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"THE WITTGENSTEINIAN REVOLUTION AND
'LINGUISTIC' PHILOSOPHY:
SOME IMPLICATIONS FOR
EDUCATION AND EDUCATIONAL
PHILOSOPHY."

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ABSTRACT

The potential relevance for Education of Wittgenstein's concept of the 'language-game', and the now widely accepted view of Philosophy as an activity to be practised rather than a body of knowledge to be learned up, are investigated principally in two of the 'component' disciplines of Education, namely Science Education and Educational Philosophy. In both of these areas, it has been claimed that Philosophy is relevant and has already been incorporated in existing work. A number of sub-optimal features are outlined for both areas, however, taking several widely-used publications as examples for detailed (though not exhaustive) critical examination; and it is argued that many of the faults revealed could be ameliorated through adoption of a Wittgensteinian approach especially if combined with Kuhn's (1962) notion of the 'paradigm' differentiated into the concepts of 'Common Assumptions Paradigm' (CAP) and 'Paradigmatic Demonstration' (or 'Public Demonstration Paradigm', PDP). This new synthesis results in a conceptual schema in which each 'family' of language-games which constitutes an academic discipline (or any intellectual or societal activity), is constrained and directed by its underlying CAP; and 'revolutionary' change in Kuhn's sense is accomplished by an alteration from CAP₁ to CAP₂ (a new CA Paradigm), signalled by a Paradigmatic Demonstration (PDP) that anomalies generated within the CAP₁ l-gs but not fully statable within them, can be both stated and resolved within the l-gs of CAP₂. This revolutionary 'paradigm-shift' is shown to involve alterations in the meanings of words, such that substantial problems of translation may arise — but may not be perceived as such — between the l-gs of CAP₁ and CAP₂, the users of the former perhaps being genuinely unable to see the latter as being other than (strictly) nonsense, i.e. meaningless, or false. The 'translation problem' between teachers and taught is viewed as a possible analogue of that between the several proponent groups of different CAPs, and some of the practical implications of this for education (notably for examining) are explored. Although the main purpose of the thesis is not exegesis nor critique of Wittgenstein's philosophy in itself, some attention is devoted to issues of interpretation, especially his doctrine of 'the unsayable' as against 'the sayable', in view of the significance for education of precise differentiation between the two; also for the abstraction, in relation to this distinction, of an adequate concept of 'reason'/'rationality'. In relation to the concept arrived at from Wittgenstein's and other published writings, concepts of 'rationality' etc. appealed to by Peters, Hirst, and others of the 'London' school of Educational Philosophy, are argued as being inadequate and misleading. In Science Education,

various limitations and faults are argued as being remediable if sufficient attention is paid to problems at the methodological/philosophical level, i.e. at the confluence between Educational Philosophy and Philosophy of Science; and educational changes needed to facilitate appropriate developments on these lines are outlined and discussed.

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PREFACE

Although Wittgenstein's Philosophical Investigations is widely acknowledged (e.g. by Passmore 1968) to be one of the most influential works in Philosophy published in this century, its true significance was not widely perceived within Philosophy until about a decade after Wittgenstein's death i.e. until about 1960. Since that time the study of Wittgenstein has spread very rapidly indeed, and since about 1970 (say) he has become the major figure of English-language academic philosophy. Coincidentally, as will be shown in the text of this thesis, from about 1960 Educational Philosophy entered upon what I shall call 'Phase 3' of its historical development, in which it regained touch, under the influence of C.D. Hardie, D.J. O'Connor, Israel Scheffler, R.S. Peters and others, with the 'parent' discipline of professional 'pure' Philosophy from which it had been largely separated for a half-century or more. It might have been expected, then, that Educational Philosophy would rapidly assimilate to itself the increasingly pervasive Wittgensteinian approach, especially since Wittgenstein's central concept of the 'language-game', with its emphasis upon developmental issues, would appear to lend itself very readily to educational purposes. But this has not yet occurred to any great extent. Although Soltis (1978), for example, makes some use of 'game' analogies, he does not extend this to the notion of the 'language-game'; and very few Educational Philosophers appear to have employed Wittgenstein's concepts or methods. A few papers of my own, some of which explicitly specify the concept of the 'language-game', have attempted to utilize a Wittgensteinian approach — but works along such lines have so far been, quantitatively, a very small minority. The predominant approach, largely due to the influence of Peters and the 'London' school, has derived from the Oxford style of linguistic analysis. This has lasted for the two decades since about 1960; but there are now signs that the potential of London-type Educational Philosophy may be reaching its limits.

In attempting to demonstrate the utility of the language-game approach within two particular fields of the general discipline of Education, viz. Educational Philosophy and Science Education, I shall attempt a synthesis between the language-game methodology and some concepts from a work which has already proved influential within a field which could be expected to relate to Science Education, namely Philosophy of Science; the work in question is one which itself embodies a Wittgensteinian approach: Kuhn's (1962) The Structure of Scientific Revolutions. From Kuhn's basic notion of 'paradigm' I

differentiate two more specific concepts, those of the Common Assumptions Paradigm (CAP) and the Public Demonstration Paradigm (PDP). With these concepts in combination with that of the 'language-game', a theoretical apparatus is set up with which the very great complexity and subtlety of educational issues may be tackled with (one hopes) an enhanced possibility of achieving a better understanding.

A problem which is peculiarly acute in Education is the intimate and perhaps inextricable intermingling of different 'dimensions': fact and value, what is and what ought to be, long-term desiderata and immediate brute practicalities. Educational Philosophy in its resurgence linked with professional 'pure' Philosophy, while gaining in sharpened perception of various conceptual issues, has tended to follow its 'parent' discipline in becoming 'exclusivist', i.e. concentrating upon matters which are in a sense uniquely philosophical: abstract and general, and criterially indeterminate. I argue against this trend, not in wishing to lessen the specifically philosophical forces deployed on the educational front, but rather in wishing to alter their nature and, especially, to swing them away from an 'exclusivist' and towards an 'inclusivist' approach which would facilitate integration both between academic disciplines and between theoretical exploration (with 'speculative philosophy', poetry, and perhaps science fiction at one extreme of the spectrum) and realistic appreciation of practicalities (at the other). Posing the issues in terms of the 'language-game' concept as used by Wittgenstein himself enables us, as I shall argue, to embrace both ends of the spectrum within a single continuum of methodology; and when our multifarious language-games are seen as resting upon and determined by a variety of Common Assumptions Paradigms of whose existence and nature we are usually only partly aware, a number of enigmatic features of education and of intellectual activity generally can better be understood. In particular, the paradox generated by a naive realism in Science Education, that we teach falsehoods some of the time, can be resolved.

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I have indicated in the text, by citation and quotation, the published works which I have found relevant to my argument — the nature of these debts to other workers, i.e. whether they have provoked my disagreement or provided

enlightenment, will be apparent, in each case, from the context. Apart from such debts to those who have published in the various disciplines from which I have drawn, and apart from emendations made in response to Supervisory comments (for which I reiterate my thanks), the arguments of this thesis are entirely my own.

D.S.

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CHAPTER ONE

INTRODUCTION: WITTGENSTEIN, PHILOSOPHY, AND SOME EDUCATIONAL ISSUES.

Wittgenstein's Tractatus Logico-Philosophicus (1922) became the 'Bible' of Logical Positivism which, having been a major force in academic philosophy for several decades (roughly 1920 - 1950, see Passmore 1968) has diffused out and become, with 'scientific method' with which it has affinities, part of the intellectual structure of 'educated' Western societies. His Philosophical Investigations, published posthumously in 1953 and encapsulating his personal but unpublished teachings which had been emanating from Cambridge since about 1930, was taken as a repudiation of many of the Tractatus doctrines, and as the paradigmatic exemplification of the revolutionary new movement of 'linguistic' philosophy which has become almost the dominant approach in academic philosophy, in one form or another, from about 1960 onwards. Pears (1971:11) remarks: "The most striking thing about his achievement is that he produced two different philosophies". The question of how different these philosophies really are will be discussed later (in relation to Toulmin's 1969 arguments that the 'different' philosophies should rather be regarded as but successive developments of what was basically a single position) - but there can be no doubt whatsoever of the central place of linguistic philosophy, and notable of Wittgenstein himself, in professional philosophy in the English-speaking countries from at least as early as the middle of the twentieth century. Passmore, himself an early critic of 'ordinary language philosophy' (see his 1952, 1954), states categorically (1969:7) that "The two most influential philosophical works published in the United Kingdom since World War II are quite certainly Gilbert Ryle's The Concept of Mind (1949) and Wittgenstein's Philosophical Investigations (1953)." Both are classified as linguistic philosophy. Kenny (1973), at the beginning of his study of Wittgenstein, says " ... by his own work, he created a new community of ideas. He published very little and avoided any kind of publicity; but the problems he discussed with a small group of pupils are now aired in universities throughout the world ... In the two decades since 1951 nine posthumous volumes of writings have been published, and the bibliography of studies of them contains well over a thousand titles."

There can in fact be little doubt about the great influence of Wittgenstein within professional philosophy and also in various other fields of intellectual activity. It is not the purpose of this thesis to attempt yet another assessment of Wittgenstein, either as an academic philosopher or, more generally, in terms of his wider intellectual influence. Instead, I want to explore some of the implications of his philosophy with regard to several current issues within Education. Some of these particular implications are specific to Wittgenstein's own methodology or technique of philosophy; others derive from the wider 'revolution in philosophy' (see the book of that name edited by Ryle 1956) to which Wittgenstein was a major contributor. One aspect of the 'revolution' which will be important to my argument is that philosophy itself is to be regarded not as body of 'inert ideas' (Whitehead 1929) to be learned up, but rather as an activity to be learned (in the sense of 'learning how' rather than 'learning that') and practised. Peters (1966) among others has indicated acceptance of this view of philosophy as being relevant to Educational Philosophy, and in this thesis I want to extend the exploration (started in earlier papers, e.g. Stenhouse, 1968, 1969a) of various implications of this. Along with the implications of 'philosophy as an activity', I want to examine Wittgenstein's concept of the 'language-game' - I shall distinguish, in this regard, between various statements by Wittgenstein about the 'language game' concept, and his own actual practice and usage of language-games - and to argue for the relevance and potential importance of this concept for both practice and theory in education. In doing so I shall concentrate mainly upon two of the sub-disciplines of Education as an academic study, viz. Educational Philosophy and Science Education; but I shall also try to indicate, if only in passing, the potential value of the 'language-game' approach in a number of other fields within education as a societal and individual activity and Education as an academic discipline.

It seems to me prima facie surprising that Wittgensteinian 'language-game' or 'ordinary language' philosophy has not so far been taken up and incorporated into educational theory and especially educational philosophy. My surprise stems in part from the nature of Wittgenstein's approach, which seems to me to be eminently compatible (as I shall attempt to demonstrate) with educational theory and practice,

and in part from the fact that Wittgenstein was for several years himself a teacher. In his Biographical Sketch, von Wright (1958) states that "After the war World War I Wittgenstein took up the vocation of schoolmaster. In 1919-20 he was trained at a college for teachers in elementary schools ... in Vienna. From 1920 to 1926 he taught in various remote villages in the districts of Schneeberg and Sommering in Lower Austria." (p.10). Von Wright goes on to show that "During his years as a schoolmaster ... Wittgenstein was not completely cut off from contact with the philosophical world": he received visits from and/or was in correspondence with Frank Ramsay and the economist John Maynard Keynes, both of Cambridge, and Moritz Schlick, founder and leader of the 'Vienna Circle', the protagonists of Logical Positivism. As a result of these contacts, he returned to the practice of philosophy. As von Wright narrates:

"Early in 1929 Wittgenstein arrived at Cambridge. He was first registered as a research student, a somewhat unusual status for a man whom many already regarded as one of the foremost living representatives of his subject. The idea was that he should work for the Ph.D. It turned out, however, that he could ... present his book, published eight years earlier i.e. the *Tractatus*, as a thesis. He received his degree in June 1929."

As I shall argue, in a later section of this thesis, Wittgenstein not only kept contact with philosophy while he was schoolteaching, but the traffic was 'two-way': his experience of teaching, especially of teaching relatively young children in elementary school, had a significant influence upon his philosophy; and in this, more upon his actual practice than upon what he explicitly stated about his practice. Amplified discussion of this issue will be offered in later pages. For the present, I shall merely remark that it seems intrinsically unlikely that a person so perceptive and so highly intelligent as Wittgenstein could have taught for five years without coming to a profound understanding of the activities and the values of teaching, and that it also seems unlikely that this experience should fail to affect his own subsequent practice in doing philosophy and also in teaching it.

With regard to this last point, it may be appropriate to emphasize that one of the most notable features of Wittgenstein's life and work, which has been remarked upon by virtually every commentator, was the extreme seriousness and intensity which he brought to his teaching of philosophy, also the idiosyncratic method of this teaching. Professor

Norman Malcolm's (1958) Ludwig Wittgenstein: a Memoir gives abundant illustration of this. His manner of doing philosophy, i.e. writing for publication, was also highly idiosyncratic; and though the form taken by the Tractatus might perhaps be explained by the fact that it was composed mainly as a notebook written on a piecemeal basis while Wittgenstein was a soldier and/or prisoner-of-war, the Philosophical Investigations were deliberately written to be published more-or-less in the form in which they finally appeared (see Malcolm 1958, esp. pp 69-70, 98). The suggestion may be offered at this point - to be amplified and discussed later - that Wittgenstein's method of doing philosophy can best be understood if it is seen as being simultaneously an activity of teaching. In short, we might say that for Wittgenstein, 'doing philosophy' and 'teaching philosophy' are interchangeable phrases. If left like this, of course, the point would be overstated: it is hardly likely that Wittgenstein would have overlooked the standard educational maxim to 'match the manner and level of teaching to the stage of development/'readiness' of the pupils'. Nevertheless, his teaching at Cambridge was, on Malcolm's account (which is supported by those of various others who had been Wittgenstein's students), very much a matter of doing philosophy:

"His lectures were given without preparation and without notes. He told me that once he had tried to lecture from notes but was disgusted with the result.... In the method that he came to use his only preparation for the lecture, as he told me, was to spend a few minutes before the class met, recollecting the course that the inquiry had taken at the previous meetings. At the beginning of the lecture he would give a brief summary of this and then he would start from there, trying to advance the investigation with fresh thoughts ... what occurred in these class meetings was largely new research." (Malcolm 1958: 24).

If it is kept in mind that the 'new research' which was done in these teaching sessions was in fact excessively difficult argument which on Malcolm's showing (pp 28-9) was "unintelligible" to many even of those who were allowed to attend - and despite the fact that most would already have considerable background in Philosophy before attempting to cope with Wittgenstein, as Malcolm says " ... one had to attend for quite a long time (at least three terms, I should say) before one could begin to get any grasp of what he was doing" - it may become more intelligible why the Wittgensteinian approach has not been taken up ~~more~~ widely, and more rapidly, within Education. After all, it was neither

widely nor rapidly taken up even within professional Philosophy; and at least most philosophy students would have known of Wittgenstein, whereas educationalists, until fairly recently, would not. Even as the various notes of and reports upon his lectures were actually published, understanding of their significance and intent was, naturally enough, not immediate. The Philosophical Investigations appeared in 1953, Moore's papers "Wittgenstein's Lectures in 1930-33" were published in Mind in 1954-5, and The Blue and Brown Books in 1958 - all the rest of 'published Wittgenstein' has appeared only subsequently to 1960. It has given rise, as remarked earlier, to a huge and rapidly-expanding body of critical and expository literature within academic Philosophy (see Pears 1971: 187-8, Kenny 1973: 233; also von Wright's 1969 survey of all Wittgenstein's extant writings, published and unpublished). Various philosophers of education have certainly been aware of the existence of Wittgenstein's work, and of the fact that it was regarded as important, at least since the time of its more extended recognition within 'pure' Philosophy, i.e. since the late 1950s, or say about 1960, to put a definite though somewhat arbitrary date to this development. For example, Peters (1966:61) in discussing the nature of philosophy, remarks that "Arguments and distinctions first formulated by Aristotle and Kant jostle with more recent ones developed by Wittgenstein"; and Hirst (1966: 37) in the same volume, quotes and apparently accepts O'Connor's (1957: 4) view of philosophy as an "... activity of criticism or clarification". But the 'language-game' concept has hitherto received little recognition, still less general acceptance, within Educational Philosophy or the general discipline of Education. It was discussed briefly in an earlier paper of my own (Stenhouse 1969b); and Soltis (1978) in several places makes use of 'game' analogies (e.g. pp 73, 94 ff), very effectively, but without explicitly linking them with the 'language-game' notion as developed by Wittgenstein.

Expanding from what was mentioned earlier, in this thesis I want to show that the concepts of the 'language game', of philosophy as an activity rather than a body of knowledge, of the distinction between 'normal' and 'revolutionary' activity in an intellectual discipline (advanced first by Kuhn 1962 in relation to science), and of the notion of philosophy as 'inclusivist' rather than 'exclusivist', can all be used to enhance our understanding of Education and some of its sub-disciplines; and at second remove to increase the effectiveness

of educating as a social and societal process. To show the application of these concepts extensively, over the whole great range of subject-matters and activities that are encompassed under the general rubric 'Education', would be an undertaking of colossal magnitude. To provide an arena of manageable proportions, attention will be focussed mainly upon two of the sub-disciplines of Education, namely Science Education and Educational Philosophy - parenthetical reference will also be made, as seems appropriate, to issues which may be extraneous to these sub-disciplines. The question arises: Why choose to concentrate upon these particular sub-disciplines of Education, these particular aspects of education as social activity? Several answers to this can be made.

In the first place, in order to demonstrate that the concepts and techniques whose use I am advocating can have a meliorating influence, it is necessary to show that improvement can be made relative to the present state of an practices within the discipline(s) in question. To do this it is necessary, therefore, to provide some depiction of their present conditions. It so happens that Science Education has received a public and objective 'vote of no confidence' over the last decade or more, in the form of the Drift from Science (Dainton 1968; further refs and discussion in Chapter 2, below). There is continuing debate over the nature and causes of the Drift, and massive efforts have been made for at least two decades to improve science curricula, the training of science teachers, and so on. Improvements have undoubtedly been made, but it appears to be still an open question whether there has been any substantial overall improvement - indeed the continuing proliferation of new curricula, etc., might be taken as circumstantial evidence that there has not. As explained in Chapter 2, the argument I shall offer in the rest of the thesis does not in any way depend upon the objective existence of a Drift or of any particular flaws in science education as actually practised. Rather, I shall postulate several characteristics of science education, which, if they occur, would have a deleterious effect; and I shall argue, on a purely hypothetical basis, for changes in various aspects of science education (especially the education of science teachers). Thus my argument is not dependent upon empirical proof of the Drift from Science nor of the existence of particular faults in science education. I happen to believe that the faults I mention are in fact frequently instantiated and that the Drift is real enough even though its nature and causation vary from place to

place and from time to time - but that these assumptions should be established as true is quite inessential to my thesis. There is extensive published evidence to suggest that the various faults I mention do in fact occur at least sometimes (and perhaps quite often) in the teaching of science. The argument I shall offer will be based, however, upon objective evidence of a different sort: the practice of science education as published in textbooks and other documents on the subject. It is no part of my intention to show that all science education is faulty, or that all textbooks on science education are misleading. My argument will be based upon the particular texts that are used as examples. These texts have been selected as being, so far as I can judge, both typical of their kind, and widely used. I recognize full well that one can find texts that are better, and others which are worse. The substantive issue is, however, one of methodology; and if the hypothetical argument which I offer should provide an avenue for the improvement of science education, that would be a bonus about which one could be pleased - but the only issue which is real and immediate is that of the illumination which may be provided by my argument simply as argument.

As I shall suggest in the concluding pages of Chapter 2, one of the most basic improvements which is needed in science education is for people (especially teachers) to be not only literate and 'numerate' - as had already been widely recognized and accepted - but also 'philosoph'icate', i.e. more sophisticated about methodology/philosophy of science than is common at present. The elaboration of what this can really mean is one of the main burdens of the thesis as a whole, and an extensive explication will not be attempted at this point. A relevant clarificatory distinction which may be mentioned, however, is that between matters of 'technique' and those of 'methodology'. Rudner (1966) provides a good introductory discussion of this distinction. Briefly, 'technique' is concerned with short-range 'tactical' issues and physical processes of observation, manipulation, measurement, etc.; whereas 'methodology' is concerned with longer-range 'strategical' issues and not with the physical processes as such but only with their outcomes, especially perhaps with questions of their appropriateness. It will be argued (see particularly Chapter 7) that teachers need to be able to understand the mistakes of their pupils if they are to make optimal use of all opportunities for teaching, that this involves being

able to see the mistakes of the pupils as not simply being 'wrong' but as perhaps being the outcome of different ways of seeing the world and as the (perhaps quite valid) inferences from alternative sets of premisses, alternative 'ideals of natural order' (Toulmin 1961) or 'paradigms' (Kuhn 1962). In other words, teachers need to be able to perceive that on most issues, considerable flexibility is possible at the methodological level, and that what is to count as the 'right' answer depends very much on the frame of reference that is invoked. To achieve the requisite level of methodological understanding and flexibility, it is argued that teachers need more education in philosophy than most appear to receive at present. This leads on to the question of the nature of the philosophical education which they receive now.

It is suggested that the most or the only contact which the majority of teachers make with Philosophy as an academic discipline is in the form of a course of Educational Philosophy at teachers college or university; and that this is most commonly derived, directly or indirectly, from the 'London school' of Educational Philosophy led by R.S. Peters, Paul Hirst and their colleagues and proteges. In Chapter 3 a brief outline of the history and present position in Educational Philosophy is given, with some preliminary discussion of the 'London' approach. Chapter 4 is devoted to some discussion (again very brief) of various shortcomings and faults in this approach, this constituting part of an attempt to put the 'London' approach into its correct historical and methodological perspective. This involves a discussion of the place of the 'London' protagonists in the history of philosophy, on the one hand, and on the other their relationship to their societal 'environment', viz. the education system within which they work and in relationship to which their philosophy has been developed. It is suggested that they have adopted and exaggerated certain features of Oxford 'analytic/linguistic' philosophy, and that this has led to weaknesses and distortions. Some of the arguments of R.S. Peters are examined in detail and shown to be defective and misleading, especially in contrast to what might be termed a 'broadly Wittgensteinian' approach.

The conservatism in methodology detected in Peters' educational philosophy is compared, in Chapter 5, with Kuhn's (1962) concept of

'normal science'. The distinction between 'normal' and 'revolutionary' science is broadened to apply to other academic disciplines and to intellectual activity in general; and the concept of 'paradigm', on the basis of which the above distinction is made, is subdivided (condensing a multiplicity of distinctions suggested by Masterman 1970) into two, the Common Assumption Paradigm (CAP) and the Public Demonstration Paradigm (PDP). It is argued that most science teaching tends to restrict attention to 'normal' science and to the CA Paradigm which happen to be current (or, quite frequently, which were current at whatever time in the past when the textbooks were written and the teachers received their training), and that this in-built orientation to the past must tend to have a counter-productive effect in science education. Similarly, mutatis mutandis, in other disciplines including, presumably, Educational Philosophy. Such past-referenced education can be expected to be conducive to rigidity of thinking, and to favour the 'convergent' rather than the 'divergent' thinkers (of Hudson 1966, 1968, 1971). Intellectual conformity is suggested as the likely outcome of both 'normal science' teaching and 'London'-type Educational Philosophy.

Chapter 6 is devoted to an examination of several passages from text-books in Science Education. In these, despite avowals as to the importance of Philosophy for Science Education, very substantial misrepresentations are found of both Philosophy and of Science itself. Some of these are explored in detail. Concurrently with this, some ideas are built up as to possible improvements which might be made, based mainly on the 'language-game' concept of Wittgenstein. One of the areas of current concern, within Education, to which the 'language-game' concept has the potential to make a highly significant contribution, is the question of integration within and between major disciplines (see, for example, Yudkin (ed.) 1969, Jevons and Turner (eds) 1972). The 'knowledge explosion' has led to the proliferation of new 'subjects' and sub-disciplines and thus to increasing fragmentation within education. Fragmentation at the level of 'mere information' is bad enough, but this has been and is being accompanied by fragmentation at the methodological level. In a real sense there is a tendency for people's thinking to be pushed towards dis-integration. As a counter to this, professional discourse and practice within all of the various subject-disciplines

can be represented as being various types of language-game (there is no suggestion that this is a necessary or the only way of seeing the plurality of disciplines). The processes of learning each professional and/or academic language-game can be shown to involve a development of 'specialist' language-games which develop from, and are necessarily dependent upon, the various 'ordinary life' language-games which are themselves the outgrowth of the simpler and more primitive language-games of childhood (and school!). Thus the language-game approach can be shown to be 'global', or perhaps more truly, 'oceanic', in that every distinguishable language-game can be shown to be confluent, directly or indirectly, with every other. Also, the language-games specific to education can be seen as necessarily central, and pervasively influential, in the proper development of all other language-games. It follows that it is vitally necessary that educational language-games should be made as effective as possible; and it will readily be seen that this type of approach can facilitate the re-integration and unification of all the various subject-disciplines, not in the sense of denying (or attempting to deny) their differences, but rather in appreciating how their differences come about, and how differences and similarities are both real and both but parts of a single continuum of methodology.

In seeking to optimise science education (and the same would go for education in any other field), we are faced with the dilemma of either accepting current practices as being ipso facto the best, or at least as fully adequate; or else we have to embark on a systematic exploration of alternative possibilities, at least some of which would be hoped to be better than current practices. In Chapter 7 this dilemma is posed and faced. It is argued that methodological/philosophical exploration must precede attempts at practical implementation, for three reasons:

(a) The criteria for success or failure of practical policy must be understood and accepted before the outcomes of practical experiments can be assessed and evaluated;

(b) Innovations in practice in education are almost universally dogged by the 'Hawthorne effect': the innovation appears initially to succeed but, as often turns out later, its success was due merely to its newness, not to its intrinsic merits; and

(c) Only those innovations which pass the test of rigorous exploration and critical appraisal at the theoretical level should be implemented, even on a trial basis, since it would be unethical to subject pupils to an educational regimen which turned out to be deleterious. (Good intentions, in education, are emphatically not enough.)

Exploration of educational alternatives at the methodological/philosophical level must demand, of those who attempt it, competence in the subject-field(s) involved, experience of and competence at the practicalities of educating, i.e. teaching, and competence in philosophy understood as involving the exploration of theoretical possibilities and alternatives of meaning/interpretation. It is here that the importance of seeing philosophy as an activity, as 'inclusivist' rather than 'exclusivist', and in terms of language-games, can begin to be most fully appreciated. The 'language-game' concept provides for methodological continuity, and the exploration of possible language-games is and must be, most emphatically, an activity. Explorations within what is, in effect, an area of multidimensional confluence between a number of disciplines and at a number of levels on the practical-theoretical continuum, demand a type of 'philosophy' which can range from the highly abstract and 'criterially indeterminate' argument which is peculiar to philosophy (and which some philosophers might take to be the only sort of activity in which they can properly, qua philosophers, indulge) along to discussion/exploration whose 'validity' (in a loose sense) can be determined only by reference to considerations of down-to-earth practicality. In brief, the philosopher's ability and habit of seeking and seeing new possibilities of meaning/interpretation must be linked with the ability to assess any envisioned possible language-game in terms of its associated practicalities; and this latter condition is necessary before even the 'meaningfulness' of utterances within a language-game can be assessed. For example, taking a tennis analogy, is it meaningful to say "He aced his opponent by means of a 'let' serve"? No; and we know this, not on a basis of practicality, but on the basis of understanding the rules and terminology of the game. By contrast, what about "He won the match without allowing his opponent a single point, by deliberately and consistently hitting net-cord shots"? I suggest that

this statement, to an experienced tennis player, would be regarded as being implausible in the extreme - implausible to such an extent, indeed, as to approach the point where it would be said to be almost necessarily false. And it would be regarded as being almost necessarily false on the basis of practicalities: nobody, but nobody, has ever been good enough to be able to hit net-cords at will and consistently. To win a match without conceding his opponent a single point, a man has to win 120 points consecutively. This is a little unlikely (though the legendary Suzanne Lenglen is said to have once demolished an opponent, losing only seven points in the process). To win all 120 points by netcords (which implies that the opponent never lost a point by double-faulting), would appear to have astronomical odds against it, odds which would approach infinity. To the extent that this could be accepted as being so, I suggest that the statement approaches 'necessary falsehood' (in an operational sense). In relation to the exploration of possible educational language-games it is of course unnecessary to establish that either 'necessary falsehood', meaningless or similar notions can be based upon considerations of practicality: ordinary implausibility is quite sufficient, if empirical implausibility can in fact be reasonably firmly established. (The little 'tennis' exploration is worth its place, I feel, in opening up the possibility of calling in question such 'standard' philosophical distinctions as that between 'logical possibility' and 'empirical possibility', and their opposites. Such distinction have their place in introductory textbooks of philosophy, e.g. the excellent and widely-used one by Hospers (1967; see Chapter 3, esp. pp 169 ff, on this particular distinction). They tend, however, to be accepted, not just as useful within particular contexts, but as being in some way final or absolute. Against this, I would follow Waismann in his "Analytic-Synthetic" papers in Analysis 1949-52, also his 1953 Logic and Language paper, in arguing that such distinctions cannot be final or absolute, since they are based upon linguistic usages which themselves have 'open texture', i.e. are not fixed; and that the assumption that they can be fixed by definition is either illusory or arbitrary, or both. Quine 1958 has made similar points, though on rather different grounds.)

In the theoretical exploration of possible improvements in educational practice, then, some of the criteria of whether or not a

suggested language-game (i.e. educational practice) is viable and worth trying in practice, can be decided upon only by persons who have a firm grasp of practicalities. There are problems here: the person with the most teaching experience, quantitatively, is not necessarily the person who has learnt and understood most, nor is he/she necessarily the best (or even a good) judge of proposed innovations. In fact, persons of long experience are likely to encounter a 'conservatism barrier' in their own thinking: they will tend to be dominated by the unconscious assumption that 'old ways are best'. This is of course quite natural. The problem can be dealt with in various ways. If philosophy can be taught as an activity analogous to psychoanalysis (see Wisdom 1953, also Chapter 2 below), in which the unconsciously-held assumptions from the past are brought up to conscious scrutiny, then if education-
alists can be educated in philosophy this should help to facilitate their giving new ideas a fair assessment. The other major alternative, which could be used independently of education in philosophy, is that of setting up societal mechanisms which would operate to 'select' for decision-making and evaluating positions those individuals who happened to have most ability for this type of judgement. (For further discussion of the systemic mechanisms involved see Stenhouse 1971a following from Leach 1968, also Stenhouse 1967).

Another desirable contribution of Philosophy to the optimization of Science Education which is discussed in Chapter 7 is that it could enhance the ability of teachers to make use of the 'mistakes' of their pupils as generating valuable teaching opportunities. This has already been mentioned (above, p.7), but several highly significant further implications need to be pointed out. One is that the possibility must be recognized, in science as in any other field, that a student might be able, independently, to anticipate a future advance within the science, especially if this is of 'revolutionary' nature in Kuhn's (1962) sense, i.e. dependent on different basic assumptions from the current 'normal' science. That this might occur even at school level is not, perhaps, so unlikely as might be thought. What has been called the 'outsider effect' is well recognized and would appear to be due in part to relative unfamiliarity with the currently-accepted paradigm helping the 'outsider' to break free from it and set up a new alternative (which may then be

supported, and perhaps substantiated, by the findings of subsequent empirical research). A pupil still at school is largely an outsider to any academic discipline - so given the requisite intelligence and interest, etc., there is no intrinsic reason why a school pupil should not think his/her way into a subject on the basis of what is, in effect, a new paradigm, and thus be the precursor, presumably unwittingly, of a scientific revolution. That teachers and examiners should be able to cope constructively with this possibility is desirable for two distinguishable but related reasons:

- (a) That the creative talent of the pupil should be fostered, for the sake of both the individual pupil and the society; and
- (b) That the potentially creative individual should not be alienated from the society and its institutions - otherwise what might have been a constructive 'scientific revolutionary' may turn into a destructive social and political revolutionary.

One of the educational strategies which can generate open-mindedness on the part of teachers and examiners, and thus be conducive to increasing their ability to deal with atypical ideas and atypical individuals, is to structure educational and professional situations so that the individual is forced to break his/her hitherto-established patterns of thought and assumption; and then, once new patterns have become established, to force the breaking of these ones; and so on, until it has become accepted that no pattern is final, no assumption is necessarily to be accepted. O'Connor makes a similar but more limited point with regard to scientific theories, when he says that no theory is ever so firmly established that further evidence could not require its revision or abandonment (O'Connor 1957).

If education in philosophy were envisaged as providing a way of attaining the above 'open-mindedness' and receptivity to new ideas and atypical pupils, on the part of educators, various questions would arise and would have to be dealt with, regarding the nature of education, of philosophy, and the nature of education in philosophy. One of the potential 'problem areas', discussed in Chapter 8, is of the relationships between questions of 'proof' and questions of 'truth-or-falsity' in philosophy. While the distinction between 'validity of argument' and 'truth/falsity of propositions/premisses' is taken to be one of the first and most basic distinctions to be made in the teaching of philosophy, a tendency of some philosophers (Bertrand Russell and D.J. O'Connor are taken as examples) to drift towards the assumption that philosophical

conclusions must be in some sense 'true', is noted and examined. While it seems clear that, even for these philosophers, 'true' cannot be meant in any straightforward empirical or semi-empirical sense, they do appear to assume that particular conclusions can in some sense or other be 'established' in philosophy (even if some/most of such conclusions might be regarded as 'negative' ones). The tendency to slide into an assumption that philosophical conclusions can be 'established' or 'true' may be associated, it is suggested, with a tendency to view science as the paradigm of intellectual activity and philosophy as being in some way or other ancillary to scientific inquiry - hence, perhaps, the characterization of philosophy by some recent philosophers as being 'second-order' and/or 'spectatorial'. This general positivistic tendency in some recent philosophy (and in 'London'-type Educational Philosophy see especially Chapters 3-5, below) seems likely, it is suggested, to be due in part to the enormous success and influence of science (and its derived technology) in the last two centuries; and certainly it seems in part also due to the influence of logical Positivism and its central opus, Wittgenstein's Tractatus. (Whatever Wittgenstein's own intent may have been, it seems to be a matter of historical fact that the Tractatus was taken to be an exposition of positivist doctrine.) But if conclusions are to be 'established' without being empirically true, it appears that they would have to be both (a) relative to a totality of 'premisses', explicit and implicit, overt and covert, consciously held or not consciously held, which would seem to be generally equivalent to Kuhn's (1962) 'paradigm' in the sense of Common Assumptions Paradigm; and also (b) they might amount to little more than 'the accepted view', 'the received orthodoxy' - in other words, adherence to particular 'conclusions' which have been 'established' may in the absence or impossibility of both empirical demonstration and strict logical proof, amount to little more than social conformism.

Some possible approaches to teaching Philosophy specifically for its benefits for wider education are discussed in Chapter 9. Study of philosophy is envisaged in a context in which 'multi-disciplinarity' is strongly emphasized; and where philosophy helps to facilitate 'inter-disciplinary translation'. The usefulness of teaching philosophy on the basis (in part at least) of its historical sequence of development, is argued as providing an 'ontogenetic sequence' for the development of the

individual which (largely) recapitulates the 'phylogenetic sequence' of the historical development of the subject itself, with the various sciences budding off at different times from the central 'stem' of pure philosophy. Competence in the activity of philosophy is seen as being a desideratum for the competence of many scientists as scientists, especially in enabling them to bring on and facilitate 'revolutionary'-type changes which involve alterations at the conceptual/methodological level. In this connection, some discussion of various treatments of the concept of 'knowledge' (necessarily a basic concept, one would think, in Education) as well as in Science, is offered. Hirst's (1974) treatment of 'forms of knowledge' is discussed at some length, and is found to be defective in various ways most of which could be ameliorated by the adoption of a more Wittgensteinian approach. Hirst's paper is used not only for its intrinsic interest, but also as providing exemplification of the strengths and limitations of the 'London' school of Educational Philosophy; and it is regarded as unfortunate that he should have neglected Ryle's (1949) important distinction between 'knowing that' and 'knowing how'. Information is normally stored (by societal mechanisms, e.g. libraries) in the 'that' form; and one of the most basic functions of education is argued as being that of teaching people how to decode 'knowing that' into 'knowing how'. This is especially important for any discipline which emphasizes its 'activity' aspects - notably, Philosophy itself if taken as 'philosophy as activity (of analysis, clarification, etc)' following Ryle (1956) as Peters, Hirst and others profess to do. Following on from this, some of the analogies which have been put up for philosophy (playing games, spectating, and so on) are discussed, and their various stronger and weaker points indicated. Throughout, philosophy is continually argued as being in intimate and necessary relationship with other academic disciplines, on the one hand, and on the other with the experiences and interests, problems, etc. of the everyday life of everyone, including both teachers and pupils; and it is argued that this complexity of involvement should be made a source of educational strength.

The relationship between philosophical thinking and other types of thinking e.g. mythic or magical, and relationships between philosophical thinking and other phenomena e.g. literacy, the written as

contrasted with the spoken word, are examined in Chapter 10. Lines of argument are explored, in which it is suggested that thinking which is both self-critical and 'open' (the latter specifically in the sense of 'actively looking for new and/or alternatives of meaning') is facilitated by the use of the written word; hence the historical development of the 'critical tradition' in association with writing; hence also a number of fairly obvious implications for education. Among the latter is the possibility of altering the swing towards 'exclusivism' in philosophy (which has been a phenomenon confined mainly to the last century), and generating, by appropriate programmes of education, a new 'inclusivist' approach centring on the Wittgensteinian concept of the 'language-game', which it is suggested could be of great value in improving the amount of interchange between philosophy and other disciplines, including Education, for the benefit of all of them. Professor Goody's (1977) arguments for an association between literacy and the evolution of what he calls a "tradition of skepticism" in modern Western postscientific thought are examined and supported, and in modified form (that of a 'tradition of moderate scepticism') this position is argued as being in continuity with the view of behavioural phylogeny offered in my "The Evolution of Intelligence: a general theory and some of its implications" (Stenhouse 1974), also as providing theoretical underpinning from the side of Social/Cultural Anthropology for much that is argued in this thesis.

It is argued that from the work of Goody (1977), Horton (1967), Wartofsky (1967) and others, a concept of 'rationality' emerges which is sharply at variance with that offered by R.S. Peters and apparently widely accepted within Educational Philosophy. Peters' concept equates rationality with 'being prepared to give reasons for ...'. This is seen as inadequate in various ways, e.g. as being conducive to mere social/intellectual conformism, as being insufficiently differentiated from Freudian 'rationalization' with its connotations of maladaptiveness, and so on. The concept of rationality being basic to philosophy, education and all intellectual endeavours, it is extremely important that an adequate account of it should be given. The work of Goody and others (already mentioned) can lead to the development of alternative and much more comprehensive and realistic concepts of 'rationality', in three

dimensions which can be summarized as follows:

- (i) A capacity for self-examination: 'reflexivity';
- (ii) Ability to perceive more than one alternative, of objects of attention, or meanings/interpretations; and
- (iii) Ability to hold in abeyance the acceptance of alternatives revealed by (ii) above, this ability, in the event that some of the alternatives are definitely rejected, amounting to a capacity for self-criticism.

These three 'dimensions' of rationality (they are not regarded as being in any way exhaustive) are argued as being both more 'true to life' than are Peters' views, having regard to the actualities of scientific, philosophical and intellectual activity generally, and also as providing a theoretical framework for a dynamic and forward-looking approach (as contrasted to the past-orientated and/or conservative and/or authoritarian approaches that have been and still are common) in educational activities.

Other aspects of educational activities are also touched upon: McLuhan's well-known emphasis upon the media of communication and the effects that the media can exert upon the message conveyed, and in association with this the important dimension of non-verbal communication and its relationship to emotional/instinctual, 'fantasy' and feeling aspects of education (see Jones 1972). None of these can be treated exhaustively, or even as extensively as they deserve - yet it is felt that they must be given at least some mention, however fleeting and inadequate in itself, so that the reader may gain at least a minimal idea of how the notion of an 'inclusivist' approach, in philosophy and in education, can be articulated. Wittgenstein's own use of the 'language-game' approach, (though not always what he said about it) has implicit within it a definite 'inclusivist' commitment, and it would appear to be methodologically desirable that a work arguing for such an approach should also endeavour to exemplify what it advocates!

Further discussion of non-verbal aspects of teaching is offered in Chapter 10, especially the use of techniques and 'tricks' of philosophy to 'put down' pupils and colleagues in what are really 'social-dominance contests' (to use the language of Ethology) as a substitute for teaching. This is the sort of thing that Kant would have called a 'heteronomy of the reason': the use of reason/intelligence to pursue objectives which are non-reasonable (in this case the social-dominance instincts and

'putting down') and, especially if the 'putting down' is a substitute for teaching, objectionable. It is suggested that wider understanding of the nature of such transactions would tend to obviate their bad effects; and that better and more widespread understanding of Ethology would be helpful in, inter alia, coming to a better appreciation of classroom interactions and of research on classroom interaction. (A need for philosophical competence in those indulging in this last type of research is attested by examination of part of a publication on this topic.)

Clarification of problems regarding Wittgenstein's life and work is used, simultaneously, to emphasize the importance he himself gave to some particular aspects of 'what cannot be said' (non-verbal communication) namely the showing and other activities that make up what he called in the Investigations 'ostensive learning' (and teaching). Questions of the interpretation and mis-interpretation of Wittgenstein's work, notably regarding Russell's controversial Introduction to the English translation of the Tractatus, are gone into in some detail, partly for their intrinsic interest and educational value, partly to illustrate the very substantial difficulties of interpretation which arise for even the greatest minds, when attempting to deal with the changing networks of meaning that constitute a 'paradigm shift' and 'revolutionary' change in the Kuhnian sense. Particularly noteworthy is Russell's support for the continuation of Wittgenstein's Fellowship at Cambridge, in view of his avowed inability fully to understand the work upon which Wittgenstein was engaged - inability to understand so often generates a negative decision!

The second half of Chapter 11 looks into some of the affinities which may be discerned between Pragmatism on the one hand (especially important for education through the influence of Dewey), and Existentialism on the other (which has assumed an increasing importance, on a world-wide basis, as an often salient ingredient in the 'Alternative Culture' of those who have become disenchanted with the 'official' culture and formal education of Western civilization). One of the most important links between all three 'philosophies' is the importance they give to actions as distinct from words, also their emphasis (notably in Existentialism and for Wittgenstein himself) upon individuation, and individual as against collective responsibility.

For Wittgenstein, actions are (largely) part of the 'unsayable', that which cannot adequately be given purely verbal characterization, that which cannot be said (gesagt) but can only be indicated, pointed to (gezeigt). Chapter 12 is devoted to discussion of the importance of 'the unsayable', interpreted in this way, both for Wittgenstein's philosophy and also for education, especially education in the sciences and for moral and 'value' education. 'The unsayable' relates very differently to these two important areas of education. It is best understood in the sense of 'direct experience' - sensory and motor experience - in the case of the sciences; and in the case of moral- and-value education, a complex mixture of direct experiences along with 'teaching by example' (the example of the teacher, also vicarious experience in form of novels, drama, film, art and music, etc.).

Perhaps the most important, and certainly the most difficult, aspect of the doctrine of 'the unsayable' is that which relates to the question of the boundaries or limits of what is sayable (hence, to a considerable extent, what is thinkable). In this sense the limits of the sayable are co-extensive with the limits of reason itself (and it was in these terms that Kant dealt with what is essentially the same issue). Here we can come to perceive the usefulness of Wittgenstein's having formulated the problem in linguistic terms (i.e. in terms of language-games), here we can see how philosophy does, in historical perspective, make real progress. If the issue is left in Kantian terms, if we talk of 'the limits of reason', 'the limits or boundaries of the rational', it is likely to seem that we are dealing with boundaries or limits which are fixed and immutable: if we have reached the limits of what is rational, then that's that! Finish. Final. Nothing more we can do - literally. Who wants to be non-rational, to go outside the bounds of reason? People do 'go out of their minds', it's true - but then, who wants to be insane? And what would be achieved by it?

By changing the formulation of the problem from 'the limits of reason' to 'the limits of the sayable', the limits of language-games, Wittgenstein enables us to see - especially if we combine his 'language-game' account with Kuhn's notions of 'paradigms' and 'revolutionary change' - how we can actually make progress. We can start off with one set of 'limits of the sayable', the limits imposed by the particular Common Assumptions Paradigm of the time and the discipline in question. These limits are 'shown' (not stated or 'said') by the

fact that anomalies are encountered (generated) in relation to certain issues. After a period of struggling with increasing anomalies a 'revolution' is likely to occur, the original paradigm (CAP₁) is abandoned and new theories and language-games based on a new paradigm (CAP₂) are promulgated. If these are more successful, notably if they are such as to constitute a Public Demonstration Paradigm (PDP), then the CAP₂ language-games will be used in a new phase of 'normal discipline' research. One of the ways in which a PDP can occur is if the new CAP₂ language-games enable the anomalies to be both stated and dealt with. This is one of the most convincing of all types of demonstration of the power of CAP₂. What was previously 'the unsayable' has become sayable, and has been said. An advance has been made. The 'limits of the sayable' have been shifted. Moreover we can see, once the issue has been put in the terms just used, that it really is also a matter of the boundaries of reason and rationality having been moved: for once we can talk of things that we were unable to talk about before, we can think about them effectively, and in a real sense our 'limits of reason/rationality' have been extended. They are relativistic. They are relative, specifically, to the Common Assumptions Paradigms which underlie and direct our various language-games; and to the extent that we can 'get at' and alter these CAPs (by means of philosophical investigations), we can alter the boundaries of the sayable, we can advance the limits of rationality itself. (It is of course also possible that, if we go about things in the wrong way, we may generate an alarming amount of 'anomaly'. But the remedy for anomaly is more, and more careful, investigation. Abstention from investigation is no remedy.) So the activity of philosophy can indeed result in 'the advance of reason', as people used to think in olden times - but thanks to Wittgenstein (and Toulmin, Kuhn and the many others who have been influenced by his example and his insights), we can now understand more clearly what these and other utterances can really mean.

The concluding Chapters deal with the question of the criteria which are used, or could be used, in distinguishing between language-games - as might be expected, these are largely such that they can only be 'shown' rather than 'said'. Wittgenstein's own usages are contrasted with his statements about such notions as 'exploring new possible language-games', 'that the philosopher's job is only to

describe ... ', and so on. A paper by Strawson (1971) on "Meaning, truth and communication" is discussed, partly for the support it offers for the general position being argued in this thesis, partly to show how its own effectiveness as a communication is in effect relative to the language-games it is seen as playing, the groups it is addressed to, and the Common Assumptions Paradigms to which it can relate. Finally, the main lines of argument of the thesis are reviewed and their implications, especially for education, are briefly outlined and discussed.

And now, to set a background for further argument and to initiate detailed discussion of specific issues, Chapter 2 offers a variety of considerations relating in one way or another to education in the sciences.

CHAPTER TWO

SCIENCE EDUCATION AND THE DRIFT FROM SCIENCE

What ought to be regarded as one of the most disturbing educational phenomena of recent years is the 'Drift from Science'. All the educational might and ingenuity of the most powerful Western democracies has been lavished, since about 1960, on the development and improvement of education in the various sciences - and what has been the outcome? Recruitment to science has suffered not only a relative but also in many cases an absolute drop. The young adults who were expected to provide the scientific work-force and the spearheads of original research for the 1980s and beyond, have been voting with their feet: they have gone, and are still going, into the humanities and the social sciences rather than the 'basic' sciences.

The 'Drift' first attracted public notice in the mid-1960s. There has been a great deal of controversy over its nature and causation. The Dainton Report (1968) documented the fact of the Drift in Britain and discussed some of its possible causes and implications. Subsequently the occurrence of a broadly similar Drift has been established in many other advanced Western countries, e.g. Holland, West Germany, U.S.A., Australia and others. The detailed patterns of drift have varied from place to place, and from time to time even within a single country. Some researchers, on finding slightly different patterns of subject and vocational choice in their own districts and/or study samples, have leapt into print to contravert the findings of earlier-published studies. Usually the only real outcome of these studies has been to reveal more of the diversity of motivations and other causal factors behind the Drift - but some of the authors, perhaps anxious to bolster their local educational Establishment or the science-teaching Establishment in general, have tried to use their findings to deny the existence of the Drift or even to argue for the occurrence of a drift to science (e.g. Neave 1973). Unfortunately perhaps, even though local and/or temporary swings in favour of science have occurred, there can be no doubt of the general and continuing trend away from the 'basic' sciences (and mathematics, though we are not substantially concerned with mathematics education, except in its effects on science education, in the present context). This is attested in a number of recent publications of which only a sample can be mentioned here: see Douglas 1975; Matthews 1975; Walsh 1976, 1977; Tampion 1977; Science Teacher Jan. 1977.

One of the disquieting outcomes of the Drift is, of course, the obvious one that there is likely to be a shortage in the scientific work-force. The shortfall is likely to be not only quantitative, but qualitative as well: the average ability level and the proportion of really high-ability people, are likely to be too low. Probably even more important, the types of ability of those entering scientific careers are changing, in ways which are likely to be deleterious for basic long-term productivity in Science.

Hudson (1966, 1968, 1971 and refs. therein) has shown that science attracts mainly those of a 'converging' habit of mind - I would go beyond this, and say that as it is often (but luckily not always) presented, science actively repels those of 'diverger' type. I have argued (Stenhouse 1969, 1969d, 1971b, 1972b, 1976) that science education is biased almost exclusively towards 'normal' and against 'revolutionary' science, using Kuhn's (1962) widely-recognized dichotomy - yet it is almost exclusively 'revolutionary' science which makes the fundamental 'breakthroughs' which can subsequently be exploited by normal science and by technology; and the great names in the history of any science are almost exclusively those of 'revolutionaries'. If science is by one means or another tending to exclude the 'diverger' and 'revolutionary' types of thinker, then in the long run its collective productivity must be expected to decline.

The possible or probable effects of the Drift upon scientific research, taken collectively, and upon technological activity, are but one aspect of the problem. Of even more ominous import in the long term is the effect of the Drift upon science education itself.

One of the basic features of education as a social process is its 'multiplier effect'. This operates as a result of the fact that each teacher every year teaches perhaps several score or, at tertiary level, several hundred students. The characteristics of the teacher and of what he is teaching exert, therefore, differential effects upon all the pupils; and whatever these effects may be, they are spread to a much larger number of individuals than the single teacher from whom they started.

The personal characteristics of the teacher can have substantial effects, and so can the subject-matter that is taught. Hudson (1968)

reported that converger-type teachers tend to be more acceptable to converger rather than diverger students. Conversely, it seemed that diverger-type teachers were better received by diverger rather than converger pupils. If these tendencies were widespread, and in the absence of overriding other factors, recruitment to science would presumably come to be predominantly of converger-type individuals - indeed this may be happening at the present time. It is not established that significant discoveries and advances in science are made exclusively by divergers rather than convergers - the true picture, as Hudson himself has emphasized (and see also Stenhouse 1974 Chap.V, 1977a) is likely to be much more complex - yet it seems intrinsically likely that diverger-type characteristics are needed especially in 'revolutionary' advances.

While the extreme diverger-type teacher may tend to baffle and irritate the extreme converger-type pupil by reason of bringing in what are seen as confusing irrelevancies (to the diverger, they are interesting and suggestive avenues for possible exploration), a modicum of diverger-type characteristics would appear to be desirable in all teachers irrespective of the psychological types of their pupils. The reason here is simply this, that in teaching, the mistakes made by the pupils have to be understood by the teacher before they can be used - as they can be, and should be - as means towards enhanced understanding. For a teacher to understand students' mistakes, he/she must be able to see them not merely as being wrong. Usually there are reasons for making a mistake, just as there are for a non-mistake, i.e. for getting it right. Of course, if education and teaching are seen as nothing more than the mere transmission of particulate and discrete 'bits' of factual information, then presumably all that is needed is negative reinforcement of mistakes, and positive reinforcement of non-mistakes - but that this is an adequate account of the processes of education would seem not to be universally accepted as yet. Getting back, then, to the question of a teacher's ability to understand and hence make use of the mistakes of his/her pupils, it would appear likely, other things being equal, that the diverger would tend to be better at this than the converger. To the extent, then, that current education in the sciences is likely to encourage recruitment to the sciences, and to science teaching, of convergers rather than divergers, science education is likely to be rendering itself progressively less effective as education.

It would be very difficult indeed to establish conclusively

whether or not this is actually happening on a large scale at the present time. A contrary argument has sometimes been put forward (see, e.g. Editorial, Times Educ. Suppl. 8/3/68), to the effect that science teaching cannot be substantially at fault, because science teachers are better qualified, in general, than are teachers of most other subjects. But this is a question-begging argument. It is pertinent only on the assumption that university degrees, classes of Honours, etc., are in fact awarded (and have in the past been awarded, to present-day teachers of science) on the 'right' basis and to the 'right' people. The possibility which has been suggested, viz. that a predominantly 'converger' science teaching force is being and has been created, calls in question the assumption that those who are teaching science are in general the 'right' people. At second remove, therefore, it calls in question the criteria used for the award of qualifications on the basis of which teachers of science (and of other disciplines too) obtain their jobs.

This brings us to the issue, complementary to that of the personal qualities of teachers of science: namely, that of the subject-matter and its presentation. Are the sciences presented in such way as to attract the 'right' people into science? Is the subject-matter chosen appropriately? If some people are being repelled from science - this is deliberately putting the matter more strongly than merely saying that science, as presented, 'fails to attract ...' - which types of person are being lost, and why, and are these the types who don't matter too much?

As Donald Schon (1968) has pointed out, in the large and complex social systems which constitute most 'advanced' societies at the present time, finding out the exact state of the system with regard to some particular issue, so that a desired change can be implemented on the basis of accurate and objective knowledge, is likely to take so long that by the time the information is available it is also out-of-date, i.e. the system has changed in the meantime. Furthermore, the processes of finding out are likely of themselves to cause changes in the parts of the system being investigated - most significantly, it probably cannot be known that changes have not occurred. This is analogous to the Heisenberg Uncertainty Principle in sub-atomic physics: we cannot know both where an entity is at a given time and where it is going In education, a further complicating factor is the slowness with which results become apparent (as has been mentioned above).

In the light of all this, a strategy of empirical-level investigation of science education, to see whether it is doing what is required (and if not, why not?) would appear to be time-wasting at best, and perhaps completely maladaptive at worst. - for by the time we have determined what is wrong, it may be too late to do anything about it. It is surely better to try to lock the stable door without waiting to see the horse galloping off across the countryside This is not to suggest that empirical-type research into science education is totally pointless, or that what is being done should be discontinued. But it is desirable to keep the basic facts in mind: not only has the implementation of various 'new approaches' in science education failed to prevent the Drift, but abundant empirical-level research on science education has also been implemented in the last two decades - and the Drift, though perhaps reduced and modified in various ways, is still in evidence. (See refs given earlier, p.23; I must reiterate however, that my own argument in this thesis is not dependent upon the fact of the Drift.)

