oxford, 1971, p58 and "Meaning" in 'Philosophical Logic', (Ed. P.F. Strawson), Oxford 1967.

(6) John Searle, "What is a Speech Act" in 'The Philosophy of Language' (Ed. J.Searle) Oxford 1971 and his book 'Speech Acts', Cambridge, 1969.

(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.

(15) H.L.A.Hart and A.M.Honore, 'Causation and the Law', Oxford (Clarendon), 1959.

(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.

(28) J.L.Mackie, "Causes and Conditions" in 'Causation', Oxford, (Ed. Sosa) 1975. p15.

(29) ibid, p15.

(30) ibid, p16.

(31) N.Rescher, 'Empirical enquiry', Athlone, 1982.

(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.

(39) H. Putnam, 'Meaning and the moral sciences', RKP, 1978. p42/3.

(40) I include, here, a list of work on similar lines:

Hugo Verdaasdonk, "Concepts of acceptance and the basis of a theory of texts" (1974 "Trivialliteratuur. Suggesties voor een probleem-stelling" in J.Fontijn (ed) 'Populire literatuur' Amsterdam, Thespa.)

R.M.Martin, 'Towards a systematic pragmatics', Amsterdam, North Holland, 1959.

R.Carnap, 'Induktive logik und Wahrscheinlickeit' Wein :Springer, 1959.

L.J.Cohen, "The role of inductive reasoning in the interpretation of metaphor" in Harman and Davidson eds. 'Semantics of natural languages', Dordtrect: Reidel, 1972.

T.A.van Dijk, 'Some aspects of text grammars', The Hague, Moulton, 1972.

J.Hintikka, "On semantic information" in Hintikka and Suppes Eds, Dordtrect: Reidel, 1972.

C.Perelman, 'The new rhetoric', Notre Dame, London, 1969.

H.Verdaasdonk, "Concepts of acceptance and the basis of a theory of texts" in 'Pragmatics of Language and literature', North Holland, 1976.

I.Porn, 'The logic of Power', Oxford, Blackwell 1970.

G.H.Von Wright, 'An essay in deontic logic and the general theory of action', Amsterdam: North Holland Publishing Company, 1968.

I. and W.Kummer, "Logic of action and the structure of practical arguments" in 'Pragmatics of Language and Literature', North Holland, 1976.

T.A.Van Dijk, (Ed.), 'Pragmatics of Language and literature', North Holland Publications, article "Pragmatics and poetics" by T A van Teun, 1976.

H.P.Grice, 'logic and Conversation', unpublished lectures. Mimeo Berkeley.

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

(41) Appendix A

(42) Julius Caesar, 'The Conquest of Gaul', translated by S.A.Handford, Penguin Books, 1951.

(43) Sidney Hook (Ed.) 'Philosophy and History', New York University Press, 1963. Article by Morton white, "The logic of historical narration."

(44) Hayden White, 'Metahistory: The Historical Imagination in Nineteenth Century Europe' John Hopkins University Press, 1973. p5.

(45) Northrop Frye, 'New Directions from Old' in "Fables of Identity", Harcourt, Brace and World, 1963.

(46) ibid, p52-3.

(47) Hayden White, "The historical text as literary artifact" in 'The writing of history' (Ed. R.H.Canary and H.Kozicki) University of Wisconsin press, 1978. p46.

(48) F.R.Ankersmit, 'Narrative Logic.' Martinus Nijhoff, 1983. p48.

(49) R.J.Collingwood. See, for example, 'The Idea of History', Oxford, 1961. p231ff.

(50) Ankersmit, op cit, p94.

(51) ibid, p86.

(52) H.White, "The Value of Narrativity in the Representation of Reality", in 'On Narrative', (Ed. W.J.T.Mitchell) Chicago, 1981. p3.

(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   <u>Deductivism</u>

        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
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.'[3]

Anscombe remarks that she is 'out of sympathy'
with the 'assimilation of practical reasoning to a
trivial move of speculative reasoning'.[4] 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'.[5] 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'[6] 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'.[7] 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'.[8] 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'.[9]

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?[10] Raz makes the point more vividly[11] 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,[12] '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[13] when he says, 'We may say
with Samuel Clarke, "that our duties are a function of
the eternal relations of fitness that hold among
things."' One is tempted to add **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[14] spells out what he feels a more adequate account must include:[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 compexities of ordinary life *and* remain within the domain of logic. What I have to say next points to the fact that it cannot.

6.20   Problems with Deductivism.

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.'[16] 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[17]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.'[18] 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:[19]

> 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[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:[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'.[22] 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',[23]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 in to 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'.[24] 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[27] 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.'[28] Milligan fastens on to the importance of this and makes 'feature wants' central to his explanation of deliberation.[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 B[30] 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, governers 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 specifi - cations 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, aswell 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 scientistic 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.

(1) Roderick Chisholm, "Practical Reason and the Logic of Requirement" in 'Practical Reason', (Ed.Stephan Korner) Blackwell, 1974. p2/3.

(2) See, for example, his paper "Deontic Logic" in 'Contemporary Readings in Logical Theory' (Ed. I.Copi and J.A.Gould), MacMillan, 1967. p303. A fuller account is given in his book 'Norm and Action', R.K.P. 1963.

(3) G.Von Wright, "On So-Called Practical Inference" in 'Practical Reasoning', (Ed. J.Raz), Oxford University, 1978. p47.

(4) G.E.M.Anscombe in her reply to R.Chisholm in 'Practical Reason', (Ed. Stephan Korner), Blackwell, 1974. p18.

(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 vii):

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.

(17) David Milligan, 'Reasoning and the Explanation of
Actions', Harvester Press, 1980.

(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.

(21) D.Wiggins, "Deliberation and Practical Reason" in 'Essays on Arisototle's Ethics', (Ed.Amelie Rorty), University of California Press, 1980. p229.

(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.

(27) A.Kenny, "Practical Reason and Rational Appetite", in 'Practical Reasoning', (Ed. Joseph Raz), Oxford. 1978.

(28) ibid, p76.

(29) Milligan, op cit, p26.

(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 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 practi tioner 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 accomodate) 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 <u>because</u> 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 contexturalise 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 obectivity, 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 possibilty 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 'aisthetic' 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 desciptions. 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 Portugese 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   <u>The Range and Scope of Narratives.</u>

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, comprehensivenees 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 particulary 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 inasfar 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 responsibilty 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 approriate 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 fundamentaly one of dialogue. This
central feature brings us to 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
sigificantly 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 that 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 forsee

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

methodolgy 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.

Chapter Seven: Footnotes.

(1) R.S.Peters, 'Ethics and Education', Unwin, 1970. p18.

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 thecoin, 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 a 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 possiblility 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 intregal 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

actions.

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, distinquishes 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 difffering 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 approriate 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 *also* the extended family.
It might be argued, contrary to this, that regardless of
what tradition might say society at large has the

ultimate responsibilty 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 hypthesize 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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