I suggest that we take as a working assumption that there really are faults in current science education. We can then immediately start to explore the possibilities as to what they may be. If we find that our initial assumption generates anomalies or obvious falsehoods, we will then be able to conclude that it was false: in other words, that current science education is satisfactory. But it will expedite investigation if we assume that the Drift from Science does have ascertainable causes, and that some of them lie within the multifarious social processes of science education itself.

One obvious but often neglected consideration may be mentioned: that the theoretical assumptions on which science education is based would need to be brought up for conscious scrutiny and critical discussion even if our main and immediate purpose were only to institute empirical-level research. The specific topics to be researched have to be decided upon, and this involves assessments of relevance. These assessments must be based on particular views - often, no doubt, only partly-consciously held - as to the nature of scientific activity itself, and the nature and purposes of education in Science. Later in this thesis various examples will be given of specific assumptions and their effects, e.g. the 'reductionist' assumption that because a biological entity such as a human body can be 'reduced' to its constituent molecules, atoms, chemical processes, etc., then the study of the chemical processes etc. is more 'basic' than the study, say, of the behaviour of the intact and living body; hence chemistry must be studied

before behaviour. This sort of view is tacitly incorporated in many science syllabuses, degree structures, and so on: chemistry is compulsory, biology and behaviour are optional. But the general point being made, for the moment, is simply that the widely-held, often unstated and to some extent unconscious assumptions about science and science education, whatever these assumptions may be, exert a strong influence upon the way we do research into science education. They determine what we think proper to investigate, and the criteria by which we judge whether or not our science (and our education) has been successful. Since the shape, techniques, subject-matter etc. even of empirical investigation are so much influenced by these unstated assumptions, it is surely desirable that the assumptions themselves should be subjected, if possible, to careful and critical examination.

The way in which we go about the examination of our unstated and perhaps unconscious assumptions about, say, education or a field of science, is very different from the way in which we go about doing empirical research. The latter, as everyone knows, involves observation, measurement, experiment, and so on - but how can we 'observe' our own (possibly quite unconscious) basic assumptions?

In fact even straightforward sensory observation of physical phenomena is not nearly so straightforward, once it is properly examined, as we often assume. And the 'observation' and examination of unconscious basic assumptions is certainly not a matter of 'just looking'. Nevertheless it can be done. The techniques we use are those of philosophy.

Mention of Philosophy (I use the initial capital to denote the academic discipline as distinct from the actual activities of philosophy) in connection with science education may bring to mind the two other academic disciplines, Philosophy of Science and Philosophy of Education. Great activity has been apparent in both fields. Philosophy of Science has been undergoing spectacular development since about 1950, while Philosophy of Education has had a burst of perhaps less spectacular progress since about 1960. (Picking specific dates is of course somewhat arbitrary - nevertheless it is often helpful to have some idea of chronology.)

Why, it may be asked, if these two disciplines, both obviously related to science education, have been making such progress - why have they not helped to prevent the Drift from Science?

Several possibilities may be explored. Perhaps neither Philosophy of Science nor Philosophy of Education has impinged upon science education, perhaps neither has had any (or enough) effect on how the sciences are actually taught. On the other hand, perhaps they have had influence but it has been either:

- (a) Positively harmful; or
- (b) Not sufficiently strong or good to prevent the Drift.

Without wishing to take the extreme position of saying that neither discipline has had any effect at all upon science education, I shall stipulate at this time that in my view neither Philosophy of Science nor Educational Philosophy has had very much effect, as yet - and to support this view I can direct the reader's attention to various features in formal education itself.

In the first place, how much education in Philosophy of Science and/or Educational Philosophy is received by those who are entrusted with the education of young people in the various sciences? The general answer must be: very little of the former, and not much more of the latter.

Some people may retort, angrily, that their science teachers have a compulsory course in Philosophy of Science and/or Educational Philosophy and/or Science Education; and that this contradicts my suggestion that the disciplines have had little effect upon science education.

Several points need to be made in reply.

One is, that I do not for one moment wish to suggest that there are no competent science educators. This is very far from being the case. There are many sound and even brilliant teachers of the several sciences, people who are indeed 'educators' in the deepest and best sense of the term.

There has been an enormous expansion in the number of science teachers, on a worldwide basis, in the last twenty years, and the formal education of a large proportion of them has been deficient in various ways. A great many of them have had no significant formal contact whatever with the philosophical disciplines in question; and in relation to the nature of these disciplines, which will be discussed in increasing detail as we proceed, even say a one-year 'subsidiary' course is likely to have had little effect. This consideration is reinforced by others, below.

Of the two philosophical disciplines in question, their relationship to science teachers may perhaps be summed-up not too unfairly by saying that:

Philosophy of Science has been very well developed - extensive and profound insights have been provided into the nature of scientific activity (see, for example, the discussions and references in Suppe 1974, Stegmüller 1976, and at an introductory level Harre 1972) - but teachers of science are given very little contact with it in their own professional training/education; whereas:

Teachers are more likely to have had some contact with Educational Philosophy - but it has not as yet attempted to come to grips with many of the problems of immediate relevance to science education, and there are grounds for arguing (as will be done in this thesis) that much of Educational Philosophy itself is not yet very well developed, either methodologically or in its range of topics.

The foregoing summary is of course nothing more than a thumbnail sketch: a forcing into two nutshells of material vastly larger than two nuts! But insofar as the compression does not involve falsification (and I do not think that it does), it can enable us to see more clearly some of the educational issues that are involved.

The class 'science teachers' may be subdivided into sub-classes according to the probable educational background of the practitioners and/ more immediately, in terms of the type of institution in which they teach, as follows:

- (a) Primary or elementary-school teachers;
- (b) Secondary-school teachers; and
- (c) Teachers in the various types of tertiary-level institutions, e.g. universities, colleges of advanced education, technological institutes, teachers colleges, and so on.

As a generalization, it might be said that, in the progression from primary to tertiary level (a --- c), the amount of training for teaching decreases, and the amount of training in science increases.

Still generalizing, but attempting to specify more clearly just what sorts of training and qualification are likely to be involved, it might be said that:

(a) The primary- or elementary-school teacher of science in the average Western country at the present time is unlikely to have a university degree in science, but probably has done at least two years (and quite likely three or even four) of teacher training and on this basis may have been granted a university degree. Whether or not a degree has been conferred is of slight significance compared to the question of the nature, extent and especially the quality of the education received; and in relation to the present argument, the question of how much has been done on science and science educating, and how well it has been done. In fact, where science education is dealt with at all it is usually in the form of a course on 'The Teaching of Science', which deals with:

(i) Various techniques of science, e.g. measuring temperatures, angles, voltages, etc., and setting up apparatus for various types of experiments; and

(ii) Techniques of teaching the techniques of science, e.g. how best to explain and demonstrate parallax errors in observation, how best to explain and train pupils to deal with 'percentage errors' in measurement, and so on and so forth.

Now here again it is not being suggested that these things should not be done, or that they should be done differently. Rather, it is suggested, and will be argued and amplified as we go along, that these should not be the only things done: that they need to be set in a much wider context, a context of methodology rather than techniques. But before we attempt to explain this further, let us complete our brief survey of the likely educational background of science educators at secondary and tertiary levels.

(b) The secondary school science teacher is likely to have a degree, but not necessarily a science degree. He/she is likely (though not certain) to have had a year's teacher training. Neither the university nor the teachers college training is likely to have included work in Philosophy of Science or Philosophy - almost certainly not, if a science degree were taken. A course entitled 'Educational Philosophy' (or something similar) may have been taken at teachers college, and is more likely if a degree or diploma in education were taken at a university - but either way, this would probably have been of limited value, for reasons already touched upon and/or to be explained later.

(c) 'Tertiary education' is a heterogeneous category. It can include the university lecturer with extensive training in science and active participation in scientific research, but with no training whatever - no official and formal training, that is - in how to teach, and probably with little or no background in Philosophy of Science or of Education, i.e. with little experience of calling in question the accepted criteria of relevance, meaning, and so on. One feature is likely to be common to virtually all tertiary-level science educators, whether they teach in university, technological institute, or teachers college: lack of acquaintance with (and thus probably of competence in) dealing with the 'criterial weightlessness' characteristic of philosophical issues.

The reason for this is perfectly simple. Very few educationalists or scientists do any philosophy as part of their formal education. Philosophical training is virtually restricted to philosophers (and, secondarily, to people who have worked in the Humanities while at university, irrespective of what they do later). Undergraduates studying with a teaching career specifically in mind are usually actively discouraged from doing any philosophy: it is not a 'teaching subject'.

It may be useful at this point to actually attempt a little philosophy, and uncover one of the presuppositions of the 'teaching subjects' policy. If prospective teachers are supposed to learn only 'teaching subjects', it appears that the teaching function is seen as that of merely transmitting information. The teacher's job is to fill himself up with the appropriate types of information while at teachers

college and/or university, and then give it out again to his pupils in subsequent years. The 'refresher course' and 'retraining' policies which are characteristic of many education systems also embody the same sort of presupposition: the teacher is just a convenient transmission channel between a 'source' of information (which is - what?) and a 'destination' namely the pupils; and teachers need periodical 'charging up' just like car batteries. (In terms of this analogy, it must be pointed out that car batteries which need recharging are either themselves almost exhausted, or else form part of an electrical system whose other components are faulty)

Thus some of the implications of particular policies with regard to teacher education (or is it 'training' rather than 'education'?) tend to depict the teacher as neither creative nor capable of autonomous synthesis even of existing disciplines. I shall not develop this line of argument further at present - but perhaps enough has been done to show the reader the 'detective story' aspect of philosophy, the probing behind what is actually said and done to reveal what is really going on. Philosophy has also been compared to psychoanalysis (see Wisdom 1953), in that in both cases the outward and 'usual' or accepted interpretations of actions, motivations, etc., are not necessarily always to be equated with the effective motivations, the 'real' significance, and so on. Philosophy can in fact be used for a kind of 'psychoanalysis' of public institutions and official policies (as has just been indicated) - and this 'psychoanalysis' analogy can help us to understand both the potential value to a society of having its unconscious presuppositions brought out for public inspection and discussion, and also the animosity on the part of an Establishment towards the individuals who perform this function of 'dragging everything out into the daylight' (dirty washing sometimes included). Look what they did to some of the Old Testament prophets, to Socrates, and in our own century to Bertrand Russell (see Russell 1975) and Sartre (see de Beauvoir 1962), to name but a few!

The only sort of philosophy to which the vast majority of teachers are likely to have been exposed at the present time, then, is Educational Philosophy. Those who have not attended a teachers college within the last couple of decades are unlikely to have had even this. If philosophy is able to do anything even approximating to what I have suggested for it, above, it might be expected that it should have

had a beneficial effect on the capacities of teachers to sort out conceptual confusions and to map and deal with the 'presuppositional geology' which underlies all educational policy and practices (including those regarding science education). This assumes, of course, that Educational Philosophy as taught in teachers colleges and university departments of education incorporates the essential features of true or 'pure' philosophy. Such an assumption may be unjustified. A second main theme of this thesis, alongside the examination of various presuppositions of science education, is the examination of current Educational Philosophy to discover, if possible, how effective it is in relation to the functions required of it; and to explore possibilities for improvement that may be available in terms of a Wittgensteinian 'language-games' approach.

It might be taken as a pointer indicating less than optimal effectiveness in Educational Philosophy as generally taught hitherto, that a considerable proportion of recently-graduated science teachers have presumably received tuition in Educational Philosophy as part of their training, without any significant number of them being inclined or being able tackle the philosophical/methodological issues inherent in the shortfall or defectiveness of science education as evidenced by the Drift from Science. (The Drift is not the only evidence, of course, but merely the most public and in a sense the most 'objective' manifestation of lack of adequacy.)

Two topics can now be outlined, to give the reader an indication of some of the arguments which will be developed in greater detail in later parts of the thesis. The two topics on which preliminary clarifications are required are:

(a) The general significance and potential value attaching to the exploration of 'hidden assumptions' - not usually explicitly stated, and often held quite unconsciously - relating to societal and individual activities, in this case particularly education and especially science education; and

(b) As a special case of (a) above, the relationships between science education, philosophy, Philosophy of Science, and Philosophy of Education. I shall argue that the study of science education (i.e. Science Education) as hitherto practised would more properly be described as the study of merely of science teaching, in that most of the deeper issues (i.e. 'hidden assumptions') regarding the nature of science,

the various possible aims and objectives of science education, and so on, have been neglected; that due to this neglect of basic issues, faults and distortions of science education have occurred which have contributed significantly to the Drift from Science; and that to achieve an adequate Science Education and improved education in science (the former being the academic study of the latter), much work is needed at the confluence of Philosophy of Science and Philosophy of Education. This is not intended to obviate the need for study of the more 'immediate' aspects of science teaching, but would provide a context and better direction for such studies.

Taking (a) first, the exploration of hidden basic assumptions:

Although science, at a superficial and 'popular' level (and even in the minds of some people who should know better) is often thought to be constituted more or less exclusively by 'facts', by 'objective reality', by what can be observed and, by suitable modifications of observation, measured, it has been shown by Toulmin (1961), Kuhn (1962) and others that even in the high 'objectivity' of science, the all-important question of the relevance of facts - which facts are to be selected, out of the open-ended and in that sense 'infinite' class of 'all the facts that there are', to be used in relation to each particular issue in science - is decided upon grounds which are not themselves in the same sense factual. They are not, we might say, external or 'objective' facts; though in another sense they are factual, in the sense that it is a fact that we do have these grounds, our selective attention to some 'objective' facts rather than others is not a matter of mere chance. The bases of selection can also in a sense be said to be 'subjective' bases, though this should not be taken as implying that they are subject without constraint to the whims of individuals. Rather they might be thought of as 'group subjective'. What Toulmin (1961) has called 'ideals of natural order' have at all times and in all places determined what people have regarded as significant in the natural universe; and even beyond that, what people have regarded as factual. "Our 'ideals of natural order' mark for us those happenings in the world around us which do require explanation, by contrasting them with the 'natural course of events' - i.e. those events which do not." (Toulmin 1961: 79). This quotation as it stands does not make the second point, which Toulmin explains brilliantly and at greater length

in his two chapters on "Ideals of Natural Order" (the very extent of his argument and examples makes him difficult to quote), to the effect that what is noticed at all, i.e. what is regarded as being a fact, is dependent upon the assumptions or 'ideals' that are held. These, as he convincingly demonstrates, change with time, and from one culture to another. Contrasting Babylonian with Ionian-Greek astronomers, for example (pp.27-38), he shows that while the Babylonians on the basis of observation and quantification were able to predict various phenomena fairly accurately (e.g. tides, astronomical phenomena) where the Greeks were not, the strength of the Greeks lay in developing a better understanding of the nature of various phenomena, so that they were less likely than the Babylonians to look for similar regularities in, say, earthquakes as in the tides. "One group of men developed the first effective forecasting-techniques, the other taught us the use of the speculative imagination" (p.30). Their different approaches were the outcome of their different basic assumptions or 'ideals'; and it was not until nearly modern times that a new synthesis of both sets of 'ideals' enabled modern science to incorporate both the Babylonian and the Greek approaches. (It might be suggested at this point that current emphasis on quantification in Science and science education may be an over-emphasis, and may involve a regression towards the Babylonian level.)

Kuhn (1962) brings out some issues even more clearly than Toulmin, especially the point about unstated basic assumptions not only giving relevance to particular facts but even in a sense bringing the facts into existence. This is not intended to suggest some miraculous metaphysical act of creation of 'things-in-themselves', but merely to emphasize that unless our basic assumptions direct our attention towards certain objective phenomena, we may be unlikely to notice them at all, and thus for us they would not effectively exist, they would not be facts. Kuhn uses the term 'paradigm' (in one of its main senses, see Masterman 1970, CAP, see Chap. 1 .) to stand for the collection of basic assumptions (stated or unstated or unconscious) about accepted techniques, accepted criteria of relevance, appropriateness of subject-matter and so on, which collectively determine the nature of a period of 'normal science'. A 'scientific revolution' in Kuhn's sense occurs when, for one reason or another, significant alterations are made in a paradigm or (to emphasize the aspect of change rather than continuity) when a new

paradigm is substituted for an old one. A change in paradigm causes a 'gestalt-switch' in how scientists see the world; and this is where 'new facts' may be brought into existence, as it were, by the new way of seeing. As an example, Kuhn discusses the different ways in which observers actually saw the behaviour of small and very light particles - bits of chaff - when in the vicinity of a large and electrically-charged object. At first, people saw the chaff 'bounce off' the large object, as one billiard-ball bounces off another; or else the chaff simply 'fell off', like Newton's mythical apple, presumably under the force of gravity. Then, after the change in paradigm to recognize the existence of electrostatic forces ('like charges repel, unlike charges attract') people actually saw the particles as being drawn to the large charged body, receiving a charge as soon as they touched it, and then being actively repelled (due to carrying the same charge).

Kuhn puts it thus:

"During the seventeenth century, when their research was guided by one or another effluvium theory, electricians repeatedly saw chaff particles rebound from, or fall off, the electrified bodies that had attracted them. At least that is what seventeenth-century observers said they saw, and we have no more reason to doubt their reports of perception than our own. Placed before the same apparatus, a modern observer would see electrostatic repulsion (rather than mechanical or gravitational rebounding), but historically, with one universally ignored exception, electrostatic repulsion was not seen as such until Hauksbee's large-scale apparatus had greatly magnified its effects. Repulsion after contact electrification was, however, only one of many new repulsive effects that Hauksbee saw. Through his researches, rather as in a gestalt switch, repulsion suddenly became the fundamental manifestation of electrification, and it was then attraction that needed to be explained." (Kuhn 1962, pp.116-7).

If we leave our picture of paradigm-change too simple, if we merely reverse the popular view of scientific progress as being nothing but the discovery of new facts and instead say that the new facts are brought into existence by an essentially subjective paradigm-shift, the problem of how and why the paradigm-change can occur becomes acute. Two solutions to this problem may be briefly mentioned here - they will be explained and elaborated later:

(i) Paradigm change, a 'revolution' in scientific thinking (or thinking in any other field of endeavour, academic, literary, artistic, even technological) often seems to be initiated by individuals who have an inherent 'negating' habit of mind. A simple 'negating' model of

scientific revolution has been presented in Stenhouse 1971; and this negating tendency has been linked with the evolutionary development of human intelligence (Stenhouse 1974, 1977). (A brief survey of evidence for negating tendencies in various eminent creative persons will be presented elsewhere.) In its simplest form, the negating model involves the rejection of one or more elements of the currently-accepted paradigm, and the substitution of something new - after which the implications of the new paradigm can be explored, and its explanatory power compared with the performance of the old paradigm: if the new proves more successful than the old, it will supplant it and give rise to a new sequence of 'normal science'. It is probably significant that the two greatest 'revolutionaries' in science in the nineteenth and twentieth centuries, Darwin and Einstein, both showed 'paradoxical dullness' at school, due presumably to their negating tendencies (Hudson 1964; Stenhouse, 1971, 1974).

(ii) Besides the 'negating' tendencies of 'revolutionarily' creative individuals, scientific revolutions can also be actuated by the inadequacies and anomalies revealed during a period of normal science. Some of these may point towards the alteration and/or rejection of some particular assumption incorporated hitherto within the paradigm; and again, tentative/provisional modification in the basic assumption may, on exploration, lead to more satisfactory results and hence to a permanent change in the paradigm. (Tentative modifications do not, of course, always result in improvement; and if not, they will probably be abandoned. A kind of evolutionary process, 'survival of the fittest', tends to operate, as Toulmin and others have suggested.)

The 'normal science' vs 'revolutionary science' distinction is important for a proper understanding of Science and science education. The same or a closely analagous distinction can be made in relation to most fields of human endeavour, e.g. 'revolutionary' writers like James Joyce, Virginia Woolf and others can be contrasted with highly competent but non-revolutionary craftsmen like C.P.Snow and Aldous Huxley (the latter helps to make the point, I feel, in that while his thinking was in many ways revolutionary, his literary techniques generally were not). The revolutionary/normal distinction is illuminating with regard to most fields of education, and applies also to Education as an academic discipline - clearly, Dewey and Rousseau (among others) were revolutionary

educationalists. We shall return often in the rest of the thesis to this very fundamental distinction; but for the moment it is time to turn our attention back to present-day science education and attempt a brief preliminary survey of what seem to be some of its major limitations.

Four major faults can be listed - but even from preliminary discussion it will be apparent that they are interrelated, that they are similar in kind, and that they might equally well have been designated under three headings, or two, or five or six. Taking four as being distinguishable, however, and as affording useful similarities and contrasts, these are:

- (1) Technicism
- (2) Factualism
- (3) 'Numeralism'
- (4) Reductionism.

In a real sense, they are all varieties of (4), reductionism. Considered separately, however, they enable us to highlight particular objective features of science education as practised today, features which readers can check up on for themselves, in textbooks, science syllabi, etc..

The importance of quantification, of the use of quantitative and often statistical techniques, and of 'numeracy' as well as literacy in recruits or would-be recruits to science, has been stressed so often as to need no repetition here. But while an understanding of probability and sampling theory and various mathematical techniques is undoubtedly important (more so in some types of science than others), there are several reasons why I choose in this context to use the possibly pejorative term, 'numeralism', rather than 'quantification'. In the first place, 'quantification' and similar widely-used terms have become tendentious: they beg the question of the value of numerical techniques. Everyone tends to assume that quantification is necessarily good; whereas - and this is my second point - questions as to which phenomena should be quantified, in terms of which categories should they be quantified, which are the appropriate units to use for quantification, and so on, tend to be largely neglected. Thirdly, I said, above, that an understanding of quantitative techniques is important, and so it is - but what is inculcated in a great deal of science education is, all too often, alas! not understanding, but rather a sort of recipe-book acquaintance with a number of mathematical techniques which tend often to be used on an ad hoc basis without any proper understanding either

of the mathematical theory and assumptions on which the techniques are based, or of the subject-matter to which they are applied. The results of such quantified research often get published, simply because they are quantified and elaborate operations have been performed on them (usually nowadays by computer, which guarantees their 'scientific' status!). What is often forgotten, or never understood in the first place, is that tests of significance, for example, depend for their relevance not upon the various calculations that have been performed, but basically upon the assumptions that have been made about the subject-matter in question. For instance, determining the number of 'degrees of freedom' in an experiment has nothing to do with mathematics: it depends upon the categorizations regarded as significant within the range of phenomena being investigated. In working out social dominance hierarchies for example, it has been assumed by various researchers: that the appropriate technique is to note the numbers of 'interactions' between each of the possible pairs of individuals of the group; that the outcomes of such interactions can appropriately be scored as a 'win' for one and a 'loss' for the other; and that the most dominant individual is the one with the most wins and fewest losses. Calculations of 'significance' based on these assumptions were found to give varying and anomalous results, not because the researchers did their mathematics wrongly or saw the 'wins' and 'losses' wrongly (though in one sense they did), but because their assumptions about behaviour were simplistic. They allowed too few 'degrees of freedom'. They neglected important categories of behaviour. In many species - including, in some circumstances, our own - the most dominant individual(s) has/have very few 'interactions' indeed. The dominants just sit or stroll around, doing as they please, neither interfering with anyone else nor being interfered with. But of course the apparent absence of 'interactions' is only apparent: there are no or few interactions only if 'interaction' is taken as implying overt aggressive behaviour (and corresponding submissive/placatory behaviour). In a stable hierarchy, the true 'alpha' (top) individual may indulge in little or no 'interaction' in this sense, simply because the rank of each individual is known by all members of the group, and the subordinates give precedence to the dominants (or at least to those at the very top of the hierarchy) without being 'forced'. (See Russell and Russell 1961, Chance and Jolly 1970, Stenhouse 1974, for further discussion and refs. See also Andreski 1974, esp. Chap 10, for further discussion of quantification and its misuse). Thus research can be conceived in terms of the wrong or an inadequate number of categories

(thus allowing too few degrees of freedom) - and all the sophisticated juggling in the world won't get appropriate categories into the data if they were not there in the first place. (See Hudson 1971 for further discussion).

What I am calling 'numeralism', then, is an emphasis on quantification without sufficient attention being given to what the quantification is for, or of. It can be represented as a regression to a Babylonian type of approach.

The fault of 'technicism' springs, like so much else, from the most laudable of motives. So many fascinating and ingenious new techniques are being invented each year that teachers and syllabus-compilers feel they just have to bring them to the notice of students. 'Bringing to notice' would be fine - what causes trouble is that it is too often felt necessary to require students to become proficient at all the latest techniques; and as the years pass, more and more techniques have to be mastered, with ever-increasing demands on time and energy. One of the results of this is that discussion of what the technique is really for tends to be reduced, finally to vanishing-point. Like 'numeralism', 'technicism' represents the elevation of something which is clearly necessary and desirable when studied and/or learned within the appropriate framework of methodological discussion in which the whys and wherefores, the how and how much can all be dealt with, into a form of dogma in which it is assumed that particular techniques (or technicalities in general) have merit in themselves without reference to purposes or functions beyond the level of mere technique. This sort of development within science and science education is akin to the development of magic and ritual, in which the mere performance of certain actions comes gradually to be regarded as having significance for its own sake, without regard to causal outcomes. The ritual sacrifice of an animal to ensure that rain falls and crops grow comes to form, in various primitive cultures, part of an elaborate and internally-consistent system of beliefs which is to a great extent self-reinforcing (e.g. if rain does not fall it is a sign that the gods are displeased, therefore another sacrifice is required, etc., etc.). The fact that the sacrifice has no causal connection with the weather cannot be accommodated, hence cannot be understood or tested, within this ideology. That is to say, the methodological understanding is primitive, even when the techniques are perfectly good: the rooster is killed on the right day and, sure

enough (in most years), within a week or so the rains start and the crops grow. Nothing is wrong with the observations being made here (and they could be backed by statistical correlations, if our primitive tribesmen had learned quantification) - but it is in the overall theoretical/methodological understanding that the deficiency lies (cf. the Babylonians). The ultimate in perfect statistical and observational correlation but mistaken methodology is the rooster who thinks that his crowing makes the sun come up!

A few further remarks on technicism:

Teaching people the latest techniques simply because they are the latest, i.e. in the absence of adequate discussion of methodology, is apt to generate an uncritical neophilia: the belief that everything new is necessarily better than everything old.

Absence of methodological understanding means that there is no basis on which to decide which techniques to continue teaching (even after they are no longer new) and which to discard.

Finally, to teach even the very latest techniques is still to impose a backward-looking orientation upon science education. Even the most recent past is still the past; and all but a tiny minority of technical innovations have an active life of a few years at most. To educate for the future it is necessary to inculcate an understanding of basic methodological principles illustrated by reference to particular techniques and particular factual material, but keeping the facts and the techniques logically subordinated to the principles. General principles have a much longer life - measured, often, in centuries - compared to particular facts and techniques.

This brings us to the question of 'factualism'.

A common burden of complaint is the fragmentation of academic disciplines. Each subject is breaking up into a multiplicity of sub-subjects, especially in the sciences. This fragmentation and compartmentalisation is alleged, probably correctly, to have a deleterious effect on student response. Nobody seems to have investigated the "Why?" of this. It seems generally to be assumed that the accumulation of factual information within each subject makes inevitable the expansion and finally the subdivision of teaching courses in the subject. The expansion of teaching courses need be proportional to the 'knowledge

explosion' only if their purpose is the mere inculcation of factual information. Everyone pays lip service, of course, to the notion that teaching leads to 'understanding principles' rather than 'knowing facts' - but in practice the teaching of science, despite all the recent advances, the 'discovery method', the 'activity' approach, and so on, remains largely a matter of imparting factual information. Or rather, information is imparted as though it were factual, when much of it really is not. Perhaps this point needs further discussion.

Every 'beginning' course in Philosophy deals with the problems of detecting the covert prescription which masquerades as a description. 'Latin is an essential part of the education of a gentleman' looks like a descriptive statement, because of the verb 'is'. We can see very easily that it is not descriptive but prescriptive, largely because we happen not to agree with the prescription. 'Science is an essential part of twentieth-century education', on the other hand, is more likely to be taken as descriptive, just because most of us tend to agree with the view expressed. In both cases the 'is an essential' really needs to be translated into 'should be regarded as essential', so as to bring out the fact that a recommendation is being made. The logic of the two utterances is made clear if they are cast in the form 'All gentlemen should learn Latin' and 'Everyone nowadays should learn science'.

Why then, it should be asked, are these statements not in the prescriptive form? Why do they masquerade as descriptive statements?

One significant reason has already been indicated, surreptitiously. If a prescription is put forward openly, people may disagree with it. There is a healthy streak of 'contrariness' in most of us. If it is put forward as though it were a descriptive statement of fact, however, most of the 'contra' reaction is inhibited. Nobody wants to look as though he is arguing against the facts! So in this democratic age, when decision-making depends on majority vote or consensus, and when time may appear to be wasted if everyone has to be argued round to the support of a particular policy, it is frequently expedient to use the deception of presenting as a fact what is really only a proposal.

Considerations relevant to democratic decision-making are relevant also in the teaching situation, especially when difficulties are exacerbated by very large classes, inadequate equipment and preparation-time, and so on. It saves trouble if theories and hypotheses are presented as facts. Nobody is then tempted to argue against them. The time and temper of the teacher are saved. The fact that under this sort of regime, education has degenerated into mis-education is either not noticed or else is 'explained away' by some facile rationalisation.

Perhaps the most common implicit rationalisation, especially with regard to the teaching of science, is to put up the notion that 'proper standards', 'intellectual rigour', can be presented only by close adherence to facts. If it were true, it would be a rigor mortis; but it is not true; and the presentation of theory, assumption or hypothesis as though it were fact constitutes a debauch of intellectual standards. No wonder so many students rebel against science teaching in this guise! The sheer 'learning up' of vast quantities of factual information is dreary enough in itself. On top of this, the brighter students undoubtedly gain some inkling of the fraudulence and sheer dogmatism of the whole business, and they cannot help but be disturbed and demoralized. Fortunately, there is still a great deal of excellent teaching, in science as in every other field - but the unhappy thought is that the proportion of good seems to be declining.

Continuing on our line of theoretical exploration, we may now ask why science should have acquired this unfortunate connection with factualism. Note that 'factualism' as used here is intended to denote, not the legitimate and absolutely necessary dependence of theory and hypothesis upon fact but the pernicious reductionist assumption that science is really nothing but fact. From recognizing that science must have a factual basis it is easy enough, apparently, to slide into the assumption that over and above the facts there is nothing but mere speculation. This dichotomy, between reputable and scientific 'fact' on the one hand, and the inchoate and diaphanous 'mere speculation' on the other - heavy emphasis often being placed upon 'mere', with hints of incompetence if not actual dishonesty - can be encountered in a good many writings and public utterances. Like many such dichotomies, this one is oversimplified and misleading. At any one time, an advancing science must contain 'theories' from every part of a spectrum. Some are so well established that they themselves are often taken as fact and are not even called 'theory'. Others are well supported and accepted, while

other theories are still regarded as interesting and worthwhile but unproven. From these we move to theories which are regarded as implausible and doubtfully 'scientific', then to the extreme of wild fanciful speculation and 'old wives' tales'. The history of science demonstrates that there is considerable hazard in making predictions as to which theories are and which are not going to stand the test of time. Very often it is just the theory which seems most solidly established that gets tipped over and thrown out. Sometimes the old wives' tales prove more substantial than the accredited theories.

The major virtue of the outmoded classical education was not "that gentlemen could read and write in Latin", but that through the study of writings in Latin and Greek they (perhaps quite unconsciously) achieved an often intimate contact with the thought of a past civilisation. From Plutarch, Thucydides, and Plato they absorbed the notion that the world is both complex and subtle and that even the most convincing of explanatory theories will not live for ever. Above all, from knowing the contrast between what the ancient world took and what the modern world takes as fact, they implicitly understood and accepted the non-factual, provisional and relativistic nature of all new explanations. This allowed them to give emphasis to what was new and distinctive within the new body of inquiry, science, which was growing up from the seventeenth century on, within the marsupium of philosophy: the search for and dependence on empirical evidence, 'facts'.

Many of the senior generation of scientists still active today, though not always themselves direct recipients of the 'traditional classical' education, still absorbed many of its presuppositions, its sophistications, its broadly philosophical outlook. Thus within their frame of reference there was no harm but only benefit in putting heavy emphasis upon fact-gathering and technical matters - they 'read-in' all the safeguarding provisos that were needed. They knew very well that scientific theory is not only determined by facts, it also determines them, the basic theoretical outlook determines what is to be regarded as a fact. This is the side of the story which has been neglected subsequently. That one's basic theoretical outlook constitutes what is to count as fact has achieved explicit recognition in recent literature (Toulmin 1961; Kuhn 1962) - but it appears that many scientists and most of the general public have not yet seen the point. Perhaps because only a

relatively small proportion of people have learned explicitly to recognize the central importance of strict inferential reasoning and have themselves become proficient in doing it, an over-simplified parody of scientific method, a dogma of 'Facts Only', is blazoned as the paradigm of intellectual activity. Newton's "Hypotheses non fingo" is wrenched from its context on the one hand, a fog of uncritical Logical Positivism (see Ayer 1959, Passmore 1966, Ch. 16) drifts in from the other, and with what are mistakenly assumed to be the sacred names of Science and Philosophy reinforcing each other, back to back, in a tight little circle - or should we say, an every-decreasing vortex? - a rationale is provided to legitimatise the factual sleight-of-hand first perpetrated on the grounds of educational expediency. Presumably this fallacious rationale also serves to disguise the personal inadequacies of those whose highest intellectual achievement is swotting up facts; indeed it might well be said to glorify these inadequacies.

Coming finally to reductionism, it was pointed out earlier (above) that reductionist assumptions are built into many curricula, degree structures, etc., at the present time. In many science degrees, chemistry and/or physics and/or maths are compulsory, whereas biological, behavioural, ecological, populational and evolutionary courses are not. There can be various reasons for this state of affairs, but in many cases it seems likely to be due to the assumption that chemistry and physics, etc., are more 'basic' than other subjects (or are 'basic' absolutely).

'Basic' is a nice vague word. If we offer two translations or interpretations of it for careful examination, I think we shall be able to get the key issues sorted out.

'More basic' can, I suggested, be interpreted as either:

- (a) 'more real'; or
- (b) 'more important'.

If we take a complex entity, say a lump of rock or a living organism, it can, by the use of appropriate physical and chemical techniques, be analysed into its various constituents. That is, the subject-matter of geology and anatomy, say, can be transformed into the subject-matter of chemistry and physics. But though we can know that the rock or animal or plant is made up of various chemicals, and ultimately of atoms and molecules, does this mean that the atoms are 'more real' or 'more important' than the gross rock or the plant or animal?

Let us see what we can say, and what we cannot say, about the issues involved.

Firstly, it is true that the atoms which now compose the rock, animal or plant were in existence long before this rock, this animal or this plant came into existence, and they will remain in existence long after the macroscopic objects have disintegrated. But does this make them more real at the present time? Surely not: there is surely no difference in the reality of any of the objects in question, whether macroscopic or submicroscopic. The rabbit, say, is no more real than the atoms of which it is composed - and it is no less real, either.

But if the long-lastingness of the atoms does not mean that they are more real than the various larger objects, living or non-living, of which they may from time to time form part, surely it must make them more important in some way?

Perhaps the key question is ask here is: more important to whom?

If the answer is, more important to human beings, what can this mean? If we have been trained in science, we can know that a particular carbon atom that is part of an animal's body today might have been part of a plant yesterday, and might have been floating around in the air the day before as part of a CO₂ molecule; and tomorrow it may be in a CO₂ molecule again. It is important to us that the 'carbon cycle', as it is called, should keep going - but why? Just because we feel we want to continue to exist; and to do so we need to eat animals and plants, breath, etc., etc..... So in one sense it is not the atoms and molecules that are important, but the 'things' they constitute. After all, our ancestors ate food, and knew that had to do so to live, long before anyone knew about atoms and molecules. Perhaps the latter have a sort of 'novelty value' just at present. In terms of function, of contribution to our own existence and wellbeing, the cabbages which we eat, and the atoms which make up the cabbages, have an importance which is exactly the same: for they are in fact identical. The only difference is, we can see cabbages, whereas we cannot (normally) see atoms. To say that a cabbage or any other physical object is made up of electrons, protons and so on is simply to offer a redescription of the cabbage - whose properties, reality, and importance are quite unaltered by our descriptions, whatever they may be.

A good analogy is with bricks and buildings. Buildings are made up of bricks (and other components - we can take 'bricks' as symbolic of

all of them), just as all physical objects are made up of atoms etc.. The bricks must exist before the building is put up, just as the atoms exist before (and after) they are incorporated in a rock, plant or animal. Thus they have a sort of 'temporal priority', though they and the building made of them are equally real once the building has been made. As to importance, the bricks and other components could be said to have a lesser rather than a greater importance than the building they constitute, because they would not have been manufactured in the first place except to build houses out of them. Also, if we had no bricks, we could build houses out of tree-trunks, or asbestos sheets, or earth, or stones, or As to the usefulness of knowledge, it is certainly desirable for architects, engineers and builders to know the properties of bricks - but the study of bricks, no matter how intensive or prolonged, can never reveal anything about the difference between a good and a bad house-plan. And it is not through study of bricks themselves, but of relationships between bricks, that the principle of the arch could be discovered.

There is nothing wrong with studying 'component' phenomena - nobody is going to suggest that physics and chemistry should be abandoned - but the useful new knowledge that comes from studying smaller-scale and 'component' phenomena is no more valuable, the phenomena themselves are no more real, than the larger-scale phenomena of which the smaller are the components. We harm our understanding and our education, not by studying the smaller-scale phenomena, but by assuming, illicitly, that they are more real and/or more important.

The fallacy of reductionism is well summed-up in the remark of a friend: "Peaches are nothing but glorified irrigation water". In this statement, the "nothing but" is contradicted by the "glorified": there just are differences between irrigation water and peaches!

W.H. Thorpe, in the Conclusion of the important book Beyond Reductionism (Koestler and Smythies (eds) 1969) writes of the unscientific character of the reductionist views of many scientists:

" ... if reductionism were right in the sense that the mental, spiritual, artistic, and ethical values which we experience to say nothing of the mere physical characteristics of macroscopic physical objects like rocks, animals and plants! really are in the electrons and other primary components of which the world is made - then all one can say is that they don't appear to be there. It follows that a great and unjustified leap of faith is required to adopt the reductionist position, a leap without any scientific evidence, to believe it."

The faults briefly indicated above as having crept into science education, have in general, it may here be suggested, had the effect of pushing recruits to science towards the 'normal' and away from the 'revolutionary' dimension. Even within normal science, the incorporation of fallacies in science education must tend to reduce the effectiveness of science graduates as scientists (and even more as educators, for those who go into teaching). That the effectiveness of scientists as scientists is declining is supported by Sir Kenneth Mellanby (1973) among others. and while various other factors could and presumably do have contributing effects, it seems likely that deficiencies in science education are a major cause of declining productivity within science. Perhaps what is needed is some 'revolutionary' thinking within Education ...

Revolutionary changes in Science are relatively well documented (see Kuhn 1962, also Koestler's The Sleepwalkers 1959 for a more extensive reconstruction of the Copernicus-Galileo-Kepler revolution) - but similar changes occur, as already mentioned, on other aspects of societal activity and institutions. A.N. Whitehead in Adventures of Ideas (1933) has shown how public ideas with regard to slavery, to take but one example, have changed through historical time and from one society to another. Rates of change vary from imperceptible to extremely rapid; and change with regard to one aspect of society can have wide repercussions on others, e.g. one outcome of changing attitudes to slavery was the American Civil War with all its ramifications of effect in American and world history.

Change is more rapid today than at any previous time in human history, as many people keep telling us. The future comes upon us so rapidly that we suffer from 'future shock', according to Toffler (1970). This seems largely true. There is an urgent general need for the

exposure and critical discussion of any and every basic assumption relating to our social institutions - if we want them to adapt to the present and future. This is demanded by a combination of two features of present-day life:

The rapidity and extent of change, in conjunction with:

The fact that many of our most powerful basic assumptions derive from the past.

The dangers and problems inherent in this conjunction may be explained as follows:

All our unconscious and therefore most basic assumptions are inherited from the past. We have grown up with them: that is why they are unconscious, or why, if they are in fact brought to our conscious attention, we assume that they are 'natural' ones. As Gordon (1964) notes: "One is reminded of the wryly perceptive comment that the fish never discovers water". But the very fact that we have grown up with them, absorbed them from the very climate of our own first breaths of education, indicates that they must derive from earlier times. This means that they may not be appropriate for the present; and makes it likely, indeed almost certain, that at least some of them will be definitely inappropriate for the future. It is desirable, then, that they should be modified progressively, to keep them in tune with the realities of the time. But how can we modify them if they are unconscious assumptions, if we don't even know what they are?

The first step, obviously, is to attempt to bring our unconscious assumptions up into consciousness, into explicit statements that can then be examined and discussed. But this first step, from unconscious to conscious and explicit, is the most difficult - and also, of course, the most important. The exhumation of often deeply buried unconscious

assumptions is a complicated business. It will be discussed at greater length in later Chapters. For the present, let us just say that, while psychoanalysis can perform the function of bringing the unconscious into awareness for the individual, one of the disciplines which has traditionally been concerned with doing the same sort of thing at societal and individual level is Philosophy. It is no accident that Adventures of Ideas, cited above, was written by one of the greatest philosophers of the twentieth century. The function of enhancing individual and collective awareness is not restricted to philosophy: the poet and novelist, the graphic artist and even the musician among others, may be able to lead us to think of things in a new way. Shelley and Blake, for example, saw many of the dangers of urbanization and industrialization which people have only started to appreciate in recent years. But while imaginative writings and other forms of art can make us aware of new things, or aware of things in new ways it is a further task still to work out and state in clear words exactly what the new vision really is - and this to a great extent is the task of the philosopher.

In saying this I am not saying that the only people who undertake this function of making clear and explicit a new way of looking at things are those who are in fact employed as professional philosophers. In some ways it does not matter what people actually do for a living, or what their educational background has been: the only real issue is, can they perform their 'mission in life', can they get their message across effectively? Individuals of powerful drive and genius have been able to exert influence almost irrespective of their place in life, their education or lack of education, etc.. John Bunyan and Michael Faraday had little formal education, yet they managed to become major figures in literature and physics respectively. For most of us, however, the education we receive and the vocational circumstances we encounter mean

a great deal. Some types of activity may be inhibited, others facilitated. With regard to the business of clearly expressing new ways of looking at the world or some segment of it, an education which facilitates our coming to see things in new ways is obviously the first requirement; and which helps us to think and express ourselves effectively is the second. Seeing things in a variety of different ways is presumably assisted by studying different major disciplines (especially perhaps if they touch upon common subject-matter). Both 'different ways of seeing/thinking' and effectiveness of expressing and communicating what is seen, should be assisted by training in philosophy - if philosophy is what its leading practitioners claim it to be.

A few preliminary stipulations can be made at this point. One is, that the amount of provision within formal education, for philosophy whether as Educational Philosophy or under some other rubric, needs to be fairly substantial and probably far in excess of what is made available under most present regimes. This is demanded by the nature of philosophy as a very complex and sophisticated intellectual activity which needs time to be learned and practised.

A second stipulation, which also cuts across current practice, is that if worthwhile philosophy is to be taught, it should be taught substantially to all students, not just on an optional basis to those who choose it. Here again we come to one of the unresolved questions within tertiary education itself and with regard to the teaching of Educational Philosophy: is Educational Philosophy only for those who wish to become professional educational philosophers or teachers of Educational Philosophy (the two are not necessarily the same)? Or is it for everyone? And can both possible aims be covered by the same type of teaching? The issues relevant to these questions seem to have been little discussed: and practice in many institutions seems to be an

uneasy compromise, which may achieve none of the aims to a satisfactory extent.

What philosophy is, is itself a philosophical question. I am not presuming to offer yet another definitive answer to this question; but intend merely to take some of the answers which have been offered, and which are widely accepted at the present time, and explore some of their implications. As an outcome of this, we may find that some modifications of current views suggest themselves.

With regard to education in science - or, for that matter, with regard to formal education in any subject whatever - even though teachers in schools would have studied 'pure' philosophy in depth in only a very few cases, there is one branch of philosophy which most of them are supposed to have studied, certainly in the last two decades, namely Educational Philosophy. If Educational Philosophy is what some of its more prominent exponents claim it to be, surely it should have provided the sorts of competencies which are needed in science education? Surely in the two decades the amount of conceptual confusion with regard to science education, and other branches of education, should have been to a perceptible degree reduced? Even if all conflicts and confusions have not been totally eliminated, surely their nature and boundaries should by this time have been clarified?

As a generalization which admits a few significant exceptions, it can be said that, in science education at least, the philosophical and methodological substratum of basic assumptions - assumptions as to the nature of science itself, and of education in science - has scarcely yet been touched. There have been little more than a few tentative scratchings of the surface; and such as there have been, have been either misunderstood or ignored by the majority of people working in this field. Attempts to come to grips with the basic methodological/philosophical issues have had virtually no impact at all in terms of the general approaches used in science education. (Possibly this would be true in other fields as well - but science education has been subjected to the public 'vote of no confidence' in the form of the Drift from Science, (hence our focussing upon it.)

If we take as the premises of an argument, that a large number of science educators (teachers) have had some training in Educational

Philosophy, that Educational Philosophy is an "activity of elucidation and clarification" especially at the conceptual level and with particular regard to educational matters, and that science education abounds in uneluciated and unclarified (and a fortiori, unresolved) conceptual confusions, we might surely be justified in concluding that either:

(a) science educators have not learned their Educational Philosophy very well; or

(b) there is something very wrong with the Educational Philosophy they have been taught.

There are grounds, which will be put forward and discussed in the rest of this thesis, for believing that both horns of this last dilemma are in fact true and applicable.

CHAPTER THREE

EDUCATIONAL PHILOSOPHY: HISTORICAL AND ANALYTIC SKETCH

What exactly is Educational Philosophy? And how can it possibly be related to science education? To most people there would appear to be little or no connection between the two. Since I am writing for a readership most of which is likely, initially at least, to be in two parts - those who are concerned with and know something about Science, and those who are concerned with and know something about Educational Philosophy - and since few of the scientists are likely to know much about educational philosophy, and vice versa, I must give some preliminary explanations of the two fields and of possible connections between them.

Science education and some of its methodological problems have been discussed in Chapter 2. To say something now about Educational Philosophy:

Some writers on the subject (e.g. Broudy 1961) start their books by giving verbal definitions, first of 'philosophy', then of 'education', and then try to combine the two into a sort of composite intended to say what 'educational philosophy' or 'philosophy of education' (in many contexts the two phrases can be taken as roughly equivalent) is all about. For a variety of reasons such a procedure is apt to cause more confusion than clarification - and since any endeavour, on my part, to explain at this point why this should be so, would probably have a similar effect, I shall refrain from further mention of this definitional strategy. Instead, it may be more illuminating to work (in a way that could be described, broadly, as 'inductive') from matters which people do know about, namely, the beliefs they hold about education.

While most people might respond to the question "What is your philosophy of education?" with an embarrassed mumble and a blank stare,

it can truthfully be said that nobody is (or can be) lacking a philosophy of education. We cannot not have one. This makes sense if we interpret 'philosophy of education' as 'beliefs about education'; since even to say "Education? I'm agin it!" is to express a belief about it. But while this sort of interpretation is both possible and intelligible, perhaps one's philosophy of education ought to consist of rather more than just having beliefs about education. To talk of one's beliefs as 'a philosophy ...' suggests that they are more than just raw undigested beliefs. But in what way can they be 'more than'?

If we investigate the beliefs people actually hold about education (or anything else), we generally find that they do not form a coherent and consistent system. Often one belief contradicts another; and some beliefs do not square with the facts. "Schooldays are the happiest days of your life," say some people, usually a long way from their own schooldays; and simultaneously "Spare the rod and spoil the child." These two propositions, if not in flat contradiction, at least seem to need a fair bit of reconciling. And "The teacher must supply the motivation for the child to learn anything" seems contrary to fact as well as being undesirable, psychologically and socially; while "Self-determination is the goal of every choice" is not only at variance with facts, it is logically so confused as to be, on virtually any interpretation, nonsense.

So there are many grounds on which it is desirable that beliefs about education should be systematized, made consistent with each other and with 'reality'. From a practical point of view, if we are going to embark on some policy of action, it is better to find and resolve conflicts beforehand, rather than to wait until we are bogged down in real-life muddle and confusion. (Recent educational history is full of examples of the harm ensuing from insufficient thinking-out of policy. The institution and abandonment of 'streaming', 'continuous assessment', the 'free-choice curriculum' - innumerable examples could be cited, at all levels of education and from every country in the world, to illustrate the dangers of unrealistic and uncritical policy-making.)

Systematizing our beliefs about education is firstly a matter of finding out what they are - stating and enumerating them - then detecting conflicts and anomalies among them; then resolving the anomalies. This makes it all sound easy and straightforward. In practice,

while stating and systematizing some of our beliefs is not too difficult, there are others which are difficult to state or even to know about - because they are held unconsciously. Some of the contradictions are difficult to sort out too, even when we know what they are.

Always we want to eat our cake, yet still have it ... The whole business, of revealing our beliefs or unstated assumptions and trying to get them into accord with each other and with the rough world of reality, forms much of the subject-matter of this thesis, and there would be little point in trying to deal with its major complexities here. But perhaps enough has been said to indicate that in the conversion of a mere jumble of beliefs into a system of 'philosophy' of education, the sorting out of logical contradictions and the testing of statements of belief (and/or fact) for mutual compatibility is a major activity. It could perhaps be said, as a tentative and provisional statement about one of the functions of doing philosophy, that it helps the individual to find out what his beliefs are and why he holds them.

But of course philosophy is not only a private it is also a social affair. The doing of it can be social, and it can also have a social 'product': the actual beliefs, attitudes, etc., of a particular society or section of society. Thus it makes sense to talk about the British philosophy of education, for example, as being different from the American or the Australian. We have to be careful in making statements about national philosophies of education. It is easy to produce facile generalizations which may be neither true nor illuminating. But because people sometimes make statements that are difficult to justify ("British education is tradition-oriented, while American education emphasizes individualism and creativity"), it does not follow that justifiable statements can never be made; nor that the very idea of a national philosophy of education is a chimaera. There is a very real sense in which it can be said that a society's philosophy of education is more easily seen in what is done than in what is stated. In other words, the collective philosophy of education is implicit behind the statements and, more importantly, the practices. Much of it is likely to be unconscious - but the attitudes, assumptions, and values which are the society's philosophy of education will largely determine what is actually done by and in the society.

Education in science is one of the aspects of education which is affected, and likely for varied reasons to be affected especially

quickly and directly, by the prevailing philosophy of education. Nobody who witnessed the enormous expansion of science education in the USA, Britain, and other Western countries after Sputnik I had been launched by the Russians in 1957, could doubt the effect of beliefs and attitudes. Overnight, and in a mood of concern bordering on public hysteria, huge sums of money were voted for the upgrading of science education. Great teaching projects, research-on-teaching projects, creativity-teaching projects, science-creativity-teaching projects, ad infinitum, blossomed and flourished throughout the land. It was believed that scientific and technological leadership had to be retrieved, and then permanently retained, by the USA and her associates ... That many of these beliefs were simplistic or downright erroneous mattered nothing: the money was voted, and the boom in science education was on. All systems go!

The fact that in many ways it seems to have misfired may be another example of the results of inadequate thinking-out. But the full significance of the 'drift from science' and other phenomena can be discussed later - the only point to be established at the moment is that there is a connection between a society's philosophy of education and the provision made for education in science.

While the educational philosophy of the community in general decides the overall scope of science education, its details are at the mercy of the educational philosophy held by the educational and scientific Establishments. On their Philosophy of Science Education depends what is actually done in science education. And it is here, as I may point out at once, that serious deficiencies are evident. They have been evident for years to those in the game who have had the historical/methodological background to perceive them; and they are beginning to be evident to the educated public, in that various symptoms are becoming obvious, of the diminishing returns of the investment in science education.

One observable which is both symptom and cause of the inadequacy in science education is the almost total absence of substantial effort in sorting out the philosophy of science education. The majority of universities have had departments of history and/or philosophy of science for fifteen to thirty years. Philosophy of Education has been taught

among the Education courses for decades; and in the last two decades it has begun to attain some reasonable intellectual standing. But the overlap of the two fields has not been recognized: systematic work on the philosophy of science education has scarcely even begun. A great deal of experiment and research at empirical levels is done on what is called 'science education' but is really only science teaching. There is no doubt that teaching techniques do need to be studied, new experiments made and new approaches devised - but until the basic questions of 'What is it all for?', 'What changes in outlook, skill, etc. are we trying to achieve?' have at least been properly discussed, much of the effort is wasted and may indeed be harmful. The basic questions have gone by default so far, and, by and large, science education is producing neither educated persons nor scientists: it is producing technicians who have been trained (more or less well) rather than educated. (This sweeping generalization, which is neither intended nor thought to apply universally, will be further explained, discussed and justified in later Chapters.)

There is, therefore, a need for the development of Philosophy of Science Education as a major field of intellectual activity. (The most urgent need is in fact in regard to biological education. Philosophy of Science as it stands at present is almost exclusively concerned with the physical rather than the biological sciences - yet the main problems facing humanity at present, which education is supposed to be a major factor in helping to solve, are biological. Controlling population, dealing with the behavioural problems of crowded living, conserving the environment, minimizing pollution - and integrating the policies necessary to achieve all these ends simultaneously and on a long-term basis - the heart of all this lies in the biological sciences. For a more extensive discussion of approaches to and philosophy of biological education, see Stenhouse et al. "Concepts of Biological Education," in preparation.) It is hoped that this present thesis will contribute something towards a philosophy of science education: some idea of what ought to be done, and why, and of the criteria that are relevant to deciding these questions.

That will come later. In the meantime, it is necessary to get back to Educational Philosophy as such and the historical context in which recent advances in this field have taken place.

Dewey was perhaps the last in the old tradition of Philosophy of Education (which we may designate as Phase I), in which people who were primarily philosophers also gave accounts of the educational concomitants of their philosophical positions. Mostly, the education was incidental to, even peripheral to, the philosophy; this was not so much the case with Dewey himself. During Dewey's lifetime the changes which had been set going by the advent of mass education (which can reasonably be dated in the United Kingdom from Forster's Act in 1870) were operating with ramifying effects within the processes and structures of education. These changes in education produced changes in what can be identified as 'Philosophy of Education' as an academic subject: changes in the type of person professionally involved in teaching and writing it, changes in the educational and general background of those receiving and giving instruction in it, changes in the institutional context within which formal Educational Philosophy is produced and changes in the media and the background assumptions associated with the informal participation of the general public in debate which in part constitutes societal philosophy of education. Many features of Educational Philosophy as an intellectual enterprise are fully understandable only in terms of this social and institutional context.

The education and motivations of educational philosophers changed with the several Phases in the historical development of Educational Philosophy. Dewey and the people of his and earlier generations were products of the old 'liberal' education. The most significant feature of this was not what was studied but the fact that study was undertaken not to achieve some specific career goal but to develop the individual's powers of discrimination and discourse so that he could take a worthy place in cultured society. This in turn, in association with the fact that 'cultured' in those days meant 'leisured', had the effect that the old philosopher worked away at his philosophical ideas, and built them up into some sort of system into which were incorporated the educational precepts which he felt would be conducive to the sort of life he advocated. Hence the integration of ideas which nowadays are characterized separately as 'philosophy', 'history', 'sociology', and so on; so that books can now be published in which those various strands of thinking, originally presented in systemic even if only loosely organized fashion, are disentangled (disarticulated?) and present separately. See

e.g. Locke's Thoughts concerning Education by R.W. Garforth, (ed.) 1964. This style of work may have left the educational views of the great thinkers scattered piecemeal through their works - but it did have the merit that the educational thinking was not separated off from other fields of thought, other subjects. Educational thought was not compartmentalized. Educational philosophy was confluent with general philosophy. (Some implications of this will be discussed later.)

The next phase in the historical development of Educational Philosophy (Phase II) began after the institution of universal education. The emphasis changed, naturally enough, from issues of high generality (such as are appropriate to the consideration of how to educate a selected minority to perform functions of scholarship and leadership within a society) to more specific, and in general more 'practical', questions. These related to the rationale to be provided for disciplinary practices, for example, or for the selection of particular subjects as compulsory or optional components within the curriculum, and similar matters. The persons who produced this type of Philosophy of Education were in general starting from the then-current practices of education - though this is not to imply they were all outstanding as practical educators, in fact the contrary may have been true in many cases - and their concern was to produce a rationale (or sometimes perhaps a 'rationalization' in the Freudian sense, with its connotations of pathogenicity) for what was in fact already being done. There is, of course, nothing wrong with a concern for practicality; but 'Educational Philosophy' of this sort need have little to do with philosophy, and, as Hirst (1963) has suggested, it is better to distinguish it under the rubric of 'Educational Theory'. (Whether the theorizing is sound or unsound, and whether Educational Theory thus conceived is methodologically viable, are other questions. They will be dealt with in due course.)

Out of the need to provide a rationale of education for the large numbers of teachers required for national universal schooling, then, grew the second phase of Educational Theory/Philosophy. It had to be attuned to the capabilities and educational background of the teachers who were its chief recipients; and since a rapid expansion of the teaching force had been necessary, the average capabilities were relatively low and the educational (or more particularly the academic) background of the teachers and teacher trainees was relatively weak.

It is not surprising, therefore, that writings and teachings in Phase II Philosophy of Education were in general also weak.

Some acute and hard-headed theorizing was performed, nevertheless, during this period. Practical problems have a habit of forcing themselves upon the attention, sooner or later; and some individuals are sure to work out ways of solving them. Some of the argumentation about practical policies was of excellent standard - present day policies could be much improved, in many cases, if they were made to accord with advice given sometimes as much as a century ago e.g. by T.H. Huxley and Herbert Spencer (both of these belong more to Phase I than to Phase II, certainly in terms of their competence) - but for a variety of reasons which can only briefly be indicated here, the bulk of theoretical writings consisted of pretentious circumlocutory verbiage which when carefully dissected is usually found to contain little significance.

The reasons for this state of affairs can be given, as a thumbnail explanation, in the following terms. An education system is a complex institution. It does not take kindly to alteration and disturbance. Even attempted improvements are apt to be classified as 'disturbances'; and people who propose improvements are likely to find themselves regarded as disturbers of the peace. People from within the system who propose improvements are therefore likely to be penalized in one way or another (not necessarily openly or spectacularly). Therefore people within the system who perceive the need for improvements must disguise their suggestions. One way of doing this is to dress them up as non-practical 'academic' discourse. Another way is to follow the example of the English constitutional reformers of the seventeenth century, and make one's revolutionary proposals in the form of appeals to (perhaps completely fictional) 'ancient usages'. Much educational theorizing of Phase II can be seen to incorporate a mixture of these strategies. Ancient names are invoked, and many learned-sounding words are rolled out - unfortunately the smoke-screen often conceals nothing except vacuity. The need to appear respectable and 'constructive' (i.e. utterly static) has caused the atrophy of intelligence: noises are made, words are written on paper, but nothing is said.

It would perhaps be more realistic and fairer with regard to our historical judgements, if we regarded most published works of Phase II Educational Philosophy as being 'public relations' exercises, and/or

morale-building activities within the teaching profession, rather than as 'philosophy' in any of the more rigorous senses of that term. In the latter part of the nineteenth century and indeed throughout most of the twentieth the teaching profession has been undergoing massive expansion, quantitatively. Its members have needed encouragement and continuing advice, as they endeavour to adapt to ever-expanding demands made upon them by societies which, it often seems, both want and do not want systems of education. The rapid and accelerating changes of the last century have caught educationalists and teachers in a painfully acute 'double-bind' situation. On the one hand there has been society-in-general, demanding expanded and improved education for its children but loath to pay for it and to a great extent lacking a proper understanding of why education is really needed at all (let alone why particular improvements are needed or why some changes have not been for the best). On the other hand, the personnel within the profession have often been uneasy as to what, exactly, they have been supposed to be doing, and of their own competence to do it. (Hence the interminable discussions and exhortations, within Phase II Educational Philosophy, about 'the aims of education': nobody was really sure what the aims were supposed to be. Doubts about the aims and functions of education have arisen with renewed force in the 1960s and 1970s, despite the disavowals of some leading Educational Philosophers that 'aims' need to be discussed.) Educators have chronically found themselves in the situation, as members of a smaller 'parental' generation, trying to teach much larger 'offspring' generations on the basis of their own personal background of education which, simply because it has dated from several years to several decades earlier, has usually been regarded as, and has sometimes been in actuality, obsolescent. Their insecurity has needed bolstering, just as their professional expertise has been thought to need periodical up-datings by means of 'refresher' and 'in-service' courses - and Phase II Educational Philosophy has had, and continues to have, functions of soothing and reassuring teachers, as well as propagandizing politicians and the general public as to the desirability of education. (But too much soothing, too much saying "Don't worry, you're doing well enough", if education is in fact not being done well enough, can lead to a dangerous form of societal paranoia ...)

After a few decades of this sort of writing, signs of change began to appear. As early as 1942 C.D. Hardie's Truth and Fallacy in

Educational Theory was published - and ignored by all but a percipient few, for fifteen years. Then in 1957 came D.J. O'Connor's An Introduction to the Philosophy of Education. (Both books, it may be noted, were the products of individuals who were in some senses 'outsiders' to the educational Establishment. Hardie for many years held the Chair of Education in the University of Tasmania; and this is a long way 'outside', geographically at least. O'Connor, on the other hand, is a professional philosopher rather than an educationalist, having held Chairs in Philosophy in several British universities.) At about this time other excellent works also appeared, e.g. books by Louis Arnauld Reid, Israel Scheffler, and others; and as the tide changed, the most prolific figure in Phase III of educational philosophy began to spread his influence over the submergent landscape.

The landscape of educational theory/philosophy had been so flattened and eroded during a half-century of isolation that a small rise in intellectual level could, and did, flood most of it. Richard Peters brought training in pure philosophy to his self-assumed task of up-grading Educational Philosophy. His expertise in pure philosophy is reinforced by a realistic grasp of institutional mechanisms. The combination has been highly successful: the low-lying deserts round the Theory-Philosophy coast of Education have been thoroughly inundated by the tide of Peters and his proteges.

What, precisely, was the improvement wrought in Educational Philosophy by the people of the 'new wave'? What are the distinguishing characteristics of the works of Hardie, O'Connor, and especially of Peters, Hirst, and their followers and imitators? (Hardie, O'Connor, and many North American and Australasian educational philosophers are in no way to be regarded as followers of the 'London school' led by Peters, even where their approaches may be similar.)

It has become commonplace to say, (following Powell 1966, for example), that the 'new people' have imported philosophical competence into Educational Philosophy. But what then is 'philosophical competence'? I shall make a stipulative assumption at this point, that this notion of 'philosophical competence' is itself far from being logically transparent or generally understood or agreed upon. My strategy will therefore be to approach the question in exploratory and piecemeal fashion, in the hope of gradually elucidating which possible interpretations are

compatible with which, and which are consonant with practical reality and which are not. As we trace out the theoretical and practical implications of one interpretation against another, the range of our understanding will be widened simultaneously as the range of what we are prepared to regard as acceptable becomes more clearly defined. Thus we do not initially commit ourselves to one view rather than another. Instead, we hold open all the possibilities for as long as we can, and delay the actual decision to adopt one particular position until after we have examined most if not all of the major alternatives.

'Philosophical competence' - it might be thought that this would be guaranteed by training in, or the possession of a degree in, pure philosophy. But while we might provisionally accept that, other things being equal, a person with a philosophy degree would be more likely to be competent at 'philosophical' Educational Philosophy than one lacking such a degree, several important provisos have to be stated and investigated. Firstly, it needs to be recognized that people lacking formal qualifications in philosophy have in the past made important - indeed in some cases revolutionary - contributions to philosophical thinking. At a more modest level a number of individuals have undoubtedly been 'self-educated' in philosophy, just as in any other form of human activity; and many of them have proved more competent than some of those with formal qualifications. Secondly and conversely, it must be recognized that people who do possess formal qualifications do not invariably and necessarily prove particularly effective in the real-life practice of their profession, whether the profession be philosophy or anything else. It is only if we make formal qualifications the sole criterion of professional competence that we can say that they guarantee such competence - but in such a case we have merely constructed a circular argument which gets us nowhere (though it may be comforting to individuals who have formal qualifications and little else).

Thus we have brought ourselves to the position of perceiving that the basic question is that of deciding upon the criteria for the award of formal qualifications in philosophy itself. Either we can take the criteria at present being used in this field as being self-validating in some way - they are the right ones simply because they are the ones being used - thus elevating the status quo to an absolute, which would

deny the possibility of improvement or progress - or else we have to search for criteria which would be logically independent of those currently being used. If we take this second alternative, one of its consequences must be faced: that present criteria for the award of formal qualifications within pure philosophy itself may be found wanting, in one way or another. For the moment it is only the bare possibility of this which is being mentioned. It would be premature to attempt at this stage to set up criteria for philosophy in general or for the teaching of it; but as we come to investigate what is done in philosophy and what is claimed for it, we must be prepared to encounter discrepancies and anomalies. Unless philosophy is different in kind from all other human activities we must expect it to change over the course of time, sometimes perhaps 'progressing' and at other times 'regressing'; and it seems possible that philosophy itself may change by the operation of similar mechanisms to those of its offspring, science, namely by the exposure and resolution of anomalies (cf Toulmin 1961, Kuhn 1962).

It will be convenient to investigate the new philosophical Educational Philosophy, first by examining what is claimed for it by its practitioners, then by attempting to assess the relationship between the claim and what is actually practised.

Hardie, in the Introduction to Truth and Fallacy in Educational Theory (1942), makes an explicit claim for the relevance of certain methods of pure philosophy for Educational Philosophy:

"The present state of educational theory, with its numerous conflicting doctrines, can hardly be regarded as satisfactory, and the present book is an attempt to resolve some of the disagreements. It has been customary to consider that disagreement in such a subject is quite respectable, just as it has long been considered to be respectable in philosophical theory. But recent years have seen a change in the attitude of some professional philosophers. The Cambridge analytical school, led by Moore, Broad and Wittgenstein, has attempted so to analyse propositions that it will always be apparent whether the disagreement between philosophers is one concerning matters of fact, or is one concerning the use of words, or is, as is frequently the case, a purely emotive one.

It is time, I think, that a similar attitude became common in the field of educational theory. That is, if two educational theorists disagree I think it should be made clear whether the disagreement is factual or verbal or due to some emotional conflict. If this is to be done it is necessary always to state each theory in the clearest possible way so that no ambiguity may be allowed to flourish undiscovered." (p. xix)

(The suggestion may be made here, to be elaborated and explained later, that Hardie's category of 'emotional disagreement' may in fact be heterogeneous, may encompass several different kinds of disagreement.)

The emphasis on analysis and clarification is continued by O'Connor (1957):

"The phrase 'philosophy of education' occurs commonly in writings about the theory and practice of education. It is however not always obvious what it means. Often, indeed, if we look critically at the uses of phrases like 'the philosophy of education', 'the philosophical basis of education', 'philosophical presuppositions of educational theory' and so on, it becomes clear that they are no more than vague though high-sounding titles for miscellaneous talk about the aims and methods of teaching. Such usages could well be dropped in the interests of clarity. But I certainly do not want to suggest that language of this sort has no proper and useful function. Phrases like 'philosophy of science', 'philosophy of history' or 'philosophy of art' are also used from time to time in a pretentious or muddle-headed way. Yet they can be used to refer to genuine and important fields of enquiry." (p.1)

He further emphasizes (p.4) that "philosophy is not in the ordinary sense of the phrase a body of knowledge but rather an activity of criticism or clarification. As such, it can be exercised on any subject matter at all, including our present concern, the problems of educational theory."

Coming in historical sequence to 1963, we find Hirst arguing for a sharp distinction between 'educational philosophy' and 'educational theory', criticizing O'Connor for overstressing the affinities between educational and scientific theories - he feels that O'Connor's account makes educational theories seem more simple, and different in nature, from what they really are - and proposing a substantial but still 'second-order' role for educational philosophy.

"From this contemporary point of view" says Hirst (1963:57), "philosophy does not directly lead to knowledge about the world or about ourselves, it is in fact a study of the meanings of the terms in which such knowledge is formed. If the sciences and humanities are said to be first order subjects because they seek to describe and explain the world, philosophy can be said to be a second order subject because it seeks to describe and explain the way in which first order subjects do their job. In this double-decker system, lower deck activities are concerned with understanding the world, upper deck with understanding what goes on on the lower deck. Seen in this way, philosophy has a contribution to make to educational theory wherever second order understanding is necessary, wherever we need to know about the nature of human knowledge, about the meaning of particular concepts and so on. If educational theory is thought of as scientific in

character, then this kind of understanding may well seem of only fringe significance. If however the theory is as complex as has been suggested above, it may well be of quite central importance after all."

The picture that emerges so far has, I suggest, several significant features:

(a) Educational philosophy, due to its nature as a 'second order' activity in relation to educational theory, becomes a 'third order' activity in relation to the practical activities of day-to-day educating. Its value for practical educators, i.e. teachers, might therefore be expected to be open to question. Thus although within his own frame of reference Hirst may be correct in claiming "central importance" for educational philosophy, he is claiming it only for theoreticians of education, not for the bulk of practitioners who do the actual educating within a society.

(b) The emphasis on 'analysis and clarification' not only forces educational philosophy into a 'second order' role - the educational philosopher must wait until the theoretician has produced some theorizing before he can get to work upon it - it also tends to force the educational philosopher into the role of a 'repair technician'. There is no point in calling in the educational philosopher until it is obvious that the conceptual/theoretical apparatus has broken down or is performing badly. He is like the TV repairman: he is needed only when the TV is not working properly. While the theoretical apparatus is working smoothly, or is thought to be working smoothly, educational philosophy is left without a function. (The fact that our 'analytic' educational philosophers do not seem to spend their time waiting for work - a flood of publication has come out in recent years, especially from the 'London' school led by Peters and Hirst - suggests either that the conceptual frameworks of education are in a shocking state of disrepair, or that there is something wrong with the 'second-order analytic' view of educational philosophy.)

(c) The third significant feature worth mentioning at this time relates to the conception of educational philosophy as an activity. The implications of this for the teaching of educational philosophy (and for the teaching of pure or general philosophy, for that matter) have been almost totally neglected. Some of the relevant issues have been

pointed out and discussed by Reid (1962) and myself (Stenhouse 1968, 1969) and a few others; and Scriven (1971) has called for a basic re-appraisal of the teaching of pure philosophy - but apart from these scattered efforts, questions of exactly what is the activity we are supposed to be teaching, how is it best to be taught, how do we know when students have reached an adequate level of performance in it, and so on, have been almost totally neglected. There must, surely, be a reason for this, one would think. Either everyone teaching philosophy and/or educational philosophy knows completely and without question what he is doing and why, and there is a unanimous consensus on this issue; or else it is so unimportant as not to merit discussion; or else there must be some other reason for the surprising neglect, on the part of those teaching either pure or educational philosophy in the universities and teachers' colleges, of these questions of what to teach and how to teach it. I shall suggest later that there are in fact 'other reasons' - or to be more precise, other causes - for this neglect; but for the moment the only point being made is that if philosophy is an activity, as is so frequently insisted, then it must needs be taught as an activity.

(There has indeed been a spate of publication debating the nature of Educational Philosophy (see Powell 1974) - but little attention has been directed to the crucial 'activity' issue.)

Before attempting to develop further these general lines of argument, it will be convenient to review briefly the actual contribution made to Educational Philosophy over the past decade by the predominant 'London school'. (This term is used to indicate a type of approach rather than a formally organized or even necessarily a consciously intended 'grouping' - many adherents of the 'school' have never attended London University, though all must be presumed to be familiar with the work of Peters and his followers.)

Perhaps the most significant early publication of the London school, from some aspects, is the book The Concept of Education (1967). In it, a number of concepts relating to education are subjected to the type of 'philosophical analysis' practised by the London people; and since the importation of this type of analysis into Educational Philosophy is its chief claim to merit, it will be necessary to give an

account of what is involved. (It should be noted that several of the contributors to this volume are independent 'pure' philosophers of considerable standing. They are in no sense 'followers' of Professor Peters, rather the contrary. Hill (1971) remarks that "John Passmore led Peters out of A.J. Ayer's seminars and down to the London School of Economics where Karl Popper taught social and political philosophy. It renewed his concern for applied philosophy." Besides Passmore, Professors Max Black, D.W. Hamlyn, and Gilbert Ryle are also leading figures in 'pure' philosophy. Scheffler is Professor of Education and Philosophy at Harvard and like Passmore, was partly instrumental in establishing Peters in Educational Philosophy. The other contributors are either members of Peters' staff in the Institute of Education or else are/were professorial colleagues (Hirst, Oakeshott, and Vesey) at the University of London. But the book does exemplify a general type of philosophical methodology - and what might be called, broadly, the 'Oxford approach' - and only the London contributors will be held to typify the methodology specific to the London school of Educational Philosophy.)

The book The Concept of Education was constructed, Peters tells us in the Preface, in this way:

"The editor Peters himself ... wrote the first article attempting to map the main contours of the concept of education, circulating it, and invited lecturers and contributors to sketch in one of the important areas in more detail."

It is clear, therefore, that Peters' own article, since it is intended to "map the main contours of the subject", may be expected not only to reveal his fundamental views on the nature of education but also to exemplify what he regards as the proper way to do philosophy/educational philosophy. We must look to the article itself, then, to discover the Peters prescription. (It may be remarked that relatively little subsequent Educational Philosophy has strayed beyond the topics mentioned by Peters, at least for the following decade.)

He starts out by remarking that:

"... 'education' is a concept which is not very close to the ground. By this I mean that it is not a concept like 'red' which picks out a simple quality, like 'horse' which picks out an object, or like 'running' or 'smiling' which pick out observable occurrences. We do not ask 'Are you instructing him in algebra or educating him in algebra?' as if these were two alternative processes. But we might ask 'Are you educating him by instructing him in algebra?'. 'Education', in other words, refers to no particular process; rather it encapsulates criteria to which any one of a family of processes must conform."

He goes on to suggest that it is more illuminating "to regard processes of education as tasks relative to achievements. This accounts for the feature of education ... that its standards are intrinsic not extrinsic to it. This task-achievement analysis I now propose to explain."

In relation to concepts like 'looking', 'running', 'learning', 'inferring', he suggests that "the end is built into the concepts."

"When a man finds something that he has lost or wins a race [sic.], he does not indulge in something different from looking or running, neither does he produce something or reach an end which is extrinsic to the activity in which he is engaged. He merely succeeds in it. He achieves the standard or attains the end which is internal to the activity and gives it point ... 'finding' is the achievement relative to 'looking' ..."

This is an illuminating argument, I suggest, especially in relation to much written and spoken discourse on education. It cuts into the frequently-encountered statements that education is "for national development" or some other alleged collective benefit, or "for self-realisation and self-determination" or some other alleged individual benefit. The notion inherent in the 'process' analogy, that there must be a recognizable 'product' of education, is also undermined. And Peters' approach avoids the methodological vice, so common in second-phase writings in Educational Philosophy, of exhortation and sermonizing. Instead of merely asserting, sanctimoniously, that "We ought to avoid thinking that education is merely instrumental to some other end", or pedagogically commanding us "Desist from the vile practices of instrumentalism", Peters very reasonably puts up a linguistic analogy between words like 'looking' and 'running', and 'educating'. It is up to us to see and accept the point of the analogy; and Peters' method at least appears to have a good pedigree, since he can mention both Aristotle and Ryle as having mapped the 'logical geography' of the concepts taken as analogous to 'educating'.

Having pointed out a similarity between 'education' and words like 'looking for' and 'running', Peters then moves on to show that there are differences:

" ... 'Education' like 'teaching' can be used as both a task and an achievement verb ... 'I taught the boy the ablative absolute construction' implies that I was successful in my task. But I can also say 'I taught him Latin for years, but he learnt nothing'. Similarly I can

work away at educating people, without the implication that I or they achieve success in the various tasks which they are engaged in; but if I talk of them as 'educated' there is an implication of success.

"But whose success are we talking about? That of the teacher or of the learner? This is tantamount to asking to whose tasks the achievements which constitute 'being educated' are relative, those of the teacher or those of the learner. Obviously both are usually involved, but it is important to realize that the tasks of the teacher could not be characterized unless we had a notion of the tasks of the learner. For whereas 'learning' could be characterized without introducing the notion of 'teaching', 'teaching' could not be characterized without the notion of 'learning'. The tasks of the teacher consist in the employment of various methods to get learning processes going. These processes of learning in their turn cannot be characterized without reference to the achievements in which they culminate. For to learn something is to come up to some standard, to succeed in some respect. So the achievement must be that of the learner in the end." (p.3.)

This again is quite illuminating - especially, one would imagine, to students who have not previously come to realize that language is not absolute, that the dictionary does not say the only and final words as to possibilities of meaning. And here again Peters seems to be adopting a very fair and open approach with regard to the teaching/educating which he is endeavouring to perform. He is in effect making descriptive statements about how we do actually use language, about how statements do actually fit or not fit with each other. Since the linguistic usages to which he refers are as much part of our cultural heritage as they are of his, it is presumably open to us to check for ourselves whether or not his assertions about them are correct. And if we carry out a check, surely we are forced to admit that what Peters has asserted is not wrong? The patterns of the language-game are indeed as he describes them.

(The question of whether we can know what linguistic usages are - correct or incorrect - prior to obtaining the results of empirical research on the actual usages of language, will be discussed in a later section. For the moment I shall merely remark, anticipating the outcome of the later discussion, that Cavell (1976) is right in arguing, vs Fodor and Katz (1963), Mates (1964) and others, that experienced speakers of a language are capable of extrapolating from already-known to novel constructions, usually (though not necessarily invariably) getting them right. Cavell's position, which is essentially that of Wittgenstein

himself, is very much compatible with, though it is in no way dependent upon, that exemplified by Chomsky (1957, 1965, 1968; see also Lyons 1970) and Lenneberg (1967) in 'scientific linguistics'.)

Moving on, Peters asserts that education "is inseparable from judgements of value".

"It is ... a logical truth that any method of education employed by a teacher must put the pupil in a situation where he is learning, where some sort of task is presented to him. But a teacher might try to condition children to 'pick up' certain things without their realizing that they were picking anything up. In saying that this is not a process of education, we would be implying that this was morally bad, because conditions of wittingness and voluntariness on the part of the pupil were missing; for we regard it as morally unjustifiable to treat others in this way. To say that we are educating people commits us, in other words, to morally legitimate procedures." (p.3.)

At this point we must cavil a little at Peters' argument. If he were saying that, in educating, we ought to restrict our procedures to those regarded as morally legitimate, he would be saying something that few would disagree with. The important questions that would clearly remain to be answered, in such a case, would be those concerning precisely which procedures were and which were not morally legitimate; and these, it would appear even superficially, are not the easiest of questions to answer. But Peters does not appear to be making a pre-scriptive pronouncement about what we ought to do, he purports rather to be describing what we do do. And if so, his statements are open to question, on various grounds. Does he really mean to suggest that, before a pupil can embark on the processes of education, he must know in advance what the end result of it all is intended to be? This seems highly implausible when one thinks of the average five-year-old just starting school. Does even the average teacher fully visualize and consciously strive for all that is involved in the educational processes going on in his classroom? Even this, when one thinks about it in real-life terms, seems a little unrealistic. And the question of 'voluntariness' - how does this relate to the fact that education is legally compulsory up to a certain age in most countries? Does Peters mean to imply that all the children undergoing formal education would have come for it voluntarily, even if it had not been compulsory? This again does not seem to square with the facts; and the implication would be, that it is needless to have statutes making education compulsory.

Alternatively, if Peters is not suggesting that all pupils come to school voluntarily, is he suggesting that what happens to those who come only because they are legally forced to do so is not 'education' but something different? Is he saying that those who come voluntarily end up educated, while those who come under legal compulsion do not? If so, again he is implying that compulsory universal education does not achieve - and logically cannot achieve - its purported intentions. Without attempting to take up all the possible arguments of detail at this point, I would suggest that Peters has slightly but significantly misrepresented the situation. He seems to have assumed a logically necessary connection between 'education' and positive moral values, i.e. 'moral goodness' in the most general terms. But here again, while we might very well want to say that there ought to be such a connection between education and moral goodness, a realistic acquaintance with what happens in actual education forces us to admit that quite often there is not. And in some of the respects mentioned by Peters it is doubtful that there ever could be. The education especially of young children must very often involve teaching them something at a time when they do not see the point of it, so that at some later time when they do need it, they have it. Thus their "wittingness" is often violated; but far from being a moral outrage as Peters seems to imply, it is the opposite course - namely, failure to teach them what they will need later but do not appreciate now - which is the moral outrage.

But what we really want to say, it appears, is not that there ought to be a logically necessary connection between 'education' and 'moral goodness', but that there ought to be a contingent but real-life connection. That is, that in educating we ought to try to inculcate moral goodness or "what is of value". This is the truism which Peters has misformulated. What is to count as 'moral goodness' is, of course, the big question - but at least if we accept that education should aim at moral goodness, rather than thinking mistakenly that it necessarily incorporates it, we shall be starting off from an unexceptionable position. (Peters' assumption that education is itself necessarily moral would make it logically impossible to talk of someone having been 'educated in crime'. This phrase would be literally meaningless, nonsensical, if Peters were right; whereas in fact it is not meaningless, even though we may deplore the occurrences which give it meaning.)

Peters appears to retreat to some extent, and in some parts of his discussion, from the position indicated above; and my initial line of

criticism must be modified accordingly.

He says:

"The connection between 'education' and what is valuable can be made explicit without commitment to content. It is a further question what the particular standards are in virtue of which achievements are thought to be of value and what grounds there might be for claiming that these are the correct ones. It may well be that arguments can be produced to show why rational men should value some standards rather than others; but at the moment there is no such established harmony." (p.4.)

It will be noted that Peters here might seem to have destroyed, by the distinction he draws between 'education' and its 'content', my earlier line of criticism. He is now saying, in effect, that while 'education itself' is always good, its 'content' may sometimes be bad. But the distinction between 'education' and its 'content' is a bogus one, and cannot be sustained. An abstract definition is useful as a preliminary to detailed argument - but to make an abstract definition of 'education' (or anything else) and then think that one can both keep it abstract (i.e. 'content'-free) and say something useful, is a complete illusion. 'Education without content' is a non-concept. When one talks about 'education' in a general and abstract way one is not suggesting that it can be 'content'-free, one is only avoiding commitment to any one sub-set out of the total set of possible 'contents'. And even when one makes a general and abstract definition with a view to later specification of content, the making of the definition can be done well or badly. 'Education is the inculcation of what is valuable' is misleading, and bad even as an abstract definition, in a way in which 'Education is the inculcation of what is intended to be of value' is not. (Perhaps 'Education ought to be ...' would be even better, as making explicit the prescriptive nature of the undertaking - but it would distract us from the examination of Peters' contribution if we pursued that topic further at this stage.)

One of the most damaging consequences of accepting the suggestion that education is necessarily good, that 'education' means something like 'the inculcation of good values', is that it logically entails the impossibility of commending education itself as being good. In ordinary language, we think that the statement "Education is good" is meaningful, and that related statements such as "It is good to provide education for persons of all ages" are meaningful and can actually function to

persuade people to support particular policies regarding education. But if 'education' really meant no more and no less than 'the inculcation of good values', then the latter could be substituted for the former in the statements where it occurred, with the following results:

'Education is good' becomes 'The inculcation of good values is good', which is not much more than a tautology.

'It is good to provide education for persons of all ages' becomes 'It is good to provide inculcation of good values for persons of all ages', which, insofar as it is more than an empty tautology, seems to be an advocacy of universal self-righteous conformism.

(If we visualize what might be involved in this sort of education - middle-aged businessmen coming into school for their 'inculcation of good values', and so on - perhaps we can see that the plausibility of this interpretation of 'education' rests on our unconscious assumption that 'education is for children'; and perhaps we can begin to think that this might be a misleading interpretation, even with regard to children.)

I am not, of course, suggesting that Peters is really arguing this line. What I am suggesting is that it is to this sort of thing that he would be committed if he persisted in his separation of 'education' from its content. And in fact most of his argument is at variance with that distinction: he is not consistent in holding to it. For example, "... if something is to count as 'education'", he says, "what is learnt must be regarded as worth-while just as the manner in which it is learnt must be regarded as morally unobjectionable." This is a clear enough statement that it is not simply an abstract and general idea of 'education' that he regards as necessarily good: 'content' is quite definitely included here. And that the necessary value of content is a substantial and central, rather than peripheral or incidental, facet of Peters' belief, is attested by passages like this:

"For it is obvious enough that the achievements or states of mind that give content to the notion of an educated man must be regarded as valuable. Finding a thimble that has been hidden is a Rylean type of achievement: but it is trivial one. The achievements involved in education cannot be of this type."

At this point in our investigation of Peters' contribution to Educational Philosophy we might be tempted to formulate a provisional judgement. Peters' work is good, we might say, in that he brings to our attention a number of different (and perhaps to some extent conflicting)

things that people are likely to say about a topic, he draws conclusions from what has been described, and he does not seem to bombard the reader with exhortations about what ought to be believed and/or done. But while this approach is an improvement on most old-style Phase II Educational Philosophy - it appears to leave more to the reader's own judgement, and it does keep contact with contemporary professional Philosophy (witness the emphasis given to Ryle) - it does also appear to be rather superficial. The investigations seem insufficiently thorough at a number of points, and the conclusions therefore must be regarded with some doubt. But perhaps this provisional judgement is unfair. The paper being examined is, after all, designed as a general exploration to provide a background for the more specific studies later in the book in question. Perhaps it is inappropriate to expect a proper analysis in such a context.

This sort of consideration can indeed excuse the omission of various ramifications of detailed argument, in my opinion. Any undertaking must be judged relative to its purpose and function; and Peters' purpose here was to give an outline, a sketch, not to fill in the details. But an outline is not of much use if parts of it are not just incomplete but definitely wrong. While I suggest that Peters can be excused for not giving us all the detailed argument, I do feel that he himself should have carried out detailed explorations on which to base his outline map. He should have avoided making suggestions which are wrong and misleading; or perhaps I should say, rather, that he should have avoided making suggestions which can so readily be shown to be wrong. Again, standards as well as techniques are relative to purposes, and a pioneer cannot be blamed for missing out on subtleties of detail. But Peters goes astray not on subtleties of detail but on a major and foundational issue; and it should not be possible to demonstrate faults so easily in the work of a professional philosopher. One certainly cannot do it in the papers by Passmore and Ryle in the same volume, to take but two examples, despite that both are working on material that is more difficult than Peters'. (Or is it, rather, that they both do philosophy better than Peters, bring out the real issues, the real problems and complexities more clearly, and thus make the reader more aware of the difficulties?)

But to make a pleasanter conclusion to this brief examination of Peters' contribution to 'The Concept of Education' - and after all, at this stage we are really concerned only to show the nature of the Peters

approach in Educational Philosophy, rather than to criticize it - let us display some continuations of his argument that seem both useful and generally unexceptionable.

In further exploration of what it is to be educated, Peters takes us on a systematic survey of various possibilities. Merely to have mastered a particular skill or knack, or to have amassed a quantity of information, are judged insufficient grounds for calling a person 'educated'. "Given, then, that being educated implies the possession of knowledge, but rules out mere knowledge, in that it also requires understanding of principles, could a man [sic] be educated whose knowledge and understanding is confined to one sphere - mathematics, for instance?" (pp.6-7). But this also is rejected as a sufficient criterion of 'being educated'. 'Education' is contrasted with 'training'; the latter

"always suggest confinement. People are trained for jobs, as mechanics, and in science. No one can be trained in a general sort of way. But this lack of specificity is just what is suggested by 'education' ... There is ... another aspect of the knowledge requirements that is built into 'education' that has implications. This is its attitudinal aspect. By this I mean that the knowledge which a man must possess to qualify as being educated must be built into his way of looking at things. It cannot be merely inert. It is possible for a man to know a lot of history, in the sense that he can give correct answers to questions in classrooms and examinations, without ever developing a historical sense. For instance, he might fail to connect his knowledge of the Industrial Revolution with what he sees when visiting Manchester or the Welsh Valleys. We might describe such a man as 'knowledgeable' but we would never describe him as 'educated'; for 'education' implies that a man's outlook is transformed by what he knows." (p.7). "Such knowledge ... must involve the kind of commitment which comes through being on the inside of a form of thought and awareness. A man cannot really understand what it is to think scientifically unless he not only knows that evidence must be found for assumptions, but cares that it should be found ... What would historical or philosophical thought amount to if there was no concern about relevance or coherence? All forms of thought ... have their own internal standards of appraisal. To be on the inside of them is both to understand this and to care ... For to be educated is not to have arrived; it is to travel with a different view." (p.8).

So much for the 'achievement' aspects of education, for what it is to have become 'an educated person'. It must be admitted, I suggest, that Peters has been able to say things that are helpful towards clarifying what is involved. One might have criticisms of one point or another, but many of them can be held off on various grounds already

indicated, and the general effect is of sober, sensible and practical if not exciting discussion. One remark which might be made here is that Peters' discussion must have its greatest effect for people raised in and on the current educational literature, the textbooks of Education as a subject in teachers' college and university. Those familiar at first hand with the works of major thinkers who have written on the topics in question (Whitehead, Dewey, and Bertrand Russell come to mind as examples commonly appropriated even by Education texts; but Gilbert Highet, T.H. Huxley, and Jacques Barzun are other less frequently mentioned writers who are still widely known) would find little that is new in Peters' discourse except the manner of its construction. He does focus attention on the words used (though his technique in this is neither totally consistent nor rigorous); and in this his Educational Philosophy appears to be in line with current fashions in professional 'pure' Philosophy.

Turning now to what he calls the 'task' aspects of education, the processes rather than the product, Peters seems to involve himself curiously in inconsistency. After having insisted on the basic importance of attitudinal change, as we have already seen, he now proceeds to suggest that 'picking up' an attitude from a teacher, "partly by identification", is not a "process of education in the strict sense" (p.11). He admits that "All sorts of things are picked up in this way - desirable things such as a passion for poetry, nuances of style and argument, objectivity towards facts, respect for persons; undesirable things such as partisan allegiances, contempt for people of different persuasions, bad manners, and class consciousness; and trivial things such as mannerisms, a tone of voice, gestures." Granted that the undesirable things ought not to be picked up - how they are to be avoided may be a problem - but surely some of the desirable ones, 'objectivity', respect for persons, etc., are at the very heart of Peters' concept of 'the educated man'? How does he suppose they are to end up in the result if they are not considered part of the process?

In dealing with this Peters does, as usual, say some useful and fairly obvious and also some less-obvious and penetrating things. Then just as he is on the point of entering an interesting and perhaps heterodox line of development, he leaves off.

First, what he does say:

"My reluctance to call such goings-on processes of education is ... due to ... the difficulty of conceiving them as tasks either on the part of the teacher or of the learner ... the fact is that so much is caught rather than explicitly taught. The best teachers are not necessarily those who are au fait with all the latest methods or very knowledgeable about their subject. They are those whose genuine concern for what they are passing on is manifest in the manner in which they do it."

This seems to suggest that some teachers are good at this, and others not, and that even the teachers cannot in any straightforward way be taught to be better in this respect. Peters has more to say which reinforces the view I am presenting.

"Education, like most meetings between human beings, is a very chancey business. The wind of the spirit bloweth where it listeth. Some catch on; others don't. It depends so much whether the learners are drawn to the teacher or not. And what is more chancey than human attractions. If the learners are so drawn, then identification, suggestion, and other such indeterminate transactions may occur. These are not 'learning' processes in a full sense; for they are different from explicit imitation or copying, where something is explicitly attended to for imitation. They happen to people; they are not achievements."

And so on.

It seems clear that the genesis of concern, enthusiasm, 'objectivity', and so on, is important. Indeed Peters has emphasized, repeatedly, that it is. Yet only a proportion of teachers, not all of them, can 'generate' these desirable outcomes. One conclusion would seem to be obvious: that the individual teachers who can and do generate these attitudinal qualities should be identified and given every encouragement to keep on doing it - and for the maximum number of pupils. Conversely, those teachers who are not able to generate the desirable qualities should be given as little influence as is feasible, with regard to the general availability of teaching personnel. In short, the problem is, to attract into and retain in teaching the maximum number of really effective teachers, and retain them in their peak effectiveness. This is to some extent a problem of personnel selection rather than education, indeed Peters himself emphasizes that nobody can be taught how to generate interest etc. in pupils. Techniques for selection and promotion of teaching personnel are the obvious need, to swing the balance in the teaching force towards those who can and do generate the desirable attitudes in their pupils. I have suggested systemic mechanisms which would do this (Stenhouse 1967, 1971) but the whole problem needs vastly more attention than it has received so far. I am not suggesting that

Peters should have embarked on an extensive discussion of it - but having touched upon the basis of the problem and indicated its absolutely vital importance, it is odd that he should sheer off it without even a paragraph on general practical possibilities.

An anomaly which arises in Peters' discussion of educational processes becomes visible first in the section entitled "Extrinsic aids". He attempts to establish a distinction between 'processes of education' and what he calls "aids to education": "I mean conditions such as praise and reward which help children to learn things" (p.10). The first response of most readers would surely be to retort that, though praise etc. does not by itself constitute an 'educational process', it is a part of many processes of education; and though on some occasions praise might very properly be regarded as extrinsic, on others the praise is an integral part of the educational process in the sense that the desired learnings would not take place without it. Sometimes and with some children the initial steps in a 'learning process' have to be accomplished surreptitiously, as it were. A task is undertaken for the teacher: from personal liking for the teacher, 'because the teacher said so', in order to obtain praise, or even from fear of blame or punishment. After the task has been performed perhaps a number of times, a dawning sense of achievement begins to supply what Peters would call an intrinsic motivation for the performance. But this in the very nature of the case cannot motivate the first performances. For these, motivation which is 'extrinsic' in this sense must be provided. But such motivation can in no sense be regarded as extrinsic to the total educational process - for without it, the educational process would often not take place. (A discussion of this issue in terms of the 'ethology of learning' is offered in Stenhouse 1977b.)

There appear to be several possible reasons for Peters' misconstruing this problem. Firstly, while the 'intrinsic' features of a specified learning task can presumably be regarded as 'task specific' and constant for all individuals involved in the task, it is a fact of teaching experience that the so-called 'extrinsic' factors vary from pupil to pupil. Some children need copious praise and encouragement before they will embark properly on a learning task; others need to be left alone to work at their own pace and in their own way; while others again need constant driving and the (to some extent artificial) generation of 'crisis conditions' to bring out optimum performance. But it seems more realistic to say that the details of educational processes and motivations

vary from one child to another, rather than make a false abstraction of one set of factors as being universally 'extrinsic' in contrast to another set which are 'intrinsic'. A distinction between factors which are 'individual-specific' in contrast to those which are 'task specific' (or perhaps even better, 'task general') seems better to reflect the realities of the educational situation.

At a second level of interpretation, Peters may have drifted into a relatively unfruitful and unrealistic distinction because of the influence of orthodox theorizing, and/or lack of intimate and aware involvement in the hurly-burly of everyday teaching. It is a commonplace among child-oriented teachers (as distinct from those who are mere career seekers) that each child has to be treated differently. It is indeed his/her ability to bring out the best in a great diversity of pupils which distinguishes the really gifted teacher - and this just cannot be done by applying the same recipe to all alike. A major factor in the changing balance between 'teaching ability' and 'subject mastery' in the transition primary-secondary-tertiary education is that, along with the increasing difficulty of the subject-material, there is a general increase in the collective ability of classes to cope with single-recipe teaching. This is partly because each individual student is more 'mature', better able to organize his own methods of study, and partly because a selection process has been operating to weed out those individuals unable or unwilling to adapt to the new regime. Finally, many of the slogans of education - 'treat pupils as individuals', 'teach children not subjects', and on - both imply that individual variation should be recognized, and presuppose that in practice it is not (or not sufficiently anyway). All in all, one would have thought that there were enough promptings towards a more realistic distinction than the one Peters actually makes; but perhaps lack of contact with the obtrusive idiosyncracies of real children, from which Professor Peters must be presumed to suffer these days, could be regarded as the main cause of what might otherwise be seen as wilful blindness. (Perhaps the inhabitants of Olympus suffer from snow-blindness.)

Oddly enough, Peters himself in his discussion of 'conditioning' reveals a sharp awareness of the false abstraction inherent in the laboratory experimentation upon which much 'learning theory' is based. "It is very questionable," he says, "how much of animal learning, outside the narrow confines of laboratories, takes place according to principles of conditioning. For how often are animals in a position where their random responses are systematically reinforced? ... The

situations in which they 'learn' things by 'conditioning' are those in which opportunities for the use of intelligence are cut down to a minimum ..." (p.13). This is very much to the point (see Lorenz 1965, Stenhouse 1974, for further discussion) - but here again Peters shies away from the line of implication he has opened up. Again it could be urged that he is merely indicating a weakness in the 'learning theory' position, so that others can expose the flaws in detail. Some of the logical inadequacies of 'learning theory' are indeed exposed by implication, later in The Concept of Education, by Ryle and by Passmore - but these later detailed arguments are in fact based upon different foundations from those touched upon by Peters. So far as the 'false abstraction' issue is concerned - which could well have been developed to reveal not only laboratories but also schoolrooms as misleadingly artificial, and not only conditioning but 'learning theory' in general as a questionable theoretical adjunct of education - Peters opens the door, and then closes it again and moves on to something else.

For various reasons I propose to leave the examination of Peters' article "What is an educational process?" (in The Concept of Education) at this stage, and to move on to other issues. Our purposes do not demand, and space does not allow, an exhaustive scrutiny of every thread in his argument. We have already seen enough, of both strengths and weaknesses, to show the nature of Peters' contribution to contemporary Educational Philosophy, and it is with the general nature of this that we are concerned. A final and overall assessment of Peters' place in the history of Educational Philosophy or Education will need, and presumably one day will get, a volume to itself. On the more specific question of the methodology propagated by Peters and the 'London school', and its adequacy for Educational Philosophy, Education, education and education in science, we can begin to work towards an answer. A number of particular strengths and weaknesses have been pointed out already - and Chapter Four will be devoted to an examination of some criticisms that have been made of Peters - so at the present stage it seems appropriate to draw up a tentative outline of what appear to be the general features of the methodology of the London school.

The main steps in Peters' procedure seem generally to be as follows:

- (1) A 'concept' is named, and some of its common interpretations and usages are described.

(2) The implications of some of these usages are stated to be dissonant with, while others are said to be consonant with, what are stated to be essential features of education.

(3) Once the first concept has been 'clarified' under step (2) above, it is used as a 'standardized component' in further explanations and clarifications. Thus:

(4) A system of concepts, each with its own 'clarified' (and 'standardized') meaning, is gradually built up, all related to each other and to Peters' views as to the essential features of education.

(5) A systematic account of educational theory and practice, and their underlying basic concepts, is thus built up; and presumably this can be taken as equating, at least in a general way, with the 'educational philosophy' of society in question.

If the above can be accepted, at least provisionally, as an outline of the methodology of the London School (especially as exemplified by Peters himself), it will be convenient to move on, in the next Chapter, to a preliminary examination of one of the earlier and more polemical criticisms of the Peters' approach.

CHAPTER FOUR
SOME CRITICISMS OF PETERS AND THE 'LONDON' SCHOOL OF EDUCATIONAL
PHILOSOPHY

Before tackling questions of the functions ^{which} philosophy could have, and should have, in education especially at the tertiary level, it may be useful to touch upon some criticisms of 'London' educational philosophy that are implicit and explicit in a paper by Adelstein (1972): and also some of the criticisms of education in general that are being made at the present time. These can then be considered in relation to the functions which education might be supposed to perform for the society and for the individual. This in turn may lead to a new synthesis and integration of our theoretical understanding of these functions, and to a new and better view of the part to be played by philosophy.

So far as explicit criticism is concerned, Adelstein mentions 'boredom', triviality, and conservatism as being bad features of 'education in educational philosophy' of the Peters school. It seems unlikely that these features by themselves should elicit the sustained criticism and such obvious and emotional antagonism as Adelstein reveals. We have all sat through boring lectures, and some of us have given them - but if a merely uninteresting and dull presentation were the only issue, there would be little point in writing books about it. (After all, one can always sleep, or not attend the lectures.) The conjunction of boredom with triviality makes the problem look more serious. The individual student does not wish to waste a year of his life taking a course which is really trivial. Granted that not every student can be expected to judge correctly at the time what is trivial and what is not - the general problem of all education obtrudes itself here, that, virtually by definition, young people are being prepared for situations and problems of the future, of which they do not yet have adequate understanding (cf Ryle 1967), hence they cannot be expected to have a full appreciation of what is being done in the present - but it is possible, all the same, for a case to be made and supported one way or the other; and the case presented by Adelstein does appear to be fairly consistent and substantial.

Again, 'conservatism' in one sense is hardly a positive fault. For an individual to prefer old ways and old values to new is not a crime. It is not a fault in an educator if he does this, even in his professional activities, provided he presents a fair picture of both old and new. Here is the central issue. A 'fair picture' must be

presented to students. They will not necessarily at first realize that it is fair. Older views may be unfamiliar to young students (hence in a sense to be new to them - and students and young people are not immune any more than their elders to neophobia). Old and new views will need to be argued, calmly and consistently, before the students can be brought to see that the old ideas are not necessarily perverse and stupid just because they are old. (Compare this with the 'paradox of past greatness': if the views of the old 'great figures' were really as absurd as they appear to modern eyes, especially when depicted in elementary textbooks, how come the people were and are regarded as great? Toulmin 1961, Kuhn 1962 and others lead towards a solution.) But given say an academic year in which to argue and discuss, it is my experience that even resistant and contra-suggestive students can be won over, not necessarily to accept a particular set of views, but at least to accept that they are reasonable.

It seems that this is precisely what Adelstein cannot accept of the Peters school: that their position and/or actual teaching is reasonable.

And this is itself curious - because 'reasonableness' and 'rationality' are appealed to so often in Peters' own writing.

Let us try to get a perspective which will embrace both Peters and Adelstein, and enable us (we hope) to achieve a frame of reference in which both parties can be understood.

Adelstein's article on the educational philosophy of the Peters school, like the other papers in the volume Counter Course (ed. Pateman 1972), is good in its diagnosis but not so good in its prescriptions. The weaknesses and failings of present-day society and its institutions are cleverly and fiercely exposed. We are urged to be merciless in criticism, in extirpating the faulty structures - but very little is suggested, on the constructive side, about what can be done to replace what should be chopped out. This is a serious deficiency. It does not, of course, vitiate the criticism as such; but it does very materially reduce the worth of the book and of its constituent papers.

It does more than this. Adelstein's paper (and others of a similar nature) can be revealed, by similar techniques of analysis and criticism to those they use on Peters, to be equally partial, and perhaps equally

dishonest, as are the Establishment practices exposed so brilliantly in the papers by Atkinson, Ingleby, and Silman (to name only three besides Adelstein himself). The sleight-of-hand which is attempted on the reader is briefly this:

Criticisms are made and (to varying degrees) substantiated.

The reader is made to feel that not to be active, in doing something about the faults revealed, is shameful.

No programmes of action are explicitly suggested (even if only as possibilities).

The reader is given to understand, however, that the authors of the papers, and their associates, are active in doing something

Therefore, by implication, the reader is invited to give an open cheque to the writers, in the form of commitment to support whatever they may decide to do.

My criticism of the Counter Course group can be put in the form of a dilemma:

Either (a) Their criticism is merely and purely destructive;
or (b) Insofar as there is any constructive implication, it involves the intellectual/emotional sleight-of-hand outlined above.

It should be noted that alternative (b) is at least straightforward: we, the readers, are tacitly invited to 'fall in' behind and in support of Adelstein et al. That this is so, is also a standard line of criticism against anti-Establishment critics, by spokesmen for the Establishment. In criticizing Adelstein and his colleagues I would not wish to be thought of as merely defending an Establishment - no more than I would wish my support of many of his criticisms to be construed as support of whatever 'revolutionary' programme he espouses. My own position is neither 'Establishment' nor 'revolutionary'; and though the term 'evolutionary' would in some ways be appropriate, indeed so far as my hopes are concerned highly apposite, I deplore the bandying of epithets which seem to be taken too seriously by some people. It would be ~~inconsistent~~ with my own philosophical position as well as with my personal views, to accept that these little labels could actually reveal anything significant about the issues involved. They are useful as labels, perhaps, simply to refer to positions which must always be given their real identification and characterization at much greater length.

If our aim is not to support either the Establishment or the anti-

Establishment positions, but rather to examine both and try to construct a methodology which can transcend both, it may be useful to extend the scope of the enquiry and pose the following as an hypothetical:

If Peters' educational philosophy (and his teaching of Educational Philosophy) has all the faults which are alleged of it by Adelstein,et al., then:

(a) How has Peters' (and, in general 'London' Educational Philosophy) managed to become so popular and so influential? Education-
alists are not all stupid. Surely if the flaws in Peters' position are real, they would have been noticed and publicized, and he would have been dethroned ...?

(b) If the flaws exist yet have not been widely detected, this makes the question of their nature and genesis even more intriguing. How could faults be real and yet remain unnoticed? How could such faults arise in the first place?

I shall devote the next major section of this Chapter to arguing that, in Peters' case, both the faults and the popularity spring from the same sources. Thus I am implicitly rejecting the suggestion that, since Peters' 'London' educational philosophy has unquestionably been popular and influential, its alleged faults cannot be real or cannot be important. But before moving on to discuss the possible explanations of both faults and popularity, it seems desirable to point out that, our major purpose being to find an alternative methodology which will transcend and resolve the controversy between Peters and his critics, we must keep in mind the need to take up any hints we can find, any clues which can lead us towards our constructive purpose. I want to suggest at at this point - so that it can be kept in mindⁱⁿ what follows - that we might adopt, as a general though only provisional principle, that the methodology we are looking for will avoid the faults exemplified by Peters and his followers and that this 'avoidance' can be stated in more extreme terms as embodying the contradictories of the faults that have been discerned. That is to say, if p, q, and r are regarded as faults, then our new methodology should incorporate not-p, not-q, and not-r.

This strategy will not by itself, of course, guarantee the formulation of a workable and improved methodology - but if we try to do the opposite of what is found objectionable in the Peters-type approach, we may be led to a point where we can see our methodologies in a new light, so that we can decide on the details of a new strategy on a basis

of other considerations besides the faults of the London approach.

With our overall strategic objective now stated and able to be kept in mind, we can return to the two questions posed earlier, of the origin of the faults in Peters' approach and of the popularity of that approach despite its faults.

While the issue of 'reasonableness' and 'rationality' will, as suggested earlier turn out to be the most basic of all - and it can be particularized in the question of whether or not a 'fair picture' is presented - it will be convenient to initiate discussion of the genesis of faults by examining first some of those that have been more superficially obtrusive.

The conservatism and authoritarianism detected by Adelstein in Peters' approach to Educational Philosophy - and similarities in this as in other dimensions must presumably underlie the grouping of others besides Peters as constituting the 'London school' - may derive from factors in Peters' personal and professional background. Thus although the 'Profile' by Hill (1971) begins with an emphatic disavowal of conservatism by Peters himself ('"The last thing I am ... is a conservative by temperament" ' - but why does he feel the need to be defensive about it? And why does Hill make it the opening declaration of the Profile?), there are hints of factors making for both conservatism and authoritarianism in the circumstances of his upbringing. His father was a Deputy Inspector of Police in India; and the Peters boys went to a 'public' boarding school, Clifton. These facts might help to explain some of Peters' tendencies towards overt anti-authority activities - most of which appear to have been fairly superficial, playing with bombs in the family garden, and so on (Hill 1971) - and also his inability to escape from a fundamentally authoritarian/conservative set of basic and largely unconscious presuppositions.

Whether Peters' philosophical training caused or only reinforced a predilection towards conservatism, it is clear that he must have found the Oxford brand of analytic linguistic philosophy highly congenial. At the present time he could be said to be one of the typical exemplars of Oxford philosophy, in the sense that he has adopted in a most straightforward way a particular methodological approach which is characteristic in one way or another of most of the well-known Oxford 'linguistic' people from 1930 onwards. Peters' methodology can be characterized without the complication of provisos, qualifications and disclaimers such as prevent the neat and final pigeonholing of people like Austin, Strawson,

and even Ryle. (It could perhaps be argued that it is the perspicacity of their provisos and disclaimers which constitutes a great deal of the value of the work of these philosophers ...) The latter are just better philosophers.

Although the 'linguistic' movement in philosophy started at Cambridge with Moore and Wittgenstein and continued with Wisdom, the most immediately influential developments from it took place at and from Oxford. A number of reasons for this can be discerned and are worth mentioning. First and in some ways most important, the Oxford people did not by any means adopt the methodology of Wittgenstein. Insofar as they followed anyone, it was Moore; and even then, they took what suited them and left the rest. What they did take they welded into a matrix of traditional philosophy plus some of the apparatus of logical positivism. (It is noteworthy that many Oxford-orientated philosophers count Russell among the founders of linguistic philosophy.) Their traditional philosophy gave them standing with, and made them readily intelligible to, tradition-oriented philosophers in other centres around the English-speaking world. Their reductionistic/'analytic' tendency, on the other hand, made them congenial to non-philosophers and especially to science-orientated people in pursuit of a rationale for their activities - it should be noted that 'linguistic analysis' is an Oxford not a Cambridge label - and has been concordant with the prevailing reductionist assumptions which have underlain most fashionable intellectual on-goings in the twentieth century. In general, the Oxford approach exemplified by Ryle and others has been more simple and straightforward than that of the Cambridge people: it has been easier to understand, and it has therefore been understood and taken up by a large number of philosophers and lay persons.

Oxford linguistic philosophy is much easier than the Cambridge parent-stream. In the Oxford distributory (this last word can be taken in any of at least three possible meanings) people follow the recipe for making a good speech: first say what you are going to say, then say it, then say what you have said. Oxford philosophy, despite the claim that it "makes a technique of being non-technical" (Mehta 1965: 27), has in fact made its general impact through the use of easily-grasped techniques and easily-remembered terminological formulations. Consider, for example Ryle's sentence-frame technique for deciding whether or not particular words are 'syncategorematic' (i.e. belong to the same linguistic category - see Ryle 1937). As Passmore (1968:443)

points out, the sentence-frame "... is in bed" can be used to differentiate between 'Jones' and 'Saturday', because the one can be inserted in the gap and makes sense, whereas the other cannot. They thus belong to different linguistic categories. But the sentence-frame technique does not, in the end, by itself enable any final distinctions to be made as to linguistic categories; and the term 'syncategorematic' and its relatives end up more as attention-getting devices rather than clarificatory concepts.

One other technique widely used by Oxford but not by Cambridge linguistic philosophers is that of 'labelling'. This is in fact useful both philosophically and educationally, so long as its limitations are understood. By 'labelling' I mean the invention or adaptation of a word (or words) to mark a specific distinction (which must previously have been established and explained at length and in detail - absence of clear and extensive understanding of the basis of the distinction is what renders labelling dangerous). J.L. Austin's demonstration and naming of the 'descriptive fallacy' is a good example of useful 'labelling': to be able to pigeonhole 'descriptive fallacy' enables us to remember easily his demonstration that though it is often assumed that the only or primary function of language is description, this assumption is fallacious. 'Knowing', for example, i.e. when I say "I know that X is Y", has commonly been regarded as giving a description of my mental state. (This assumption is widely prevalent in Education.) But as Austin shows (see Austin 1961), the 'description' view is highly problematical, and it is more useful to regard statements of the "I know ..." kind as being performative, in that I can justify my claim to knowing, not by examining and then describing my mental state, but by performing some activity. I show that I know the boiling-point of water by: telling "It boils at 100°C"; lifting the saucepan off the stove, saying "It's boiling", when the bubbles are rushing up vigorously from the bottom ... and so on. Austin later modified his performative/descriptive distinction into one between performative utterances and 'constative' ones, the latter being a somewhat wider category than 'descriptive'. Later still, he argued (in a slightly different dimension) for a distinction between 'locutionary', 'illocutionary', and 'perlocutionary' acts. For the moment the only point being made, however, is that of the usefulness of having our distinctions (those that have been demonstrated as being useful to make, anyway) 'labelled' by the use of specific terms.

Although Austin followed the practice, typical of Oxford philosophy, of 'rounding out' his investigations and leaving everything neat and tidy and with all the issues 'labelled', he and Waismann (another Oxford philosopher originally from Cambridge) were closer to Wittgenstein in their emphasis on the need to do full justice to the great complexity, subtlety and the 'open texture' of language (the phrase is Waismann's: today we might rather say 'open-endedness'). They recognized, more than the rest of their Oxford colleagues, that however careful and extensive our investigations of actual and possible linguistic usages, language itself changes and cannot be completely specified and tied down. This aspect of their approach seems, however, to have been less influential than its more typically 'Oxford' features of specifying and labelling.

The main direct influence of Wittgenstein seems to have gone, so far, into philosophy of science rather than 'pure' philosophy. Toulmin and Kuhn have probably made more impact, even within 'pure philosophy' circles, through their works in the history and philosophy of science, than Wittgensteinians such as Malcolm and Gasking who have remained 'pure'. There are signs that this situation is changing. The simple Oxford-type approach is being increasingly regarded as simplistic. Complex problems are increasingly being recognized as demanding not only complex solutions but also complexity of treatment and investigation. Perhaps Kuhn's The Structure of Scientific Revolutions (1962) will turn out to be the precursor of a new wave of complexity-regarding work. In America there are signs of a reaction against simplicism that may well become massive. The reasons why this should happen at this time and in America appear multifarious: the continuing influence of 'Continental' (European) philosophy, especially perhaps the Existentialist writers and Hegel and Kant; reaction against the formalism of the entrenched ranks of American formal logicians and against British empiricism and associated types of 'analysis'; perhaps most important of all, a reaction against the pernicious mixture of simplicism and double-think emanating copiously from politicians, self-elevated 'experts', and opinion-manipulators of all sorts using the mass media of communication. (Watergate and the Nixon resignation are other symptoms of such a reaction.) The fact is, that a revival of Wittgensteinian rigour-with-complexity looks like occurring in America (and perhaps also Australasia) - and the best recent systematic example of Wittgensteinian (as distinct from Oxford-style) 'language philosophy' is an American one (Cavell 1969, 1976).

So the dominance of Oxford-style philosophy may be at an end (though Oxford itself will no doubt continue to move with the times). It will be unfortunate for Educational Philosophy if it should prove that its recent anastomosis with 'pure' Philosophy has been with a senescent portion of the system. This looks like being the case, however. The 'philosophical analysis' which Peters has imported into Educational Philosophy is not only lacking many of the subtleties and qualifications which are insisted upon by the leading figures of Oxford philosophical analysis, it is in any case a virtually worked-out and exhausted mode. Little more can be expected from it.

Lest it should be thought that this contradicts my earlier assertions of the value which Peters has contributed to Educational Philosophy, let me hasten to reiterate that Peters and the London School do represent a worthwhile advance on what 'Phase II' Educational Philosophy had previously been. They have been able to achieve a worthwhile advance partly because the previous state of the discipline was so poor. Having achieved this advance, Educational Philosophy has been put momentarily in step with some branches of pure Philosophy. The tempo in the parent discipline itself is changing, however, and as I have suggested, the rate of change is likely to increase dramatically within the next decade. Educational Philosophy will therefore need to advance beyond the 'London' methodology, if it is to retain professional contact with Philosophy - and it will need to move rapidly.

There has always been much in the Oxford school of 'philosophical analysis' to attract those of conservative and authoritarian mind. Passmore writes (1968: 44 ff):

"... not all ex-students of Wittgenstein look with kindness on the 'ordinary language' philosophies which have latterly dominated the philosophical scene at Oxford, for all that they show clear signs of Wittgenstein's influence. At Oxford, Wittgenstein's ideas entered a very different philosophical atmosphere from that which prevailed at Cambridge. Oxford philosophers, for the most part, have learnt their philosophy as part of a course of study which is based upon classical scholarship; in particular, the influence of Aristotle has been strong ..."

"Now when Aristotle considers such a question as 'whether the virtues are emotions', he makes use of what it would be natural to call 'an appeal to ordinary language' ... 'What we say' ... is the decisive factor. Arguments of this sort are everywhere to be found in Aristotle's Nicomachean Ethics, and were freely employed by the most influential Oxford Aristotlians Add to these special influences the quite general consideration that classically-trained men are always likely to place great stress on 'correctness', which has a reasonably definite

meaning within a dead language, and it will no longer seem surprising that 'ordinary language' philosophies made such rapid headway ... At Oxford, then, Wittgenstein's ideas were grafted on to an Aristotelian-philological stock ... In the eyes of many of the Cambridge 'old guard' of Wittgensteinians, Oxford philosophy has desiccated into scholasticism."

No wonder, then, in the light of this, if Peters found any natural tendencies towards conservatism being strengthened by his training in Oxford philosophy. The charges that Passmore makes against Oxford 'linguistic analysis' in general are similar in many ways to these that have been brought against Peters by Adelstein and in the earlier chapters of this thesis. Not that Peters is alone in his basic conservatism and authoritarianism, even in Educational Philosophy: these are general characteristics, indeed, of the London school and their very wide circle of loosely-affiliated adherents. Conservatism and authoritarianism might well be regarded, in fact, as the peculiar hazards specific to the Educational sub-discipline of Philosophy. They are very real occupational hazards which are apt to be generated, it would seem, by the usual circumstances by which individuals come to be Educational Philosophers.

People come to be Educational Philosophers, it seems, mainly from schoolteaching. They move from schoolteaching to lecturing in a teachers training college, find that Educational Philosophy is a subject which is widely taught but seems to demand little more than a nodding acquaintance with some great names from the past (and/or the works of Professor Peters), and move into it. It has offered - up until now - a relatively easy road to status, especially for individuals with verbal fluency and good oneupmanship skills. So long as one does not attempt to publish, one's reputation can be sustained almost indefinitely. (And of course, one's reputation need not be hazarded in the truly learned journals: Education abounds in little parochial and 'bandwagon' magazines which are chronically, and understandably, short of copy) Thus after a year or two of practising 'edfil' upon hapless teacher trainees, and perhaps after taking one of the special 'quickfil' courses becoming widely available these days (some even carry the tag of Ed.D. or Ph.D. at the end of them), the aspiring Educational Philosopher moves into a university department of education to ply his trade (or practise his art - whichever it is he does). There, he is again in a good position, provided his colleagues in Education know little about Edfil and provided he avoids the Philosophy Department. (He can always emphasize

that he is a Philosopher of Education, if cornered by philosophers; and if he is lucky, his friendly neighbourhood philosophers may themselves interpret 'philosophy' as 'knowing about philosophers' writings' rather than 'being able to do philosophy themselves', so they won't be able or inclined to trouble him. Purefil as well as Edfil has its less worthy practitioners, too, in these days of rapid expansion of universities.) But even the most self-complacent can be aware, deep in his subconscious, that he is living on shaky foundations, that basic philosophical competence is lacking - so he is likely to put up defensive screens, in the form of authoritarianism and conservatism. (Conservatism is safe: the old doctrines have been defended by plenty of competent people. One needn't depend on one's own judgement and argument; one can simply 'learn up' the standard commentaries.)

So long as Educational Philosophy is regarded as something apart, an esoteric 'second-order' spectatorial activity having little real relationship - despite what its practitioners may claim - with the day-to-day activities of education, it is quite natural that it should seek nurture and protection from the most authoritarian-seeming and prestigious brands of pure philosophy. Hence the umbilical cord between London and Oxford. The many London-type Edfillers do not really want to be part of the rough and tumble of ordinary education. Like many Scientists (the capital is intentional), they want to be isolated in their 'logical laboratories'. They want to have closed-shop societies in which they can devote undivided attention to each others' work, i.e. they want to play oneupmanship games. They want the uplift of a mystique.

But what are the alternatives?

Against conservatism, authoritarianism and the mystique of the London school, what viable form of educational philosophy can be offered?

Some of the people who might ask this question would be asking it, I feel, only rhetorically. Their view would be, that there is no alternative. It is Peters or nothing. (This, it may be pointed out, does not exemplify a very philosophical approach ...) But in fact there are many alternatives in philosophical methodology. Within the 'linguistic' tradition itself, the main stream of Wittgensteinian methods, having been overshadowed for a couple of decades by the cut-and-dried Oxford approach, has in the 1960s been demonstrating its power in wider and wider areas of American philosophy (see, for example

Cavell 1976), and also, in Ethics, in the work of Philippa Foot (1958) for example. The simplicism characteristic of much earlier thinking has led to the inevitable realization of the need to grapple with complex issues through complex argument - simplicism having amply demonstrated its inadequacy - and the necessary time, effort, and subtlety have now been put into the more difficult Wittgensteinian type of philosophy. Powerful demonstrations of its effectiveness have already appeared e.g. Kuhn's The Structure of Scientific Revolutions (1962), which is already influential on a world-wide scale. Cavell's Must We Mean What We Say? (1969) is cited by Bates and Cohen (1972) as "the best discussion and defense of the procedures of 'ordinary language philosophy' " that has appeared so far. And though Wittgenstein-type philosophy appears still to languish in Britain, an offshoot has kept alive in Australasia, in the teachings and writings of Gasking, Ziedins, and a few others.

It appears possible that the Wittgensteinian approach, spreading within pure Philosophy in America and with a significant revolution already achieved in Philosophy of Science (in the work of Kuhn, Toulmin, N.R. Hanson and others), might have a great deal to offer in Educational Philosophy. Some considerations supporting this view will be given later. One point which may be mentioned immediately in connection with Philosophy of Science, is that a commonality of approach and methodology between that field and Educational Philosophy could be expected to lead to highly fruitful reorientations and new syntheses. There are several channels of relationship, all of vital importance, between science and education. Education as a social and individual process is much affected by various findings of science - empirical-level research into educational and related issues has even been entitled 'educational science'. Then on the other side, how science is conducted is greatly affected by how scientists have been educated. A considerable volume of discussion and research has been going on for over two decades on how to teach science - very little has been done, however, on fundamental questions about what science education is really intended to achieve, or even on what we want science itself to do for us. In other words, philosophy of science education does not yet exist as a major field of intellectual dialogue and academic study. As it is brought to birth - as it must be - it will eventually inherit genes from both its parents, Philosophy of Science and Educational Philosophy. For our future wellbeing, indeed for our very survival, we must try to ensure that the two sets of genes are mutually compatible. Very substantial channels of communication will

have to be opened to connect right through the full spectrum of first-order science, science education, Philosophy of Science, Educational Philosophy, Education as an academic discipline, Philosophy as an academic discipline, philosophy as a personal and social activity; and education as a social and personal activity. A Wittgensteinian 'language-game' approach offers a means - I believe the only means - of achieving this sort of immensely difficult but vitally important communication. This thesis is offered as an attempt to exemplify the sort of thing that is needed. (It is not intended to do all that is needed, needless to say, only to give an example of the type of approach.)

Before we move on to examine the constructive contribution which might be offered to educational philosophy, and education generally including science education, by a Wittgensteinian 'language-game' approach, a major issue with regard to Peters and the London school remains to be dealt with. This is the question of 'reasonableness', 'rationality'. (There may be not one but several questions here - we shall find out as we proceed.)

For a number of reasons (which, again, will emerge more clearly as we proceed) it will be convenient to base our investigations on the work which is perhaps Peters' most characteristic and most influential contribution to educational philosophy, Ethics and Education (Peters 1966; references will be to the 1970 edition); and within this to concentrate mainly on the Chapter on "Equality".

Peters himself says of the book that it "presents a distinctive point of view" (Preface, p.7) - namely, his own. It was written relatively early in his career, at least in the sense that it was produced while he was still in process of establishing his dominance in Educational Philosophy (indeed it contributed very substantially to that process); and although he acknowledges his intellectual debts to various of his colleagues and friends, the book itself appears to present in the purest available form Peters' own views and methods, uncontaminated (relatively) by the impact of later controversies and criticisms. Yet though Peters emphasizes the rapidity of composition of the book (three years instead of the five years it "should have taken" p.7), he had been working the general area of ethics-and-educational-philosophy since the 1950s; and Authority, Responsibility and Education (Peters 1959) contains material that was originally presented as early as 1956, as is stated in its Preface. Thus Peters had been active in this area for about a decade when the MS of Ethics

and Education left his hands in October 1965, and it cannot, therefore, be regarded as a product of hurried thinking. Neither can it really be regarded as unduly subject to pressures towards brevity, such as can, perhaps reasonably, be assumed to apply to Peters' introductory contribution to The Concept of Education which was discussed earlier. All in all, and accepting the provisos which Peters advances in his Preface regarding possible reactions to Educational Philosophy by practical teachers on the one hand and professional 'pure' Philosophers on the other, it seems not unreasonable to take Ethics and Education as exemplifying, perhaps at paradigmatic level, the characteristic methodology of its author.

Peters himself seems inclined to claim a certain priority for his chapter on "Equality":

"... the ethical foundations of 'equality' and 'fairness' may well seem more solid than those of the content of education From an expository point of view ... there is much to be said for starting with the principles of equality and fairness" (p.117):

and

"... the justification of fairness or justice ... is as near as we can get to rock-bottom in the attempt to establish ethical foundations" (p.118).

We are looking, it must be remembered, not so much at Peters' arguments for equality and fairness, in themselves, but rather as they exemplify his basic notions of 'rationality' and 'reasonableness'.

Having looked at 'equality of consideration' as perhaps being an "empirical generalization about the nature of man" and having found it "either vacuous or patently false" (p.118), Peters remarks that " 'All men are equal' ... seldom functions as an empirical generalization ... its logical function is usually to lay down a rule ... It usually amounts to saying that men ought to be treated equally" (p.118). He then examines the possibility that 'equally' might mean 'identically', and finds that it would be "unwise and unjust" to treat everyone identically. His grounds here are given by example rather than explicit general statement, but could be summed-up as the consideration that people's needs differ: they are unequal in their needs, so must be treated unequally. Peters himself puts it thus:

"The principle of distributive justice can thus [in the light of the examples he gives] be formulated which lays it down that equals should be treated equally and unequals unequally." (p.118).

From this the problem emerges: how do we decide which people are 'equals', which are 'unequals'? Peters invokes the device of 'categories'. Persons within the same category are equals and must be treated equally; inequality of treatment is permissible between categories. Peters recognizes that this merely generates a problem of category-multiplication, and to get over it (since he feels that no 'positive' grounds for equality of treatment can be found, p.120) he explores the possibility of restating the principle of equality "more negatively". The formulation he offers is: "no one shall be presumed, in advance of particular cases being considered, to have a claim to better treatment than another."

He himself goes on:

"This reformulation of the principle gets rid of the necessity for finding positive grounds for treating people alike; but it would amount to no more than a verbal trick if no reason can be produced for showing that this is the proper way to state the presumption ... The only way to show this is to establish that the general principle ... is a presupposition of practical discourse ..." (p.121).

'Practical discourse', is said by Peters to be discourse associated with attempting to answer the question "What ought I to do?". Peters appears to presuppose (though he does not state this) a significantly moral dimension in the situation i.e. he wants to exclude questions like "Should I get a pink or a white icecream?" He does say that he is concerned with people choosing an alternative " ... as distinct from merely 'plumping' for it ..." - here, 'choice' is apparently being contrasted with arbitrary and/or random selection. "A person who uses the discourse of practical reason seriously", says Peters (my emphasis added) "is committed to choosing rather than plumping, the notion of 'ought' being more or less equivalent to the notion of there being reasons for something." His use of 'seriously' seems to imply a recognition that the same 'discourse' (i.e. forms of words) could also be used 'unseriously': and this would presumably mean that Peters' conclusions about 'reasons' would not then follow. But Peters does not explore the implications of this, beyond saying that " ... this could not be the general practice or practical discourse could not have the function in social life which it in fact has." (p.122). This looks like an appeal to 'mere usage', in which, perhaps, the reader is invited to concur. We may as well do so, in order to get on with the argument - but we must note that Peters has not really offered us reasons for doing so, beyond his own assertions!

Discussing a person in a 'choosing' situation, Peters says " ... if he is deliberating about the characteristics of A rather than B in order to choose, he must presuppose that there might be features possessed by either A or B which would make his choice correct or wise" (p.122). (We may note in passing the connotations of conformism in Peters' use of 'correct' in this context.) "Choice", says Peters, "cannot be a matter of individual fiat if there is to be a possibility of its being shrewd, wise, correct, intelligent, or far-sighted". Why ever not? we may ask. Surely choice must always be a matter of 'individual fiat', otherwise it is not choice. And surely individual choice does turn out to have been shrewd, wise, etc. at least sometimes - we can agree that it does not always do so. But why is Peters so much against 'individual fiat'? What does this phrase mean?

One possibility is that deciding by 'individual fiat' means ignoring the experience of other people which most of us normally make use of, vicariously, in a great many of our decisions. If this is what Peters means, he would be correct, presumably, in suggesting (as he may be doing) that a person is unwise to neglect or reject other people's experiences - but this would not entail that a choice made by 'individual fiat' must necessarily turn out to be disastrous. And presumably the individual can learn by his own experience (even if, totally by himself, he would not learn much).

Another possibility is that, while the individual could presumably make a choice without reference to other individuals or to the social group and its traditions, this choice could not be talked about, communicated, etc. unless the individual were able and willing to 'talk the language' of the social group. Only under these conditions could his choice come to be called shrewd, wise, etc. Now while this and the previously-mentioned possibility overlap to some extent, e.g. the individual cannot draw upon the past experience of others unless that experience can be communicated to him, in neither case is the actual choice anything other than a matter of 'individual fiat'. Various considerations, of fact or of other individuals' or the total group's values, can be brought to one's notice - but the choosing individual must still choose whether to be swayed by them (or, by which of them to be swayed). Even to 'do what one is told' involves choice and decision: the decision, namely, to do what one is told. Only if one acts unconsciously in a literal sense (and this can be the result of any of a variety of causes) can one be said to

be not acting or deciding by 'individual fiat' - and in this case one could not be said to be 'deciding' or 'choosing' at all!

The foregoing is not offered as a conclusive and exhaustive rebuttal of Peters' argument, but rather as a suggestion, backed up by a sketch of an analysis, that he may be mistaken in his analysis of our actual usages. His anxiety against 'individual fiat' might be seen as another symptom of conservatism and authoritarianism. Our main intention at present is to ascertain and be able to discuss his position with regard to 'rational' and 'reasonableness', however, and it is in relation to this that the 'individual fiat' issue assumes importance.

As we read further in Peters' chapter, it becomes apparent that his emphasis with regard to choice and choosing is not so much on making the choice (which is, as suggested above, inescapably a matter of 'individual fiat') as in justifying it. If we read Peters with this in mind, much of what he says which seems obscure or paradoxical can be made intelligible, e.g. his emphasis on choice in relation to "social life" (p.122). One's first thought on reading this and similar passages is that choices can be and often are (and often have to be) made without reference to any particular considerations of 'social life'. (It is their emphasis upon the individual's 'cosmic solitude', the 'leap in the dark' involved in important life choices, which has been influential in Existentialist writings, see Grene 1959 for an accessible and perceptive discussion.) But the question of justifying one's choice, whether before or after the event, does presuppose a social context and, as Peters insists, general principles. "This is what any individual assumes", says Peters, "who is seriously worried about what to do" (p.122; emphasis added). 'Seriously', which was noted earlier as occurring rather enigmatically in Peters' argument, can now be seen as a device for indicating the individual's awareness and concern for a social context and its associated principles of conduct, also his awareness that justification may be demanded of him. Peters goes on in his next sentence to say: "There must, therefore, be general principles on which he can rely in making his choices."

But what sort of 'principles' are involved that can be 'relied' upon so confidently? For chosen actions to turn out well - and this is partly what one is concerned about when asking "What ought I to do" - one must understand what might in a general way be called the 'laws of

Nature', i.e. what sorts of consequences generally follow from what sorts of causes. But the 'laws of Nature' which underlie human behaviour whatever they may be, seem not to be understood well enough for us to rely on them: human interactions are notoriously fraught with unexpected difficulties and disasters. And even the 'laws' governing inanimate nature (it may be as well to keep in mind the metaphorical sense of 'law' in this context) are not always too reliable as we apply them in everyday real life, e.g. Newton's laws of movement don't guarantee that we avoid having accidents with our cars, falling off high buildings, etc. And anyway, though an understanding of the relevant 'laws of nature' is necessary if our actions are to turn out to be characterizable as "shrewd, wise, correct, intelligent, or far-sighted", they are in some sense irrelevant or peripheral to questions of justification. The need to distinguish, within judgements of morality, between questions of fact and questions of value, has been generally accepted for a very long time (see Hare 1954, among many other works). Let us merely stipulate that while knowledge of facts (including 'laws of Nature') does come into questions of justification, even if we equate 'principles' with 'laws of Nature' we cannot be said to be able to rely on our knowledge of facts in making our decisions about what we do. (In one sense we do rely on such knowledge, simply because we must: we haven't anything better to go on. But we find we are often mistaken, things turn out differently to what we expected, hence we cannot in a strong sense rely on 'principles' interpreted as above.)

What then can Peters mean when he talks of there being "general principles on which a person can rely" in justifying one course of action as against another?

In the immediately-following passage (p.122) he enunciates a principle of "fairness or justice" which, without going into detail, asserts that all persons ought to be treated alike unless there are relevant reasons for treating them differently. Clearly, this principle on its own, as Peters recognizes, will not suffice for determining relevancies from irrelevancies - on its own, this principle of "fairness or justice ... is really the principle that there should be principles", namely, principles in terms of which the questions of relevant/irrelevant could be decided. Appeal to the 'principles of justice' on its own is therefore inadequate as a way of justifying a choice of action: the appeal must be to the 'principle of justice' plus some other principle(s).

Peters himself puts the matter this way:

"Both positive and negative aspects of the principle of justice have to work with the notion of 'relevance'. This indicates its formal character as a principle for the creation and application of principles ... But the considerations which make a difference relevant cannot be determined by the principle of justice itself." (p.123).

"Another way of bringing out this second-order character of justice in relation to other principles is to note that there is never an issue of justice if a situation is not in some way rule-governed." (p.123).

Justice is determined, then, in terms of whatever 'principles' or 'rules' are decided to be relevant. "It is a principle regulating the operation of rules, which stipulates the desirability of categories to be made on relevant grounds and the undesirability of exceptions made on irrelevant grounds" (p.124). With regard to the rules which are to be justly operated, however, Peters notes that " ... there must always be a question about the desirability of the rule which is to be discriminately and impartially ['justly?'] administered." (p.124). In other words, the other principles and rules, over and above (or, apart from) the principle of justice itself, have to be decided upon - and presumably only those that are 'desirable' should be accepted and administered. Peters notes that a torturer can be 'just' in the sense of distributing torture impartially; but "The rule relating to torture ... is wrong" i.e. torture itself is wrong, and a rule allowing or enjoining it is undesirable, unjustifiable.

Since Peters also says: "Justification is a specific form of human activity which is tantamount to the demand for reasons" (p.125), it looks as though what is justifiable is that which is 'desirable' and for which 'reasons can be given'; while what is unjustifiable is the opposite. However, an important and very basic ambiguity begins to emerge at this point, with regard to both 'desirable' and 'giving reasons' (especially, in the latter case, if 'giving reasons' is taken to mean 'subsuming the action under a general principle' or something similar).

Let us look first, briefly, at 'desirable'.

One possible interpretation of 'desirable' e.g. in the proposition 'X is desirable', is that X is actually desired. But desired by whom? If the answer is: 'desired by anyone, by someone or other', if 'desirable'

means 'desired by someone or other', then we are on dangerous ground if we try to rule out any type of conduct - for how can we be sure that there is not someone, somewhere, who desires it? (Even torture!) In a similar way, if what is justifiable is equated with what somebody, anybody, could 'give reasons for', then any conduct is justifiable. Even if 'giving reasons' is taken to mean 'giving a general rule under which the particular action/conduct could be subsumed', it is still possible to formulate a general rule to cover any action whatever. 'Generalizability', like Kant's 'universalizability' is not a sufficient justification. (See Korner 1955, chap. 6; Paton 1953).

If we change our interpretation of 'desirable' from 'what is in fact desired by somebody or other' to 'what could or might be desired', we open the door even wider (if possible) to any and every type of action/conduct being justifiable. This is obviously contrary to both virtually every notion of 'morality' (if any and every action were acceptable, there would and could be no morality, and there would be no problem of moral choice), and also to Peters' intentions. It is obvious that some way must be found to 'close down' the filter of what 'acceptable', 'desirable', and similar words can mean in the relevant context.

If we can take 'desirable' as meaning 'what could or might be desired, not by some individual, but by a society', it seems we might be able to make progress. It might be assumed that by taking the society as the unit, rather than the individual, we would be able to 'iron out' the individual idiosyncrasies which might otherwise cause confusion. However, we still have ^{an} ambiguity. In referring to 'a society' we could mean, not an actual, existing society, but rather a possible society which might perhaps come to exist at some time in the future. 'What is desirable' would then mean 'What would be desired in that society'. No doubt Peters, and others, could ask for reasons why some things would be desired, other things not desired, in that society - and everyone could argue about the merits of the reasons that could be given, one way or another. The basic point about this interpretation of 'desirable', however, is that insofar as it refers to a possible society which is not in anyway specified, we have no means of knowing what would be desired, and what not desired, until someone actually specifies this in detail. In the absence of such specifications, 'desirable' is meaningless. Peters himself offers no such specification.

The alternative interpretation of 'desirable' with a societal frame of reference must, then, involve an existing rather than an hypothetical

society. Here we can clarify another ambiguity as we go along, for 'desirable' in relation to an existing society can mean either:

- (a) 'Actually is desired within this society'; or
- (b) 'Ought to be desired within this society'.

We seem to be making progress here. If we are talking about what is (or is perhaps only alleged to be) desired in a real existing society (or what is 'regarded as desirable' - though this formulation does plunge us again into some ambiguity), then we can find out, by appropriate research, what is desired; and even, with a little more complication, what is regarded as desirable. Any good textbook of social anthropology will tell us how to go about the appropriate type of research. There is no difficulty in principle. There is a difficulty, however, not in finding out what people desire (that can be done by watching what they actually do, as well as by asking them using various techniques subtle or unsubtle), but in coping with diversity within a society. Few actual societies are homogeneous. What percentage of the people within a society have to desire something (or regard it as desirable) before we can say that the society desires it or regards it as desirable? 100%? This is pretty unlikely to occur. 51%? This comes close to the 'democratic' assumption of majority rule (in fact under some particular 'democratic' constitutions at the present time a government elected by only 37% of the voters can, and does, claim to represent the whole society!) - but what about the other 49% (or 63%)? Clearly, while in practice a particular group within a society may be able to enforce its views as 'the society's views', this cannot be accepted as a rational or philosophical justification for ignoring even a small minority. Peters offers us no justification for adopting such a procedure - but then, he seems to be unaware that there is a problem about 'desirable' at all.

Looking now at alternative (b), that 'desirable' might mean 'ought to be desired within this society', we can note that it is explicitly prescriptive (rather than descriptive, as was alternative (a)), hence different means must be used to justify it. Empirical or quasi-empirical evidence was appropriate with regard to (a) - for (b), Peters' notion of 'giving reasons' appears to be apposite.

But what does 'giving reasons' mean, in practical terms, for Peters?

Here we come back to his notion of actions being 'rule-governed'. To 'give reason' for an act so that it can be 'justified' means, apparently,

that a 'rule' or general statement under which the action can be subsumed, must be formulated. No problem so far. But as pointed out earlier, any and every act can be given a general description under which it would be subsumable. 'I can murder my neighbour so that I can take his car' can be generalized to 'People can murder their neighbours so that they can take their cars or other goods' - but nobody, surely would accept that mere generalizability is sufficient to make the action morally acceptable?

What Peters appears to assume, and what would make his discussion square with actual practice, is that the 'rule' or general statement under which the act is subsumable is not just any general statement but one which is actually accepted within a given society. We could again, at this point, go through the possible interpretations of 'acceptable' just as we did for 'desirable' - but they would, I suggest, exactly parallel those earlier explorations. The same distinctions, the same sorts of implications, would be found to apply. Summing it all up, if Peters wants to justify a decision or action by 'giving reasons', then either:

(a) The mere giving of reasons is sufficient justification - but in this case any and every act is justifiable so long as someone is prepared to 'give reasons' i.e. talk about it; or

(b) The reasons given have to be 'acceptable' or 'desirable'; and as we have already seen, these words can mean either 'might be accepted by someone or some society, somewhere, at some time' (in which case, again, virtually anything is justifiable) or 'actually is accepted by an existing society or segment of society'.

In this last case, Peters' notion of justifying does make sense, whether couched as 'giving reasons' ('acceptable' ones) or as 'appealing to rule or principle' (again, rules and principles which are actually accepted within a particular extant society or portion thereof). It makes sense, but it is clearly a very conservative position that Peters has adopted; and to the extent that he himself is in effect claiming to speak for the society, an authoritarian one.

The foregoing analysis can make no claim to have proved the case conclusively. It is suggested, however, that such that Peters does say, and what he doesn't say, is consistent with the essentially conservative position and methodology that has been ascribed to him.

What he does and does not say must be examined against a background of what might have been said. He repeatedly makes statements like " ... it is hoped to develop a particular form of argument to justify the manner and matter of education" (p.117). Why should he see philosophy of education in this way? Why not say "It is hoped to expose and discuss some of the underlying assumptions of current education, so that any conflicts and inconsistencies between them can be resolved: so that, in the light of the general principles which emerge, changes and improvements in practice may be suggested"? This is, of course, an explicitly extreme alternative, in that the possibility of change and improvement in educational practice is specifically mentioned - but much the same philosophical approach could have been taken without actually mentioning the prospect of practical change. As another of many possible examples, Peters' focus on 'the educated man' is either tendentious, or empty. Either we know what sort of man (or woman?) is being held up before us - in which case the notion is meaningful but is likely to involve preservation of the status quo: we are being asked to preserve the type of education which produced the example offered. Or else we don't know which of the many different types of 'educated men' is supposed to be the exemplar - an Eskimo, Australian aborigine, Shia Moslem from Iran, a Marist Brother from New Zealand, a Taoist scholar from Japan, a Brahmin from Calcutta, each of whom may be highly educated in the sense of being supremely competent within his own society and environment - and if Peters doesn't specify which type of educated person he is talking about, we are either left in doubt or (if we come from Peters' own society) we may by default accept the same sort of model as he does.

What about 'rationality' and 'reasonableness' in all this? For Peters, if philosophy is rational, it has to do with 'discourse' (= 'talk', whether spoken or written); and if we can assume that philosophy is the epitome of rationality and also a 'second-order, spectatorial activity' (Hirst 1963, a view which Peters also seems to espouse), then the height of rationality is 'talk about talk'. But obviously it cannot be mere talk. Philosophical talk must itself be rational. What does 'rational' really mean, then, for Peters? Presumably he would want to assert that his own discourse is rational, especially written material which he has had time to think over and amend where necessary. This would apply, no doubt, to the chapter from Ethics and Education which we have been examining. 'Giving reasons' is apparently rational. But, as we have seen, just giving any reasons won't do - they must be 'rule governed' and

logically consistent. Although he does not say so, presumably Peters would demand that all acceptable reasons should be mutually consistent with each other, i.e. they must be able to form a logically coherent system, even though they would not actually be enunciated as such. But, to be acceptable and thus to be used actually to justify, the 'reasons' and the sets of rules (specified or unspecified) must as we have seen be consistent with the rules and reasons which are in fact accepted in an existing society, namely the society of which Peters himself is a member. We have explored in some detail what he has said on an issue, 'equality', which he himself takes to be important: it therefore seems fair to assume that Peters has endeavoured to exemplify, as best he can, the 'rationality' and 'reasonableness' which should be characteristic of philosophy. In short, presumably he has done this piece of philosophy as well as he can. What sort of 'rationality' does it seem to exemplify?

We can say, perhaps, that Peters has exemplified what he advocates, in the sense that he has 'given reasons' (he has talked to us about the topics in question) and he has appealed, though not very explicitly, to 'rules'. But having said this, we must also say that 'giving reasons' and 'appealing to rules' are not, in Peters' usage, two things (as we might have thought), but one. 'Giving reasons', for Peters, is 'appealing to rules'. All along, the 'reasons' he offers us are in fact appeals to 'rules', namely the 'rules' of linguistic usage and of belief, attitude, etc., of Peters' own society. He manages to disguise this social conformism, mostly by the use of ambiguity either of word or of the construction of his whole argument.

Take the word 'justify' for example, which is one that occurs frequently in his discourse. This, in terms of the task/achievement analysis which Peters has adopted from Ryle (see Marshall 1976 for a recent discussion, and refs), would probably be taken by the average reader to be used in the 'task' sense. When Peters talks of 'justifying' something, especially in the context which he gives to the present chapter of choosing, trying to answer the question "What ought I to do?" (see p.121), the reader would normally think of 'justifying' in terms of looking for reasons which would help towards making a decision. But before we can decide whether a particular reason is for or against doing a specific action or adopting a particular policy, it has to be decided which reasons (out of an indefinitely large class of 'considerations', as we might call them) are relevant. Peters very properly

emphasizes the need to decide relevance, as we have noted. But if we are really involved in the task of deciding what to do, i.e. if we have not yet decided, then we do not know which out of the many possible considerations are really relevant to the possibilities in question, and which are not; i.e. we have not yet decided which considerations are reasons (for or against). The consideration "Rain is predicted for Thursday" is relevant to the decision whether or not to have a picnic, play tennis, etc., on that day - it would in fact be a reason for not going or playing, though not, of course, necessarily a conclusive reason - whereas it is not relevant to a decision whether one should enrol for a Mus. Bac. degree, invest in government bonds, or vote Social Credit at the election. It is not a reason for or against decisions of these sorts (not usually, anyway).

Now if Peters were really involved in 'justifying' in this task sense, he would need to examine a pretty wide range of considerations relative to education, to be able to decide (and to allow his readers to decide) which of them were and which of them were not relevant. But he does not do this. On the nominated topic of the chapter, 'Equality', he could, if he were really embarking on a 'task', have laid out a number of different possible interpretations of 'equality', he could have gone into the various implications (pro and con) of each one, and he and his readers could have made up their several minds about the various possibilities. Peters could have stated which interpretation he preferred, and could have given his reasons; but if he had laid out an array of other alternatives, readers could have come to their own, and perhaps different, conclusions and choices.

But Peters does not do this sort of thing at all. We have seen what he does do. And over and above the deficiencies in that, it seems highly significant that when he does mention a paper in which possible alternative meanings of 'equality' are explored (a paper by Lieberman 1961, discussed by Peters on p.140), he dismisses it virtually out of hand. For example, one of the meanings/interpretations put forward by Lieberman is, says Peters, "a sham".

This is highly significant, and must be investigated.

"Lieberman suggest two meanings for 'equality of educational opportunity. One is that A and B have it when 'they live under conditions which do not provide either person with any material advantage over the other in selecting or pursuing his

educational goals. He argues that such equality is almost non-existent, though many imagine that it exists. The other is that A and B have it 'when the material advantages which one of them possesses over the other in selecting or pursuing his educational goals cannot be removed without endangering other important values.' Because 'other important values' vary from person to person, so also does the interpretation of 'equality of educational opportunity'. This second meaning, however, is a sham. For people have not got it in any actual sense and are not prepared, for reasons that have nothing to do with equality, to take the steps necessary to ensure that they have it ..." (p.140).

Now this is an extraordinary state of affairs. Even if we were to allow that Peters is correct in asserting what he does in his last sentence, this does not in any way change the validity of Meaning2, as a possible meaning or interpretation of 'equality'. Even if the relevant state of affairs were not actualized at all, it is still a perfectly intelligible meaning, with no 'sham' about it; and it seems quite likely that a considerable number of people would actually support it. (I would, for one; and so presumably would Lieberman.) But the point is not whether or not anyone would support this interpretation: the issue is whether or not Lieberman has given an intelligible and plausible account of a possible meaning/interpretation of 'equality of educational opportunity'. This I suggest he clearly has done, by any standards. And in fact Peters tacitly admits as much. He says, in the last sentence quoted (above) that people do not actually, as a matter of fact, have 'equality' in this sense. For him to be able to assert this, he must have been able to understand Meaning 2. It was not unintelligible to him. Therefore, as a possible meaning/interpretation there was nothing wrong with it: Peters could and did understand it. He did not accept it, of course — but he is entitled to call it "a sham" just because he did not accept it? Is it proper for a philosopher to call 'sham' whatever he happens not to believe in?

But even worse, perusal of the pages of Peters' chapter immediately preceding his mention of Lieberman reveals that Peters has actually said things pretty similar to Lieberman's Meaning 2. For example Peters says:

"Arguments for adjusting institutions in order to remedy such obvious sources of injustice depend upon contingent factors which are more the concern of the social scientist and psychologist than of the philosopher. They only get philosophically

interesting when a question of principle is raised about the limits to which equalitarians are prepared to go in order to remove such actual sources of inequality" (p.139).

This last sentence raises exactly the same issue as does Lieberman's Meaning 2 — why then does Peters call Meaning 2 "a sham" when he himself has been dealing with the very same issue? This seems irrational, to say the least.

One possible way of resolving this difficulty would be to say that Peters seems to have difficulty in distinguishing between questions of meaning/interpretation and questions of fact. He goes on, after the paragraph about Lieberman on p.140, to say:

"The obvious fact is that, descriptively speaking, there is no equality of opportunity and never can be unless equalitarians are prepared to control early upbringing, size of families, and breeding. Without taking such steps there will always be ineradicable differences between people which will affect how any system works in practice."

Is Peters here revealing an underlying belief that inequality of opportunity ought to be eradicated in this way?

He goes on:

"Were there not such differences the principle of equality would have little point ... what makes differences relevant is, as has been shown, a very complex matter which necessarily involves recourse to other principles. [My emphasis added; what price Lieberman here? Is not this one of the main points of his Meaning 2?] To extend the meaning of equality of opportunity in the way suggested by Lieberman has the effect of soft-peddalling the importance of such other principles in discussions of educational provision" (pp.140-141).

Really??? One would have thought precisely the opposite, viz. that it is Lieberman's Meaning 2 which explicitly recognizes the existence of conflicts between principles and the need for compromise.

Peters' next sentence reads as follows:

"It [i.e. 'extending' the meaning of 'equality ...' as in Meaning 2] is to fall in with the modern fashion of assuming that the only moral issues involved in educational provision are issues of equality" (p.141).

But Lieberman's Meaning 2 explicitly contradicts this! It is he who brings out the possibility of conflict between various principles or

values. How can Peters' possibly ascribe 'soft-peddling' to Lieberman, let alone 'falling in with the modern fashion ...'? Could there be an element of 'psychological projection' here, in the Freudian sense? Is Peters projecting onto Lieberman his own view, perhaps even his own practice, of falling in with the fashion and demanding equality at virtually any price? Certainly some of his statements appear to support the view that he wants equality to be imposed, if necessary by stringent methods, e.g. his calling Lieberman's Meaning 2 "a sham", and saying that if a state of affairs corresponding to Meaning 2 were to be called 'equality ...' this would "misrepresent the facts of life in order to retain a shibboleth". (p.140). As to its "misrepresenting the facts of life", one would be inclined to say that this is simply untrue: equality of educational opportunity is in fact limited by people's adherence to other principles which conflict with the equality principle. To say that this is the case is not, of course, to say that one agrees with adherence to the other principles — but one can hardly refuse to recognize the existence of other people's principles just because one disagrees with them. Peters goes on:

"The plea for 'equality of opportunity' is more properly understood as either an attack on irrelevant aids to opportunity (e.g. wealth) or as a demand for replacing unreasonable by reasonable grounds for providing access to opportunities" (p.140).

Apparently for Peters 'equality' and 'wealth' are mutually exclusive, and wealth is not only an 'irrelevant aid' to educational opportunity, it must also be 'attacked' — the means of attack are not specified — alternatively or in addition, it is not 'reasonable' that wealth should aid educational opportunity.

(It might be pointed out that without wealth-in-general, there could be little education at all, as we know it. But the 'wealth' which Peters wishes to attack is apparently wealth held by individuals. It is perhaps a weakness in his position that he gives no indication as to the basis and means by which individual wealth is to be reallocated. Since he has stated his opposition to the influence of individual wealth with regard to educational opportunity, even a brief indication of what he sees as a 'reasonable' method of dealing with this issue would have been helpful.)

Reviewing Peters' position at this point, then, it seems clear

that he has a very strong commitment to equality of educational opportunity (dare we suggest that there is a compulsive quality in this commitment? — it does seem to distort his argument and his judgement): yet along with this he seems unwilling to set out for his readers any alternative interpretations of 'equality of opportunity', and to this extent he seems unwilling to practice equality of opportunity, i.e. provide it for his readers.

Are we yet in a position to come to conclusions with regard to Peters' use of 'rational' and 'reasonable'? On his own showing, for a policy to be reasonable it must be able to be shown to conform to a rule. This rule must be an actual rule in a real society, not just a possible rule in an hypothetical society, otherwise Peters' procedure of argument would, as we have seen, need to be very different from what it actually is. (Also, depending on the precise interpretation given to 'possible rule', the way might be opened for anything and everything to be justifiable.) It may be suggested that the actual rules and the real society are in fact those 'rules' (including values, beliefs, etc.) accepted not in contemporary British society as a whole, but in whatever subdivision of it that includes Peters. It is not possible to prove, in any strict sense, that this is the case: there are too many obscurities and ambiguities in Peters' writings. One can only suggest that readers check up on what has been argued here by reading Peters' own works. But a case could be made, and justified (in the 'task' sense, i.e. justification could be offered) at greater length than has been attempted here, for saying that 'reasonableness', in Peters' writings, means 'conforming to Peters' views', and 'rationality' means 'being prepared to talk like Peters, on the same grounds, within the same frame of reference, etc.'.

In relation to this, and to indicate the grounds on which I believe an extended indictment of Peters' methodology could be developed (consistent with but going beyond the considerations already offered), I can say that there are two features in particular that 'bother me' as a philosopher. They are not unrelated, though it is convenient to distinguish between them; and I shall amplify my discussion of them in later Chapters. These two features are:

- (1) Peters' failure to provide explicit exploration and discussion of alternatives of interpretation of the words and concepts he uses; and
- (2) His tendency to treat philosophical issues as though they were matters of fact.

This last point brings into the open the question of the differences between philosophy whose function, on most philosophers' accounts, is not simply to describe facts, and science which is concerned with describing facts. This general question and some issues relevant to it will be taken up in the next Chapter.

(Critical discussions of various aspects of the published works of Peters and other 'London' philosophers of education have been offered by a number of scholars. My main intention in the preceding Chapters has been to display some of the more fundamental aspects of Peters' own writings, with a few lines of critical evaluation which are relevant to the general theme of this thesis, viz. the relative merits of a Wittgensteinian language-game approach; so I have not attempted to integrate with the extant critical literature, in detail. I have been concerned at the extent to which Peters (and his associates) have been accepted uncritically and very widely within Educational Philosophy and Education; and I have been concerned to develop an alternative major strategy, rather than engage in tactical battles; hence my seeming neglect of some of the critical literature. For an entry to this literature, however, the reader may be referred to any of the following:

Cooper 1973, Edel 1972, Goldstone 1970, Kennedy 1975, Kleinig 1973, Marshall 1975, Murphy 1973, Robinson 1972, Soltis 1978; and Powell's (1974) Select Bibliography (3rd.ed.), also any recent issue of the main journals mentioned by Powell.)

CHAPTER FIVE

"SOME FUNCTIONS OF PHILOSOPHY IN SCIENCE EDUCATION".

If for Peters 'rationality' and 'reasonableness' mean 'being able to give reasons in conformity to the currently-accepted rules', it appears that there may be considerable similarity between his position (within the disciplines of Philosophy and Education, and within his own societal context) and what Kuhn (1962) has called 'normal science' as distinct from 'revolutionary science'.

In 'normal science' also, as with Peters, there is emphasis on the acceptance of a particular framework of basic assumptions, accepted procedures (i.e. 'rules'), and so on. Not all of these are necessarily explicitly stated or even consciously held — as indicated earlier (Chap.2), many of our most influential beliefs exert their strongest influence precisely because they are unconscious. Collectively, within a science, a particular set of such assumptions, beliefs, etc., has been designated by Kuhn as a 'paradigm'. Masterman (1970) in a searching analysis of the 'paradigm' concept, has discerned a number of variations in its interpretation. Some of the finer distinctions need not concern us here, but it will be convenient to distinguish two major interpretations. The usage of 'paradigm' indicated above, viz. the total set of assumptions and beliefs regarding rules, techniques, what is regarded as appropriate subject-matter, etc., may be denoted, for present purposes, as 'Paradigmatic Assumptions' or as a 'Common Assumptions Paradigm' (CA Paradigm). This will be contrasted with another significant major usage, 'Paradigmatic Achievement' or the 'Public Demonstration Paradigm' (PD Paradigm): the public demonstration, by means of argument and/or experiment, of the significance and heuristic value of a particular set of Paradigmatic Assumptions (CA Paradigm). Paradigmatic Achievement will be explained and illustrated more fully in a later Chapter — for the moment our concerns lie mainly with the 'Common Assumptions Paradigm'.

'Normal Science', for Kuhn, involves by definition work undertaken within a particular Common Assumptions Paradigm. All of the workers within the science at a particular time — researchers, students, and teachers — accept the Paradigm, and their work consists of using the accepted techniques, innovating within limits that are broadly set by the

Paradigmatic Assumptions, and applying the techniques to subject-matter (phenomena) which are again limited in a general way by the Paradigmatic Assumptions. (It will be appreciated that while the 'normal'/'revolutionary' distinction can be made relatively clear-cut at the conceptual level, in practice it may sometimes be difficult to characterize a particular piece of research, or a particular researcher, as definitely one or the other. Often 'normal science' research leads towards a 'revolutionary' revision of basic ideas, perhaps even contrary to the intention and the knowledge of the researchers involved. Koestler (1964) shows how the great 'Copernican revolution' occurred almost in despite of the people who effected it. But there is no need to use the distinction on an 'exclusive either/or' basis at the descriptive level. Kuhn himself characterizes the progress of normal science as involving the accumulation of new factual/observational knowledge, the application of the accepted paradigm to new subject-matter (this is always in principle tentative, and may turn out to be either unsuccessful, or to lead to 'revolutionary' changes), and enhanced precision of measurement (Kuhn 1962: 23-34). Thus the basic conceptual framework is set and accepted (perhaps only tacitly and uncritically, by many practitioners), and the actual work of normal science occurs in terms of technique, at the tactical rather than the strategical level. Even innovation in technique occurs within an accepted framework of common aims, criteria of relevance and acceptability, etc.; and this applies also, as explained earlier, to science education as well as to science in itself.

'Revolutionary science', by contrast, involves, besides the use of techniques at the level of normal science, also conceptual-level clarification and alteration along the following lines:

- (a) The basic assumptions, or at least some of them, of the hitherto-existing normal science have to be exposed and made explicit.
- (b) Possible modifications of these, and the implications of such modifications, are explored and appraised. This, it must be emphasized, relates to the total set of basic assumptions not just to any particular one or a few that may have been formulated explicitly. What is in question here is whether some assumptions should be rejected or modified, or whether an addition or additions should be made — and in considering such possibilities, it is necessary to keep in mind the interaction between the assumptions as they may be stated in any particular explicit formulation.

Once the implications of the changes have been explored at the imaginative or theoretical level, the move back to the level of technique can be made in order to:

(c) Test the new CA Paradigm by means of experiment and/or observation; and if such tests are successful — especially if they are completely decisive in refuting the old CA Paradigm (or Paradigms, if several had been in competition) — they plus the new CA Paradigm will come to constitute a new Public Demonstration Paradigm. In short, the successful Paradigmatic Achievement leads to general acceptance of the particular set of Paradigmatic Assumptions which actuated it.

Now while a scientific revolution as outlined above usually does gain wide acceptance for the new CA Paradigm (Paradigmatic Assumptions), there are usually, as Kuhn has emphasized, a number of individuals who, despite being acquainted with the Paradigmatic Achievement, still do not accept it as decisive and thus do not accept the new Paradigmatic Assumptions. Hallam (1973: Chap.8) discusses some of the current dissidents from the recent scientific revolution in the earth sciences, in which 'continental drift' (plate tectonics) has finally been established. As Max Planck says, the last representatives of the 'old' paradigm often die without ever accepting the new (quoted in Kuhn 1962: 150); and this poses a significant problem. One might be inclined to 'write off' the members of a rearguard of an old and widely-rejected paradigm as merely stubborn, ultra-conservative, stupid, or senile — and this might be true of some. But to dismiss the 'non-progressives' like this is too facile. They may indeed suffer from the faults mentioned. It will bring us a step further into an understanding of 'revolutionary science', however, and of philosophy itself, if we examine more closely the 'processes or activities of philosophy' which are involved in an individual's taking up what can be called, whether in a particular science, in philosophy, or in any intellectual discipline, his or her 'personal position'.

In philosophy, an individual is said to take up a particular 'philosophical position'. For present purposes, let us equate this with adopting a position in science, as accepting a particular CA Paradigm (a particular set of Paradigmatic Assumptions) — it must be kept in mind, of course, as indicated earlier, that not all of the assumptions of the paradigm are likely to be explicitly articulated or even consciously es-

poused. But this is true also, as will be shown, with regard to a person's philosophical position; so for the moment we shall conflate 'scientific position' (i.e. acceptance of a particular set of Paradigmatic Assumptions) with 'philosophical position', and examine what is involved in attaining the latter.

There are two distinct usages or meanings of 'philosophical position' which are relevant to the present discussion. On the one hand the phrase may denote a particular doctrine, of greater or lesser generality, as when we remark that 'The "expressivist" position has been under attack', or 'His position is, that a statement's truth is a function of the number of people who believe it'. On the other hand, we can and do also talk of a person's, an individual's, philosophical position as being something of even greater generality than is common in the other usage. In this sense, his 'philosophical position' might be thought of as a kind of summation of his several philosophical positions in the first sense. That this 'summation' view can be somewhat misleading will be apparent as we proceed; but for the moment the only point being established is that we do use the term 'philosophical position' with reference rather to the individual than to any one of the particular doctrines he holds. This second usage of 'position' equates with the usage of 'philosophy' in the personal sense, as when one says 'Smith's is an idealistic philosophy', or 'Green's philosophy tends towards intuitionism'. I propose for convenience to use the phrase 'particular position' to replace the first usage of 'philosophical position', 'personal position' to replace the second.

There is another difference which is most important. A 'personal position' would tend in principle to be less explicitly formulated, to have more areas of uncertainty, than does a 'particular position'. At any given time, one's 'personal position' is normally not determined with regard to various specific issues to which one has not yet given thought. One can give thought to them if required: but one's position with respect to them could not be regarded, even before giving thought, as entirely open, since the particular position eventually adopted with regard to the issues in question will be affected by one's particular position on other matters. The essence of a particular position, it might be said, is that it is pretty clearly delineated — but one may not know whether or not one

holds it; whereas one's personal position is certainly held, it is constituted by what one adheres to — but precisely what one holds may not yet have been formulated, may not yet even be known.

Let us consider some hypothetical situations regarding philosophical positions in general.

A. We say 'Smith is a logical positivist', (see Ayer 1936) but it turns out that he has done no more than read about logical positivism in a popular magazine; and when he is presented with a few arguments against logical positivism but in favour of, say, transcendental idealism he immediately says 'No, I'm not a logical positivist, I'm a transcendental idealist'. Are we willing to admit that Smith had adopted a philosophical position here? It seems unlikely. But why is it unlikely?

B. Jones is a logical positivist. He has read a statement and some discussion of the 'verifiability principle' in a chapter in a book about philosophy. The verifiability criterion seems to make sense to Jones; and though we assail him with a barrage of transcendental idealism, and though we prove to him that the statement of the verifiability principle is itself unverifiable, he obstinately refuses to abandon it. 'How do you know? How can you verify that statement? It doesn't make sense to me', he says to every idealist argument we put forward. Has Jones adopted a philosophical position? It would appear that he has; though we might query how far it could properly be described as 'philosophical'.

C. Brown, too, is a logical positivist. Although he received no formal academic training in philosophy, he has read a great deal in the subject over a number of years, and he finally adopted the positivist position after having been in turn a Platonic idealist, a Cartesian dualist, and a Santayanan naturalist. (!) In argument about logical positivism Brown is a formidable antagonist. He does not simply dismiss or ignore arguments against his own views, on the contrary he listens attentively and allows them full weight — but he can develop arguments, sometimes of great subtlety and ingenuity, to defend his own positivist position. His positivist arguments may not always be convincing to his opponents, but they are seldom trivial; usually people have to admit that there is 'something in them'.

Brown's position is unquestionably a philosophical one.

On the basis of the foregoing it is suggested that one's personal position, and whether one has a personal position at all, is to be decided not merely in terms of subject-matter, the actual beliefs that one holds, but in terms of one's behaviour. A personal position involves both tenacity in adherence to it, and competence in arguing for it. Absence of either tenacity or competence tells against one's claim to hold a personal position: Smith has neither, Jones had tenacity but little competence, Brown had both. (Facility in argumentation, without adherence to a 'particular position', would seem the opposite or negation of 'holding a personal position' — this combination has therefore been neglected.) Competence in arguing for one's position is a product of personal ability and the quality of one's philosophical training.

Further, it is clear that one's personal position is not arrived at, or adopted, by argument, but only through argument. By this I mean to point out, that logical argument can lead one along the entailment paths between particular positions and can show whether or not one position is consistent with another. If one position is revealed to be inconsistent with another, one or other may be abandoned, or both, or neither — but the decisions at this point are decisions which are not themselves entailed or logically determined by the arguments which led up to them. One does not abandon the position because of an argument against it which one cannot at the moment refute; but one must, of course, eventually have some arguments for one's position. These 'decisions' are the expression of the basic attitudes of the person making them. Some logical consequences of the opposite view to this must be regarded as decisive against it. If we were to suppose that the holding or adoption of any one particular position — the holding, as distinct from the arguing for — were to be a matter of logical necessitation, then there could be only one possible particular position on any topic in question, and all others would have to be, literally, illogical or nonsensical. If, for example, there were some logical necessitation behind the adoption or holding of an objectivist position, then a subjectivist statement on the same topic would not really be a statement at all, it would be a pseudo-statement, a collection of words without sense or meaning — and whatever hot-tempered objectivists may on occasion say in the

course of a disputation, this is simply not the case!

It is true that some statements must be regarded as logically necessary in terms of the 'logic of language' — 'Anything that is red is coloured' is a standard example — but statements of one's philosophical position hardly come into this class. Arthur Prior (1949) demonstrates the consequences which would follow if a statement of a particular position were necessarily true, in his discussion of Moore's refutation of the 'naturalistic fallacy'. "To represent an opponent's position in such a way as to make it not only false but self-contradictory is a dialectical triumph which can never be obtained without being duly paid for; and the price is the representation of one's own position as not only true but a truism." (Prior 1949, p.8). Prior quotes J.N. Keynes: "The denial of a contradiction in terms ... yields merely what is tautologous and practically useless" (J.N. Keynes, 1906, pp. 119-120). Thus there is presented the dilemma:

either

(a) The statement of a particular position is logically necessary — in which case its contradictory (i.e. any statement of an opposing position) is self-contradictory, nonsensical, or meaningless;

or

(b) The statement of a particular position is not logically necessary. It may (but of course need not) be significant, meaningful and substantial — but its contradictory (i.e. any statement of an opposing position) must also be significant, etc.. Further, if the statement is not logically necessary, then the position itself cannot have been adopted purely and exclusively by a logical determination, therefore its adoption must have been due to other factors besides purely logical considerations. Clearly, there is an immense diversity of such factors, many of them 'accidental' in the sense of being in themselves philosophically irrelevant (e.g. when Smith adopts a position antithetical to Brown's because of personal dislike); but the factors which are not accidental in this sense would seem to be appropriately grouped under the heading of 'basic attitudes' or 'basic assumptions'. (This is simply a generalizing of Nowell-Smith's 'pro- and con-attitudes', and should be understood in terms of his argument and provisos. See Nowell-Smith 1954, esp. pp. 111-121.

The similarity to Paradigmatic Assumptions should also be fairly clear.)

The adoption of a particular position is not, then, in the strict logical sense determined by the arguments which bear upon it. One's personal position in the more general sense must lie in an even looser relationship with particular lines of argumentation. But one's personal position is not independent of particular arguments. It is necessary to arrive at one's personal position through argument, for a variety of reasons. Firstly, there is the obvious consideration that one cannot know to what one is committed, in adopting a particular position which will have to be argued for, unless one is acquainted with a reasonable range of the arguments on either side. But this is not merely a matter of psychological preparedness, of 'forwarned being forearmed'. Much more than that, one cannot know what a particular position is except *through* the arguments for and against. But even beyond this, a particular position and especially one's personal position is actually discovered and indeed constituted by the arguments one uses and formulates in its defence. Philosophical decisions have similarities to the legal decisions made by judges under the common law, in that each decision not only applied a law or a rule, it also (in the courts through the mechanism of precedents) sets up a particular definition or interpretation of the law and thus contributes to what constitutes the law. The courts, in interpreting the law, also make it. Similarly with respect to a particular or one's personal philosophical position, in a sense one's position is formulated and revealed to oneself only as one argues for it. That this should be so is in accord with the fact that one's views normally undergo progressive modification over the course of time. If one's position were taken up in something approaching an all-or-nothing fashion, a subsequent alteration would have to be classed as vacillation, or worse; but what really happens is that one gradually comes to see more extensively the implications of a position and either accepts them (thus attaining an amplified understanding of one's position — but we might as well say that the position itself, as one holds it, is amplified or extended); or one rejects some of the newly-seen implications, which again comes down to amplified understanding or, in other terms, to saying that one's position itself has been extended by being more clearly understood. It is useful to know, besides what one does accept, what one cannot accept!

Although argument cannot in the strict sense determine one's particular and personal positions, then, the range and effectiveness of the arguments with which one is familiar must have a fundamental influence upon them. The personal position based upon desultory even though extensive reading is at best precarious; and the teacher who does not try to give his students experience of good hard arguments, spread over at least a range of particular positions, is failing in his duty to help them towards establishing their own personal positions. There is truth in the view that a philosophical position is only as strong as the people with whom it has been argued. One's philosophical position, in both the personal and the particular sense, is a function or outcome of one's basic attitudes — but one must have tested these against a large range of 'persuasive redefinitions' (see Stevenson, C.L. 1938, p.331).

So far, attention has been concentrated upon the question of 'developing a philosophical position' ('personal position' and 'particular positions'). The examination of the doctrines of the great philosophers of the past can be represented, in this context, as a form of window shopping plus 'home trials without obligation': one tries out a range of philosophical positions to see which of them one likes best. In these terms, and particularly since time for the study of the alternatives is always limited, in teaching courses in Philosophy it is usually going to be most profitable to look at fairly extreme possibilities, fairly extreme positions. In a one-year university course, two or three philosophers, say, would be as much as the average student could come to grips with; while in a three-or four-year degree perhaps nine or a dozen could be studied in reasonable depth. The skill of the teacher comes in the selection of which philosophers for which group of students, and which parts of the work of these philosophers, and so on (see Mabbott, 1966, Introduction). For advanced students, whose own philosophical positions are becoming clarified, it may be desirable to get away from extremist positions and the great figures, and instead study the more workaday productions of lesser men — especially those who are working contemporaneously. But at that stage, of course, the objectives of an education in philosophy have changed to some extent. The pupils' own positions are becoming delineated, and it is becoming desirable not to

overawe them with too much 'great work'. Also, it is desirable at that stage to put them in touch with current discussions and controversies in the learned journals, and to let them see mediocre and poor examples of philosophy as well as good, against which to judge their own performances. For by that time the practical emphasis in teaching philosophy has swung round very firmly — or ought to have swung round very firmly — to the doing of it rather than the learning about it. The development of the individual philosopher should by that time be recapitulating the historical evolution of the subject, and should have reached the twentieth-century 'revolution': philosophy as an activity rather than a body of knowledge.

If education for 'revolutionary' rather than 'normal science' were to be attempted, a study of the history of science could well be used in a similar way to the standard approach, outlined above, in the teaching of philosophy. The great figures from the past, their doctrines and the evidences and interpretations associated with these doctrines, could form the basis of teaching courses. It would be absolutely essential to incorporate adequate methodological-philosophical discussion of the relationship between doctrine and evidence: this is why courses of study are usually constituted as History-and-Philosophy of Science. If the history of science is left as a mere account, a statement of various doctrines leading up to modern views, the 'paradox of past greatness' is generated. This can be stated as a question: If the great people of the past held the ridiculous views that are ascribed to them, how did they ever come to be regarded as 'great'? Empedocles' 'earth-air-fire-water' doctrine, for example: how could people have believed such silly rubbish? Associated with this approach to the history of science is the view that the earlier doctrines were not based on evidence, were the result of mere 'armchair philosophizing'. This misrepresents both the science and the philosophy of earlier (and even more recent) times, and leads to the fallacy of 'temporocentrism': the belief that in one's own time ultimate knowledge and wisdom have been achieved. This is a dangerous form of arrogance; and if the study of history teaches anything, it teaches that arrogant complacency is inevitably followed by some sort of collapse. For example, as Toulmin and Goldfield (1962) have pointed out, physics in the mid-nineteenth century appeared to have reached the limits of knowledge:

"The distinction which lay at the heart of the nineteenth-century world-picture appeared absolute. On the one hand, there was matter: on the other hand, radiation. Between them, these two concepts exhausted the realm of physics" (1965: 295).

Yet within a few decades the 'classical synthesis' in physics was utterly destroyed. As Toulmin and Goldfield put it,

"... by 1905 J.J. Thomson and Rutherford were rapidly undermining the structure of the classical system. The existence of atoms was finally accepted, just at the moment when they proved not to be 'atomic'."

(i.e. when they were proved not to be the fundamental units of matter). As the authors in question remark:

"Except that science is a perpetually optimistic activity, the situation would have had all the elements of tragedy"! (p.303).

To give yet another elementary example of the dangers of simplicism and of ignoring methodological and interpretational issues, take the 'flat earth' question. That people should at one time have believed in the truth of the proposition "The earth is flat" is often represented as a quaint absurdity. Within our present-day frame of reference and linguistic usages, of course, it is an absurdity: for us, the word 'earth' means 'the spheroidal-shaped planet on which we live, whose orbit lies between those of Venus and Mars in the solar system...'. Thus for us the proposition 'The earth is round (spheroidal, or whatever)' is an analytic proposition, and necessarily true. But back in ancient times the word 'earth' did not have its present meaning. For the people of earlier times, 'earth' (or whatever equivalent word in the languages actually used at that time) meant something like 'what you can see if you go out and look around you as far as you can in all directions, what you see if you travel widely ...' — and in this sense, if we stretch 'flat' to include the relatively minor bumps and irregularities of hills and valleys, etc. (try plotting a cross-section of even a mountainous country using the same scale for both horizontal and vertical axes), then the earth is flat. What's more, its 'flatness' in this sense is empirically verifiable, in a way in which 'roundness' (sphericity) is not empirically verifiable, not by the ordinary person-in-the-street, anyway, though astronauts can actually see it as spheroidal.

In the light of the foregoing, it may begin to be apparent that while the continuing supporters of an old paradigm which has been supplanted by a new one might in some respects be regarded as merely stubborn or obtuse, the 'gestalt switch' which Kuhn talks of as occurring at the heart of a scientific revolution is not a simple matter. It is like the gestalt switch at the perceptual level, when we can look at one of the standard examples of ambiguous drawings and see, e.g. either a wineglass or two profile faces in confrontation — but only in some ways. The likeness comes from the fact that once we have seen the figure as being one of the alternatives, it becomes difficult to see it as the other. Also, the 'switch' itself, from seeing one to seeing the other, is only in some peculiar way and to a limited extent under the control of the will. Nevertheless, the perceptual-level switch is easier of accomplishment than the gestalt-switch of a scientific revolution, because it is easier for a person to 'come upon' an ambiguous picture when thinking of something else and hence lacking the mental set which pushes the 'seeing' in the usual way — thus the different way of 'seeing' can 'come through' — whereas in a scientific revolution the very meanings of the basic terms of the science in question usually change, often in subtle but nevertheless fundamentally important ways. And the change is unlikely to be restricted to one word, where it could relatively easily be identified, explained, argued about, and so on. The whole network of linguistic usages within the science is likely to have changed, including not only the obviously 'technical' terms but even some of the 'ordinary', 'everyday language' words as well.

(For an example of a contemporary and on-going revolution within the sciences of human behaviour, the word 'instinct' and its derivatives can be singled out as being used in different ways. Those who deny the significance and even the existence of instinct in human behaviour are usually assigning one of two meanings to 'instinct':

- (a) Behaviour which is like that of insects in its rigidity and 'mechanicalness'. People who take this view are invariably unacquainted, needless to say, with the true range of variation in the behaviour of insects (see Thorpe 1963 discussed in Stenhouse 1974: 163-4).
- (b) 'Instinct' used as a 'blanket' explanation of any and every kind of behaviour, without further specification or analysis of particular mech-

anisms. The word was used in this way by various writers early in this century, and it is indeed a non-useful usage — but for Montagu 1968, for example, to impute this sort of meaning to post-1935 ethologists is quite wrong. It is noteworthy that the references he gives are all either pre-1930, or else are 'popular' rather than scientific or scholarly writings.

The fact is, that 'instinct', 'instinctive' and their relatives have been given new meanings since the time of the 'ethological revolution' which started with Lorenz (1935) and achieved its classical exposition in English in Tinbergen (1951); and anyone who wants to talk about 'instinct' nowadays, and be taken seriously by scientists, has to learn ethology.)

The general message which seems to emerge from all the various considerations mentioned so far is that, if people want or need to be able to understand, accommodate to and even take part in scientific revolutions, they need to develop skills of understanding which are essentially philosophical. (They need not be called so.) It would appear that the general public should be educated to be able to understand 'revolutionary' changes in science and other fields ('revolutionary' is being used throughout in a Kuhnian sense, unless otherwise specified), if public decision-making involving science is to continue to be both informed and democratic. (Has it been so far? See Snow 1971.) It has been implicit in earlier Chapters that improvements with regard to the revolutionary dimensions of science are urgently needed in science education, indeed within education generally. If such improvement demands the teaching of philosophy in some shape or form, incorporated somewhere or other in the curriculum, one of the questions which must be answered is: What sort of philosophy should it be?

The next section of this Chapter will be devoted to a discussion of this issue.

It is commonly supposed that there are about as many different ways of doing philosophy as there are philosophers. Perusal of any standard work on the history of philosophy might seem to reinforce this view. But closer examination reveals that the diversity and disagreements among philosophers are more a matter of their different conclusions, basic assumptions, and particular philosophical positions, rather than differences

in how they do philosophy. If what we want to teach is philosophy as an activity (in line with the twentieth-century 'revolution in philosophy' which has stressed exactly this, see Ryle 1956, among other works), then perhaps we have less of a problem of selection among diverse models than we might have thought. Peters (1966b), for example, cites Socrates as asking "What do you mean?" and "How do you know?" — everyone who heard the late Professor C.E.M. Joad on the "Brains Trust" repeatedly saying "It depends on what you mean by '....' ...", will see similarities between fifth-century B.C. Athens and London in the 1940s. And as Peters goes on to point out (1966b: 61): "Arguments and distinctions first formulated by Aristotle and Kant jostle with more recent ones developed by Wittgenstein". In my own teaching (mainly with postgraduate students) I have on many occasions read passages from the (sometimes distant) past and had them 'identified' as emanating from living philosophers!

In some ways it may not much matter, then, which philosophers are studied in the process of learning to do philosophy ("Learning to philosophise", as in the title of Emmet's 1964 book), provided the philosophy is good stuff and well taught. (A proviso relevant here will be mentioned and discussed later.) Indeed it need not always be mandatory that the material (i.e. writings) actually used as 'raw material' upon which students are to practice their nascent skills of philosophical activity should be categorized specifically as philosophy. Especially in an introductory course, the main lesson to be taught and learned, besides that of detecting the various common flaws of argument (as in Thouless' 1930 Straight and Crooked Thinking, Susan Stebbing's 1939 Thinking to Some Purpose, and others), is that words do not have each a single unitary 'meaning' which can be looked up definitively in the dictionary, they have a variety of meanings which slide off one into the other depending on slight differences in context and manner of use. This is not in itself a new idea especially to students coming to philosophy with a good background of critical study of literature whether in English or any other language: anyone who has brushed with Empson's (1930) Seven Types of Ambiguity can be under no illusions as to the fixity, straightforwardness or simplicity of language. But many students come to university lacking this sort of appreciation; and especially (in my opinion) if they are hoping to become educators, it is desirable that their understanding should become more sophisticated, more in touch with the realities.

Students taught Philosophy within any of the many 'traditional' approaches will eventually, if they are going to become good philosophers and if they do enough philosophy, gain an appreciation of and competence at working within their medium, viz. language. In thinking to use philosophy as a major influence within education, especially but not exclusively education within the sciences, it may not, however, be practicable to consider an extensive and long-sustained training in philosophy. Time spent in one field means lost opportunity-costs in another; and while one of the major intentions of this thesis is to argue the value of philosophy to all education, there can be no thought of exclusive concentration upon philosophy. The various other subjects must have their places — and in fact one of the specific theses being developed in the course of the present argument is that philosophy must have its place as one of the major integrative influences among and within the other subjects. Clearly, an exclusive concentration upon philosophy would work against that intention. What must be looked for, then, is a way of teaching philosophy which can simultaneously be good as philosophy and also achieve the several educational goals already indicated, among them being the need for economy in time.

One of the main points in the twentieth-century revolution in philosophy, linked with its emphasis on philosophy as an activity, is that it is an activity necessarily involving and dependent upon the use of language. This has been mentioned repeatedly in the foregoing pages. Why not, then, base our educational endeavours using philosophy as an integrating influence (among other important functions), upon linguistic philosophy?

This possibility would appear rather obvious. Why has it not already been adopted and implemented, then? Is there some hidden snag which might vitiate an educational programme incorporating the methods of linguistic philosophy?

Several things have to be said here. As already indicated, there is not one 'linguistic philosophy' approach, but many. Thus a question of choice must arise. Secondly, it must be pointed out that the type of philosophy with which the largest number of educationalists are already acquainted, viz. Educational Philosophy of the 'London' school, is (or at least purports to be) a form of linguistic philosophy — and we have

already found reason (Chap. 4) to question its worth both as educative and as philosophy. (This is not to suggest that it is worthless. I have already emphasized the advance upon previous education in this field which has been associated with the rise of the 'London' school — not that the improvements have been solely due to London influences, either. Scheffler and Soltis are but two names that can be mentioned from the U.S.A., Hardie and L.M. Brown from Australia, D.J. O'Connor and many others from Britain, as being influential in educational philosophy without being 'London' products. But the important question is not whether the London approach has been better than what went before, but whether it has been good enough, and of the right sort, for the functions required of it. To this latter question an answer in the negative is being offered.)

If the London approach is regarded as inadequate, it might seem that the Oxford approach from which it derives is also, by implication, to be rejected. Indeed various considerations advanced in earlier Chapters have hinted, more or less explicitly, in this direction. This is only partly the case, however. For one thing, Oxford philosophy is generally of better quality, as philosophy, than much of the London edphil — indeed this as it stands could be taken as a crass understatement of the true position. Also, and from the educational point of view more importantly, London educational philosophy has taken up only part of the spectrum of Oxford pure philosophy: one is tempted to say (bearing in mind that one is speaking metaphorically) that some of the best bits have been left out.

The suggestion being put forward in this thesis is, substantively, that the philosophical approach best adapted to the educational requirements that have been outlined — and also possessing credentials of the highest order in pure philosophy itself — is that of the 'Cambridge' school and notably that of Wittgenstein himself centring on his concept of the 'language-game'.

A number of significant problems arise with regard to the extension of the Wittgensteinian concept of the language-game into Educational Philosophy and into education itself, and these will be discussed at greater length in later Chapters. For the moment, it seems appropriate to attempt a brief preliminary review of what appear to be some of the principal

benefits and dangers which may be associated with the language-game. (Perhaps I should emphasize that discussions at this stage are very much just part of a cursory and preliminary survey: no attempt at a definitive account is being made, indeed the thesis as a whole cannot attempt to do more than establish a prima facie case for a language-game basis for educational activity whether in general education, science, or philosophy itself.)

One point of possible misunderstanding must be dealt with right at the start. Some people have assumed, from the fact that Wittgenstein and others have talked about 'language-games', that there are language-games which can be played as separate and distinct from the 'serious' and 'real life' use of language. Language-games, on this view, would be optional: one could choose to play or not to play. This is diametrically opposed, it appears, to Wittgenstein's own view (at least as expressed in his later-published works); and it is the exact contradictory of the position argued in this thesis. The 'game' analogy is, as I hope to demonstrate, a useful and powerful one — but the analogy does not extend to optionality. Language-games are not optional. We cannot choose not to play them. They are co-terminous with, and in some senses identical with, language itself. To talk of 'language-games' is to talk about language itself. One may or may not be talking about a particular language, or about specific actual usages, but one is always talking about language as it is or might be used. The 'game' implications of optionality extend, then, to any particular usage — but the use of language-in-general is not optional.

One of the most important benefits which will be argued for, as regards a 'language-game' approach, is that virtually any and every part of education and of life in general can be seen as confluent with, in potential communication with, every other part. Both philosophical language-games and science language-games arise out of and retain connections with the complex variety of 'ordinary life' language-games. Similarly for all other 'specialist' language-games. Thus if we can make the requisite 'gestalt-switch' and 'see' all our communication, thinking, etc., in terms of language-games, the barriers between 'subjects', between all the different academic disciplines and between them and all the other activities that make up our lives, can, in principle, be surmounted. Interdisciplinary communication becomes in principle a matter of translation between language-games: the various disciplines are not intrinsically different in

kind. (People who have themselves acquired a multi-disciplinary background have always known this.) In particular, the language-games which constitute Philosophy, while they share various 'family resemblances' which enables us to distinguish them (or, more accurately, which enables us to distinguish typical members of the family) from members of other families, e.g. mathematics language-games, typical science language-games, music language-games, and so on, are not set apart from language-games which constitute the other parts and activities of our lives.

A major effect of this development is to modify very substantially the view of Philosophy as essentially 'second-order', 'spectatorial' and by implication different in kind from other activities and other academic disciplines. This 'exclusivist' view has been accepted almost universally within Educational Philosophy (see O'Connor 1957, Hirst 1963, Peters 1966 and elsewhere), though it has never been universally held (not as an explicitly articulated doctrine, anyway), within pure Philosophy. In discussing the teaching of J.L. Austin, one of Oxford's most influential philosophers, for example, Cavell (1969: 99) remarks that

"... Austin himself was, so far as I know, never anxious to underscore philosophy's uniqueness, in particular not its difference from Science; he seemed, indeed ... to like denying any such difference ..."

Nevertheless, 'exclusivist' views of philosophy have been widely held, within the profession of Philosophy as well as outside it. It may be seen as paradoxical, then, that an extension of the latest 'revolution' in professional 'pure' philosophy — namely, the Moore-Wittgenstein 'linguistic' revolution — should now be argued as leading to a breakdown in the 'exclusivist' trend in professional philosophy and to a resumption of the 'inclusivist' approach which had been prominent during most earlier times in the mainstream tradition of philosophy.

(It may be more effective, initially, to speak of an 'inclusivist' approach as having been characteristic of the individual philosopher (e.g. Descartes, Locke, Bacon, Kant) rather than applying the term to any of their particular works. The reason for this is, that some of their works are now generally classified as philosophy, others as science, political theory, etc.; and it may seem implausible to suggest that a particular work, e.g. The Critique of Pure Reason, is in any sense 'inclusive' of other fields besides philosophy. We think of these works as philosophy

pure and simple (or not so simple, in the case of the Critique). Other works by the same individual are classified as, say, scientific or theological, e.g. Berkeley's Theory of Vision or Kant's Religion within the bounds of Reason. But what must be pointed out is that these classifications are made on the basis of our views as to the meanings of 'philosophy', 'science', 'psychology', and so on. Thus we read into our categorizations of past works an interpretation which would not necessarily be accepted or even recognized by the authors of those works or by their contemporaries. As Walsh, E.B. (1961) points out with regard to Kant:

"... it would be quite wrong to think of Kant as taking only an amateur interest in scientific developments. The truth is, of course, that the dividing line between philosophy and physics or "natural philosophy" was by no means sharply defined at the time and was often crossed and recrossed by the same people. Many of Kant's first essays would be classified as scientific by modern standards, but he never kept his philosophy and his science in watertight compartments. The same had been true of Leibniz..."

But even in what would now be regarded as unequivocally philosophical works, Kant, like many of his predecessors, often mixed what many would nowadays regard as heterogeneous considerations. The first Critique and the Prolegomena contain many references to empirical science, to mathematics, to theology, and so on. These references are not extensive accounts of those other non-philosophical fields, of course — nevertheless to be able to draw useful philosophical inferences from such fields, one needs to know enough about them so that the inferences may be justifiable: one has to get the basis of the inference right, and this means that one has to know and understand the particular science, mathematics, or whatever. (More on this later — See Chap. 8.) So 'inclusivism' applies within particular works of philosophy, not just within the total output of an individual philosopher.)

The development of the 'language-game' concept within philosophy has led to our seeing philosophy as an activity, an activity of exploring and describing the various language-games which have sufficient family resemblances to be categorized as philosophical language-games. But while the boundaries between language-games within philosophy have been revealed as neither sharp nor 'watertight' — there is no one 'right' way of tackling a particular philosophical problem, there is no one 'correct' language-

game which must be played, various language-games are possible though it is also true that some are likely to be better than others from various points of view, and some language-games may not be possible at all in some contexts — it is also becoming clear that the boundaries between the whole assemblage of philosophical language-games and all sorts of other language-games are also becoming both imprecisely locatable, and permeable.

To attempt to illustrate, briefly but in several fields, what seems to be involved:

The very extensive impact within Philosophy of Science which has resulted from the works of Popper, Toulmin, and especially Kuhn (to name but three), has derived from the fact that various aspects of their theories square with the actual experiences of scientists when doing science. Popper's (1963) emphasis on refutation strikes a responsive (if somewhat plaintive and apprehensive) chord in the practising scientist who knows that he lives in perpetual danger of being proved, in public, to have been mistaken. Toulmin, especially in Foresight and Understanding (1961), has presented scientific activity as more adventurous and risky, and much more interesting, than the quasi-mechanical churning out of hypotheses (where do they really come from, anyway?) and testable inferences from them, which is what it had been made to seem by various logical positivists and formalists (e.g. Carnap 1966, Woodger 1937). Kuhn's The Structure of Scientific Revolutions (1962) has emphasized the provisional indeed the ephemeral nature of scientific knowledge (without over-emphasizing the refutation angle) and has put it in a social context of group cohesion and shared beliefs, while still allowing for competition between groups and between individual and group.

These philosophers (and others, e.g. N.R. Hanson, Feyerabend) have gained strong followings among scientists because they have achieved insights into scientific activity as it is seen from the inside, i.e. by practitioners. Kuhn in particular appears also to have drawn a sympathetic resonance for his own work from many who are outside and perhaps critical of science, in that his emphasis on scientific revolutions highlights the impermanence and changeability of 'normal science' paradigms and thus sets limits to the pretensions of science as a modern-day religion. The condescension and arrogance of many individual scientists, tempted by popular

and uncritical adulation to set themselves up as High Priests of a secular pantheon (e.g. Crick 1966), receives a sharp check when Kuhn warns them implicitly, that they in their turn will be contraverted and supplanted, or bypassed, in a continuing scientific 'evolution by revolution'.

The strength of the language-game approach stems basically from its recognition that all language-games are ultimately in free communication with each other. They form a continuum. Boundaries can be erected along particular lines; and/or relatively empty 'no-man's-land' zones can be formed by the withdrawal of usage from particular areas — but these are all to a great extent arbitrary, the result of historical accident, artefacts of the educational regime at a particular time and place. In reality one language-game intergrades with another, and that one with yet others ... and so on indefinitely. This can be illustrated with reference to what might be termed the ontogenetic development of language-games in the individual: the process of learning more and more language-games as maturation and education (both formal and informal) proceed.

A baby learns very simple language-games to start with: 'Mama', 'dada', and so on. It gradually builds up to a level where they approximate to the 'primitive' language game described by Wittgenstein (1953: Sections 1 - 50), where objects and actions are indicated, commanded, etc., by the use of single words. Later come simple sentences, then more complex. But the key issue is that all the acquisition and development of subsequent language-games depends, necessarily, upon the language-games that have been learnt before. Each new language-game is acquired through the use of previously-learned language-games — and this is so even when the acquisition (= teaching-and-learning) of a new language-game is not purely verbal. 'Ostensive' or 'experiential-learning' is not usually purely ostensive/experiential. "Come and see this", and pointing, are both uses of previously-learned language-games, even though what is seen and pointed to may not hitherto have been part of the individual's language-game repertoire. It might be said that the mere seeing, experiencing, of the 'something', whatever it might be, is not itself part of a language-game, and if the seeing/experiencing 'just happened', if it were fortuitous and not the result of suggestion or directions given (via language-game) by someone else, then to that extent the claim that the experience is not part

of any language-game might be allowable. As soon, however, as the individual who has had the experience wants to describe or allude to it, it is clear that the new experience has been (must necessarily be) incorporated in a language-game. And surely this must be true, even if the individual is merely attempting to characterize the experience in his own mind.

An anecdote may help to illustrate the principle being discussed.

Once when lecturing on History and Philosophy of Science I said something to the effect that "Science necessarily involves language, and the whole of science can be seen as a large number of interrelated language-games".

"But", said a physics lecturer in the audience, triumphantly, as though exposing a fatal flaw in my whole argument, "A scientific discovery can often be expressed in the form of a mathematical formula or equation — and many of them are expressed in this way!"

I replied mildly, that I was including mathematics within the general category of 'language'. My opponent (for so he seemed to regard himself) looked unconvinced and suspicious. "Just juggling with words", I could see him thinking. If I had wanted to make the point strongly and convincingly — as I later realized when thinking over the incident — I should immediately have written on the blackboard:

" $E_t^P : K = (p^x - q^y)^{x-1} + HK x^y$ ", or something similar, and then asked the physicist to tell me — and the class — its meaning.

Since it has, so far as I know, no meaning — I just made it up — the physicist would presumably have to say that he did not know the meaning; and, provided he did not suspect that it was intentionally meaningless, he would have to ask me to define the meaning of the symbols used. I would then point out that these explanations would have to be given in ordinary language or something closely approximating to it; and that the symbols by themselves, i.e. if not linked up to some already-known language-game, are literally meaningless.

What, if any, it may be asked, are the advantages of the language-game approach over other more traditional ways of doing Philosophy?

One possible part-answer would be: none. The great quantities of traditional philosophy which have been soundly argued are not diminished by

contrasting them with language-games approaches — not, anyway, in terms of their substantive conclusions. Transposing them into language-game terms may, however, by bringing out explicitly the importance of particular linguistic formulations, make them easier to understand, and in particular may help to reduce the possibilities for misinterpretation and illicit extrapolation.

An important advantage of the language-game as compared to traditional approaches is, as already indicated, that it helps to eradicate the notion of hard-and-fast frontiers between philosophy, science, education, ordinary life and other 'realms of discourse'. If all such 'realms' are seen as being made up of indefinite numbers of language-games, many of them full- or part-alternatives to each other, and if it is appreciated that many particular language-games cannot be classified categorically and exclusively into one 'realm' or another — in other words, if many such language-games lie right athwart the frontiers between realms, and cannot be said to be unequivocally on one side or the other — then the possibility of communication and meaningful discourse between the different academic disciplines, and between them and 'ordinary life', must be increased. Thus fragmentation between and even within disciplines could, in principle, be reduced; and so could the 'ivory tower' effect, of seeming isolation of all academic disciplines from ordinary life.

Communication, or the lack of it, between established adult practitioners within the various sciences and other academic disciplines is one thing (and does not depend exclusively upon linguistic considerations); but the education of persons into academic disciplines, the professions and trades, and into citizenship and adulthood, is something else again. It is in all areas of education that the language-game approach can be most valuably transformative. Several examples of the enhancement of educational effectiveness and of intellectual clarification are given in later Chapters. Many of these relate, broadly, to science education. At this point, and in preparation for a more extended discussion of the significance of the language-game concept in philosophy, it may be illuminating to consider the language-game approach, briefly, in relation to the teaching of mathematics.

The 'trad' approach to the teaching of mathematics was quasi-empirical and inductive, and tended to assimilate mathematics to science. In-

deed mathematics seems often to have been regarded as virtually a form of science. (In many universities, mathematics is still taught within the Science Faculty.) The young child was taught that two apples (or counters) plus three apples made five apples (or counters) — and it was not made clear that the apples or counters or whatever were mere examples. The child was allowed or indeed encouraged to assume that the 'two', the 'three' and the 'five' were somehow inherent in, and to be derived from, the apples (or whatever physical objects were being used). In general, mathematical propositions were presented as though they were descriptive generalizations derived inductively from experience of the physical world. This worked well enough in some ways, but tended to generate anomalies and blocks to learning as soon as the more complicated levels of mathematics were reached. For example, even the rationalization of expressions involving both multiplication and addition tended to be problematical (for the pupils, though presumably not for the teacher). Why should multiplication and division have priority over addition and subtraction? The only answer was: it is the rule (or, from the more percipient teachers: it is the convention). But there seemed to be nothing in physical reality that necessitated having precisely this rule or convention — thus mathematics came to seem a dissonant compound of inductive empiricism coupled with arbitrary convention. (Probably the conflicts generated by this situation forced many maths teachers into the ultra-authoritarian role which many of them adopted, unhappily for their pupils and, one suspects, for themselves. But they had to ensure that their pupils would 'get their maths right' — and since many parts of mathematics could not be learned, like science, from observing the world, the alternative was simply to enforce compliance with the established conventions.

The conventions regarding priorities of operations could perhaps be accepted without too much trouble — nevertheless the underlying discontinuity as to what sort of undertaking mathematics really was seems to have been at least partly apprehended, if only at an unconscious level, by many pupils — but when it comes later to concepts like 'n-dimensional space' and matrix algebra, or even to avowedly fictitious assumptions about an 'instant' which is both timeless and a-very-short-time and a 'point' which is both a-very-short-distance and no-distance-at-all (both of these concepts being basic to calculus), resistance on the part of pupils tended towards infinity. And on the empiricist/inductivist basis on which most of their mathematics had been taught, their resistance was

both rational and justified. To remove the grounds for this resistance, it was necessary to present mathematics as something quite different from empirically-based, inductive quasi-science. The change was made by presenting it not as a system of abstract but 'real' and 'objective' rules and relationships — teachers who have fallen into that trap have had scant success, and have given 'new maths' a bad name — but rather as a collection of abstract games, some of them with different rules, and with differing types of applicability (or not necessarily any applicability at all) to the real world of everyday life. In other words, new maths if presented as an open-ended series of hypothetical systems, a series of games of 'lets pretend such-and-such, and see what happens', can catch not only the understanding but also the imagination and enthusiasm of pupils, and can give them a competence at mathematics far beyond what was once thought possible.

The link between new maths conceived as a series of possible games, and language-game philosophy which sees all language and all human activity as a series of games, is sufficiently obvious. (It might be remarked that teachers trained in language-game philosophy, especially if also trained even at a fairly elementary level in formal logic, are likely to attain enhanced effectiveness, notably in flexibility of approach and in ability to appreciate the problems and mistakes of their pupils, in teaching most subjects including mathematics).

In later Chapters the possible functioning of a 'language-game' approach in science education will be explored and discussed in detail; and various other issues regarding the nature of philosophy, and specifically of the place of language-game philosophy in relation to education, will be dealt with as seems appropriate. This present Chapter can usefully conclude with a brief review of some of the contentions which are being put forward:

- (1) Present-day practices in education in the various sciences (and mutatis mutandis in other types of discipline, too) tend to favour Kuhnian 'normal science' at the expense of 'revolutionary science'.
- (2) Creativity even within normal science may tend to be inhibited by many of the current approaches (Stenhouse 1971); and insofar as major advances in science are equatable with revolutionary rather than normal science, current education within the sciences tends to be counterproductive at basic or fundamental levels.
- (3) Better understanding of the nature and history of science, and of its methodology/philosophy especially with regard to revolutionary science

(Could we abbreviate this to 'revsci'? As contrasted with 'normsci'?) should improve Science Education as an academic discipline, and should also lead to improvements in teacher education hence also in actual science teaching in schools and tertiary educational institutions.

(4) As a special case of (3) above, better understanding of history and philosophy/methodology of science, and in general a better understanding of the multiplicity of possible language-games, would enable teachers (of science and other subjects too) to understand better the mistakes and errors of their pupils and thus to be able to use such errors, positively, as a means towards improving the pupils' understanding (see further discussions of this in Chap. 7).

Thus a 'snowballing' effect can be expected, an emphasis on methodology/philosophy, especially if cast in terms of a language-game approach, leading to:

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Better appreciation of 'revsci'
      ↓
Improved Science Education (as an academic discipline)
      ↓
Better teaching of science
      ↓
Better teaching of all subjects.
  
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It may be suggested, in conclusion, that the language-game approach provides a framework within which can be incorporated, besides the verbal interchanges on which attention has hitherto almost exclusively been concentrated, also the non-verbal elements of communication which are now being revealed by the new science of Human Ethology. More on this later.

CHAPTER SIX

APPLICATIONS OF THE LANGUAGE-GAME APPROACH IN SCIENCE EDUCATION

Having taken a look, in Chapter 5, at the functions which philosophy in general might be able to perform in relation to science education, and having suggested that, while any of the established approaches within philosophy could be efficacious in achieving the results desired, Wittgenstein's 'language-game' methodology might for various reasons be expected to have special potential in terms of education, it will now be appropriate to test this suggestion. As preliminary tests which will occupy the present Chapter, passages from several widely-used textbooks in Science Education will be examined and shown to be defective or misleading in important ways. They will then be re-formulated in terms of a language-game approach, these re-formulations will be discussed briefly, and readers will be left to assess for themselves the hoped-for improvement.

(An extensive survey of textbooks in Science Education would necessarily be superficial, especially if attempted before the basic methodological issues have been explored and understood. Attention will therefore be concentrated initially upon only a few texts — some general remarks, supplementary to Chapter 2 above, will be given later.)

Firstly, what influence, if any, has philosophy already had upon Science Education?

Anderson et.al. (1970) in the Preface to their widely-used textbook Developing Children's Thinking Through Science, state that:

"Throughout the 1960's philosophers, learning theorists and child development specialists entered the field of elementary-school science. The efforts of these people, in co-operation with science educators, scientists, and elementary school teachers, resulted in the development of many curriculum projects. This development work has concerned itself with numerous areas that have an impact upon elementary-school science instruction, such as philosophical considerations Because of all these new developments, it becomes necessary for the prospective and in-service teacher fully to explore the areas that will enhance teaching effectiveness. Full understanding of these areas will result in more positive teacher-student and student-student interaction." (p. v. — *emphasis added.*)

It is clear from this — and from many similar passages from other textbooks, professional education journals, official utterances of various sorts that could be quoted, also from changes in policy and practice

e.g. the proliferation of compulsory courses in Educational Philosophy within teacher training programmes — that Philosophy is regarded not only as a desirable influence upon educational policy and practice, but also as having had an appreciable effect.

Two questions immediately arise:

- (a) Precisely what effect(s) are these changes thought and/or intended to have had?
- (b) What effects have they really had?

Before attempting to answer these questions in detail, several further quotations can be taken from the Preface to Anderson et al. These will provide indicators to some of the underlying assumptions of the authors; and through them, to some of the assumptions widely held among educationalists at the present time.

"The one theme that permeates the manuscript is the goal of develop-children's thinking", state the authors (p.vi). "This can be illustrated in the first chapter, which deals with the establishment of a working philosophy of science teaching and in which it becomes obvious that both content and process, in varying degrees, are essential to the development of the thought processes of the child"

The notion that it is possible in one chapter — and that the first in the book — to "establish a working philosophy of science teaching", may strike readers trained in contemporary 'pure' philosophy as somewhat unlikely. Doubts may arise on several grounds. If philosophy is taken as a 'second-order spectatorial activity of clarification of concepts' (cf O'Connor 1957, Hirst 1963, Peters' various works; and others), one might ask where is the 'first-order' material upon which the 'spectatorial activity' is to be performed? In the later chapters of the book, perhaps. Then why not put the first chapter last? But even if this were done, is philosophy really the sort of activity that can adequately be taught in one chapter (irrespective of its placement in the book)? Further, the authors speak of "a working philosophy", and they clearly assume that it can be "established". Now if they mean, merely, that they can 'establish' (i.e. state and argue for) their own position with regard to science teaching, this might not seem utterly unreasonable — though perhaps a rather ambitious objective for a single chapter no matter how substantial. But they seem to claim that they can teach the students who read their book to 'establish a philosophy of ...'; and their claim to 'develop children's thinking' is quite explicit. If, then, the 'establishing of a philosophy of ...' involves and is dependent upon the 'development of thinking' — and

this position would in general seem likely to be acceptable to a majority of presentday professional philosophers — it would seem to follow that the 'establishing a philosophy' would be the outcome of, rather than a precursor to, the 'development of thinking'. (Cf discussion of 'philosophical positions' in Chap. 5.) Moreover, it might well be assumed to be a longerterm process than the mere reading of even a rather large book.

The invocation of 'Philosophy', then, in this as in many other educational textbooks, might turn out, on close examination, to promise more than is actually performed — and perhaps even worse, might be substantially misleading in promising or appearing to promise something quite different from what is actually performed. If the textbook-writers' views about philosophy are defective, and if (as they themselves seem to assume) philosophy in some guise or other is important to the educational task they are attempting to perform, is it likely that the outcomes of their endeavours will be successful? At least a prima facie doubt seems not unreasonable.

A number of distinguishable but related questions ramify from this point:

- i) Is it in fact desirable, necessary, or even possible to teach children 'the philosophy of' the various subjects they are to learn in school?

Clearly, one's answer to this is going to depend, among other things, on one's view of what philosophy itself is — and so we have to ask:

- ii) What is 'philosophy' (and/or 'the philosophy of ...': education, science, biology, or whatever)?

and

- iii) How could/should philosophy be taught?

This last question has in fact been posed to professional academic philosophers in recent years by Michael Scriven (1971), and a variety of answers have been offered. We shall examine and discuss several of them as we proceed.

Moving on now to a brief consideration of Chapter 1 of Anderson et al. 1970 Developing Children's Thinking Through Science, we find that the Chapter starts with a list of "Performance Objectives":

"Upon completion of Chapter 1, you should be able to:

1. State either verbally or in written form your concept of the scientific enterprise.

2. State in your own words a philosophy for the teaching of science ... and identify the assumptions and propositions upon which this philosophy is based."

The list continues to:

- "6. State either verbally or in written form why past approaches to the teaching of science in the elementary school are no longer valid for today's children". (This seems tendentious, and a 'hard sell', to say the least! Emphasis added.)

While one can but applaud the attempt to get students, teachers (and, hopefully, parents, citizens, and everyone concerned with education whether in science or in any other field) to think consciously and explicitly about 'the scientific enterprise' and about the assumptions etc. on which science and education are based, it would be seriously misleading if the readers of the book obtained the impression that a comprehensive list of such assumptions could in fact be made. It seems unnecessary to argue this in detail. The whole trend of the works of Toulmin (especially with regard to his 'ideals of natural order', Toulmin 1961) and Kuhn (1962), to name but two of the leading contemporary philosophers of science, is to show that the most pervasive and influential of the assumptions behind science at any one time are so deeply engrained as to be susceptible only with great difficulty to conscious scrutiny. This 'exhuming of unconscious assumptions to conscious scrutiny' may in fact take centuries to accomplish (see Whitehead 1933); and it constitutes a major component in the long-term progress of science. To expect students at high school, teachers' college or university to be able to write more than a very sketchy and superficial list after reading a single chapter of a book, then, appears to be quite unreasonable. And to put this expectation forward, with the tacit implication that the mere compilation of such a list would constitute a substantial educational achievement, cannot but distort students' understanding of education, science, and philosophy. The making explicit of unconscious assumptions can be beneficial in various ways (see Chap. 2 above) — especially if it is sustained until it becomes habitual — but its most significant effects cannot be expected to be so immediate that they can be written down after reading a chapter in a book. (Neither are they likely to appear to a very material extent in the written examination at the end of even a year's study.)

To put the matter at an extreme, if students were able to exhume a substantial array of the assumptions, hitherto unconsciously held, upon

which the teaching of science — and therefore, it would seem, science itself — is based, they would be well on the way to being important 'revolutionary' scientists in Kuhn's (1962) sense! I am not suggesting that appropriate education cannot go a long way towards achieving this: but it seems unlikely to be achievable merely after reading a book, or, in particular, one chapter of a book.

Later in the chapter various nominated approaches to the teaching of science are described and discussed.

The

"Factual Approach ... is primarily concerned with imparting the findings of science to students. The end [= outcome, intention?] of instruction would be their acquisition of the following type of information: A hydrogen atom has one electron



Venus is the planet nearest to the Earth.
Dinosaurs were reptiles." etc. (p 14).

It may be noted in passing that the authors do not condemn this approach outright. They recognize merits in it, but question whether it provides students with a true picture of the 'workings of Science'.

The "Conceptual Approach" is the next to be considered:

"The principal findings of science can be tied together into a limited number of conceptual schemes.

Typical of these are: All matter is made up of particles.
In most reactions, the sum of the matter involved remains constant.
The celestial bodies move in predictable paths.
Living things are affected by their environment.
Matter changes state by absorbing or releasing energy."

etc. (p 15).

Perhaps the first reaction of a reader (especially one who has in fact obtained a real grounding in present-day philosophy) is that these two lists of propositions do not appear to differ very much. Do Anderson et al. really regard these latter propositions as representing or expressing 'conceptual schemes'? Do these propositions set up 'conceptual schemes' whereas propositions from the first list do not? It would appear that the authors would answer this question affirmatively — yet on the

face of it, there appears to be little difference, apart from generality, between "All matter is made up of particles" and "A hydrogen atom has one electron". Both appear to be equally abstract, in the sense of not being susceptible to observational verification in any direct way. Both appear about equally open to didactic presentation and rote learning, etc.; and insofar as explanation would (or could) come into the relevant educational transactions, the explanations required for the one would be no different in kind from the explanations required for the other. There is a verbal difference between them, to be sure, in that "All matter ..." is explicitly a universal proposition, while "A hydrogen atom has one electron" might appear to be particular (and hence, perhaps is it assumed, observable, or verifiable in some more direct way, than the "All ..." proposition?) But a moment's thought reveals that the difference is indeed at the 'merely verbal' level. What is really being asserted is "All hydrogen atoms have one electron" — and without attempting to justify it at this point, I shall simply assert that this is what must be asserted. The procedures by which the structure of 'the hydrogen atom' has been determined did not, in fact, deal with single atoms, nor was it ever possible that this could have been done. The structure of 'the hydrogen atom' was arrived at inferentially, on the basis of observation of populations of atoms and electrons, etc.. (See Toulmin and Goodfield 1962, for discussion and further refs.) So that there is less difference between the two propositions cited by Anderson et al. than might appear at first sight to be the case; and in fact it may be suggested that there is really no significant difference between the two sets of propositions they offer as typifying the 'Factual' and the 'Conceptual' approaches respectively. The authors themselves virtually admit this, when they say (p 16): "Teachers who emphasise facts often teach conceptual schemes. Unfortunately, though, the concepts are usually taught as facts." (Emphasis added; and cf Chapter 2 on 'factualism'!) In short, the differences which Anderson et al. are dealing with are not adequately to be characterized in terms of differences between propositions (or between types of proposition); they are to be characterized, rather, in terms of how the propositions are introduced to the pupils, how they are explained, how the pupils come to understand them, test the, interpret them, modify them, and so on.

Contrasted with the Factual and Conceptual approaches, Anderson et al. propose what they call the "Process Approach":

"Both factual and conceptual approaches to science teaching emphasize the product of science. But science is also a dynamic enterprise. It includes the process, or way in which these products were formulated." (p 15)

Perhaps the first — and seemingly minor — point to be made about this formulation is that science is being presented as "... the process..." (note the altered emphasis); i.e. it appears to be assumed that there is one process which is operative throughout all science. Objections can be raised against using the word 'process' at all; but even if we leave aside such objections, and accept 'process' provisionally and for the sake of argument, it is very widely accepted that there is no single 'process of science', not even a fixed and determinate set of processes (see Beveridge 1967, Toulmin 1954, 1961, Kuhn 1962, Harre 1972 and refs therein). It may be, however, that the simplicism exhibited in the textbook is nothing more harmful than a necessary easing of the preliminary and introductory notions, and that a more realistic complexity will be revealed as we proceed. The passage from which the last quotation was taken proceeds thus:

"A process approach to science teaching is based on the examination of what a scientist does; therefore, the end of instruction [presumably 'aim' or 'objective' is meant] would be to have the student behave like a scientist. The specific behaviours are derived from an examination of what a scientist does and are called processes of science. Some of them are:

Observation	Measurement
Prediction	Classification
Communication	Inference." (p 15)

This is just a list. But is it randomly organized, or is the order of the items significant? If the order is significant, how are we supposed to read it? Across the rows or down the columns? Either way, 'observation' seems to come first (as it does in most simple stereotypes supposedly representing scientific activity). But does science always start with observation? It seems to be assumed, by the writers of educational textbooks, that it does come first, and that it is primary in two senses:

- (a) In coming first in time; and
- (b) In being a 'basic', irreducible 'process' or activity.

But this view, of the primary of observation, is disputed not only by philosophers of science (e.g. Popper Conjectures and Refutations 1963, see also Harré 1972 and refs therein), but also by various findings from science itself.

The key issue can be put (in nice philosopher's terms!) like this:

'Observation'	-	of what?	
'Measurement'	-	of what?	
'Prediction'	-	of what?	and so on.

Now this is not just 'being difficult', or 'asking questions for the sake of asking them'. These questions are being asked in order to expose what is really one of the most pervasive and most pernicious unstated assumptions in books about science teaching: that the subject-matter of science is obvious to all, is just given. But in fact the subject-matter of science is obvious only when viewed post hoc. Looking back, after the discovery has been made, perhaps it all seems very straightforward, perhaps it seems almost inevitable that things would be found to be as in fact they are. But of course this is a false and misleading picture of how science actually happens. That it should be assumed to happen this way is largely due, no doubt, as Kuhn among others has pointed out, to the systematically misleading nature of reports of scientific achievement (and especially textbooks) used in the teaching of science.

It is perhaps desirable to point out, parenthetically at this stage, that the 'selective omissions' (of all the false casts, unsubstantiated hypothesis, etc.) in scientific research reports are not necessarily harmful when these reports are read by fellow researchers. Persons who are themselves experiencing, or who have for a substantial period experienced, the everyday realities of scientific research with all its frustrations and disappointments, the bright ideas which turn out to be duds (or to be only 'partly-baked ideas', see Good 1961), and so on, are likely to 'read in' all the bits that have been left out. They know the untidy realities of scientific research. But a great many people who are teaching science and writing books about science education have not had such first-hand experiences of research — or they have, it has been the relatively structured and directed experience of the 'research student' doing a Master's degree or Ph.D. It was experience of limited scope and duration, and may have been at a much earlier period of the individual's career (this is the impression one gets about the authors of many texts in science education, anyway). It appears, therefore, that many perhaps the majority of science educators are not deliberately misleading their readers when they present an oversimplified and distorted picture of how scientific activity (i.e. research) actually occurs: the distortion occurs mostly because they them-

selves quite genuinely do not realize what they are doing. They simply do not realize to what an extent the picture is actually false. They are misled by both research reports and textbooks. They may not have the experience (or if they had, they have forgotten it) to be able to give a proper interpretation, in the experiential dimension, of what appears in the texts and the learned journals — it is not to be wondered at, therefore, that they cannot communicate this dimension when they are teaching or when they are writing textbooks in science education. So their students get a systematically biased (and falsified) picture of how science happens. (Perhaps some of them gain an inkling, only half-consciously, of the fraudulent nature of the teaching they receive; and maybe it is this that prompts them to change their career orientations from science to arts. Hence the 'Drift from Science'?) Getting back to our examinations of the textbook by Anderson et al. and this business of "Observation", "Measurement", "Prediction", and so on:

It is one thing to be honest with students, and tell them what to observe, what to measure, etc.; and preferably tell them why these observations, these predictions, these measurements, etc., are regarded as significant. But the teacher who does this is admitting, tacitly at least, the post hoc nature of the business. He is admitting that he 'knew the answers all along' — and why not, indeed? Isn't that why he is employed to be a teacher? This type of approach, approximating in some ways to 'trad' schooling, is at least honest. It is also honest for a teacher to tell his students — if indeed he knows — about how different real scientific research is from the learning about science which goes on in schools and universities; but to pretend that one can give, in the school classroom, or the lecture-room or laboratory at the university, an experience of what it is actually like to do original scientific research — this is inherently impossible, and whoever pretends that it is not is, intentionally or otherwise, deceiving his students.

The 'discovery' approach exposes most clearly the logical/procedural absurdities of the situation. Teacher: "In this period, you will spontaneously and by your own efforts discover that oxygen is necessary for combustion [or whatever]. Take out your notebooks and apparatus!" This is logically about as absurd as the old jokes that used to be made about Communist countries: "Come along, we'll be late for the rehearsal of our spontaneous demonstration of the solidarity of the workers — and the Commissar won't like that!" There is just no way in which, in the set forty-minutes-to-two-hours session in the laboratory, the students are going to discover for themselves what the teacher or the syllabus expects them to discover.

Education can take one horn of the dilemma or the other, but it cannot take both. If the pupils are truly to be left to spontaneous exploration, they may indeed make various discoveries: that John is better than Richard at wristwrestling, that Mary has pink panties on today, that thermometers held up to the bunsen flame squirt ^{mercury} out of the other end, or that all sorts of interesting things can be done with the batteries and electric circuits provided to illustrate Ohm's law. On this horn of dilemma, discoveries may be made, but they are rather unlikely to relate to the official syllabus. If on the other hand it is felt desirable that specific information, skills, etc., should be acquired by the pupils — whether part of the official syllabus or not — then it is best to be honest about it, and impose a structure on the teaching situation which does not pretend to leave the acquisition of information, skills, etc., to the independent and spontaneous discovery of the pupil. There is no need to revert to the authoritarian, arbitrary, dogmatic or even brutal enforcement of 'rote learning' which is supposed by some writers of texts (so it seems) to be the only alternative to 'free and spontaneous discovery'.

It is noteworthy that Anderson et al. do not give much emphasis to the discovery approach. It is downplayed quite heavily. It is not mentioned at all in the chapter on "Establishing a Working Philosophy", not even in the somewhat tendentious section entitled "Historical Overviews of Elementary School Science" (pp. 17-23). Its only mention, a short paragraph, comes in the "Methods and Techniques" chapter, on p 58.

"This approach stresses the learning of concepts, theories, principles, and content in science through discovery rather than through rote memorization".

Note the simplistic and tendentious polarization between 'discovery' on the one hand and 'rote memorization' on the other. The paragraph ends:

"It is this engagement with discovery that affords students the opportunity of grasping the true spirit of science."

This sort of statement is, of course, mere window-dressing, since the real problem of 'discovery'-type education, as outlined above, is not even touched upon in the paragraph in question. This very basic issue is simply dodged. Lip-service is paid to 'discovery' and 'the true spirit of science', but the actual nature of real-life science (let alone its 'true spirit') is neither described nor comprehended. It is not surprising, therefore, that there is little substantial attempt to interpret real-

life science into a programme of education.

Instead of facing the harsh dilemma posed by the 'discovery approach', Anderson et al. retreat into the ambiguities and obfuscations of what they call the 'Process Approach' to the teaching of science (pp 15ff). Some of this has already been quoted above: I repeat some quotations in order to make some new points regarding them.

"Both factual and conceptual approaches to science teaching emphasize the product of science. But science is also a dynamic enterprise. It includes the process, or way in which these products were formulated."

It may be pointed out that to equate 'process' with 'way ... these products were formulated' seems to leave us still very much stuck at the 'products' level: if 'process' is nothing more than the formulation of products, i.e. a matter of alternative ways of expressing what has been discovered, then we are not getting much deeper into the actual activities of scientific research. Yet Anderson et al. go on in the very next sentence to say:

"A process approach to science teaching is based upon the examination of what a scientist does; therefore, the end of instruction would be to have the student behave like a scientist." (p 15)

This as it stands is heavily ambiguous. The "therefore" suggests that the second part of the statement is entailed by the first. The first could be taken as unexceptionable. Basing teaching on an examination of what scientists do (again, in my formulation I am tacitly recognizing that there may be diversity in what they do: whereas the textbook's" ... what the scientist does ..." could be taken as suggesting much more uniformity) — but to repeat, to base education in science upon an examination of what scientists do is clearly so sensible that it could be said to border on the truistic. This, however, is a very different thing from suggesting that science students should actually replicate how scientists go about doing research — yet it is made to appear that, "to have the student behave like a scientist" is entailed by 'basing science education on the examination of what a scientist does'. The conclusion simply does not follow from the premiss. The argument, such as it is, is invalid.

Now it can be objected that 'having the student behave like a scientist' does not necessarily translate into 'having the student replicate how scientists go about doing research'. My interpretation implicitly

emphasizes the possibility of students replicating the totality of real-life scientific research. The phrase 'behaving like a scientist' on its own need not entail behaving totally like a scientist. But if total replication is not involved, what aspects are involved? On what basis are they to be selected from the totality? Wearing laboratory overalls and fiddling with bunsen burners and test-tubes does not make a person a scientist any more than wearing a white coat and a stethoscope makes him a doctor. Anderson et al., however, appear to think that by imitating some of the 'behaviours' of scientists, children/pupils are going to get to know what doing science is really like. They say (I apologize for the repetition, but it is necessary to pay close attention to the details of their statements):

"The specific behaviours are derived from an examination of what a scientist does and are called the processes of science. Some of them are:

Observation	Measurement
Prediction	Classification
Communication	Inference."

The items of this list are collectively neither necessary nor sufficient. Measurement, for example, comes into a great deal of science, but not all. It is thus not strictly a necessary, though it is a common part of science. The question of how the categories to be measured are decided upon, i.e. the criteria of measurement ('quantification' would be a better general term) and of units of measurement, is not even mentioned (cf Chapter 2, above) — thus the list is not sufficient to characterise scientific activity. Various other major categories involved in science are not even mentioned: e.g. imagination, risk-taking, rejecting or 'sitting loose to' the accepted orthodox theories, etc., etc.. It is true that some categories similar to these are listed in an earlier section, "Characteristics of Scientists" (pp 12-13). Scientists are supposed to be characterised by 'Creativity', 'Persistence', 'Fastidiousness', 'Open-mindedness' and 'Knowledgeability'. But the various questions as to what these terms really mean, and how they are to be fostered in pupils, are neither faced nor resolved. There appear, indeed, to be substantial inconsistencies lurking not far beneath the surface of this chapter (and of the book as a whole): for if 'knowledgeability' is truly a necessary or desirable characteristic of the scientist, the rejections of the 'factual' and especially the 'conceptual' approaches in science education (see pp 14-15) would appear to need more justification than they receive. (Even the condemned 'rote-learning', to the extent that it does in fact result in real

learning — which it can and often does — could be regarded as contributing to 'knowledgeability' and hence to be acceptable.)

It may be suggested, then, at this point, that at least two rather different and perhaps mutually incompatible strategies of science education are being conflated. These may be distinguished as:

- (a) Telling the students about scientific activity: how research is done, the methodology/philosophy, the psychology, the strategies, tactics and techniques of science.
- (b) Allowing/inducing students to actually experience the doing of scientific research.

I suggest that the second of these two alternatives, if we take 'scientific research' to mean what it says, viz. the endeavour to solve problems etc. to which the answers are not already known, is manifestly unrealistic as a general policy for the education in science of the vast majority of pupils. (It may be possible for a minute fraction of 1% of pupils, say in later secondary or tertiary education — but this tiny minority is likely to be creative anyway, they don't need to be educated into it, see further discussion Stenhouse, 1974, Chapters V and X). It is of course possible for students to be given experience of various of the techniques of science — this is what is normally done in laboratory classes, field trips, etc. — but this is something quite different from carrying out original scientific research. There is no 'creativity' about it; thus the listing by Anderson et al. of "Creativity" as one of the characteristics of scientists is misleading if pupils assume that it is intended to apply to their own immediate education in science. (Similar considerations apply to "Openmindedness"!.) Students are in fact not normally given the opportunity to conduct original scientific research (i.e. 'experience the activity/process of doing scientific research') until their post-graduate years at university — and often only to a trivial extent even then. It appears likely that the virtual abandonment of the 'Discovery Approach' in Science Education is a tacit recognition of its impracticability (and even, considering that only a small and shrinking proportion of professional scientists are allowed to conduct independent non-supervised research, its undesirability).

If alternative (b) above is impracticable and perhaps undesirable, what about alternative (a)?

Alternative (a) was formulated as a generalization: "Telling the

students about scientific activity ..." This was later particularized to include, among other things, "... the strategies, tactics, and techniques of science". It is in fact only at this last level that direct experience comes in. 'Direct experience', the 'behaviours' of scientists that the students are supposed to replicate, the 'activities' or 'processes' of science which are apparently intended to give pupils an 'inside view' of the nature of science, are limited almost exclusively to the techniques of laboratory experimentation and (to a lesser extent) field observation. Pupils' direct, active and in a sense 'autonomous' experience of the 'processes' of science are limited almost entirely to technicalities. Questions of strategy are often not discussed at all; and even tactics (the shorter-range alternatives, compared to the longer-range and wider-scope considerations of strategy) tend to be told to students, either by teacher or textbook, rather than being decided upon by the individual student. The methodology and/or philosophy of science is almost never given systematic discussion even on limited topics specific to a particular branch of first-order science currently being taught — the briefest, most fleeting mention of methodological issues is about the limit, in most courses. And where philosophy/methodology is brought to notice, its treatment is almost invariably as shallow as in the Anderson et al. volume which we are discussing. What is quite frequently done is to preface the presentation of firstorder science with a brief history of the science in question — but this, in the absence of methodological discussion of suitable depth, merely generates what has been called the 'Paradox of Past Greatness' to which we have referred earlier: if the 'great names' of the past dreamed up theories which to our sophisticated modern eyes seem so peculiar, often so silly, how does it happen that these people are indeed regarded as great? How do their names get into the history books?

The paradox can be dissolved, of course, if one goes into the background presuppositions, the evidence available to the old-time people and how they interpreted it. Once this is done, their achievements can indeed seem colossal. But the proper context cannot be given in a few brief paragraphs (or even a few brief pages — though if they were the right pages, i.e. if the required type of discussion were given, even a few pages would be a big help). The type of discussion which is needed is definitely of a methodological/philosophical nature. Is it reasonable, it may be asked, to suggest that this should be done, or even attempted, in teaching science

in schools? Are the criticisms of Anderson et al. (sketched but not elaborated in the foregoing) too sophisticated for school-level consumption? There are several answers to this.

Firstly, Anderson et al. are not writing for elementary/primary school children directly, but for their teachers, or prospective teachers. The latter are themselves undergoing tertiary-level education in teachers' colleges and/or universities (or have done so in the past). In short, the readership to which the book is directed is a professional group; and if they are incapable of appreciating the argument of the foregoing, then they should not be attempting to do what the title of the Anderson et al. book suggests, viz. Developing Children's Thinking Through Science. Better stick to the three R's, done properly, (or the secondary-level equivalent) rather than attempt something beyond the capacity of the teacher and in consequence miseducate the children in science. Secondly, there is a difference between presenting sound argument in simplified and easily-assimilated form, and presenting argument which is basically unsound. The material presented by Anderson et al. falls, as has been shown, into the latter category. The position they adopt is intrinsically untenable. Some of the assumptions on which their case is based are close to being mutually incompatible — some of their explicit statements even come close to being mutually contradictory. Though their chapter-heading, 'Establishing A Working Philosophy', implicitly sets up a claim to competence in Philosophy of Science (and/or Education), it is clear that such a claim cannot be substantiated. The authors seem to understand little about philosophy, or even about science — as has been indicated in the foregoing.

Are there, then, alternative ways of dealing with the issues involved which are both intrinsically sound and capable of being understood by the audience to which this type of book is directed?

In order to approach an answer to this question, it will be convenient to shift our examination from the text used so far (Anderson et al. 1970) to one which allows problems, deficiencies and possibilities of a different type to become apparent. As before, we shall proceed by letting the book speak for itself, in quotation, while we interpolate points of criticism, alternatives of interpretation, and so on, thus building up a detailed picture of the different alternatives of educational approach with which we are faced.

The following comes from Selberg, Neal and Vessel, Discovering Science in the Elementary School (1970, p.xvi):

A fact is attained through observation from percepts derived through sensory organs. Example: "Grass is green."

Let us accept that 'Grass is green' is a fact. Do we 'attain' it, get to know it, "through observations"? A number of comments seem to be called for.

Firstly, it would be odd to deny that we get to know that grass is green through sensory observation. But surely it would be almost equally odd, in many situations, to assert that we do so? In an ordinary 'talking about grass' situation, I just say "The grass is a bit brown" (implying that the drought seems to be continuing) or "The grass is green" (implying that at least it seems to be growing again and perhaps soon there will be more feed for the sheep). In this type of situation, if I say "I know that the grass is green through sensory observation", people will look at me strangely and wonder which they should take me to, the optician or the psychiatrist. Far worse, if I started babbling about "... percepts derived through sensory organs".

It may be objected, and quite rightly, that the writers of the textbook are not operating in a 'talking about grass' situation, and that considerations which would hold in the context of that type of language-game may be inappropriate in the context of what they are trying to do. This brings us to the second point:

The authors are not attempting actually to teach young children how to use language. They are not trying to initiate anyone into 'talking about grass' language-games. What they are trying to do, presumably, is to initiate student-teachers into the use of 'teaching science' language-games. And the question is, are they going about it in an optimally-effective way?

It seems desirable to distinguish two possibilities here. The textbook could be either:

- (a) Initiating the readers (i.e. student teachers) into the language-games of science; or
- (b) Initiating them into the l-gs of science education.

The latter possibility could again be subdivided into:

- (b1) Teaching the readers the language-games science educators use when they are, as it were, talking among themselves about their work of educating children in science; or

- (b2) Teaching the readers (i.e. student teachers) the actual language-games that they could/should use in initiating children (relatively young children in this case: those in elementary/primary school) into the l-gs of science which are appropriate to their age and stage of development.

It would appear, prima facie, that we should be able to discard possibility (a) above, on the grounds that initiating into the l-gs of science itself is normally a function of textbooks, teaching courses, etc., in the various first-order sciences; and the text in question does not purport to be a textbook in science, but rather (and quite explicitly) in science education, science teaching. Maybe it is presumed that the readers already know some of the l-gs of first-order science — otherwise, should they really be thinking of teaching science to children? So we can narrow the alternatives to (b1) and (b2).

Here some further clarification can usefully be attempted. On the one hand we can ask which of the alternatives we could reasonably expect the authors to be using, considering the presumed function of their book; and on the other we could attempt to determine, from examination of the book itself, which alternative they are in fact following.

Considering the first possibility, it would seem reasonable to expect, in a book which purports to help its readers to teach science to young children, to find both the language-games which science educators use when talking among themselves about science teaching (b1) and also some examples of the l-gs actually used in such teaching (b2). Furthermore, functional effectiveness would seem to demand that examples of the second sort (l-gs actually used in science teaching) should be introduced and explained by — in general, set in a context of — l-gs of the first sort, viz. language-games which explain and discuss, comment upon and argue about, the l-gs actually used in teaching.

Looking at the book itself in the light of the foregoing, we find no clear distinction made between these two functions, these two types of language-games (b1 and b2). Even within the brief first paragraph quoted above, the talk about "... percepts derived through sensory organs" would be merely mysterious and confusing to elementary/primary school children. It cannot, surely, be intended for them? And if not, then it must fall into category (b1) and be talk about science education intended to be intelligible mainly to science educators. For this, the type of language used is not inappropriate (whatever one might think of the use to which

it is put). But if the paragraph is aimed at science educators, its final component "Examples: Grass is green" seems out of place, and more appropriate to the teaching of young children (category b2) than to the initiation of student teachers who are working, presumably, in institutions of tertiary-level education. Irrespective of questions of educational or institutional context, there appear to be fragments of two different types of language-game embedded in the same paragraph. Talk of "... percepts derived through sensory organs" and talk of "Example: Grass is green" are not as they stand mutually compatible; and though it might be objected that they could be made compatible, by the elaboration of argument connecting the one with the other — this is indeed a possibility — nevertheless one can only point out that no attempt has in fact been made, by the authors of the textbook in question, to do so. The paragraph is simply flung at the reader. The next following paragraph makes no attempt to explain the first, nor to reconcile the anomalies revealed. It goes on to present different material (which, partly by the very fact of its greater elaboration, can be shown to be even more faulty than that of the first paragraph. See below.) So the small paragraph we have been examining is apparently intended to stand on its own, with all its imperfections.

Before we leave it, even more of the imperfections must be exposed and discussed.

Perhaps it would be convenient if the paragraph in question were re-quoted:

"A fact is attained through observation, and from percepts derived through sensory organs. Example: Grass is green."

Now whatever it may mean to say that 'a fact is attained through observation' — and a precise meaning is not so easy to perceive as might be thought — I certainly do know what it means to say that I know that the grass is green through observation, or that "The grass is green" is true by observation — assuming always that the grass is green, and that I can in fact see it. But how do I come to know that (given the provisos) "The grass is green" is true? Not just by observation. If I just observe, then I observe that the paper I am writing on is white with faint blue lines across it, that the table is a reddish-brown colour, and so on.

Someone objects that I should be observing grass, not the paper or the table — alright, but I shall then retort that what was being suggested

was that I could tell that the grass was green 'just by observation'. I have tried 'just observing', and it did not tell me that the grass is green. Now it seems to be suggested that it is not just observation, but observation of the grass which is required. Now then, do I know what grass is, what 'grass' refers to? Clearly, I must already have learned about grass; and clearly the whole business is turning out to be a lot more complex than was initially suggested. I must know what 'grass' refers to, presumably I must know the meaning of 'green' and 'is', and I must know what 'observation' means, and how to do it, before I know that I can tell by observation whether or not "The grass is green" is true.

To put the same issue another way, it could be said that mere observation tells me nothing: my attention has to be directed towards specific features of the environment before I can know whether statements about them are true or not. Indeed my attention has to be directed before I can be in a position even to make such statements. Additionally, it is clear that it would be impossible for me actually to make such statements (whether true or false) unless I had first learned the appropriate language; and even if the statements were made by other people, I could not be in a position to decide whether they were true or false unless I had first understood them (and in particular, unless I knew what they referred to).

Furthermore, if the possible circularity of using 'green' as a defining characteristic of 'grass' is to be avoided — if "The grass is green" is to be a synthetic not an analytic proposition — then the learning of 'grass' must at some stage or another be ostensive, experiential. And this cannot be merely or purely experiential: the learning of how 'grass' and grass fit into the appropriate language-game is what is in question. In other words, when learning a 'Grass is green' language-game, a person (normally in practice a child) has to listen to statements being made about the grass, has to observe the grass and what the people do in relation to it, has to do and say things in relation to the grass and what people are doing and saying about it and about what they are doing and saying, has to be corrected about what he has said and/or done, has to try again, perhaps be corrected again, has to have the experience of his doings and sayings being accepted and tacitly and/or explicitly being reinforced ... and so on.

In short, learning a 'Grass is green' or any other sort of language-game is an immensely complicated and sustained real-life social process; and though there could be said to come a time when the relevant learning

curve flattens off into a plateau, it seems likely that we can never be sure that a need for more learning will not arise. No matter how many things we have already learned to say correctly about grass, we don't know that it is impossible to say new and different things. For example, after 1839 people began to talk about the cellular structure of grass, after 1901 about its genetics (See Dawes 1952) — and there have recently been suggestions (Watson 1974) that we should be able to talk about its psychology and its musical sensibilities! (Whether these particular language-games do in fact develop, remains to be seen.)

The foregoing is of course an extremely minute and rough thumbnail sketch of how a language-game works and of how people learn it. It may not readily be intelligible to anyone unacquainted with Wittgensteinian linguistic philosophy. But even if it were to be provisionally accepted as having some validity as philosophy, the question would still need to be asked, whether a language-game type of approach would offer any improvement educationally, and if so, improvement in precisely what?

Part of the answer to this lies in the distinction made earlier, between the language-games of science educators when talking among themselves about science education, and the l-gs actually used in teaching science to young children. It should be apparent by now that what is appropriate for one of the l-gs is not necessarily appropriate for the other and may indeed be highly inappropriate. Above all, it is essential to remember that they are really different l-gs. They can be 'mixed' only with the greatest care and in the full consciousness of what one is doing. It is possible to use 'teaching children' (type b2) l-gs within a context of 'talking about science education' (type b1) l-gs, but not vice versa. In talking about science education (or any kind of education) one can give examples of 'teaching children' l-gs. If one tried the converse, however and included 'talking about science education' (type b1) l-gs in one's attempts to teach science to young children, the results would be disastrous. The children would not understand them, would be mystified and confused and hence sooner or later would probably become antagonistic both towards the person attempting such a misconceived endeavour and also towards science education, and science, and education. (It seems, alas! that precisely this happens, all too often, in actual teaching situations.) Selberg, Neal and Vessel and the authors of many other textbooks of science education fail to take sufficient account of the distinction between these several language-games. They mix together bits of different and often incompatible l-gs, with confusing results.

The confusion is not only between the different types of l-gs which we have already distinguished. Between types (b1) and (b2) as indicated earlier we can also characterize a third type of l-g which can appropriately be discussed at this point. This can be called a 'teaching science educators' language-game, and is in some respects to be regarded as intermediate between, or as a combination of, types (b1) and (b2). There are dangers in saying 'a combination' of types (b1) and (b2), because some of the faults of the Selberg, Neal and Vessel approach have been caused, as we have seen, by the unfortunate conflation of different language-games: everything depends on which bits of which l-gs are put together, and how. What we might conveniently designate as the type (b3) language-game, the 'teaching science educators' l-g is like (b1) in that the dialogue is between science educators and about science education. It differs from (b1), however, in that some of the educators involved (the writers of science education textbooks, lecturers in science education at teachers college or university) are not just talking about science education to the other science educators (or would-be educators, viz. the students who are learning how to be science educators), they are in fact endeavouring to show the latter how to go about the business of educating young children in science. If this is kept in mind, it can be appreciated that some parts of (b3), the 'teaching science educators' l-g, need to be like (b1) in that they can be relatively sophisticated talk about science education; while other parts need to be like (b2), or indeed need to be actual examples of (b2), the 'teaching children' language-games. The would-be science educators need to be shown what 'teaching children' l-gs are like. Both good and bad examples need to be shown to them, and as part of their 'teaching science educators' l-g they need to elucidate and discuss the precise nature of the merits and demerits of the several 'teaching children' l-gs which are shown them. As an example of how this could be done, and to further amplify the points made in the foregoing discussion, let us now examine a longer paragraph from Selberg, Neal and Vessel, the one which immediately follows the very brief paragraph on which we have already expended so much attention. This reads as follows:

"A conception or concept is a person's mental image of an object or event. Example: Each person has in mind a slightly different meaning for words such as chair, or boiling. A conception may embody characteristics, properties, qualities, or attributes. Examples: "Green plants produce food material". A conception may be a symbol, such as the words molecule or atom, or it may be stated in the form of number symbols, such as area equals width times length: $A = W \times L$." (Selberg, Neal and Vessel, 1970, p. xvi)

Now, neglecting various possibly-rather-pedantic issues of the inconsistency in the use of quotes in the above passage, and so on, perhaps the first thing that would strike anyone with previous training in Wittgensteinian philosophy is the "mental picture" theory of concepts. Taking this to have been conclusively disproved within 'pure' philosophy, the question arises whether or not it is a permissible, whether it might in fact be an effective, explanation within the context not of philosophy but of elementary education. It is often desirable to present notions in simplified form to young children, and to add the complexities and provisos as they attain a sufficient basis of their own experience to need and make sense of the amplified explanations. But then the question becomes: Is the presentation of concepts as being mental pictures a convenient simplification, educationally justifiable or perhaps even necessary? Or is it a distortion and falsification which is definitely misleading, which cannot be amplified and added to but which sooner or later would have to be repudiated and rejected outright?

There is no doubt that the story of concepts being mental pictures is intelligible, in some form or another, to children. But granted this, two further issues become salient:

- (a) To what extent is the notion 'concept = mental picture' really helpful even in a preliminary and provisional way to young children and:
- (b) If we grant that the notion has no 'ultimate validity' (this having been established within philosophy), then even if it were used as an initial teaching device, it would at some subsequent stage in the educational process have to be critically dissected, its faults exposed and its pretensions deflated — in other words it would have to be later rejected as misleading. And then we have to ask: Is this in fact done, in the textbooks where the 'concept = mental picture' doctrine is promulgated in the early chapters? Also, are the people who use this doctrine (seemingly a large proportion of teachers at all levels in the education system) competent to handle the presentation-plus-subsequent-debunking of the 'concept = mental picture' doctrine? And do they really attempt the necessary debunking?

One of these questions can be answered very simply: No, the debunking of the 'mental picture' doctrine is not usually even attempted, in either textbooks or teaching courses. (It may be done occasionally).

And so we arrive at the more interesting and important question, of whether the 'mental picture' doctrine is really in fact as helpful in building up children's understanding as people assume that it is.

It seems to be widely accepted among educationalists that if the notion of 'concept' has to be explained to children (or to teachers' college students or university undergraduates) then the 'mental picture' story is the way to do it. But if the 'mental picture' theory is fundamentally wrong, as the philosophers seem to have shown, then we are indeed in the midst of a typical philosophers' dilemma, asserting the conjunction of a proposition and its contradictory!

Let us therefore examine in detail what is implied or presupposed in the paragraph from Selberg, Neal and Vessel.

For brevity, I shall assume that 'conception' and 'concept' are equated, in the passage in question, with 'a person's mental image or mental picture of an object or event'. Also, it may be stipulated most emphatically at this point that nowhere in the argument that follows am I suggesting that people do not in fact have mental pictures of objects and/or events. Most people seem to have such pictures: this is not disputed. What is to be disputed is that having a mental picture is to be equated with knowing or understanding a concept.

"Each person has in mind a slightly different meaning for words such as chair ..." say Selberg, Neal and Vessel. (We may note that according to standard convention, since it is the word 'chair' which is being discussed, and not an actual chair, the word should have been set in quotes — as has been done in the earlier part of this sentence.) If 'meaning' equals 'mental picture', then we might be prepared to concede, for the sake of argument, that if somebody utters the word "chair" probably most of the audience will experience mental pictures of chairs and that most of these mental pictures will quite likely be different from each other. But how could we go about establishing this? Easily enough. We could simply ask each of the listeners to sketch, on paper provided, whatever mental picture(s) he/she experienced when the 'stimulus word' was uttered (or flashed on a screen, or whatever). Psychological experiments along these lines have been performed hundreds of times. The probable results could be represented in simplified form, as follows:

Person	A	B	C	D	E
'CHAIR' =					

Now if such an experiment were performed and with results along these lines, it would certainly have established that people do have different mental pictures associated with a given word or concept. But does it prove that people attach different meanings to the word or concept in question? Quite the contrary. Leaving aside the question of whether or not all the people attach an identical meaning to the given word, it is clear from the above example that they must all be using meanings which are substantially the same — otherwise how could they all (with the exception of E, which will be discussed later) produce pictures to which the word 'chair' does correctly apply? Clearly, they must all have taken roughly the same meaning from the word, otherwise:

- (a) They would not have produced pictures which have so many basic features in common (i.e., they were all chairs except E) and
- (b) They would not be able to agree — as it is here suggested they would agree, and would have to agree — that the word 'chair' is correctly associated with each picture (Again, E is the exception...)

In short, our experiment would have proved, not that each person's meaning of 'chair' was different, but that they were all substantially the same. Furthermore, the demonstration that the mental pictures were different would actually be dependent on the meanings being the same: otherwise we could not recognize all the mental pictures as being pictures of chairs, which is a prerequisite to our being able to characterize them as pictures of different chairs.

Our finding, therefore, is that it is probably generally true that the mental pictures are different — but recognition of this differentness is logically dependent upon the meaning being the same (or at least largely the same).

It seems to follow from this that the meanings and the mental pictures are not identical.

Where then does the notion of 'concept' lie in relation to 'meaning' on the one hand, 'mental picture' on the other?

I suggest that, in relation to the considerations offered in the foregoing, the concept of 'chair' is general, applying to any or all of an indefinitely large number of instances, in the same way that 'meaning' is general, and that it is not specific in the way that a mental picture is specific. 'Concept' is therefore to be equated with 'meaning' rather than with 'mental picture'.

It might perhaps be put forward as an objection to the argument about the mental pictures, that when a particular word (i.e. 'chair') is mentioned, each person experiences not one mental picture (as represented in the diagram) but rather a rapid series of mental pictures of different types of chair. That this could happen is true enough; but far from weakening the case being argued here, this consideration actually strengthens it. What this shows is that not only people in general take a common meaning for 'chair' (i.e. hold the concept in common), but that each individual holds a single 'meaning' in terms of which all of the mental pictures are recognized (or in the first instance, evoked) as being chairs.

It is time now to consider the disputable case, E. Is E's picture a chair, or not? Some people would say it is not a chair but a stool, a high stool. Others might argue that while in a narrow or strict sense of 'chair' it is not a chair but a high stool, there is also a broader, more generic sense of 'chair' in terms of which E's picture could be said to be includible within this category. If we recognize that for some purposes and in some contexts one of these positions would be more appropriate, while in others the other might be better, it seems that we might not need to come to an all-or-nothing decision to the effect that one is right and the other is wrong. But in recognizing that there can be differences in the broadness/narrowness or 'latitude' of meanings we are getting ourselves further and further away from a 'mental picture' theory. We are implicitly recognizing that there are 'intermediates', possibilities which lie in a sort of no-man's-land or boundary zone and which might be included, for different sets of reasons presumably, on either one or the other side of the boundary. This possibility would be very difficult to account for in terms of the 'picture' theory of meaning. E's picture is just as much a picture as D's is, or the others. Each of the pictures is no more and no less a picture than any of the others. What is in question, in the 'boundary dispute', is not what might be termed the 'pictoriality' of E's picture, but whether or not the word 'chair' is properly to be associated with it.

This suggests that what we are looking for might be rules for associating words with pictures (mental or physical), or with objects and events — or, more generally, just rules for using words.

Is it possible to formulate rules for using a word like 'chair'? Perhaps we could pose this initially as: Can we distinguish features which

are common to all chairs and which distinguish them from other objects (especially objects which are somewhat similar)?

"A chair has a flat surface that a person can sit on" — how about that as a distinguishing characteristic?

We must admit that this is true of chairs; but it does not enable us to distinguish between chairs on the one hand, and empty boxes, high stools (E's picture), and the floor... (In other words, this is necessary but not sufficient as a distinguishing characteristic.)

To differentiate chair from floor we can say that a chair must have its 'sitting-surface' raised above floor level. To distinguish chair from high stool we can say that a chair must have a back-support as part of its structure. And so on, to cover various other possibilities.

But how about a 'bean-bag' chair? Is it really a chair or not? If we just look at it, we might say "It hasn't got a back-support: it can't be a chair." But if we sit down on it we find ourselves sitting down in it: the 'sitting-surface' has sunk down under our weight and by doing so has caused a back-support to be left upstanding around the rearmost parts of the bag: a back-support has been generated, as it were dynamically, so that in function it is present even though not apparent in terms of rigid structure.

How about function and purpose then? Does a structure have to be constructed specifically for people to sit on, if it is to be called a chair? Suppose I build a small table to keep books on. Suppose I make an upright supporting bit at one end, against which to lean large picture books, atlases, etc.. Suppose the end result is perfectly functional as a chair; and when I give my bookcase away to a friend, he/she uses it as a chair not as a bookcase. What is it properly to be called? It would seem reasonable to think that its actual function at any given time should determine our answer here. While it is being used for books it is a book-table, and if used for sitting on it is a chair. Even so, it is quite possible that someone should ask "Why are you keeping books on this chair?" or "Why are you sitting on the book-table?" — but if the object were really as equi-functional as is envisaged, they would be likely to ask such questions only if they had seen the other function being performed at some earlier time. Otherwise, present function would be taken at face value.

The foregoing exploration of hypothetical possibilities has, it is hoped, clarified some of the rules for using the word 'chair'. In ordinary

real-life, of course, the rules are not usually explored in this explicit fashion, neither are unusual possibilities dealt with unless they happen, by some chance or other, to arise. Neither do we learn the rules for using words by being handed a printed list of them. (In exceptional circumstances we do learn some rules in this way, e.g. definitions of units in science, mathematics or formal logic, etc. But such occasions presuppose that we do understand language in general. Our initial and basic learning of language does not and cannot proceed in this way. You cannot teach a baby to talk by handing it a dictionary.) Most of our learning of the rules for using words is tacit rather than explicit, and much of it probably occurs partly below the level of consciousness. Language-learning is a complex business (see e.g. Minnis (ed.) 1971 for a number of points plus further references), yet most people manage to learn a language up to some level of adequacy.

Coming back now to the specific issue of Wittgenstein's suggestion that knowing or understanding a concept means, not having a particular mental picture or set of mental pictures, but rather knowing the rules for the use of the particular word (or words) used to denote the concept, we can ask: "Does this view of what a concept is provide us with better educational understanding than does the mental picture view?"

Various objections might be made against the 'concept = rules for use of word' position. 'It is impossible to teach all the rules, especially to younger children.' Yes, of course it is — but then surely nobody would claim to be teaching all about a particular concept, irrespective of how 'concept' might be defined? Also, if we see the rules for the use of a word as being not usually a closed and finite but an open-ended list, this again equates better with our experience: for most concepts involve a great deal of progressive extension, specification, differentiation, refinement etc. in getting to know them. We get to know them better and better as our education proceeds. This 'objection' can actually lead us to perceive more clearly another weakness of the 'mental picture' theory: that if it were true, it would generate severe problems in accounting for the correction of errors during education. If 'knowing the concept' means 'having the appropriate mental picture', how is a teacher to know whether or not all of the students have acquired the appropriate mental pictures? By telepathy? Not very plausible. In practice what happens is that the teacher asks the students to use the relevant words, whether by speaking or writing, perhaps associating words with pictures, diagrams or symbols, and then checks the

students' utterances to see whether or not they fit the rules i.e. are intelligible and correct. Thus educational practice in this respect seems much more in accord with a 'rules for using word' position than it is with a 'mental picture' position.

And consider teaching itself. If the teacher is teaching concepts which he/she knows, to the pupils who initially do not know them, on the 'mental picture' theory this would need to involve the transference of the teacher's mental pictures into the minds of the pupils. Again, this might seem to necessitate telepathy; or else, alternatively, it might merely involve the teacher talking to the children, getting them to talk, reinforcing their correct talk and correcting the incorrect, talking perhaps about pictures, diagrams, symbols, maps, graphs, etc., reading and writing as well as talking but, in general, using language, learning the language-games by participating in them. In the latter case (the language-game, non-telepathic) it is clear that the language-game is in fact the area of interaction between teacher and pupils (and between pupils). The language-game is the interface through which all the transactions of the educational situation have to pass. To put emphasis on mental pictures (nobody denies that we do have them) is to distract attention from the central and vital issue of how each individual participates in the language-games. The 'mental picture' theory of meaning, even if it were adequate in itself, would thus be peripheral — at best — to the understanding of educational transactions. In fact it is inadequate and untenable as a theory of meaning, or of what concepts are, and if given emphasis it can lead only to confusion and misunderstanding. Even if it were true, in terms of individual and subjective psychology, that 'concept' = 'meaning of word' = 'mental picture(s)', attention given to the 'mental picture' notion would still be superfluous if it were not downright pernicious.

To appreciate more fully the difficulties of an educational approach based on 'mental pictures' assumptions (in which the question of the transfer of such pictures from the teacher's mind to the students' would necessarily be central), the reader may be referred to John Wisdom's classic, Other Minds (Wisdom, 1952). Prospective readers of that work should be warned that it can cause 'criterial weightlessness' (Stenhouse, 1968) in the reader and probably even physical dizziness!

Turning now to another possible objection to the language-game approach, people might say that there is too much emphasis on language, mere words. As a counter to this objection, it could be pointed out that education does involve using a lot of words, in talking, reading, writing, discussing, arguing, etc., and that it is desirable to learn how to use

words, and language in general, properly. This is true, but it does not go quite far enough. An objector can still point out that much of education consists of doing, not of talking or writing. The Deweyan maxim "Learn by doing" still has a lot of force. And so indeed it should. But the language-game concept as elaborated by Wittgenstein (1953) is emphatically not a matter of mere words — see Sections 1-50 or more of the Philosophical Investigations. The language-game encompasses the people, the various objects that they wish to deal with, the actions they perform, their motivations, their social relationships, the relationships they have been brought up and educated to regard as significant — among which are the relationships between particular words, objects and actions — and so on. Learning the language-game does involve 'doing' as well as talking: the words can be learned meaningfully only when set in their matrix of actions, objects, people, and so on.

Let us briefly consider the very simple language-game with which Wittgenstein commences his Investigations:

"Let us imagine a language ... [which] is meant to serve for communication between a builder A and an assistant B. A is building with building-stones: there are blocks, pillars, slabs and beams. B has to pass the stones, and that in the order in which A needs them. For this purpose they use a language consisting of the words "block", "pillar", "slab", "beam". A calls them out — B brings the stone he has learnt to bring at such-and-such a call — Conceive this as a complete primitive language." (Section 2, p. 3)

Wittgenstein goes on to point out that we can clarify our understanding of what language is and how it works if we get away, temporarily, from the immense complexity of real-life language and study simplified models:

"It disperses the fog to study the phenomena of language in primitive (i.e. simple) kinds of application in which one can command a clear view of the aim and functioning of the words.

"A child uses such primitive forms of language when it learns to talk. Here the teaching of language is not explanation, but training." (Section 5, p. 4)

By this last remark it seems that Wittgenstein is making a similar point to that made earlier (above) when I said that you cannot teach a baby to talk by giving it a dictionary. Neither can you teach it to talk by exclusively talking to it. You must do other things as well, besides talking. You must play with it: at 'naming things' games ('nose', 'mouth', 'ear', and so on), 'action' games ('throw', 'come', 'give', 'fall', and so on) — see how the two basic notions of 'language' and 'game' come in at

the very beginning and basis of the education of every individual who ever lived! (Or certainly the education of every individual who successfully 'made it' into the adult world.)

Note also how the teaching of the baby necessitates not only saying but doing: the adults have to enter into the language-game, and do things with the baby e.g. say 'nose' when he/she points to it, point to something and look at the baby expectantly so that a name may be uttered and then either reinforced or corrected, and so on. Wittgenstein himself describes the business thus:

"We could imagine that the language of § 2 / the language of the builder and assistant quoted above / was the whole language of A and B; even the whole language of a tribe. The children are brought up to perform these actions, to use these words as they do so, and to react in this way to the words of others.

"An important part of the training will consist of the teacher's pointing to the objects, directing the child's attention to them, and at the same time uttering a word; for instance, the word "slab" as he points to that shape. (I do not want to call this "ostensive definition", because the child cannot as yet ask what the name is. I will call it "ostensive teaching of words" — I say it will form an important part of the training, because it is so with human beings; not because it could not be imagined otherwise.)" (Section 6, p. 4)

Yet another feature of the language-game may be pointed out as implicitly recognized by Wittgenstein: the basic importance within the l-g, of non-verbal communication, e.g. "pointing", "directing attention". One can instance an enormous range of behavioural elements which are important in this way: gesture, posture, tone of voice as distinct from the words uttered, facial expression, and so on. Good teachers have always known how to use these means of non-verbal communication in their teaching. They are (or ought to be) of particular interest to teachers of science, since the findings and methods of the new science of ethology (Tinbergen 1951) are being increasingly applied to human behaviour (see Eibl-Eibesfeldt 1970, 1971, Hinde 1974, Argyle 1975, Blurton Jones 1972, among many other works) and are likely to affect a substantial revolution in our understanding of and control over human behaviour. Little research has yet been attempted on non-verbal communication in contexts of formal education (but see Blyth 1976, Omark et al. 1976 and refs therein), but it seems clear, on a variety of grounds, that the ethological dimension is going to be of great importance to education in the future. As was argued earlier, the 'teaching' language-games are the interface through

which the processes of education actually occur; and in these language-games the interaction of verbal and non-verbal communication and of the dimensions of learning, instinct, and intelligence, are of crucial importance. Bruner (1960, 1972) and Jones (1972) among others have urged the desirability of paying attention to the processes of education instead of concentrating exclusively upon its outcomes. From the standpoint of philosophy, the Wittgensteinian language-game approach embodies the clearest possibilities of appreciation of the actual processes of education. It allows for the incorporation of considerations from philosophy, at one extreme of abstraction, to ethology and sheer commonsense practicality at the other. It allows for unification in approach: one can teach Science Education and Philosophy without having to 'change gear': the differences in subject-matter can be used for mutual enlightenment when they are set within a methodological continuity, that of the language-game. But as pointed out in the foregoing, we always have to be careful as to which language-game we are using — many texts, in Science Education as in other fields, confuse their readers by mixing up bits of different and sometimes incompatible l-gs. Being aware of the fact that there are many different l-gs is, of course, the first step towards understanding them, avoiding confusion, and integrating our fields of knowledge.

Returning to the passages quoted from Selberg, Neal and Vessel (1970) Discovering Science in the Elementary School, we can now take up the question of whether the same points can be made, and made at least as effectively (or better), in terms of a language-game approach.

Before I offer an l-g reformulation of the passages in question, let me make an important stipulation as regards educational methodology. I do not regard it as good educational strategy, in general, to begin as do Selberg, Neal and Vessel, with definitions. (There are exceptions to this: axiomatic systems of formal logic, Euclidean geometry, etc. by their very nature begin with definitions. Nevertheless even here, while the subject-matter to be taught begins with and is based upon definitions, the teaching of it should begin with an explanation of what sort of an activity 'doing logic' or 'doing geometry' is going to be. This will be explained more clearly as we proceed.) In education it is necessary to begin with what the pupil already knows, has already experienced. This is truistic — but does not thereby cease to be true, and important. Offering a definition at the beginning of a lesson is likely to mystify the students (if the subject of the definition is unfamiliar to them), or else bore them (if it

is something that they think they already know, something commonplace); and either way, their attention and interest are likely to decline. It is of course possible, and sometimes desirable, to offer some preliminary explanation of what one is going to be dealing with — but to do this is to do something rather different from giving a definition. Better just to talk (or write) about the subject-matter, explore some of its characteristics (or possible characteristics) and then perhaps move on to attempt a rigorous and comprehensive definition, if that seems to be desirable ... but to commence with a definition, especially of such an abstract notion as that of 'fact' or 'concept' as an education procedure is always suspect. Premature definition is dysfunctional.

With this proviso, the implications of which will be clarified and amplified as we proceed, let us attempt a reformulation of the textbook passages already quoted, as follows:

"A fact is what you can state to be the case, so that other people can understand your statement and check up on it for themselves. Many facts can be checked directly by observation (e.g. "Grass is green"); but this depends on all the people involved having already learned to use language correctly, so that they can know what is being talked about, what sort of things can be said about it, and so on. For them to have learned to use language correctly, they must have learned how the various words relate to material objects (e.g. grass), to the things people do (e.g. talking about grass, colours, whether the grass is growing and may need to be cut soon, whether it may be dying because the weather has been dry, and so on), and how the words can be put together to communicate with people so that they can act in various ways. Learning to use language is thus not just a matter of 'putting words together' — it involves learning how people, actions, words, things and the world in general can interact; and this complex totality can be called the language-game.

"Within the general language-game we can distinguish an enormous variety of particular language-games, each with its own special words and ways of talking and behaving: motor-mechanics' language-games; sporting language-games ('goal', 'pass', 'passing shot', 'drop-kick', 'drop shot', 'ace', 'tackle', etc.); and education l-gs among many others.

"Knowing a concept is knowing the rules for the use of a word. These rules are learned mainly tacitly: we learn the language-games by hearing, seeing, and generally, by participating in them. 'Mental pictures' may be associated with particular words, but must not be thought of as constituting the 'meaning' (i.e. rules for the use) of the word. When I read, hear, or think of the word 'cat', for example, I may have a mental picture of a favourite pet,

grey with a white bib and a quick and loud purr — but if the mental picture constituted the meaning, I would have trouble in thinking about e.g. lions as cats. In fact I know that 'cat' is applicable to an indefinitely large range of variation of the ordinary domestic cat; and can also be used in the generic rather than specific sense to apply to lions, tigers, leopards and other animals as well. And in knowing this, I do not have to pass rapidly through my mind a whole series of mental pictures ..."

The above reformulation is rather longer than the original by Selberg, Neal and Vessel — but not only does it avoid the misleading implications of the 'mental picture' theory of meaning, it provides a basis for a very much wider development of complex ideas as to the very nature of scientific activity. For example, most textbooks on Science Education present science on a basis of what might be called 'naive realism': facts are collected, classified, general statements initially designated as 'hypotheses' then later (if they are found to be in accord with further facts) as 'theories' or 'laws' are formulated, these form a foundation for the discovery of more facts, and so on. Now this is not too grossly misleading as an account of 'normal' science in Kuhn's terminology, but it makes any explanation of 'revolutionary' change highly problematical. It leads to misunderstandings, like thinking that Einsteinian relativity physics has in some way falsified or disproved Newtonian physics: and if the Newtonian physics was true, how can it now be false? If it did fit the facts, how come it no longer does so? (The alternative horn of the dilemma is, of course, that Newtonian physics never did fit the facts, never was true — but this is implausible too, for how could people have been deceived for so long?) The key to the paradox is to get away from naive realism and to appreciate that 'facts' are not simple, we do not simply receive them, passively, like bits of objective reality being injected, as it were, into our consciousness. What we perceive is conditioned and influenced by what we think, and what we think is conditioned by how we talk, how we use language. As Kuhn points out, people came to observe electrostatic repulsion only after the idea had been talked and written about — previously they had seen the phenomenon as simple mechanical 'bouncing off' or else as 'falling off' due to gravitation (Kuhn 1962: p. 116). In other words, while the material-objects parts of our language-games affect the 'linguistic' or verbal parts, the converse is also true: the verbalizations affect how we perceive even the material objects, and also have a widely-recognized effect on our more 'evaluative' perceptions,

i.e. whether we perceive certain things, persons, animals, etc., as being 'good' or 'bad', 'benign' or 'threatening'. To give a general account of the situation, it may be said that the question as to which 'facts' are relevant to a given issue, and even as to which facts we are to take as 'existing', as being facts, is to be settled by the nature of the relevant language-game. In one language-game one set of 'facts' is constituted; whereas in a different l-g these first facts (or some of them) may not be relevant and in a sense may not even exist, while a different set of facts comes into relevance and/or 'existence'. (This issue has been further elaborated in relation to the 'normal'/'revolutionary' distinction in science in Stenhouse 1971). One basic reason for this is that, as the language-game changes, the meanings of different words change too.

To sum up this section, the following outline of the several related but logically-distinct language-games mentioned in the foregoing may be found useful:

Language-games of Science and Science Education:

- (a) 'Teaching Science' l-gs, as found e.g. in Science textbooks.
- (b) 'Science education' l-gs:
 - (1) 'Science educators talking about science education' l-gs;
 - (2) 'Teaching children Science' l-gs;
 - (3) 'Teaching Science educators' l-gs.

It may be pointed out that there are at least two more

major categories in the family of Science Language-games, viz:

- (c) 'Reporting outcomes of empirical research' l-gs; and
- (d) 'Arguing for 'revolutionary' (i.e. conceptual) change' l-gs.

As Kuhn points out, science textbooks often present summarized versions of (c) and (d) in a systematically distorted and misleading form — so we must be cautious and critical in accepting the version of science presented under (a)!

Some people might wish to query whether deficiencies in basic methodological position — even granted that such deficiencies have been demonstrated — need make much difference to the details of Science Education especially at elementary levels. "You can shoot holes in the philosophy of Science Education", they might say, "But this doesn't make much difference at the practical level. The facts and techniques we are dealing with remain the same, irrespective of what sort of methodological/philosophical position we adopt. We can continue, in practice, just as we always have."

Against this, a number of retorts may be made — and should be made, need to be made.

Perhaps the first and in some ways the most important rebuttal could take this form:

Assume if you like that the methodological/philosophical issues discussed in the foregoing are irrelevant to practice, and/or that the actual arguments given are faulty in some way or another — still, even at the level of facts and practice it is evident that something is wrong with science education (and at second remove, with Science Education). The Drift from Science is itself a fact which indicates that something is wrong (and there are other facts as well which tell the same general story) — if you don't want to accept the diagnosis just given, then some other explanation must be offered which might account for the observed phenomena.

It is true that there are scientists and educationalists who would dispute the methodological diagnosis given in this thesis and even the existence of the Drift from Science. (But remember that the diagnosis has been put forward, as stated quite explicitly in Chapter 2, on a purely hypothetical basis. The methodological issues that have been discussed would be significant, as has been argued, even if there had been no factual indication of inadequacy in the shape of the Drift from Science. And the texts which have been discussed are themselves evidence, as objective as can be got, of methodological short-fall.)

For example, some scientists dismiss the Drift as being due to lack of intelligence and/or perseverance in the young people of today. "Science is too tough for them!" could sum up the attitude involved.

This easy option, of blaming the problem on the young people who 'vote with their feet' and go into the social sciences and humanities rather than the 'pure' or 'basic' sciences, not only neglects the possibility that there may be real deficiencies in current science education, but also tends to be internally inconsistent. Since the protagonists of this view want to insist that some young people do opt for the sciences, i.e. they do in a real sense choose, and some of them are presumably both 'intelligent' and 'hard-working' (as those terms are used by the supporters of current practices in science education) since some of them end up with 'good' classes of Honours in their degrees, it would appear to be difficult to sustain the view that all those who do not enrol for science are either

unintelligent or lazy or both. 'Intelligence' can be measured independently of students' aptitudes for and interest in Science — though 'diligence' as against 'laziness' is more difficult to assess, without begging the question — and I know of no study published within the last two decades which establishes that only high-IQ students go in for 'pure' science and only low-IQ ones into the other academic disciplines. In other words, this particular defence of the status quo in science education, in that it merely offers a blanket answer ('students not choosing science are either stupid or lazy'), is no answer at all. If it is granted that some students do choose science, i.e. they are intelligent and diligent enough to succeed in science courses and, presumably, in most other courses offered at tertiary level, and they choose to do science, then the facts of the situation demand that we recognize that other students, of equivalent ability so far as independent evidence allows us to assess this, do choose to keep away from the pure or basic sciences and go instead into the social sciences and humanities. It is desirable to dignify the choice of 'non-science' as being a choice, no less rational than a choice for science, instead of trying to pretend that a choice of 'non-science' is not really a choice ('the students concerned couldn't have succeeded in science anyway') or that it is in some way non-rational. If we recognize the reality of choice, both ways, it is possible then to get down to investigating the basis of choice as exercised by both those who opt for science and those who do not. In this thesis we are not concerned with empirical investigations of this sort: we are concerned with the conceptual framework within which all such investigations must take place. We have stipulated, right from the start, that the choices made by students with regard to science/non-science careers are real choices, that there are grounds for rational choice (this is not to say that particular individual's choices may not turn out badly, or that all individual's choices are fully rational), and that as a working assumption we would take it that present-day science education does have room for improvement, i.e. it is not perfect, i.e. it does have shortcomings and/or faults. We have been looking into the possibility of there being faults in methodology. There are other possibilities that need looking into. For example, it is possible that the techniques of classroom teaching in science are sometimes/often faulty. This could be investigated. It would need a different type of investigation from the present one, however, for obvious reasons. The fact that this thesis has not investigated this sort of possibility does not indicate that I think all is well in terms of classroom interaction in science teaching. Individual differences in teaching style are great, are known to influence

pupil responses (see e.g. Hudson 1968), and have not as yet been adequately studied. The new and rapidly-developing science of Human Ethology (see Russell and Russell 1957, 1961, Fletcher 1957, Eibl-Eibesfeldt 1970, Morris 1977) will have a great deal to offer in terms of enhanced understanding of classroom interactions, in the teaching of science and all other disciplines. (But conceptual-level problems, of the relationships between the major behavioural categories relevant to education, viz. learning, instinct and intelligence, are substantial; see Stenhouse 1977a and b). Quite apart from the interactions between the persons involved in the educative processes, there are questions about the content, the 'facts' and techniques being taught in science education, which can profitably be discussed in detail only in relation to the particular sciences in question. But while it is true that such matters of detail are intelligible only in the context of a specific science it is also true that this 'context of intelligibility' also necessarily includes methodological considerations, of two sorts:

- (a) those relating to the methodology (and philosophy) of the science(s) in question; and (b) those relating to the methodology/philosophy of education.

One of my main theses is that all endeavours in science education do necessarily involve presuppositions about methodologies, the aims of such education, and so on (this is in the sense in which it can be said that everyone does have a 'philosophy of education', one cannot not have a philosophical/methodological position — see earlier Chapters) — yet because the various methodological commitments have not been made explicit, have not perhaps been brought even to the level of conscious awareness, they have often embodied selfcontradictions which become manifest in the form of the confusions exposed in the textbooks discussed earlier in this Chapter.

As a means towards exposing the complex, intimate and strategically influential mingling of the methodologies of science and education, let us briefly consider some of the implications of several distinguishably different aims for science education. For convenience, let us restrict ourselves to a context of, say, the final two years of secondary schooling (though it will be apparent that many of the considerations relevant at this stage will also be relevant, perhaps in modified form, at both earlier and later stages in education).

Among the many possible aims for science education we can distinguish

the following:

- A. That pupils should learn about the findings and techniques of science.
- B. That pupils should be given experience of actually conducting scientific research.
- C. That pupils should be given training to enable any of them to get a scientific job.
- D. That pupils should be educated so that they can develop their capacities for creative contribution to science.

These four possible aims have been chosen, out of an indefinitely large number of alternative formulations, because they offer significant similarities and differences among themselves and between each of them severally and the actual realities of policy and practice in science education. Without attempting a comprehensive discussion of all aspects of possible comparison, the following points are offered as significant:

The aim of giving students experience of actually conducting scientific research (Aim B) runs into the dilemma of either restricting itself to an essentially artificial and indeed fraudulently-structured situation (that of 'discovery learning' as discussed earlier in this Chapter, in which the teacher knows what the 'discovery' is supposed to be, but pretends to the students that they are going to make real discoveries); or else the 'scientific research' which the students carry out is nothing more than a series of technical exercises. The latter possibility would bring Aim B into line with Aim A (teaching students about the findings and techniques of science — Aim B emphasizes the need to have experience in actually performing the techniques), and is also in reasonable accord with much of what actually happens in the teaching of science. Limitations of time and facilities usually entail that pupils' direct first-hand experience of techniques is restricted — they are told about (Aim A) more than they directly try out for themselves (Aim B) — nevertheless it is generally accepted that some direct experience is better than none. A combination of Aims A and B would thus be more generally acceptable, in relation to practicalities, than would either A or B on its own.

Moving now in another direction, Aim D (that pupils should develop their creative potential) has been widely espoused, on two main grounds:

- (a) Societal needs for innovation and discovery in basic science and in the derived technologies; and
- (b) The desirability of providing, in science education as in all other aspects of education, for the actualization of individual potentiality.

It is of course recognized that not all pupils have potential for creativity in science — but since the diagnosis of creative potential is more of a hope than a reality, if any are to be given education appropriate for the actualization of their potentiality, then (more or less) all must be given the same. In practice, all are not given appropriate education for developing whatever scientific creativity they may possess, due to the operation of optional specialization. Rather than make this a reason for attempting to impose greater uniformity (i.e. abolish optional courses), there would appear to be better long-term prospects, at both individual and societal levels, if students were encouraged and enabled, to tackle a larger variety of 'specializations'. The drawback is not in optional specializations in themselves, but in the institutional devices and the societal ideology which limits individuals to only one 'specialization'. More on this later. The general trend of argument here is simply the encouragement of diversity.

On the creativity issue, I have argued on psychological grounds (Ev. Int. Chapter V and X) that potentially creative individuals of some types are unlikely to respond to efforts specifically directed to enhancing their creative powers, and indeed may respond negatively. The desirable educational strategy that seems to emerge from such considerations would be one of the endeavouring not to destroy or distort, rather than positively to encourage, individual creative potential. Unfortunately, a number of considerations suggest that much science education at the present time does actively tend to discourage creativity. Questions of whether or not and to what extent this is true, are beyond the scope of this thesis — we are assuming that it may sometimes be true, and are exploring, on the basis of that assumption, various possibilities for improving science education — but at this point we are dealing with the much more limited question, and using the creativity issue merely as an example, of how various different aims for science education may be in conflict with each other. Unless their practical implications are very carefully thought out, attempts to implement partially-incompatible aims simultaneously can cause nothing but trouble.

It seems that we might be able to say that too much emphasis on Aims A and B (knowledge of the findings and techniques of science, and proficiency in performing the latter) would be inimical to the accomplishment of Aim D (encouragement of creativity), especially if 'creativity' were to be envisaged in terms of revolutionary rather than normal

science. A heavy emphasis on the existing normal science and its underlying paradigm must, it would appear, tend to make people less likely to question the paradigm and thus less likely to come up with revolutionary new ideas. (Perhaps if 'revolutionary' thinking in Kuhn's sense within science is stifled, some potentially-creative individuals are more likely to turn their talents towards revolution in other, non-science, spheres.) To the extent that 'normality' within a science is over-emphasized in the educational processes and in the 'rites de passage' that precede the admission of practitioners, that science is going to become increasingly dependent on the 'outsider effect' for its theoretical advancement. 'Rigidification' (Morris 1970) of behaviour is just as maladaptive for a society (or a sub-group within a society) as it is, at individual level, for the individual. But what, at the operational level of educational practice, does 'over-emphasis on normality' (and related terms) really mean?

Without going deeply into the matter (a book on this topic is in preparation), it may be suggested that 'over emphasis' is only partly a matter of what is told to students: mainly it is a question of what is demanded of them, what they are made to do, e.g. in the form of examinations. Also, apart from questions which are in a sense 'internal' to the examinations, exercises, etc., that students are made to do, there is the 'external' question of what use is made of the results of examinations and other forms of assessment. If for example research opportunities are to be restricted (as increasingly they are) to individuals who have been awarded First Class Honours, then a great deal of already-extant research (refs in Stenhouse 1971, 1974, 1977a) suggests that substantial 'amounts' of creative talent will be excluded from contribution to science (or, more accurately, from contribution to the so-called 'pure' or 'basic' sciences: for on present showing many potential creatives are going into the social sciences and humanities; and probably a significant proportion are opting out of further academic study completely). Hudson (1968, Chap. 8) has reported an experiment in which the normal mechanisms and criteria for selection of candidates for university study were ignored and a group of students admitted on 'sub-standard' performance at the usual tests (other criteria were used in addition). By the end of their university courses these students were performing, if anything, above the average of those admitted on orthodox criteria. Their 'scatter' seems to have been greater — but, significantly, some of them were reaching the best standards achieved by those specifically selected on the orthodox criteria as being of the very highest

potential. The general moral is, that it is unwise to take the implicit predictions of exam results too seriously.

Returning finally to our set of hypothetical aims for science education, the likelihood of conflict between Aims C and D must be noted. Aim C is 'that students should be able to get scientific jobs', while D relates to creativity in science. Creativity tends to imply non-conformity to the 'rules' within the discipline (and does^{so} virtually by definition where 'revolutionary' creativity is involved, cf Kuhn 1962, Stenhouse 1971). For the vast majority of those working within science, however, even those whose function is nominally 'original research', there are demands of varying severity for conformity both intellectual and social. Various authoritative estimates put the proportion of professional scientists who are in a position to make reasonably independent decisions regarding their research as around only 5% of the total; and some estimates are much lower.

Again, provisos:

It is not being suggested that conformity to the current paradigm within a science is bad in itself (nor that social conformity is bad in itself.) Clearly, the majority of individuals prefer to be 'in step' with each other; many prefer, indeed, to be told what to do. And just as well, too! All Chiefs and no Indians tends to be counterproductive. But the danger to be guarded against is, not that the majority of scientists will conform to the current paradigm (Kuhn has argued that concerted effort within a paradigm leads to greatest productivity — see further discussion below), but that exploration of alternatives will totally cease, i.e. even the small minority of non-conformist 'revolutionary' thinkers will be squeezed out. (Out of science, and/or out of non-conformity, see Ev. Int. Chap. V.) This need not be bad for the individuals involved — they may find other fields in which to actualize their creative potential — but it would certainly be bad in the long run both for science and for the societies involved.

It may be worth emphasizing, in this connection, that progress in science need not be tied to 'progress' in the sense of quantitative expansion in technology and in economic activity. The achievement of zero population growth and a 'steady state' economy (or even 'negative population growth' and a contracting economy, as may be required) must depend upon both scientific and especially educational progress — especially the latter. A permanent and stable adaptation of the human population of the

world to the resources available to support it may well demand much more from humanity, in terms of intelligence, imagination, planning and restraint etc., than has been required for all the scientific and technological achievements so far. Putting a few people on Mars is a trivial achievement compared to the problem of ensuring the continuation of Homo sapiens populations here on Earth.

If we are to achieve long-term adaptation to a world in which some of the resources which have been essential for the technological advances of the last couple of centuries are in ever-diminishing supply or are totally exhausted — and if we wish to retain as many as possible of the technological benefits already achieved by our civilization, along with whatever modifications of life-style and technology as may be forced upon us — it will be necessary to continue and even perhaps increase the present levels of collective effort in scientific research. The subject-matters of research will certainly need to change quite substantially, with probably less emphasis than at present on the physico-chemical sciences and much more on the bio-behavioural. Methodology within science and especially within science education will also need to change (some of the reasons for this have already been indicated). But there are no grounds for concluding that the total 'quantity' of scientific activity will or should decrease — rather the contrary.

It should be clear from the foregoing that the all-important details of day-to-day teaching in the sciences must almost inevitably be a compromise between the various functions that a society demands or sets as aims. The four possible functions/aims discussed briefly above are, as already stated, only a few out of the large number that could be formulated. The actual educational practices used in science education have in fact always been compromises between different and partially incompatible aims — but they have usually been compromises arrived at largely as a result of expediency, tradition, etc., and usually without conscious and explicit discussion and rationalization. To the extent that practice has not been the outcome of adequate examination and rationalization of conflicting aims and presuppositions, etc., then, it could be expected that it would be less than satisfactory — and this brings us to the question of the benefits of methodological/philosophical discussion. Put in the form of a simple dilemma, we can say that either current practices in science education are satisfactory, or they are not. If current practices are not satisfactory, then conceptual-level exploration of the nature of science and of education in

science, in relation to various alternative aims/functions, may enable better compromises between the various aims to be achieved in relation to resources available. Success in this is not, of course, guaranteed, any more than it is in any creative endeavour: and it may need to be emphasized that what is sought is not necessarily one particular pattern of compromise: a plurality of compromise-patterns might well be best.

Looking at the other horn of the dilemma, what can it mean to say that current practices 'are satisfactory'? At one extreme this might mean merely that we are prepared to put up with them, uncritically and without even looking for possibilities for improvement. At the other extreme, it could mean that possibilities for improvement have been explored exhaustively, and none have been found. In this latter case it could be said that we know that our practices in science education are satisfactory. But to what extent could we be justified in asserting that current practices are satisfactory, in the absence of an extensive (if not literally exhaustive) exploration of possibilities for improvement?

Thus an attempt to deal properly with either horn of the dilemma must involve us in methodological/philosophical exploration — avoidance would commit us to mere hand-to-mouth expediency at the level of practicality; and there has been too much of that, in science education so far.

It is necessary to recognize, however, that a number of people involved in Science Education believe, no doubt sincerely, that science education as practiced, and Science Education as an academic discipline, have already had the benefit of philosophical investigations. Some of our examinations of textbooks, earlier in the present Chapter, might serve to throw doubt on the merit of much that has been attempted along these lines — but further examination of the relationships between science, science education, and philosophy needs to be undertaken. It will form the substance of the next Chapter.

CHAPTER SEVEN

PHILCSOPHY AND THE 'OPTIMIZATION' OF SCIENCE EDUCATION.

In order to decide whether or not a particular science is being taught in a way which is reasonably near the optimal, it is necessary, as was pointed out in the previous Chapter, to have explored a fair proportion of the possible ways of teaching it. Such explorations need not be full-scale and long-term 'real life' exercises. There are grounds for arguing that largescale practical implementation of exploratory approaches should not be undertaken, or at least that not all the possibilities should be explored: because some approaches, though possible, would be definitely unsatisfactory, ex hypothesi; and it would be unethical to subject students to instructional approaches which were known in advance to be sub-optimal.

The ethical/moral issues here are clearly important. They are similar in many respects to those that arise in say medical research: how far can one be justified in using unproven techniques upon other human beings? How far can one use people as guineapigs? Some extreme views on this sort of issue are taken, e.g. some people would say that it is never justifiable to use an unproven technique i.e. to use another human being 'experimentally'. But while some extremes of experimentation are obviously immoral (e.g. Nazi experimentation in concentration camps), one point that has to be recognized is that a great deal of what we actually do, with each other and to each other, is necessarily experimental, in the sense that we simply do not know in advance what the outcomes of particular interactions (medical, educational, etc.) are going to be.

This consideration cannot be used as an excuse for mere ignorant meddling. Probable outcomes must be assessed as accurately as possible before any real-life experiments are attempted. In the case of educational experiments (whether in science or any other field), one problem is endemic: that of 'Hawthorne effect': that almost every new approach seems to be better than the old 'standard' approaches — but only while it is new, while it is still an experiment. If it is implemented on a wide scale and thus itself becomes standard, the effectiveness of the innovation often falls off (sometimes below the previous standard). In short, it is

the newness, the novelty of the approach, and not necessarily its intrinsic merit, which causes the initial improvement. This suggests that educational research ought to place greater emphasis on theoretical rationale, and less on what seem to be 'empirical' results, than is done in the various physical and biological sciences. Practical or 'empirical' results can be peculiarly misleading in education.

With regard specifically to science education, what is involved in 'exploring possibilities'? If we are thinking of this in terms of theoretical exploration (as against practical experimentation, which could be done, and often is, very much on an ad hoc basis), we come up against the problem of how to characterize scientific activity in general terms. As soon as we have recognized that the recipe 'scientific method = observe, hypothesize, test (by experiment and/or observation), modify hypothesis, ...' is so simplistic as to be virtually fictitious, we are forced to recognize also that, among those who have attempted to make general accounts of the nature of scientific activity (viz. philosophers of science), there is considerable disagreement about many of the basic issues. But perhaps the most important point is that of realizing and accepting that the theoretical exploration of possibilities for science education does involve us in Philosophy of Science. Some of the possibilities which we have to examine as ^{at} educationalists are, precisely, the possibilities which form the substance of Philosophy of Science. So we are involved, whether we like it or even are aware of it or not, in a confluence between Philosophy of Science and Philosophy of Education.

Does 'exploring possibilities for science education' mean, then, that we should study Philosophy of Science and Philosophy of Education, and then come up with a blueprint listing what the possibilities are and which of them are preferable, and so on? No, I suggest it does not. In the first place, what we are really dealing with is not 'Science', but many sciences. We cannot assume in advance that 'Science' is unitary, that there is such a thing as 'the scientific method' — these are issues that are in debate in philosophy of science at the present time. It would seem to be more sound, both scientifically and philosophically, not to commit ourselves prematurely to either 'unitary' or 'pluralist' assumptions about the basic nature of scientific activity, but rather to accept, on the quasi-empirical level, that we are faced with a number of sciences which have in fact developed as separate disciplines (this does not commit us to saying that they are necessarily separate ...) and that our 'explorations of

possibilities' must start — though they need not necessarily continue — with each discipline, each science and perhaps even each sub-science, taken separately.

It is clear that in order to explore the possible ways of teaching any science, the science in question must itself be known and understood. Since no one person could possibly know more than a small part of the total spectrum of all the sciences, it is clear that explorations of possibilities for teaching would have to be done, initially independently, by a largeish number of individuals each working on the basis of his own special field. This kind of programme would entail neglect of the question of integration between the sciences — itself an immensely difficult and important question for science education — but it seems best to concentrate for the moment on how 'exploring possibilities' for different educational approaches could be accomplished in relation to any branch of science, any particular scientific discipline, taken on its own.

So what would be required, for a substantial range of the possible educational approaches for the teaching of a particular science, to be explored?

I suggest that we should stipulate that such explorations can be achieved only by persons having sufficient background in:

- (a) the science(s) in question;
- (b) Education, both theory and, especially, practice (practical experience that has been thought about, see below); and
- (c) Philosophy — again, competence in the activity of philosophy is what is needed.

Without arguing the need for each of these severally and at length, it may be convenient to take them for the present merely as stipulated bases for argument, and go on to explore the sector of inter-relationship between disciplines that is relatively novel at the present time, viz. the interrelationships between Education (and education!), science, and particularly philosophy. I shall try to show that philosophy is important not only for the abstract exploration of new educational approaches, but also that it is of vital significance for the solution of urgent practical and already-existing problems within day-to-day science education in the classroom.

It has been implicit in much that has been said already that the teaching of science, and probably of most other subjects, would be improved if the teachers thereof had a deeper understanding of the methodological/philosophical and historical background of their subjects. It

is arguable that teaching even at school level, certainly in the later years of secondary school and with the more academically-orientated classes, could well incorporate more explicit discussion of these aspects: a significant proportion of secondary pupils are certainly capable of understanding abstract argument and the changeability and 'relativity' of concepts (cf. the 'flat earth' vs 'round earth' antinomy discussed earlier). What is the 'formal operations' stage of Piaget, with its emphasis on abstraction and inferential schemata, if it does not allow for, and indeed demand, the exercise of abstract thinking upon concept-systems which are changeable? It is recognized that not all pupils attain to the level of 'formal operations', but those who continue to the advanced levels of secondary and tertiary education must be presumed to have reached that stage, in most cases anyway. If many of them appear to be somewhat lacking in flexibility of mind (i.e. 'reversibility' etc.), a strong case could be made for regarding them as having been condition^{ed} into rigidity as a result of their formal education (and perhaps other social influences). As a great many people have remarked, formal education does tend to put an over-heavy emphasis on 'getting the right answer', 'knowing the subject thoroughly' (= become deeply embedded in the currently-accepted paradigm), and so on. This can be related to rigidities of discipline within the school; and 'trad' education has often been castigated as a generator of intellectual as well as social conformism. There need, however, be little relationship between the social and disciplinary structure of a school (or any other institution) and its intellectual effects. Naturally, an extreme of harsh, arbitrary and unintelligent discipline can be harmful to all aspects of personal development — but a firmly and intelligently structured social milieu can be highly conducive to intellectual flexibility, originality and creativity (cf. 'Hudson's anomaly', Hudson 1968: 70-71, discussed in Stenhouse, in press). It should be kept in mind that most of the creative thinkers even of this century have been the products of 'trad' schooling. If children are made to learn about the world and learn especially the intellectual techniques which have been used for dealing with it, they do at least have a basis upon which they can, if inclined and if able, practice innovation and alteration: they are in possession of paradigms which they can attempt to negate. But the extreme of permissiveness in education — letting children do only what takes their interest — is likely to leave too many of them with insufficient knowledge and skills to have anything to rebel against, or even the ability to rebel effectively and constructively.

Whether or not philosophy should be taught in schools — and I reiterate my own belief that it could and should be taught, though perhaps better not under the name 'Philosophy' — there seem to be the strongest grounds for urging that more and better philosophy should be taught in institutions of tertiary education, especially to students who may themselves take up careers in teaching. One of the most important practical benefits of this — again to reiterate — is that it should improve the ability of the would-be teacher to understand and interpret the questions and also, perhaps most importantly, the mistakes of his pupils.

The importance of this last point can hardly be over-emphasized. Much of the rigidification at the intellectual level which occurs in schools results from the fact that pupils' queries and mistakes are simply not understood. Often enough the teacher conscientiously gives an answer or attempts a remedial explanation — but it is not an answer to the question that the pupil was actually trying to ask, or not a remedy for the problem that was actually afflicting the pupil. Educational philosophers even of pre-1960 vintage were aware of the fact that in educational discourse (they were thinking, it seems, usually of discourse between educationalists), the arguments and discussions of different people, especially if they express views from different sectors or levels of the educational enterprise, often "pass each other without really meeting" (Broudy 1961: 23). They are not mutually intelligible. This question of communication and mutual intelligibility between people on the adult side of the educational fence has received quite a bit of attention. It has usually been posed as a matter of inter-disciplinary communication. C.P. Snow depicted its most extreme manifestation as a rift between the "Two Cultures", the 'Arts' on one side, the Sciences on the other. But while this is undoubtedly of very great importance, the problem that is always with us — and which, for that very reason, tends to get less than its needed attention — viz. the problem of communication between children and adults, the taught and the teachers, is relatively neglected.

The situation is analogous in some ways to comparative experimental psychology prior to, say, 1970. All sorts of experiments were being performed upon white rats, pigeons, and a few other animals. The results were analysed using the most elaborate statistical techniques — but the outcomes of much of this research were largely indecisive and/or anomalous

(see Hilgard and Bower 1975, Lorenz 1965, Kuo 1967, among others). The reason for this was, generally, that the experimenters had failed to appreciate and understand the natural behaviour of the species in question, hence were not able to pose their questions (i.e. design their experiments) in terms that were intelligible to the animals. Hence the animals were often 'answering' a question quite different from the one being 'asked' by the experimenters; and, naturally, considerable confusion ensued. The remedy was for the experimenters to turn to Ethology and take Tinbergen's (1951) advice simply to spend time watching, listening, and getting to know the normal behaviour of the species under natural conditions: a time-consuming but vitally necessary prerequisite for all research. (For clarification of the essential issue, see Stenhouse 1974: 352.)

It must be emphasized that many teachers do understand the behaviour, the difficulties and the questions of children; and they deal with problems and provide models for the advance towards adulthood, in the most admirable way. And often without the benefit of formal study of the behavioural sciences: it might be said that these sciences hitherto, if taken too seriously, would be more likely to distort than improve communication between adults and children. Luckily, it seems we not only have inbuilt templates to guide our interpersonal behaviour towards a reasonable degree of adaptiveness (Morris 1977 and refs. therein), but many adults retain the ability to see a situation for what it is and to change their own behaviour if it is not achieving the desired results. They retain the ability to learn from experience and to discard, where necessary, various 'scientific' policies which have been foisted on them.

A proper understanding of non-verbal communication (see e.g. Hinde 1974) on the part of the teacher can make him/her aware of problems and failures in communication — but the full diagnosis and sorting out of the problem often involves the cognitive/intellectual dimension as well, and the verbal means of its communication. Both verbal and non-verbal elements, the cognitive/intellectual and the instinctual, enter into the language-game. Some understanding of the 'language-game' concept and its application in practice, e.g. the fact that a variety of different language-games enter into the study and teaching of any academic subject such as one of the sciences (as was shown in Chapter 6), can provide a valuable advance in insight into the transaction of education and of communication generally. But while recognition of the nature of language-

games is necessary, it is not a sufficient condition for effective teaching: teachers, indeed adults generally, need to be able actually to play the appropriate language-games effectively, in 'real life', if the children in their charge are to achieve proper development. With regard to children's questions and problems, one of the salient issues is that of 'translation'.

This question of translation, of working out which language-game the pupil is actually using on a given occasion, is both crucial and sometimes very difficult. Using the term 'translation' can indeed be quite misleading. It suggests the relatively familiar process in which we take the words of one known language and transliterate them into (more or less equivalent) words of another known language. For most of us, the languages we translate between are fairly similar, they are likely to be from the same linguistic families, e.g. European languages. Translation between highly dissimilar languages, e.g. English and Chinese, is a more complex and more problematical business. And the most appropriate analogue to the situation where the teacher is trying to understand the problems and/or mistakes of the pupil is probably the situation where a translator is faced with a completely unknown language. The first problem is to decide what sort of language it is, what its structure consists in, and so on; and only after this has been sorted out can the task of translation, in the immediate sense, be attempted. In short, the problem which faces the teacher may sometimes be more like the decipherment of Linear B (see Chadwick's 1958 book of that name), rather than a simple translation from German or Latin, say, or even Chinese, into English.

To the extent that these considerations apply in the teaching situation, it is apparent that the successful teacher must satisfy a number of criteria. He/she must be acquainted with the nature of the 'translation' problem springing, as it does, from the existence of a number of possible language-games. He/she must have developed some ability actually to do the required identification (of the language-game being used by the pupil) and translation — this is where extensive experience in teaching, provided the other major criteria are satisfied, can make a tremendous and positive difference to a person's effectiveness as a teacher. Clearly, the teacher must have both patience, and concern for the individual pupil, to take the time and trouble to engage in enough 'translational dialogue' to get the pupil's use of language-games sorted out; and the institutional context must allow for such dialogue (which need not always be on a one-to-one

basis). And so on.

What is significant for our purposes here is related to the matter of 'experience', just mentioned. Other things being equal (and often they are not), the more experience a teacher has, the better. That is, when one particular individual is considered, this person is likely to be a better teacher roughly in proportion to the amount and variety, etc., of the experience he/she has had: the experienced state is better than the inexperienced. (It is of course quite illicit, and does immense harm, to extrapolate from the intra-personal — one person at different times — to inter-personal comparisons. To assume that A is likely to be better than B, simply because A has had more time in teaching, 'more experience' in a quantitative sense, is quite wrong. Some people learn nothing after decades of 'mere experience' — others can gain remarkable insights and competencies in two days.)

Experience of teaching is one thing, then, and experience of thinking about and learning from one's teaching is something different. The one does not necessarily lead to the other. Nevertheless it would appear unlikely that anyone could possess teaching ability in the total absence of experience. There is a similarity here with the relationship which was explored earlier (Chapter 5) between a person's experience of the activity of philosophy and the development of that person's 'philosophical position' (I am deliberately conflating for the moment the earlier distinction between 'personal position' and 'particular position').

With regard to the translation of children's language-games, the extent and diversity of the teacher's experience of such language-games must obviously increase his/her (hir?) potential for coping with them effectively — other things being equal. But this type of experience, from its very nature, can be expected to provide an understanding mainly of the average range of variation likely to be found in the classroom. From several important points of view, however, coping with an average range of possibilities is not enough. With regard to the ideal, widely avowed in educational circles, that every individual pupil should be assisted and guided in the full actualization of his potentialities ('each to his full stature', is the slogan), should those pupils whose potentialities are non-average simply be neglected (as they often are, under present regimes)? If not, then teachers ought to be prepared to cope with language-games which may be 'highly non-standard'. From a societal point of view, too, the extremes of divergence from the average

need to be dealt with: a society has a duty to help those at the lower extreme of ability (this is widely recognized, though the appropriateness of what is provided may sometimes be open to question); and a society which fails to encourage the talents and elicit the allegiance of those of higher ability — especially the tiny minority having major creative potential — is unlikely to prosper and may indeed fail to preserve its own existence as an autonomous society. In their humanitarian endeavours to aid the handicapped, some educational policy-makers have neglected those pupils who are 'exceptional' at the top end of the ability-scale. They have failed to perceive, perhaps, that the productivity of a society must be sustained if aid is to be available for the handicapped; and that productivity is basically a matter of the efforts of the main bulk of 'average' people working to implement the innovative ideas of a creative minority. If either the main body of a society fails to work well enough, or the flow of new ideas dries up, the society will be taken over by other societies which are more viable. But even before this, aid will simply not be available for the handicapped.

The ability of teachers to deal appropriately with 'non-average' language-games emanating especially from the more gifted of their pupils is, then, of quite crucial long-term significance. The nature of the problem may be illustrated with reference to possibilities which arise with regard to examining and assessment in the upper levels of the educational pyramid, and especially with multiple-choice objective tests. Let us consider some of the possibilities of these in detail. We shall assume a context of science teaching (more specifically, science examining as part of science education) at a tertiary or upper secondary level; and I shall assume an 'objective' multiple-choice test of the form in which candidates have to choose the one 'right' (or 'correct' or 'most correct' or 'most nearly correct') answer out of say four or five alternatives, for each question. From the examiner's point of view, the problem is, to surround the 'correct' answer with alternatives (the 'distractors') which are plausible enough to be selected as correct by students having an insufficient (or an erroneous) understanding of the subject.

In an objective multiple-choice question, the distractors have to be untrue, or unsatisfactory in some way, in order to be distractors and avoid being the correct answer. A dilemma arises in relation to the currently orthodox theories and presuppositions of normal science, in that distractors tend to be either:

- (i) Definitely and unequivocally false —in which case they must tend to function less well as distractors, especially for more able students; or
- (ii) Highly effective as distractors — but their equivocal nature, on which the distracting effect depends, must then tend to render at least some of them only problematically false (or only problematically to be rejected, which is not the same thing at all).

The second horn of the dilemma is the interesting one.

It may be illuminating to make explicit the difference between two logically distinct classes of distractors:

- (a) Those that are known not to be the case.
- (b) Those that are not known to be the case.

Both (a) and (b) are in contrast, as they must be, with the 'correct' answer which might be characterized as being 'known to be the case'. But (a) and (b) are in contrast with each other in terms of various possible implications which may severally be drawn from them. The exact position of the word 'not' can be important. For example, distractors of class (a), propositions which are known not to be the case, are for that very reason likely to be less effective as distractors than those of class (b). An effective distractor needs to have some plausibility. But distractors of class (b) run into a different problem: some of them may in fact not be false. And if they are not false, they are not distractors, they are true alternatives and ought to be accepted as such.

This possibility, regarding propositions of class (b), those which are 'not known to be the case', can be put in other terms by saying that some distractors of class (b) may in fact adumbrate advances in scientific understanding. This deserves more detailed discussion.

'Advances' in science have been argued by Kuhn (1962) to be of two kinds, 'normal' and 'revolutionary'. These have been characterized (Stenhouse, 1971) as being 'additive' and 'transformative' respectively. It will be instructive to examine the possible relationships of distractors of class (b) with each of these types of advance.

Kuhn distinguishes three types of advance within normal science (Kuhn 1962 23-34):

- (i) The discovery of new facts.
- (ii) Enhanced precision of measurement.

(iii) Extension of the range of application of current theories.*

In relation to this, distractors of class (a) might be characterized as propositions which are the logical contradictories of propositions asserting known facts. But apart from mere formal negating of factual (i.e. descriptive) propositions which are known to be true by observation, what is to count as a 'contradictory'? A mere logical contrary or subcontrary will not do, since they fall into class (b): they are not known to be true, but neither are they known, as is the contradictory, to be untrue. (See any elementary logic text, e.g. Copi 1972, on the relationships between a given proposition and its contrary, subcontrary, contradictory, etc..) To make his distractors definite enough to qualify for class (a), then, an examiner must ensure that they cannot be made true by the discovery of new facts, by improvements in measurement, or by an extension in the range of application of a current theory. To do this successfully demands great care in phrasing and formulating both the distractors and the correct answer. A sound knowledge of formal logic is necessary for bare adequacy; and sensitivity to the nuances of the relevant language-games (of the particular science or other academic subject-field) is a pre-requisite for high-level discrimination. Neglect of the subtleties of formulation — what are sometimes pejoratively described as 'mere verbal quibblings' — lies behind many of the abuses in ⁱⁿexamining described by Hoffman (1962) and others.

In particular, it appears likely that many examiners make the simplistic assumption that all distractors are of class (a), i.e. that they are known to be false. From this arises the situation where knowledge or perception beyond the 'normal', on the part of the examinee, can be penalised. The student with unusually deep knowledge of the field, or unusual penetration of the ambiguities of the question, chooses one of

* Kuhn himself talks of extension in the range of application of the current paradigm. A paradigm could be explained, briefly, as a collection of theories related through their collective dependence upon some or all of the members of a set of basic assumptions, explicit and implicit (the latter being usually in the majority), and this set of assumptions itself. I shall talk simply of 'theories' in the present context, but with the proviso that these are to be understood as including the implicit, unstated, and often unconsciously-held assumptions which are logically required as backing, i.e. the Common Assumptions Paradigm (CAP), see Chap. 5 above.

the distractors instead of the one decreed to be correct, and loses marks.

It is not in general very likely that students will be able to bring to their examinations a knowledge of new facts or of improvements in measurement which would enable them to impugn well-constructed test items. It is of course possible that they may have special knowledge which, while not new in an absolute sense, may be newer than, or in general 'better' than, the knowledge upon which a particular examiner has based his 'correct'/'incorrect' decisions. Provision can be made in the test forms, as indicated below, for argument about particular issues. The possibility of extending an existing theory into new ranges of applicability (a Type (iii) advance, see above) is intrinsically less unlikely, however, partly because there need be less dependence, initially at least, upon technological sophistication. How many students have been penalised, for example, since 1951, for extending the applicability of ethological assumptions to include human behaviour? How many, for that matter, are still being penalised, and will be for perhaps a decade more? Yet the movement which started with works by Tinbergen 1951, Lorenz 1935, 1950, 1952, Bowlby 1952, Fletcher 1957, Russell and Russell 1957, 1961 and other pioneers is now rapidly expanding in the human social/behavioural sciences in work by Blurton Jones 1967, 1972, Goffman 1969, Tiger 1969, Tinbergen and Tinbergen 1972, Bowlby 1971, Freedman 1974, Eibl-Eibesfeldt 1970, Morris 1977 to name but a few — and it is clear that a major link-up is occurring at the present time between ethology and the human sciences (especially perhaps on the clinical side). The details of this extension of range are of course still being worked out — my own The Evolution of Intelligence (1974) is an attempt to deal with one of the major boundary disputes (see also Stenhouse 1977 a and b) — but the fact of the extension can scarcely be in dispute. (Though diehard conservatives may still be found, who would dispute it. For example, in one university a textbook is used in one course, which denies any significant influence of instincts in human behaviour (Lefrancois 1975) — while simultaneously, courses in Human Ethology are being taught and are listed in the University Calendar!)

Another example of a scientific revolution in recent times comes from the earth sciences. In the early 1950s the possibility of continental drift was referred to, briefly, as 'Wegener's theory', an historical curiosity. Twenty years later, under the rubric of 'plate tectonics', it is accepted as established fact (see Hallam 1973 for an excellent discussion.)

This brings us to consideration of one of the most exciting possibilities in education, in science and other fields: that students may in thinking about their coursework and examinations adumbrate not merely the advances of normal science, but even the more radical conceptual reorganizations of a 'scientific revolution' in Kuhn's (1962) sense.

It is necessary to be careful of our terminology when talking about possible relationships between students' examination performances and scientific revolutions. It is not being suggested that students should be encouraged to think of participating substantially in the actuation and accomplishment of scientific revolutions. (They often have sufficient tendency to daydream along these lines anyway!) Against this, it is necessary to insist upon the need for extensive background in existing disciplines (preferably in more than one), and upon the slow, painful, and laborious accomplishment of most revolutions — even small ones. As Kuhn remarks, it seems that the established adherents of an old paradigm have literally to die off, before the new one can become established; and its protagonists by then are old men.

Nothing but harm can result from facile and shallow thinking about revolutions, in science as in any other field of endeavour. But while vainglorious thoughts of 'leading a revolution' are to be discouraged, what is both possible and desirable is that students should be encouraged to perceive alternative possibilities of interpretation within the orthodox terminologies. They should be allowed to explore possibilities within and extending from the existing language-games in the subjects being studied. In doing this, it is likely that they will sometimes see, and can draw attention to, theoretical avenues of revolutionary change. It is in relation to this sort of opportunity that the 'distractors' in objective examinations take on an especial significance.

As soon as reliance is placed upon questions of interpretation rather ^{than} simple straightforward truth-or-falsity (and often even those are not so straightforward as they seem), a road is opened for an interpretation to be made which is in some way different from what is standard and acceptable in terms of the normal science of the time. If a student interprets an item differently from the examiner, then, and as a result selects one of the distractors as the best answer, there is at least a chance that:

- (a) A theoretical framework (= language-game) might be constructable, in terms of which the student's answer would be the best answer of those offered; and

- (b) The new theoretical framework might in a number of other respects be superior to that characterizing the normal science of the present time.

The question of how it is decided that one theory or paradigm is 'superior to' another raises complex issues which cannot adequately be dealt with here. For extensive discussion the reader is referred to various major works in the history and philosophy of science: Kuhn 1962, Toulmin 1954, 1961, Lakatos and Musgrave 1970, Suppe 1975, various symposia (Minnesota, Boston etc.), and other works. The one point that it is essential to mention and appreciate, however, is that an 'appeal to facts' very seldom yields a decisive result. As Kuhn (1962: 146) remarks: "To the historian [of science] ... it makes little sense to suggest that verification is establishing the agreement of fact with theory. All historically significant theories have agreed with the facts, but only more or less." In short, to assume that current theories are 'true' and that all other theories are 'false' is to exhibit historical/methodological naivety approaching an extreme of absurdity. (Yet this assumption is implicit in a great deal of teaching and examining at the present time!)

There can be no question, then, of a student being able to establish the new viewpoints and interpretations of a scientific revolution within the confines of an examination script. As Kuhn himself points out (1962: pp20ff), the initiation and explication of a scientific revolution requires very extensive discussion, of book length at least. It is only after a new paradigm has become well established that communications of the length of a journal article, say, become intelligible. So how could it be practicable for a student in an examination, even assuming that he/she has perceived a new possibility of interpretation, to demonstrate any 'validity' for the new insight?

The short answer is, of course, that it is not practicable. But this cannot be left as our final answer; otherwise we shall be guilty of casting the role of education, by implication, as essentially conservative, reactionary, status quo with regard to subject-matter and, at one remove, educational methodology. But what could be done, practically speaking, to open-up a multiple-choice objective test to accommodate the possibility of an examinee anticipating the direction of an on-coming scientific revolution?

One possibility is to provide space, say at the end of the test

booklet, for examinees to criticise any of the questions within the body of the test, and/or to provide a brief rationale for any of their answers which they feel might need explanation and support. If the test itself, exclusive of this 'open' section, were such that only the top few students were likely to complete it in the time available (this is a common practice with objective-type exams), then it would have to be arranged, and explained very clearly to the examinees beforehand, that time and effort spent on the 'open' section of the test should be regarded as a high-risk investment. If say five 'open' spaces were allocated at the end of a hundred-question test, the possible marks per 'open' item should perhaps be double those allotted to each of the normal multi-choice questions — but it should be pointed out that the chances of obtaining high marks would generally be greater for examinees who concentrate on the orthodox multi-choice questions. A case has been argued (Stenhouse 1976) for multichoice objective tests being attractive to examinees of high creative potential, and for the efficacy of such tests in the diagnosis of such potential. It could therefore be explained to examinees that their basic capabilities would probably be adequately assessed by a combination of multi-choice objective, essay-type and other examinations ('multi-modal' examining, Stenhouse 1969), and that there would be little need, therefore, for them to press for originality, non-conformity, etc.... Nevertheless the opportunity to innovate, and to criticize and argue with the examiners, would be provided in the 'open' (and of course optional) section of the multi-choice paper.

One valuable outcome of provision of an optional criticism-and-argumentation section in an objective test would be to allow the examinee(s) the opportunity to comment upon ('hit back at!') the performance of the examiner. This could release some of the pent-up tensions associated with the exam situation, and provide a safety-valve for the 'affectional dimension' (Kratwohl et al. 1964) which tends to be under-accommodated or totally ignored by much present-day examining (see the principle of 'all-factor effect', Stenhouse 1969). Some of the current antipathy towards examinations and tests, and towards education itself, could probably be reduced if examinees felt they could comment on the examiners, just as the latter can comment on them. At present, examinees usually have no 'come-back' at all: no wonder they feel like helpless victims of examiners' whims, and are resentful.

It is interesting to point out that one of the features of examining

in the medieval university, viz. the public arguing-out of the candidate's thesis between the examiners and the candidate, has been lost in much modern ⁱⁿexamining. Even in those universities in which a PhD candidate is at least allowed to meet his examiners in the oral exam (or, to put the matter in what may be a truer light, where the examiners are forced to face up to the examinee), the transactions are not open to 'public' view even within the academic community and the discipline in question. One result of this is that while some of those who are awarded their degrees can later be seen, publicly, to be unworthy of them, misjudgements of the opposite sort — individuals worthy of the degree, who are denied it — are much less easy to discern. Hudson (1964) gives a couple of notable examples of the latter. If the number of errors of the first sort is anything to go by, ("A learned blockhead is a greater blockhead than an ignorant one", B. Franklin 1734), then there must be a tremendous wastage of talent in the form of highly able and creative people who have been wrongly denied appropriate degrees!

It is clear that, especially in the 'open' section of an objective test, heavy responsibilities would be thrust upon the examiners. These responsibilities are multi-dimensional. The examiner would have to preserve a fair and open mind, perhaps under criticism and even abuse of a personal nature. It is not suggested that students should be encouraged to think that they might gain marks as a result of ad hominem criticism of examiners. Nevertheless many people tend to identify personally with the views they hold, it is implicit in Kuhn's notion of a paradigm (in the sense of shared assumptions, beliefs, etc., see Masterman in Lakatos and Musgrave, 1970) that many of the assumptions about their science are deeply and strongly believed in by the practitioners of that science — hence any questioning of such beliefs, especially if it comes from 'junior' and/or 'non-initiated' members of the scientific community (i.e. students) may arouse strong feelings of personal resentment. The attack on the tenets of the science may be perceived as an attack on the scientists; and the scientist/examiner may respond accordingly. The background to this deserves further discussion.

One of the disappointing things about R.S. Peters as an educational philosopher is that he always stops half-way. He has good insights, and then refrains from developing them. One such insight is of education as initiation (see Peters 1964). The notion of 'initiation' is a powerful one as developed within the discipline of Social Anthropology. It involves the acceptance of the individual, the 'initiatee', into the social group wherein, once accepted, he can maintain the cultural norms of the society,

or modify them in one way or another to a varying extent. Thus initiation is initiation into a society, a group of people (not all need be equal within the group; and societies differ in the extent to which they are open to change from within). For Peters, however, initiation is not so much a matter of admission to the social group (or to a sub-group, but often a dominant sub-group, within it), but of acceptance by the initiatee of the cultural patterns, norms, etc., which happen to be current within the society. Thus Peters' view of initiation has strong implications of social conformity and conservatism which are not necessarily present in anthropological views. For the social anthropologist, conformity and conservatism are contingent features of particular societies at particular times, etc.; whereas for Peters, they are intrinsic to the very notion of initiation.

It is not my intention to attempt to sheet home this particular interpretation of R.S. Peters, but rather to sketch in some of the possible implications for our understanding of education, of a move towards the viewpoint of Social Anthropology.

Kuhn (1962) has stressed that the practitioners of a particular 'normal' science must be regarded as forming a social group (or sub-society) united by their acceptance of a particular paradigm of 'common assumptions'. (This has been designated as a 'Common Assumptions Paradigm', CAP, in contrast to other interpretations of the word as distinguished by Masterman 1970; a different cluster of interpretations may collectively be designated the 'Public Demonstration Paradigm' or PDP, see Chap.1.) As a social group, the scientists practicing a particular discipline must be presumed to be affected, mutatis mutandis, by similar factors as affect the structure and behaviour of any other social group; this is not to say that they are affected identically, or by the same factors, as any other group: groups differ. Among the factors affecting any group are those related to:

- (a) Group cohesiveness; and
- (b) Rank order, and other structuring, within the group.

The social group which comprises the practitioners of a particular science could be expected to show increased group cohesiveness, in terms of standard ethological mechanisms, when presented with what they perceive as an 'external threat' (cf Malleon 1961). This mechanism is of course well known and frequently used by politicians: when their own group is getting restive and inclined to be critical of the leadership, they conjure

up some 'enemy outside the gates' (possibly largely imaginary, but the mass media can easily deal with that problem, witness Dr. Goebbels' services to his master Hitler), and the group closes ranks to repel the threat ... Now it is not being suggested that the members of a scientific community behave like a baboon troop in the face of external threat — I have myself emphasized (Stenhouse 1974) the crucial role of 'evolutionary intelligence' in holding in check ('negating') the various instinctual tendencies — but nevertheless the tendencies may still be there, even among scientists dispassionately devoted to the pursuit of truth. Beveridge (1957) cites examples of innovators in science who have been opposed quite irrationally, and even persecuted; and Koestler (1971) gives an account of how even an established biologist, Kammerer, was hounded into death and oblivion by antagonistic colleagues, in the twentieth century. These matters are not pleasant to think upon — but science must always take account of the facts; and one set of facts is those concerning the human nature, the various behavioural propensities, of those who study and/or teach in the various sciences (cf Goodfield 1977). Psychology has been revealing many unpalatable and embarrassing facts about human nature at least since the time of Freud (plenty of good features have been revealed and investigated too) — and there is no reason to think that scientists and educationalists are immune to the less-desirable tendencies, or that they have a monopoly of those regarded as more-desirable. It must therefore be assumed that, in the absence of an objective Public Demonstration of a new (or alternative) C.A. Paradigm, the adherents of a generally accepted C.A.P. are likely to perceive criticism of it as an 'external threat', and close ranks accordingly.

Since a student is, more or less by definition, not (or not yet) a member of the social group whose 'subject' or 'field' he/she is studying, any criticism emanating from a student, is, again by definition, 'external'. Whether it is seen as a 'threat', depends on a variety of factors, among them those relating to the other major social-group phenomenon already mentioned, viz. the rank-order structuring of the group.

It has become a commonplace, in studies on rank-order and related phenomena, that overt aggressiveness is generally most exhibited by those individuals who are not at the extremes of the 'pecking order'. Conversely, alpha and omega individuals are often quite tolerant of what could be perceived, by middle-rank individuals (who make up the bulk of the group), as threats or 'insubordination'.

One of the more disturbing points that should be kept in mind is that the main bulk of educating and examining is done by persons who are certainly not in alpha positions in the social hierarchies of their institution and/or discipline. They are in that respect, therefore, unlikely to have sufficient self-security to be able to view heterodox views as non-threatening. Thus the student who advances an argument at variance with the current orthodoxy within the field in question is likely to be perceived as a subordinate who has 'stepped out of line' and needs correction, i.e. needs to be brought back into conformity with the accepted 'correct' views, rather than as a pioneer exploring new intellectual possibilities. Alternatively, and depending on the very complex networks of possible relationships, personalities, institutional ecology, manner of statement, etc., which subsist between examiner and examinee, the heterodox student may be 'promoted' to being regarded as an external threat (rather than merely as a wayward subordinate). Either way, the examiner's reaction is likely to be adverse and, more importantly, it is likely to be in a real sense irrational. This needs further explanation.

For an action to be irrational, it is not necessary that the agent should be unable to 'give reasons' for it. The very concept of 'rationalization' in the Freudian sense implies that 'reasons' are given: but they are not the real reasons (which are usually socially unacceptable, or thought to be so, when Freudian rationalization is resorted to) — rather they are 'cover-up' stories of a socially acceptable nature, under which the action(s) in question can be subsumed with at least some degree of plausibility. Freudian rationalization in its milder forms is of course very common. It is only when an individual rationalizes (in this sense) a large and important proportion of his actions, and when the rationalizations become obviously implausible, that a truly pathological condition is being reached. Nevertheless, even though an individual's habit of rationalization may fall well short of the level where he/she would be diagnosed as mentally ill, particular instances of rationalization may be very harmful to other individuals and through them to institutions and society in general.

So with the examiner who reacts against the heterodox examinee: he can probably give excellent reasons for disallowing any answer which is at variance with the currently-accepted paradigm. The whole strength of the paradigm and of the professional scientists who accept it is behind him. But — and this is a 'but' of the very greatest significance —

examiners who simply reject new theoretical possibilities in this way ought to realize that they may, in doing so, be rejecting from their science the individuals who might otherwise have actuated an important revolutionary change in that science. In short, too much emphasis on the orthodoxy of the moment, the current normal science, may be — in fact almost inevitably will be — counterproductive for the long-term success and continuing 'evolution through revolution' of the science in question.

It may be remarked that similar considerations to those adduced above with regard to academic examiners apply, with scarcely less long-term significance, to the editors of scientific and/or educational journals and to the readers and editorial staffs of publishing houses. It is within the power of persons in such positions to allow new interpretations and ideas to be promulgated — this is not to say that all will prove of major importance, indeed the likelihood is that few would do so — but if heterodox views are denied publication, then the advances potentially intrinsic in a minority of them may be long delayed or may even be lost forever.

In connection with progress in normal science itself, Kuhn argues (1962; Chapter XIII) that researchers are better to keep their attention within the bounds (of subject-matter, techniques, etc.) set by the accepted paradigm, rather than keep an eye open for different approaches associated with alternative paradigms. Talking about the education of 'normal scientists' — and we must keep in mind that Kuhn's view is that of the historian, retrospective, and that he is concerned with the physical not the bio-behavioural or social sciences — he says:

"... one cannot help but notice that in general it has been immensely effective. Of course, it is a narrow and rigid education, perhaps more so than any other except perhaps in orthodox theology. But for normal-scientific work, for puzzle-solving within the tradition that the textbooks define, the scientist is almost perfectly equipped. Furthermore, he is well equipped for another task as well — the generation through normal science of significant crises. When they arise, the scientist is not, of course equally well prepared ... scientific training is not well designed to produce the man who will easily discover a fresh approach."
(Kuhn 1962: 165 — emphasis added.)

That Kuhn can acknowledge the narrowness and rigidity of science education, even on a retrospective view of the successes of (mainly) the physical sciences in the earlier parts of this century, should make all

his readers wary of accepting uncritically his initial assertion that it has been "immensely effective". The provisos that need to be made are such as virtually to nullify whatever might seem to have been claimed. In the first place, the historian's retrospective eye picks out the advances, the successes, that have in fact occurred. But as Kuhn himself remarks with regard to science textbooks, hindsight can make it seem that only those theories, those developments, which did in fact prove successful, were ever in question. The science textbook gives scant attention, if any, to theories and hypotheses which did not work out — or, which have not yet worked out, which is a very different matter. The historian of science can do better, in that the unsuccessful theories can be mentioned and discussed — but only if they have been published, or at least written-up and preserved. But a proper assessment of the effectiveness of education would demand consideration of the individuals whose talents were not developed at all, who opted out of science or who failed to get to the point of even formulating new theories, etc.. And in the second place, Kuhn makes it clear that he is talking about sciences in which fruitful paradigms have already been established. The students are trained in terms of these paradigms. But what about education in sciences which are actually in process of revolutionary change, or in which several alternative paradigms are in competition? Does current science education provide an adequate basis for its graduates to cope with these situations? Kuhn's own answer appears to imply a fairly emphatic negative.

If we remove ourselves for a moment from immediate considerations of examining and educating in the sciences as they stand at present, and look briefly at the wider perspective of the human situation in the world as a whole as we face up to the next fifty years (to look no further into the future), it becomes apparent, I suggest, that while the physico-chemical sciences upon which Kuhn focusses, and their derived technologies, have contributed heavily to the causation of most of our problems, these sciences are not the ones which will have to provide the answers. The answers have to come in the first instance from the bio-behavioural and the social sciences — and from teaching people to think. In one of the earlier books on the population-vs-resources crisis (Stenhouse 1966) I posed some of the problems, e.g. regarding the regulation of the birth-rate, which are only now coming to be seriously debated as real-life political issues — and even now only by small minorities of concerned individuals. But behind a first-order switch in emphasis within science

education, from the physiochemical to the bio-behavioural sciences, it may perhaps be of even greater importance to achieve a paradigmatic change in the very basis and orientation of our thinking. Many of our problems at the politico-social level are generated by our adherence — mostly quite unconsciously — to nineteenth-century physics and engineering assumptions. One of these is that 'social engineering' (significant phrase!) can be accomplished by drawing up plans, ('blueprints' is a favoured term) and then implementing them, usually by governmental and bureaucratic means, in a matter of months or a couple or five or ten years at most (cf the 'Five-Year Plans' of the Soviets and others).

Toulmin (1961) in discussing 'ideals of natural order' (these equate with a generalized and deep-level use of 'paradigm' in Kuhn's terminology), points out that people's notions of what is 'natural' and hence not in need of explanation, are different with different times and cultures. Today, the growth of a plant from embryo to maturity is felt to need explanation in terms of the physicochemical mechanisms involved: physiology, biochemistry, etc.. This is the mechanic's view. In Aristotle's day, the notion of 'growth' was itself basic, things' growing was natural, taken for granted, not something that needed explanation. This is the developmental biologists' view. Toulmin instances cooking as being explainable, in the biologist's paradigm, in terms of growth: the ingredients of the cake are like seeds which, given the right environmental conditions, will fulfil their potentiality for turning into a cake. Stretching the metaphor, we might say that the great machines of a modern factory, if fed on the right sort of 'nutrients', will after a shortish gestation period give birth to fully-formed cakes, refrigerators, motorcars, etc.. (It may be pointed out that the greater the degree of automation, the more plausible this analogy is likely to appear. Electronic computers, for example, are already addressed on first-name terms, and have to be greeted using the appropriate amicable formalities before they will consent to operate.)

It appears, then, even from this brief glance at recent developments, that changes are already occurring in our deep-layer paradigms, our ideals of natural order. Paradoxically, the turn to complexified and subtle modes of thinking, similar to Aristotle's 'biologicalism' in some ways (but not in others), is most evident among hydraulics and electronics engineers — to name two groups of my own direct acquaintance — whereas many biologists have swung even in the last few decades away from

the 'biologicalism' which was encouraged by evolutionary theory and into a 'mechanicalism' often of crude and simplistic nineteenth-century type. But this particular trend must be reversed. Especially in our societal, political and educational thinking we must change to a complexified and biologicalistic and, perhaps even more important, an 'evolutionistic' paradigm. This idea cannot be much elaborated in the present context; as an illustration, one point may be mentioned, however, to show the change in emphasis and expectation which is needed. We need to be prepared to wait longer for results. The complex systems of large societies are like natural ecosystems in that they often change slowly, but with a kind of massive inertia: once they have started to swing, it is difficult to stop them. And the nature of the changes involved may be unexpected. We are seeing this at present, increasingly, in various environmental changes — pollution, erosion, depletion of soil fertility, etc. — which we are finding difficulty in controlling.

In education, whether in the sciences or in other disciplines, it is becoming clear that while short-range 'behavioural objectives' deserve some attention, the really important outcomes are long-term. The success of our children as parents, and thus at second remove our success as parents and educators both formal and informal, can be assessed only when their children have reached adulthood, i.e. perhaps half a century from now. In a different and more easily perceived — but certainly not more important — dimension, our success in fostering creativity in our pupils cannot by the nature of the case be assessed for at least several decades. Although it has been shown that highly creative individuals often have their best ideas quite early in life, i.e. late 20s-early 30s, this can be seen only ex post facto. Whether the ideas are any good, whether they are even new, cannot usually be determined for at least a decade. But decision as to which individuals are to receive further education, support in their research projects, etc., etc., have to be made much earlier: this is where we come back to the intelligence of examiners and, in general, of the various evaluators of young people's abilities. How well can examiners evaluate a line of argument which is fundamentally new, 'revolutionary' in the Kuhnian sense?

Some short answers, again, are all that can be offered at present. Most depends on the individual examiner. Some have real flexibility of mind, real insight into new possibilities: other have not.

Secondly, and other things being equal, an examiner having multi-

disciplinary background, and an understanding of history and philosophy especially perhaps in relation to his own fields of study, should be better than one lacking such background. But unless such a person has learned philosophy as an activity, and especially as an activity involving the exploration of possibilities on meaning/interpretation, philosophy itself can cause rigidification, e.g. through over-emphasis on formal logic, or on proving conclusions beyond a shadow of doubt.

Thirdly, I would suggest that an examiner needs a high endowment of the P-factor of 'evolutionary intelligence' (Stenhouse 1974), the pause-and-consider factor which helps provide the opportunity for new possibilities to be thought out.

Fourthly, and finally, a receptive and open-minded examiner may expect to receive 'reinforcement' (reward), in that from trying to appreciate examinees' often heterodox expressions of their ideas (even though the ideas themselves are not new, and may indeed be untenable by any standards), he himself may come to perceive new possibilities for revolutionary developments in his own field of research.

Thus one of the rewards, one of the causal outcomes, of good teaching and ⁱⁿexamining may be enhanced effectiveness in research, especially perhaps in Kuhn's 'revolutionary' dimension.

Perhaps the most significant outcome in the long run, from the societal point of view if not always that of the individual teacher/examiner, is that the open-mindedness or 'divergence' of thinking which good teaching and examining demands, could and indeed should lead to reassessment of educational methodology. By taking seriously the mistakes made by students and, more generally, the reactions of students to the education they are receiving or have received (including their decisions to opt out!), one can be led to perceive not only new avenues of first-order research within one's science or other field, but also new possibilities with regard to its teaching, and to education generally.

It is being suggested, in short, that given the right education for science educators, they can be induced to become the actuating agents of revolutionary and constructive change within both science and education. The key issue is: being able to think out (or, more important, being in the habit of thinking out) alternative interpretations, alternative procedures, over and above those that are standard within a science or within an education system at a given time and place. If we cannot visualize different possibilities we certainly will not be able to find better ones!

CHAPTER EIGHT

QUESTIONS OF PROOF AND TRUTH.

If we grant that it is desirable to teach more philosophy in all educational programmes, then we have to return to a question that has been touched upon earlier in this thesis, namely: what sort of philosophy should be taught?

In an earlier brief discussion of this question I suggested that the differences between various philosophical approaches are often portrayed as being greater than they really are, and that though they do differ in their starting points and their conclusions, they do not differ so much in the dimension that really matters from an educational point of view, viz. in the fact that they all involve careful argument. But even here there are differences. Not all argument is sound. Not all argument is appropriate to the purposes required in a particular situation. Even some of those who teach Philosophy in universities fail to differentiate between the type of argumentation which is appropriate to the teaching of philosophy to students, and the type of argument which is appropriate to attacking other professional philosophers at a conference or in the learned journals. Also, as we have seen in our brief examination of some of R.S. Peters' work, the rationality which is supposed to characterize philosophical argument can become little more than 'rationalization' in the Freudian and pathological sense. I still want to insist that the educational value of philosophy lies in the argumentation involved — we must recognize, of course, that there can be good argument and poor argument, the latter perhaps grading off to the point where we might begin to query whether it really did deserve the title 'argument' — but a more detailed examination of two issues in particular may help to illuminate the nature of both philosophy and science, and education.

The two issues in question are:

- A. Whether and to what extent philosophy involves proof of its arguments and conclusions; and
- B. Whether and to what extent philosophy is concerned with truth.

One of the first lessons which is usually taught in 'beginning' courses in philosophy is the distinction between questions of truth or falsity of statements on the one hand, and questions of validity or

invalidity of argument/inference on the other. This is often given simple illustration by a syllogism like the following:

All flamingoes are birds

All flamingoes have beaks

Conclusion: Therefore, all birds have beaks.

The point is made, when we observe that each of the statements in the above argument is true, but that the argument as such is invalid, i.e. the conclusion is not logically necessitated by the conjunction of the premises. This can be seen intuitively by most people, but can if necessary be demonstrated by means of Euler's circles or Venn diagrams.

Examples like the above are usually given in order to teach the lesson that truth (of statement) is quite distinct from validity of argument, i.e. proof. But is philosophy then concerned only with questions of logical proof? If a conclusion or an argument as a whole cannot be definitively proved, should it not count as philosophy? Or, not proper philosophy? Even if we were to concede, for the sake of argument, that Philosophy can rightfully contain matters which have not yet been decisively proved, is it still to be taken as a criterion of the activity of philosophy that what we ought to be doing is trying to make our arguments rigorous to the point, eventually, of constituting proof in the strict sense?

If our general answer to that set of questions approximates to "Yes", then it might be said that we are tending to assimilate philosophy to formal logic. The latter is indeed usually taken to have a fairly intimate connection with philosophy — but whether they are really identical (or, perhaps, whether they ought to be identical) is not proven. Whether they are or should be, or not, is itself a philosophical question that needs proper exploration. We have already looked at some arguments which appear to disprove the thesis that philosophical positions are usually proved in the strict sense (Chapter 5, pp120-3) — but does this mean that, in the activity of philosophy, we are not concerned with proof, but rather with truth?

A number of utterances of twentieth-century philosophers might seem to support the view that emphasis should be on truth rather than proof. Bertrand Russell (to start at what many people would regard as 'the top') in his Report of 08/5/30 to the Council of Trinity College, Cambridge, concerning continuance of a research grant to support the work being done

at that time by Ludwig Wittgenstein, has this to say:

"The theories contained in this new work ... are novel, very original, and indubitably important. Whether they are true, I do not know ..." (emphasis added). (Russell 1975: 440. Wittgenstein got his grant continued.)

Russell's use of 'true' here is perhaps all the more surprising in that he himself was primarily a logician, and most of the work in question by Wittgenstein was of a logical nature — though its nature may have been somewhat different from what Russell thought — see below, Chap. 11. One would expect 'valid' or some roughly equivalent word to have come more readily to Russell's pen in such a context. One might be tempted, indeed, to think of the use of 'true' as perhaps a mere slip of the pen — but this is rendered fairly implausible by the fact that Russell certainly used 'true' on other occasions, e.g. in a letter to G.E. Moore of 05/5/30 about the same work by Wittgenstein, he says:

"... His theories are certainly important and certainly very original. Whether they are true, I do not know; I devoutly hope they are not, as they make mathematics and logic almost incredibly difficult. ..." (Russell 1975: 437).

This second passage certainly rules out the possibility that 'true' is a merely verbal mistake, a slip of the pen. Whatever meaning Russell may be assigning to 'true', he certainly takes it as being substantial: otherwise his 'devout hope' would not make much sense.

Even O'Connor, in his 1957 book which remains one of the best works so far in Educational Philosophy, although he does not (so far as I can find) actually talk of philosophical conclusions as being 'true', nevertheless does talk of them as being the possible subject of "final agreement" (p.28), and suggests that the meaningfulness of any question (presumably including philosophical questions) depends on there being an appropriate framework of "evidence" in terms of which possible answers can be assessed (p.32). In this and other ways he does seem to have a tendency towards 'positivism' and the assimilation of philosophy to science, despite the fact that he states that "There is indeed no ... established body of positive philosophical knowledge: and philosophy being the sort of enterprise that it is, there never could be." (p.24). He certainly comes down very definitely and explicitly against the notion of 'proof' in philosophy. He says that the "... belief that philosophical discovery was essentially a

process of proof took a long time to lose its plausibility ...", and he seems to assume that it has in fact by now lost any such plausibility:

"There can be no philosophical proofs because philosophy cannot proceed either by the axiomatic method of mathematics or by the experimental way of the scientist. And there are no other kinds of proof but formal deduction on the one hand and the establishment or refutation of hypotheses on the other." (p.30).

Perhaps the key to O'Connor's position is revealed by his use of the word 'positive' in the quotation above (O'Connor 1957: 24). Even though he may believe that there is no body of positive knowledge built up through the history of philosophy, he does seem to believe that a number of negative conclusions have been established. That there can be no philosophical proofs could be taken as one example of something which O'Connor himself, as is obvious from the quotation which constitutes the preceding paragraph, takes as having been established — and if it is 'established', might we not just as well say it is 'true'? Most of his discussion of the history of philosophy and of science (his Chapter 2) is of the mistakes made in the past; and it seems that he accepts that these have been properly and firmly established as mistakes. In particular, he contrasts the cumulative development of 'established bodies of positive knowledge' in the sciences as they have grown up since the seventeenth century with the absence of such 'bodies' in philosophy. (It might be regarded as an open question, whether the more important issue, for O'Connor, is the absence of 'positiveness' from philosophy, or the absence of 'bodies' of knowledge, i.e. organized systems.)

Could it be said, then, that for O'Connor the achievements of philosophy are all negative: the exposure of mistakes as mistakes? But if this were the case, if an affirmative answer were to be offered to this particular question, surely it could then be said that, even if only on a 'double negative' principle, philosophy has in fact made positive advances? It seems not unreasonable to claim that the establishment of error as error is a positive achievement; and if we accept this, we would in effect be rejecting O'Connor's claim that philosophy has made no positive advances. This would swing the weight of interpretation of O'Connor's statements about philosophy — if they were to retain positive significance — towards emphasis on the lack of organized systems of results in philosophy. In terms of this sort of emphasis, O'Connor's allegations do have a great deal of plausibility, especially with regard to twentieth-century philosophy in

English up to say 1960. In most fields, in both science and philosophy and also in intellectual activity in general, the major weight of effort has been on analysis rather than synthesis and system-building — this has been attested in scores of publications, and the twentieth century has been tagged as 'the Age of Analysis' — and it would seem to be true, in general, that extensive systematized sets of conclusions, definitively established and widely accepted, have not been produced by the philosophical activity of this or indeed of any other century. But does this mean that philosophy has had only negative results or even that it has been more or less pointless? O'Connor does not appear to think so, and neither do I. But what account can we give of Educational Philosophy, what account can any philosopher give of any branch of philosophy, which rings true to both students and practitioners of the discipline? Can we do for Philosophy itself what Toulmin, Kuhn, Popper, Feyerabend and others have done for Science, namely, give an account of the subject that is reasonably true to life, reasonably in accord with what actually happens?

One preliminary clarification may be offered at this point, based upon the model of philosophical activity discussed early in Chapter 5, in which a distinction was made between 'particular positions' in philosophy and an individual's 'personal position'. Briefly, the suggestion made earlier was that the individual clarifies and in a sense constitutes his personal position through the process of exploring the implications of various particular positions: some positions are decided to be untenable because their implications are unacceptable, and as a wider and wider range of particular positions is explored in this way, the individual gradually acquires a clearer understanding not only of the positions he does not want to hold, but also of those he does ... But his gradually emerging 'personal position' is not taken as having been completely and comprehensively specified by any of the formulations of particular positions.

In this process of exploration of particular positions, questions of literal truth do not normally arise with regard to statements of particular positions: these would usually be taken as only hypothetically true, 'true for the sake of argument'. Questions of truth and falsity would come into the process of exploration in terms of the various descriptive statements of a more-or-less straightforward empirical kind, statements about the world as it is ordinarily experienced, against which the several implications of any given statement of a particular position could be matched to determine their relative degree of plausibility. We need to know how

our philosophical views square with our real-life experience. (This is not to say that we would abandon a particular philosophical view at the first hint of possible conflict with what we have hitherto accepted as 'real life': we might decide that our previous 'realistic' or 'common-sense' views had been mistaken.) But if a clearly-established implication of a particular position-statement is flatly at variance with what we know to be true, then the particular position must be untenable.

Questions of empirical truth and falsity (or, as is often the case, questions of relative plausibility/improbability, since clearcut truth/falsity sometimes cannot be determined) come into philosophy, then, in this sort of way. How about questions of proof?

The question of proof can also, depending on how it is interpreted, be accommodated quite readily in terms of the model of philosophical activity we have been using. If one of the 'components' of philosophical activity is the exploration of implications stemming from statements of 'particular positions' (these latter being initially adopted purely hypothetically), then it must be recognized that the implications being explored must be valid implications. There is no point in exploring invalid implications: in a very significant sense invalid implications are not implications. To the extent, therefore, that the implications we explore must have been proved as valid, we are in fact dealing with proof. This is of course proof only of the inferences as such, i.e. as valid inferences — there is no question of anything being proved absolutely, of proving that any particular conclusion is necessarily true in itself. Proof is only relative to the premises, and these are hypothetical. Nevertheless, we are concerned that the 'tracks of inference' which we are exploring should be proven as valid.

It must be stipulated that, relative to the foregoing argument, the implications or 'tracks of inference' between a particular position and its logical consequences can be understood only as being relative to the position as formulated. Inferences can be drawn only from statements. We have to say what a particular position is before we can begin to explore its implications. An unstated position is no position. (This is in the sense of 'particular' not 'personal' position, cf Chap. 5.) But from the recognition that the exploration of the implications of a position has to start from a statement of that position, we can begin to sketch in some of the complexities of our model of philosophical activity, complexities which will bring the model closer and closer to the reality of philosophical investigation.

We start with a statement of position. This might be fairly extensive, perhaps occupying several pages of a book, for example; or it might be more succinct, perhaps only a couple of sentences. Now, either way, in order to explore its implications we have to decide precisely what it means. We can formulate what is essentially the same issue in somewhat different terms, by saying that we can start exploring its implications only by exploring its various possible interpretations (assuming that, as is usually the case, more than one interpretation is possible). The key issue is just this, that most statements do admit of more than one interpretation — unless they have been carefully and elaborately formulated, like legal documents and papers for the philosophy journals, in such a way as to reduce ambiguity to vanishing point. (But the trouble about that kind of statement, as anyone who has struggled through the fine print of a legal document or a philosophy paper knows, is that in proportion as ambiguity is reduced, ease of comprehension of the meaning of the statement tends also to be reduced!)

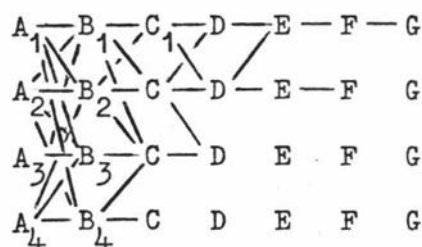
Not only do statements, as collections of words, usually involve a multiplicity of possible interpretations, but so also do single words — as one can readily see by looking into any dictionary. In principle, then, problems of interpretation could become pretty massive. Let us again take a simple model to try to gain an appreciation of the nature and extent of the problem. Let us assume that we are dealing with a statement of position which contains say seven significant words, and let us assume that each of those words can have any of four different meanings/interpretations. How many interpretations are possible for the statement as a whole?

In an hypothetical example set in these terms it is possible to give an exact figure for the number of possible interpretations of the statement as a whole: the number of possible interpretations is two thousand four hundred and one (2401).

If we accept that four significantly different interpretations per word is not an unreasonably high figure — Masterman (1970) in a most valuable paper discerns twentyone (21) distinct usages/interpretations of 'paradigm' (at least twentyone, as she says (p.61), and possibly more) in Kuhn's 1962 classic The Structure of Scientific Revolutions — and if we also accept that a statement of a particular philosophical position can contain at least seven significant words, then we are stuck with the possibility of having to explore 2401 different interpretations. (No wonder Bertrand Russell felt that Wittgenstein might be making mathematics, logic

and philosophy "almost incredibly difficult"!)) This might seem to make philosophy almost impossibly complex; and it would be misleading to deny that it is a complex and difficult business. As we examine our model in more detail, however, it will become apparent that the 'amount' of complexity need not, perhaps, be quite so great as it initially appeared.

The way in which we go about the exploration of the alternative meanings of the individual words as they fit into the statement as a whole can offer us a means of reducing the complexity and extent of the whole business — depending upon our perceptiveness with regard to interactions between meanings/interpretations in the context of the given statement. If we visualize the model of the given statement as being a series of words denoted by the capital letters "A..B..C..D..E..F..G..", with the possible interpretations of each word being denoted as 'A₁', 'A₃', etc., then the total number of combinations would be represented by a network of possible interpretations roughly as follows:



In the above model or matrix, the 2401 possible combinations of meanings would be represented by lines joining each of the four interpretations of A to every (i.e. all four) interpretation of B, each of the possible interpretations of B would be joined to every possible interpretation of C, and so on. To give partial illustration of what is involved, a few of the lines in the left-hand area of the model have been drawn in. It will readily be appreciated that drawing them all in would tend to decrease rather than increase the clarity of the diagram.

In practice, the magnitude of the problem tends to be reduced by the fact that we read the total given statement seriatim. Thanks to the 'linearity' which McLuhan professes to find as an undesirable constraint in the written word, we read the words of the statement in series, starting, in European languages, from the upper left and reading from left to right and down the page to the lower right-hand corner. All written languages are linear, so far as I know, in this sense, even though the

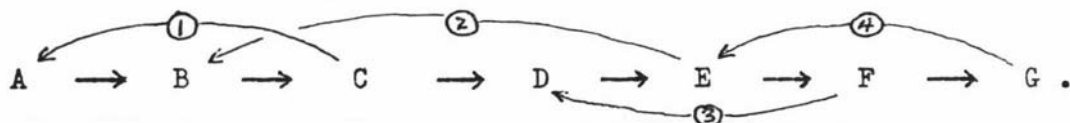
directions in which they are written and read may vary from one to another. (I always get a slightly weird dissociated feeling when I look into, say, the Japanese translation of my The Evolution of Intelligence. It is a beautifully produced book, but the familiar diagrams are set amidst pages of ideograms which have to be read down the columns and from right to left ...)

Irrespective of the order in which written statements are read, they are always read in some particular order, and the important implication of this is, that as one progresses through the statement (from A to B to C ...) the interactions between the possible meanings of the first words begin to set up a context which usually tends to reduce the number of possible — or at least the number of plausible — meanings that have to be explored with regard to the later words in the statement. Persons who are reasonably sophisticated in a particular language, and especially if they are also experienced and sophisticated with the multi-dimensionality of meanings which is characteristic of studies in philosophy, literature, etc., will usually start to sort out the more likely from the less likely interpretations of a statement quite automatically and unconsciously. Disentangling ambiguities 'on the run' becomes habitual, given the right background — though this does not mean that conscious, deliberate and even agonizingly arduous effort may not be required on occasion. But at least many of the more obvious implausibilities can be discarded, even on a first cursory reading; and this reduces the number of 'serious' possibilities that need to be investigated. Furthermore, simultaneously with the reduction in the number of possible meanings/interpretations that are deemed significant on initial read-through, there is also the likelihood of a more positive outcome, viz. that a limited number of more definitely relevant possibilities can be perceived as emerging from the welter of 'mere possibilities': the 'probables' are provisionally becoming differentiated from the 'possibles'.

To illustrate the role of the 'internal environment/context' in the elucidation of significant interpretations of even simple statements, let us examine a few examples.

"In order to go to the university ..." — from this beginning it is impossible to determine whether the statement will be completed by "... take the first turning on the left after you cross the bridge", or "... you will have to satisfy the requirements for ad eundem admission, or get a scholarship". If however the order of the words (clauses) of the

statement is altered, one or other of the above alternatives is automatically ruled out, e.g. "The requirements for ad eundem admission must be satisfied if you want to go to the university" rules out the 'geographical' interpretation of 'go to the university'. This example is perhaps so simple as to be too obvious, and it may be psychologically unconvincing for that reason. The logical issue is, however, perfectly clear; and a significant proviso is also, because of the brevity and simplicity of the example, readily to be appreciated, viz. that while the objective 'linearity' of the written statement has been emphasized and remains important, the statement as read need not be taken in a fixed linear sequence. In other words, and reverting now to the 'A,B,C...' model, the reader is not constrained to read on all occasions straight through from A to G and, as it were, assimilate all 2401 possible meanings in one gulp. Instead, as he reaches C, say, and one particular pattern of meaning seems to be emerging, he can go back and start at A again, testing out the plausibility of the emergent (and at this stage very tentative) interpretation as he re-reads the sequence A-B-C and then goes on to match the emergent pattern with D-E-F-G — always with the possibility of doing further check-up repetitions on the basis of 'feedback loops' being generated by the later terms in the series, as and if they seem to necessitate modification of the total pattern of interpretation. Thus the sequence of assimilation or understanding of the given statement, as distinct from that of its objective presentation in the written form, may diverge very considerably from simple linearity. A pattern of assimilation/interpretation of the following form would still be fairly simple, compared to the intricacies which are often required:



To be able to arrive at a satisfactory overall interpretation on the basis of only say four or five feedback or check-up 'loops', as in the above model, presupposes a fairly simple statement (even granting the seven significant terms with an average of four possible interpretations of each term). In relation to the interpretation of philosophical argument, or of methodological issues within science, a model as simple and straightforward as the above would equate merely with careful reading of a text — the real complexities of exploring major possibilities of interpretation would begin only subsequently to this. In short, this model equates with mere reading

of the statement or text. Nevertheless, the model does give a working picture of the type of process which seems to be involved in reducing the total complexity of possible interpretations, even when these include methodological/philosophical considerations of the highest generality; and even when the exploration may take decades or even centuries.

The history of philosophy may be seen as the exploration of the possible interpretations of the various significant 'great works'. Commonly, a highly original work is not understood at all on its initial publication. Hume's Treatise "fell still-born from the press", and he wrote many revisions and popularizations during the rest of his life in an attempt to gain understanding for his ideas. Kant's Critique of Pure Reason (1781), probably the greatest single work of modern philosophy, also failed to obtain early support. Lucas (1953: ix) writes:

"Kant expected the Critique to have a revolutionary effect and anxiously awaited its impact on the world of learning. In fact he found it was being received in silence ... and he heard privately from friends that his book was being found obscure and unintelligible, and put aside as unreadable."

In science the same thing occurs with regard particularly to works classifiable as 'revolutionary' in Kuhn's (1962) sense. For example, on 1 July 1858 the papers by Charles Darwin and Alfred Russell Wallace in which the Theory of Evolution by Natural Selection was first promulgated, were read to the Linnean Society of London — yet when the President of the Society was later reviewing the year in which these epoch-making papers were presented, he regretted that there had been none "... of those striking discoveries which at once revolutionize, so to speak, the department of science on which they bear." As Bell (1959: xi) remarks: "Few judgements can have proved so erroneous..."

The publication of commentaries, criticism and explanation of the various great works of philosophy — and similarly of lesser works, too — can best be understood as a sustained endeavour to clarify the interpretation of the original work in question and to discover, if possible, a single interpretation that is internally consistent. The long process of exegesis and critical discussion can be seen as a sustained and highly complicated version of the model given above: alterations are made in the interpretation of some of the terms, and these alterations necessitate exploration of their effects which may ramify throughout the rest of the total statement of position — which may in fact be a long book, convoluted in

argument, like the Critique of Pure Reason. If we were to attempt to make the model fit the actual historical process of criticism and explanation, etc., we should have to point out that, very often, it is not so much a matter of exploring combinations of meanings/interpretations each of which is as it were already given, i.e. the model assumes that meanings $A_1, A_2, \dots$ are known, but rather a matter of coming to perceive the possibility of new meanings, new interpretations that were inherent in the words but had not hitherto been made explicit, had not hitherto been consciously recognized. In short, the exploration of the possible meanings of a given statement (book, article, or whatever) would often involve adding new meanings. In terms of the model, new meanings $A_5, A_6, B_5, \dots$ might well be added to the average of four meanings per word which had previously been recognized.

The addition of new meanings for individual words would on the one hand increase the scope and complexity of the explorations seeking a single and self-consistent interpretation of the given passage as a whole — but on the other hand, the addition of one particular new meaning might just provide the 'last piece of the jigsaw', in relation to which the original total statement might suddenly fall into a coherent, intelligible and self-consistent pattern. A new meaning for one word might catalyze a 'gestalt switch' in people's understanding of the original statement. As an example here, the word 'intuition' as it occurs in Kant's Critique of Pure Reason and the Prolegomena may be mentioned. The present-day reader of these works in English normally finds difficulty in making sense of passages in which this term is used. We are accustomed to a more or less 'intellectual' and cognitive meaning of 'intuition': something like "immediate unreasoned perception; instinctive knowledge" (Penguin English Dictionary, 1965. See also further discussion in, e.g. Westcott 1968). When Kant says:

"Thus it is only through the form of sensible intuition that we can intuit things a priori, but through it we can only know objects as they appear to us (to our senses), not as they may be in themselves ..." (Lucas 1953: 38),

most of us would be baffled as to what he might mean. We would tend to interpret the word 'sensible' in the above passage as 'intelligent, realistic or reasonable' or even perhaps as 'prudent'; and we would give emphasis, in the first clause, to 'sensible intuition', stressing its sensibleness/intelligence etc., as a means of getting to understand the key issues. But this sort of interpretation would in fact be quite mistaken

as an interpretation of Kant's doctrine. For him, the emphasis of the first clause comes on 'form'; and for him, 'intuition' means something pretty close to 'what we can know by means of sensory experience'. As a rough paraphrase, we might say that Kant's message is, in brief, that 'We can get to know about physical objects only through having sensory experience of them' — which is a sensible (in our modern sense) thing to say, though perhaps to us rather obvious and truistic. What Kant was arguing against, in philosophy, however, was the view that we might be able to know 'things in themselves', some 'reality' beyond and superior to ordinary sensory-based knowledge. He was insisting that, even when we are talking of knowing objects that we are not actually experiencing (i.e. perceiving at the moment) we still can talk, think, etc., of them only as they would be experienced. Putting the issue the other way round, we can say that it does not make sense to talk of, say, unperceived properties of an object: if they are unperceived, we cannot know that they exist, that the object possesses them. (With modern scientific technology, 'perceive' has to be stretched to include what we can perceive by means of our special gadgets, e.g. phase-contrast or electron microscopes, telescopes, sound spectrographs, and so on. But the same principle applies: we can know an object's properties only by perceiving them.)

It would be inappropriate to allow ourselves to be drawn into a discussion of the substantive epistemological issues which are relevant here — the only point being made is the methodological one regarding meanings. To reiterate, exploration of the passage from Kant would probably get nowhere if limited to the current meanings of the English words used. Once the key difference in meaning of 'intuition' is injected, however, i.e. a new meaning not in any 'absolute' sense but merely in the sense of being new to, different from, current English usages, then the whole passage is transformed and becomes, indeed, fairly straightforward. Körner (1955: 80 ff) gives an excellent discussion of the 'intuition' question; and his whole book, with its included references, provides a most valuable introduction to Kantian philosophy.

We may note, in passing, and still on the 'intuition' issue, that the dictionary definition which was quoted above, viz. 'intuition' = 'immediate unreasoned perception', could be brought into fairly close agreement with the Kantian usage merely by deleting the word 'unreasoned'. Or, again to clarify by putting the matter the other way round, perhaps

our modern usages have generated an unnecessary puzzle by bringing in the notion of 'reason' at all in this connection. Modern usages certainly stress immediacy with regard to intuition — do they need to approach the notion of 'unreasoned reasoning'? Why not deliberately emphasize the 'perception' metaphor, and talk of 'intuition' as 'immediate intellectual/cognitive perception', distinguishing it from ordinary 'physical/sensory perception'? By stressing the metaphorical and analogical nature of the reference to intuition, we would be emphasizing both its reality and importance, and also the fact that we cannot at present give much of an explanation of what is actually involved in it. To the extent that we assimilate intuition to 'reasoning', by contrast, we seem to be putting forward a claim, tacitly at least, to understand whatever it is we are talking about. Everybody seems to assume, as we have seen in previous Chapters, that 'reason' is universally understood. 'Reason', 'rational' etc. are constantly appealed to by Peters and various other philosophers: obviously, they believe this family of concepts to be logically transparent. Whether or not they are justified in this belief is, of course, another matter, which will be further investigated in later Chapters.

The other dictionary interpretation for 'intuition', viz. 'instinctive knowledge', may prove to be worth further exploration especially with educational contexts in mind — but this also must be deferred to later pages. It is tempting to treat 'intuition' as being a derivative of 'tuition', 'teaching', and perhaps explore the notion that it might be a sort of 'inner tuition' or 'endogenous self-tuition'. Chomsky, Lenneberg and others have argued in recent decades that language-learning is endogenous or 'instinctive' in the ethological sense (further discussion and refs in Minnis (ed.) 1971); and Ewer (1969) and Barnett (1968) have cited evidence which may indicate a specific 'instinct to teach' among some species of the higher mammals. Perhaps we could argue that the human species has gone further, and developed an endogenous self-teaching behavioural tendency. More on this later.

It may be pointed out, as a matter of some importance in relation to Educational Philosophy and Education, that the model given above for the exploration of possible meanings of a given statement does in fact equate quite well with the characterization of philosophy (by Hirst, Peters and others) as a 'second order' and 'spectatorial' activity. In terms of the model as developed so far, the philosopher waits until he is presented with a statement, and then he explores the meaning, or the various

possible meanings, of it. He is presumably able to say, after he has completed his explorations, which if any of the possible combinations of individual-word meanings are meaningful or 'legitimate'. One important implication needs to be noted at this point: that if the philosopher is really and literally restricted to 'second order' considerations, i.e. if a rigidly 'exclusivist' role for philosophy, as outlined in this book, is enforced, then the philosopher is precluded from testing possible meanings of some statements against reality — e.g. he cannot test statements from within science education as a first-order scientist would, in terms of observations, experiments, measurements, etc..

A corollary of this is that if the philosopher is indeed strictly constrained to the 'second order' level, he is restricted to dealing with purely formal questions of validity of argument; and questions of truth/falsity can be posed and dealt with only in terms of an extreme Coherence Theory of truth. In this sense, and to the extent that they do want to insist on the 'second order' character and exclusivism of philosophy, the adherents on the 'London' school of educational philosophy push themselves into an extreme position akin in some ways to that of the typical Idealism of the late nineteenth century. Indeed if exclusivism and second-orderness are really taken literally, it might become problematical whether even a coherence test of truth could be applied in any useful way. Coherence could be judged in terms of the rules of grammar — but it has often been demonstrated that statements can be grammatically correct yet semantically meaningless or misleading. So if the Educational Philosopher follows the criteria laid down by the London school, and takes his function as being the second-order one of testing the validity and clarifying the implications of statements made by first-order educationalists and/or first-order scientists, then either:

(a) He restricts himself to adjudicating on the grammatical propriety, or otherwise, of the statements (strictly, the assemblages of words) offered up to him; or

(b) He attempts to assess the validity of the given statement in terms of the criteria of formal logic — but in this case, either:

(i) The significant terms, i.e. words, are assumed to be consistently used, and the criteria of formal validity/invalidity can be applied in a straightforward way — but this is unlikely to be the case, and even if it were, the philosopher's role is then a pretty trivial one (one is tempted to remark that it might just as well be performed by a computer!); or else:

(ii) It is not established that the significant terms are consistently used, therefore all possible inconsistencies would have to be sorted out and resolved before the criteria of formal validity/invalidity could be applied — and this sorting out of possible inconsistencies would in practice tend to equate with either (a) above, or (c) below.

The last alternative is as follows:

(c) The philosopher attempts to do more than the grammarian and the formal logician (or computer!), but to the extent that this involves understanding and sorting out the first-order statements that are offered up to him — statements from science and/or from education, for example — then it is difficult to see that an exclusively second-order type of functioning for the philosopher can be sustained. Putting the issue in terms of language-games, we might say that if the philosopher is to be able to deal usefully with first-order language-games, then he has got to be able to understand them properly. The philosopher's discourse itself, when he is dealing with his subject-matter (i.e. first-order l-gs), can be termed 'second-order' as Hirst (1963), Peters and others have done — but it needs to be made clear what can, and what cannot, reasonably and usefully follow from this distinction. The 'London' school and many other Educational Philosophers seem to assume that the distinction itself is in some way logically both necessary and transparent, i.e. self-evident. Against this, the present argument is that while the philosopher qua philosopher is occupied with an activity which is different from that of the ordinary scientist, say, or the educator (i.e. the ordinary classroom teacher), the suggestion that these different activities can adequately be characterized and differentiated by use of the terms 'first-order' and 'second-order' is simplistic and misleading. In particular, it is categorically false that the philosopher's second-order discourse can achieve very much if it stands on its own. This is not to say that useful and important points cannot be made within what could be described as a 'purely second-order' realm of discourse — as an example here, one might perhaps instance a distinction mentioned earlier, which must surely be among the most fundamental (in some senses at least) in all philosophy. Certainly it is among the first to be taught in virtually every introductory teaching course in philosophy in every university in the Western world: the distinction between what might be called 'truth/falsity' on the one hand, and 'logical validity/invalidity' on the other — see the

earlier pages of this present Chapter. If any distinction is a purely philosophical one, this is. It might not be too much of an exaggeration to say that the whole of philosophy as a distinctive intellectual activity and as an academic discipline depends upon it (though not, of course, solely upon this particular distinction — I am not concerned to argue this out in the present context, anyway). Whether or not the suggestion of the immediately preceding sentence is true, I take it that few indeed would dispute the assertion that the 'truth/falsity as against validity/invalidity' distinction is indeed a peculiarly and characteristically philosophical one. But can even it be made purely and exclusively within a 'second-order' realm? Can even this basic distinction be made wholly within the second-order realm, without reference to 'first-order discourse'?

It might be thought that the concepts of 'validity' and 'invalidity' could be explained in purely formal terms, i.e. entirely at second-order level; and for the purposes of present argument I shall simply stipulate that they can. (See, however, Strawson 1971 discussed in a later Chapter.) But a distinction cannot be understood in terms of only one of its components. Both sides of the dichotomy need to be covered. This means that people have to be able to understand and use the notions of 'truth' and 'falsity', as well as those of validity/invalidity; and they can learn to use 'truth' and 'falsity' and related words only by participating in the language-games in which these words are properly used, and appropriate. This means that they must learn and be able to practise first-order language-games, they must be able to use first-order discourse — because it is only in first-order discourse that we can learn when it is appropriate to say, of some descriptive statement 'X is Y', that it is true, or that it is false.

In short, second-order discourse comes subsequently to first-order not only in the temporal sequence of its being learned, but also in being necessarily dependent upon prior acquaintanceship with, and at least some degree of competence at, first-order discourse from which it derives its own subject-matter. All language-games, in fact, even the most erudite and esoteric, derive ultimately from those of ordinary everyday life, and these in turn from the 'primitive' language-games that the baby starts to play within a few days of its birth. The temporal sequence is also, and necessarily, a logical sequence: the later stages cannot be understood without the earlier.

If the history of philosophy is represented, as has been done in

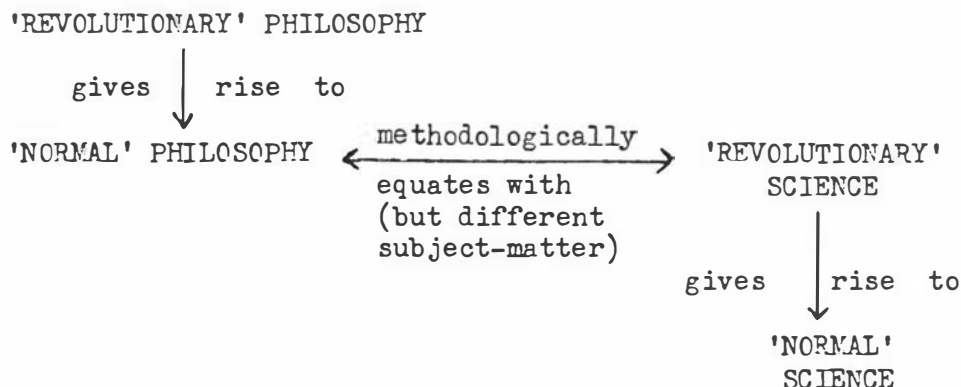
this Chapter, as a history of the gradual exploration and clarification of the possible meanings/interpretations which were inherent in the various 'great works' in the subject — for example, works by Plato, Locke, Hume, Kant, and Wittgenstein — then there might seem to be a distinction to be drawn within Philosophy, analogous to the distinction drawn by Kuhn within Science, between 'normal philosophy' and 'revolutionary philosophy'. Can such a distinction be made? How far can it be substantiated? And how useful could it be?

In an earlier Chapter we took as a simple example of revolutionary change the two statements 'The Earth is flat' and 'The Earth is round' (i.e. spherical, spheroidal ... depending on how accurate we want to be). It was pointed out that the revolutionary change consisted in the word 'Earth' changing its meaning, so that both statements, when made at the appropriate time and in the appropriate circumstances, would be analytical statements. In the first case, 'Earth' means 'flat', as you can see from the top of a tree or a tall building ...'; while in the second case 'Earth' just means 'the spheroidal planet that circles the sun ...'. In science, on the Kuhnian view, the revolutionary alteration in the meaning(s) of the words, the 'gestalt switch' as he puts it, is followed by a period of 'normal' science — usually, one would think, very much longer than the time occupied by the actual revolution — during which the implications of the new meanings are explored. In science, this exploration is largely empirical and technical. New techniques are developed, methods of observation and measurement are improved, and the paradigmatic methodology is applied to new subject-matter (Kuhn 1962:23f). In other words, the range of application of the new paradigm is extended; or, in other words again, new discoveries are made, at the empirical level, and scientific knowledge is increased in a quantitative way, i.e. additively (Stenhouse 1971).

If we assume a parallelism between science and philosophy with regard to a 'normal'/'revolutionary' distinction, it might be worth investigating in detail the precise nature of the similarities and differences.

'Normal' philosophy has been represented, tacitly at least, in the foregoing, as involving the exposure of ambiguities and inconsistencies, or, in general, as exploring the various possible meanings/interpretations of the various major works of 'revolutionary' philosophy e.g. The Critique of Pure Reason. To the extent that it is a matter of 'exploring different meanings', then, it might seem that normal philosophy would be equatable with revolutionary science, and that a relatively simple schema of

relationships could be drawn up:



One of the assumptions of the above schema is that 'exploration of new combinations of meanings/interpretations' is common to both normal philosophy and revolutionary science. The actual subject-matters are assumed to be different; and it might be assumed that this is the only significant difference between the two. Closer examination will reveal, however, that more than this is involved; and that the relationships between the four major categories of the above schema are in fact both more complex, and to some extent different in kind, than might initially be expected.

A first look at the several relationships in terms specifically of educational practices (or: of possible educational practices — since these are not as yet often actualized) might seem to support the view that a fairly straightforward equation of revolutionary science with normal philosophy is tenable. Take for example a question from vertebrate zoology (which might be an examination question, or a question posed rhetorically in order to set a conceptual framework for teaching a particular type of lesson): "Are the Monotremata (or Prototheria: Platypus, Echidna, etc.) really classifiable as Mammalia, or should they be regarded as Reptiles?" This question calls for a number of considerations which may be outlined briefly as follows:

- (a) A list of the actual criteria which are used, in practice, by systematists/taxonomists for the differentiation between Mammals and Reptiles (two major Classes of the phylum Chordata).

In terms of these actual criteria, the Monotremata are in fact classified as Mammals, being given a special sub-division of their own within the Class Mammalia to mark their peculiar nature. The answer to the question, then, in terms of actual usages, is an unequivocal assignment of the

Monotremes to the Class Mammalia. But since the question was presumably meant to elicit more than a statement of current usage, what could it really be getting at? We would be led, it seems, to ask:

- (b) Why should these particular criteria of classification be used to differentiate between Mammals and Reptiles?

Giving an answer to this question involves us in a discussion of the purposes of taxonomy/classification, and in particular of the various criteria which might be used in making the particular distinction in question (i.e. that between Mammals and Reptiles). The many different groups of Mammals and Reptiles have a considerable variety of similarities and differences, and the problem is to decide which of the differences are to be counted as decisively significant. The Platypus, for example, as everyone knows has fur like a mammal but lays eggs like a reptile — so what are we to call it, i.e. how are we to classify it?

From consideration of the diversity of characteristics of the many living animals which we want to classify, we are eventually forced by the logic of actual taxonomic practice to ask:

- (c) Which criteria could possibly be used to decide upon the classification of all animals, both living and dead, the live animal in its natural habitat and also the (often fragmentary) fossilized bones found in a rock stratum from previous ages?

At this point we can come to appreciate the reason for what might otherwise appear to be arbitrary criteria. In fact the criterion for an animal to be classed as a Mammal is if its lower jaw is composed of only the dentary bone. Reptilian lower jaws are composed of several bones, including the dentary but with others as well. Obviously, the use of the bone-structure of the lower jaw as the definitive criterion leaves out of account the greater bulk of interesting and important other features of the structures and functioning and ecology etc. of both mammals and reptiles. But this criterion is one which can be applied to both living and fossil material, and is thus necessary if we are to be able to build up an evolutionary picture of the phylogenetic history of the animals in question.

However, one last query can be made:

- (d) Is it necessary to be able to assign every individual animal, every scrap of fossilized bone, to one or other of the categories of classification?

Perhaps it will suffice for me to suggest that here, in asking this sort of question, the zoologist is coming round to doing much the same sort of thing as the philosopher. He is asking a question the real nature and significance of which is not as clear as might superficially appear. Taking the above question as the example, what are the possible answers which might be given? Or, putting the matter more in the way a philosopher might put it, how many questions, and which questions, specifically, are being asked within the general format of question (d), above?

At one extreme, we might say that it is necessary to have absolute hard-and-fast boundaries between taxonomic categories, so that every animal and every fossilized fragment of a dead animal can be assigned to a definite category. This implies that the boundaries between one species and another are razor-edged, or like a Euclidean line having length but no breadth, so that it is logically impossible for any particular animal to straddle the boundary. This 'sharp-edged' approach in taxonomy has its merits; but it also leads to what we may call 'the Adam-and-Eve paradox'. To explain this briefly: if we assume a continuous line of descent from a pre-human to the true or full human condition of the species Homo sapiens (irrespective of the diagnostic characters we happen to use), and if we also insist on 'sharp-edged' taxonomy, then we are forced to recognize that, at some point in the evolutionary line, a pair of non-human (pre-human) parents got together and produced their first offspring, and this offspring was the first human, the first member of the new species Homo sapiens. If it was a boy, why not call him Adam? Why not, indeed. Thus evolutionary biology and Genesis can be reconciled. But there are some further consequences to be faced. What about Eve? (Or if Eve happened to be the first born, what about Adam?) If more true humans are to be produced, then Adam and/or Eve must find a mating partner, and reproduce. But who are they to mate with? If they are to mate with other full humans, it looks as though they will have to commit incest. The alternative is that the girls and boys of the first human family must reproduce with others who are still not quite human. (And it must be remembered that their own parents are not quite human either!)

These difficulties can be avoided if we accept that, though it is still desirable to get our criteria for classification as clear and sharp-edged as possible, we do not have to use these criteria in the way outlined above. We do not have to insist that every individual animal must

go definitely into one category or the other. We do not have to insist that two non-human parents must at some time in the evolutionary past have produced the first fully-human child. Instead, we can simply take the phylogenetic line as a continuum, with definitely non-human (pre-human) beings at one end, definitely human beings at the other (but remember the range of individual variation in all species especially Homo sapiens), and with a transition zone somewhere in-between. In other words, provided we understand the nature of the changes that have taken place, we are under no compulsion to draw sharp-edged divisions between the various taxonomic categories especially as these apply to a single line of descent (= phyletic line). In short, while it is desirable to get our criteria as clear as possible, we can avoid the fallacy of thinking that the natural phenomena to which we apply these criteria are themselves neatly packaged and sharp-edged.

Thus the activities of the scientist and those of the philosopher come to be pretty similar as soon as the scientist begins to run into questions involving the meaning and the application of his terminology (i.e. language). It is surely a reasonable conclusion to draw from this, that if the scientist has been educated in philosophy in addition to science, he will not only have a 'broader' education (whatever that means; and I'm not suggesting it is meaningless), but he will also be more efficient as a scientist.

It will be clear from what has already been said about Kuhn's conception of 'revolutionary science' (see discussion and references in this and earlier Chapters) that once a revolution is imminent, the philosophers' skills (and habits) of looking for alternative meanings/interpretations are likely to be useful to scientists in helping them to facilitate and accelerate whatever paradigm-shift is necessary to accomplish the revolution and allow them to get down to normal science again. It need not be emphasized, presumably, that to say this is not to imply that the nature of the paradigm-shift, the revolution, is known in advance. Skill in navigation is useful towards knowing where one is, and how one might get to various other places — the navigator's art is not concerned with deciding on ultimate destinations, but only with being able to get there efficiently.

It should also be clear from the foregoing discussion, however, that the usefulness of philosophers' skills is not confined to phases of science that are already recognized as revolutionary or as 'crisis

situations'. On the one hand, background in philosophy is going to be intrinsically useful to the scientist as a theoretician (as distinct from his role as technician or even technologist) even without normal science; and on the other, it should, other factors being equal, assist the progress of science in helping 'normal' scientists to perceive emerging anomalies etc. more quickly, bring on any necessary revolutions more quickly and, as it were, get them over with more quickly.

Adding education in appropriate types of philosophy — presumably, 'normal' philosophy — to the education of scientists could be expected, then, to be conducive to these benefits. But it is evident that in terms of the schema of relationships presented earlier, there is no simple one-to-one correspondence between normal philosophy on the one hand with revolutionary science on the other. Normal philosophy is useful to both normal and revolutionary science (though perhaps more so to the latter). The relationships are complicated. Training in philosophy probably will not turn the average normal scientist into a revolutionary scientist. It may, however, by making him more conscious of the need for accuracy and appropriateness of formulation, of both problems and of findings, make him a better scientist — but on the other hand it must also be recognized that some scientists operate at a non-verbal, intuitive level (Beveridge 1957 cites Rutherford as an example here), and for such persons too much emphasis on verbal formulations and the accuracy thereof might be inhibitory. Here we encounter a need to make finer distinctions within Kuhn's 'normal'/'revolutionary' dichotomy: Keeping in mind that non-verbal 'intuitive'-type individuals may be able to make important contributions both normal and revolutionary.

One point that needs to be recognized, especially with regard to science education, is that a scientist can be innovative, creative, etc., without being in any substantial way 'revolutionary'. One of the most valuable outcomes of the work of Kuhn and many other philosophers and historians of science, indeed, is that scientists, educators, administrators and the lay public now have a much better chance of appreciating the difference between 'revolutions of theory' on the one hand, and technical innovation on the other. Hitherto, scientific progress seems often to have been visualized purely in terms of technical innovation and factual discovery: Edison, rather than Einstein or Darwin, has been the paradigmatic creative scientist. (Unfortunately this tends to be the case still, for large sections of the public, simply because of the

influence of the mass media especially TV. A new gadget, and physical models of newly-discovered 'structures', can actually be seen — whereas a new way of looking at the world or some part of it cannot itself be seen; and as discussed elsewhere in this book, there are difficulties in getting people to make the gestalt-switch which will give them a new way of seeing the world. Incidentally, it needs to be remembered that the physical models of newly-discovered structures, mentioned above, are often models of purely hypothetical entities which may turn out to be partly or even entirely wrong. They may turn out to be not only hypothetical, but fictional. But nowadays the fundamental distinction between technical innovation and theoretical revolution is at least 'available' to scientists, policy-makers, educationalists, and others, even if they do not always make use of it or understand it properly. And there is a danger that, as and if the 'normal'/revolutionary' distinction comes to be widely taken up (I say 'taken up' rather than 'accepted', because the latter word and its relatives tend to imply understanding on the part of those who accept a new notion — whereas education is plagued by people 'jumping on bandwagons' without necessarily understanding the ideas which they 'take up') — there is a danger that valuable talents of non-verbal innovation, in science and in other fields besides, may be misunderstood, mis-directed, and wasted. Especially when dealing with physical objects (e.g. scientific or technological apparatus), the non-verbal hemisphere of the brain may be important (see Ornstein 1972). Relatively non-verbal, inarticulate scientists can make valuable contributions to all science. Their innovations and discoveries can add to normal science and can lead to the generation of 'crises' which may actuate revolutionary advance.

An interesting and potentially important possibility is that the relatively non-verbal and 'intuitive' type of scientist may, precisely because of his presumed lesser reliance upon words and greater reliance on (say) an active spatial imagination, vivid picturing of imagined structures, be in a freer relationship to the accepted paradigms (CAP_1) of his time, and may thus be more able to break away to operate in terms of a new paradigm (CAP_2). There are many reports of advances originating in spatially-structured imaginings, some of them dreams, e.g. Kekulé's famous dream of snakes taking their own tails in their mouths to form rings of their bodies, which led to his demonstration of the carbon atoms linking up to form the ring structure basic to many organic compounds (see Beveridge 1957 for further details of this and other examples).

Independence of and perhaps even ineptitude in the verbal dimension, then, may be a positive advantage toward making unusual discoveries in science; and the fact that a number of notable scientists have been 'non-verbal' (we must avoid exaggerating this: nobody is suggesting that such persons are mute, or communicate only by grunts and gestures; 'non-verbal' in this context is intended to indicate only that the individual's primary mode of thought is other than verbal) may have encouraged the formation of the 'two cultures' gap and some of the harmful tendencies mentioned in Chapter 2 as afflicting science education.

But all advances, whether normal or revolutionary, have to be published or communicated in some way to the scientific and/or general community; and enhancement of the individual's ability to do this cannot but be beneficial. A certain amount of education in 'normal philosophy', provided it is of the right type, should therefore enhance the ability of the 'normal scientist' as a scientist — but both the philosophy and the way it is taught must be appropriate, see the various other sections of this thesis where this important issue is further discussed. The most sensitive area is probably that of examining: if the formal education of scientists were to include a significant amount of philosophy, their examiners in the philosophy sections of their degrees would need to be prepared, and be able, to see beneath the explicit verbal level of what was offered to them and judge in terms of the level of implicit/tacit understanding that was revealed. Polanyi's notion of the 'tacit dimension' is of immense importance for education, and less recognized than it should be (see Polanyi 1966).

Returning to consideration of 'normal' science and philosophy as compared to 'revolutionary' science and philosophy, it has been suggested in the foregoing that the 'normal' phases of both science and philosophy are constituted by the exploration of the possible meanings/interpretations, and the various consequence thereof, of the several 'revolutionary' statements that have from time to time been made. Such 'statements' are usually, as Kuhn (1962:20ff) has pointed out, in the form of relatively lengthy book publications. As examples, most of which have already been mentioned, we might take in philosophy say Kant's Critique of Pure Reason and Wittgenstein's Philosophical Investigations, and in science say Darwin's The Origin of Species, Einstein's several 1905 papers (see Einstein and Infeld 1938), and in the behavioural sciences, Freud's The Interpretation of Dreams, (see Wollheim 1971), Jung's 1906 The Psychology

of Dementia Praecox (see Storr 1973), Thorndike's 1898-1913 books and papers (Hilgard and Bower 1975), and Tinbergen's 1951 The Study of Instinct. Each of these has given rise to a substantial body of work in normal science or normal philosophy. Their implications, their strengths and their weaknesses have been explored in detail by the 'normal' research which they have both stimulated and focussed; and they have led to great increases in both factual knowledge and theoretical understanding. But granted that the relationship between 'normal' and 'revolutionary' is roughly as has been depicted, the question must now be asked: how does the 'revolutionary' statement, of whatever length it may be, from which all the subsequent 'normal' research derives, come to be formulated in the first place? How do people come to make 'revolutionary' statements?

We have characterized both normal philosophy and normal science as involving the exploration, in terms of meanings and empirical experiment respectively, of statements which have, thus far in the present argument, simply been taken as given. It has been implicit in some of the earlier discussion that particular origins have been assumed for some of the types of 'statement' that have been in question. Thus much of Philosophy of Science is concerned with the discourse of first-order science: statements from science are examined, to discover their logical status, their presuppositions and implications and so on. Similarly, much Philosophy of Education has been concerned, not perhaps with the actual discourse of first-order education (in the sense of the processes and activities of educating, the language-games of teaching-and-learning), but rather with the multitudinous statements that have been made about education, its aims, presuppositions, etc., etc. by educationalists, politicians, and the general public. (One of the purposes of this thesis, which may be mentioned parenthetically at this point, is to argue that Educational Philosophy should take much more account of, and should see itself more as being in an intimate relationship with, the language-games of real-life first-order educating; see Chapter 6, above, for example.) Thus some sectors of 'normal philosophy', viz. philosophy of science and philosophy of education, are in part not derived from 'revolutionary' philosophy, and to this extent they are at variance with the schematic model with which we have so far been dealing. It must be recognized, however, that these disciplines do also in part fit the model, in that within them, and providing part of the material for subsequent 'normal'

philosophizing, there are published works specific to these disciplines which also meet the criteria for being 'revolutionary'. Examples from Philosophy of Science would be Toulmin's 1961 Foresight and Understanding, Toulmin and Goodfield's trilogy The Fabric of the Heavens, The Architecture of Matter and The Discovery of Time and, of course, Kuhn's 1962 The Structure of Scientific Revolutions itself — indeed a large proportion of major works in this field could well be classed as revolutionary, as the field itself is a relatively new one, and it has attracted a sizeable constellation of intellectual talent. In Philosophy of Education, 'revolutionary' works do not appear to be so numerous. Hardie's 1942 Truth and Fallacy in Educational Theory eventually, after about two decades' delay as mentioned in an earlier Chapter, stimulated a 'normal' tradition in this field which has seen some variations (e.g. O'Connor's 1957 book, works by Scheffler, Soltis, the 'London' school, a nascent Australian school, and various others) but which has not so far, to my knowledge, been challenged by any radically different — and hence potentially revolutionary — alternative approach. An increasing amount of highly competent work is being published in Educational Philosophy, but it would generally be classifiable as 'normal' rather than 'revolutionary'. And since an increasing proportion of it seems to be going over much the same ground, establishing finer and finer distinctions but with diminishing marginal increases in enhancement of total illumination, the discipline would appear to be in a condition analogous to the end of a 'run' of normal science, where a 'gestalt-switch' is needed to get things going again, in a new frame of reference and with a new methodology.

The question is, how is a 'revolutionary' statement to be generated?

Before we attempt to work towards an answer to this particular question, it may be useful to draw together some of the considerations which have emerged regarding the model proposed earlier for the relationships between normal and revolutionary science on the one hand, and normal and revolutionary philosophy on the other.

The model (see earlier in this Chapter, p.226) suggested a methodological similarity between revolutionary science and normal philosophy, on the grounds that in both cases one of the chief concerns is with the exploration of possible meanings/interpretations of the discourse of the relevant subject. This does seem to be the case — with the proviso that, while normal philosophy can fairly readily be seen to be dealing with different interpretations and their several implications, revolutionary

science often would seem not to be directly concerned with questions of meaning/interpretation, but rather with either methodological assumptions or 'the facts' themselves. It is usually not too difficult to show that changing the assumptions (i.e. parts of the CAP) and/or how the facts are to be seen, involves changes in the meanings of some at least of the terms used. But work has to be done to show this. This interpretative work is the function of the philosophers and historians of science, not usually of scientists themselves (but see Hallam 1973 for an exception).

In addition to the similarity between normal philosophy and revolutionary science, however, there are other similarities which the schema of the model does not stress (and which it might, if pushed too far, actually misrepresent). Normal philosophy and normal science are similar, for example, in that in neither case are the basic assumptions of the time called in question. In both, the persons doing 'normal' work accept the positive and (usually less consciously) the negative guidance provided for them in the shape of the CA Paradigm as this is manifest in the form of the (possibly multiple) Public Demonstration Paradigm(s) which have already occurred and been recognized. As Hallam points out in the book mentioned above, the success of the theories, experiments and observations collectively designated as 'plate tectonics', has led to a huge proliferation in the numbers of people working in this field since the early 1960s. (And so he hints, many of these researchers probably accept the assumptions of the new paradigm as uncritically as they earlier rejected them!) In the behavioural sciences, Human Ethology is well on the way to becoming a popular bandwagon; and apart from the persons who jump on the wagon simply because it is popular and who have little real knowledge of Ethology, there is a danger that even properly-trained Ethologists may, on moving from the study of animal to that of human behaviour, apply concepts and techniques appropriate to the one uncritically to the other, and hence needlessly bring discredit on the human-applied science. There are, of course, plenty of normal-science workers who make no mistakes and bring no discredit to their discipline — this is because they work within the limits established by the Public Demonstration Paradigm(s), or, in the event that they do break the limits, because they present their results explicitly as being anomalous. They can then be taken up by persons equipped by natural endowment and/or training for functioning in the 'revolutionary' dimension. As an example from Philosophy, a great many academic philosophers have clearly been influenced by the paradigmatic examples of 'analytic philosophy' which have emanated from Oxford since

World War II, can do sound work within this tradition but find difficulty in really seeing the point of work done in the more purely Wittgensteinian tradition now being carried on mainly outside Britain, in North America and Australasia. Normal work, in both science and philosophy, need not be characterized as uncritical (though some may be), but rather in that the basic assumptions are not questioned and may not even be consciously considered at all.

At the opposite extreme, revolutionary science and revolutionary philosophy have their similarities, too, as well as their more obvious differences. In both cases, some at least of the basic assumptions (i.e. parts of the CAP) are called into question and/or provisionally negated (cf Stenhouse 1971, 1974). It is surely no accident that so many of the people who are significant revolutionary figures in the history of philosophy are also significant, and revolutionary, in the histories of the various sciences. One thing that many intellectual revolutionaries seem to have in common is that they often do not see themselves or their work as revolutionary — hence their surprise, noted elsewhere in our discussions, when people fail to understand them.

Further exploration of the similarities and differences between the several categories of the model will be offered later. This brief review has been useful, I hope, in showing that the simple model of science and philosophy in 'normal' and 'revolutionary' dimensions, is both illuminating in itself, leads on to significant insights, and must ultimately be transcended in favour of a much complexified model (so much so, that it might become questionable whether the use of the term 'model' would still be justified). For the moment we can return to the issue of how an intellectual revolution, in science, philosophy or any other discipline, can originate. We have been assuming that revolutionary statements do eventuate, that they provide new approaches to the world, new ways of seeing objects and events, and so on, and that they can provide the raw material, as it were, for the various activities of the 'normal' disciplines — but: We have to repeat the question, How is a 'revolutionary' statement to be generated?

To obtain an answer to this question, we must turn first to Kuhn's notion of the 'paradigm' — in the sense of 'Common Assumptions Paradigm' mentioned in Chapter 7, itself a summation of some of the distinct meanings of 'paradigm' noted by Masterman 1970 — and also, I suggest, to my own earlier arguments regarding the central function of 'negation' in the

genesis of novelty, in discovery and creativity (Stenhouse 1965, 1971, 1974).

In a scientific revolution, one or more of the Common Assumptions of the Paradigm of the previous phase of normal science are either rejected outright or else altered in some way. In either case, some particular assumption(s) must, in terms of logic, be at least provisionally negated, even if only so that an alternative assumption (i.e. a different one) can be put in as a replacement. Once an altered/new assumption has been incorporated into the Common Assumptions system on a tentative basis — thus constituting a provisional new CAP — it can be tried out, either by first exploring its theoretical implications and then testing these empirically, or else by moving to an immediate empirical test. (Logically, there must always be two distinct steps, seeing the theoretical implication, and then making the empirical test; but often the 'seeing the implication' step seems to be telescoped, psychologically and temporally, into either the original 'revolutionary thinking' itself, or else into the actual making of the empirical test: this would be so especially when a particular implication-and-test is 'obvious'.)

One of the strengths of Kuhn's thesis of 'paradigm' is that it provides an implicit distinction between a 'paradigm' which, in the Common Assumptions sense, is only partly explicit and conscious, and a 'theory' which, by contrast, is largely (though it may not be entirely) explicitly stated. We do not talk of a theory without being able to say, even if only roughly, and even if only partially, what theory it is. But a theory as an explicit formulation grows out of, is an expression of, a Common Assumptions Paradigm. Every theory necessarily presupposes a substrate of CAP — possibly of several alternative CA Paradigms. The Common Assumptions Paradigm(s) find expression only in the form of explicit statements, i.e. theories. Any particular theory, we may assume, can express only part of the CA Paradigm upon which it is based. Thus the 'statements of revolutionary science' about which we have been talking in the earlier parts of this Chapter must be equated with 'theories'. The differentiation of 'theory' from 'paradigm' thus enables us to see that the statement we are looking for must be a statement of theory, or a statement of a particular theory. Behind it, if it is indeed a revolutionary statement, there must lie a new Common Assumptions Paradigm. But if CA paradigms are usually largely unstated, and to a significant extent not even consciously known, it seems that a problem arises: for how can we know whether or not

we are really working in terms of a new CA Paradigm, how can we know whether or not a statement of theory really is revolutionary?

The problem takes on appreciable urgency when we remember a point made earlier, viz. that one result of revolutionary change is that the significant words of the relevant discipline take on new meanings. The 'gestalt-switch' of moving from one CA paradigm to another means that at least some of the language of the discipline is altered, perhaps in fairly subtle but nevertheless significant ways. How then are we to know whether we are using the same language-games as before, or not? It would appear to be possible, in principle, that we might slip from using the language-games of one Common Assumptions Paradigm (let us call this the CAP₁ l-g) into the language-games of a new CA paradigm (the CAP₂ l-g) without being able to detect that we had done so. The several l-gs, after all, must presumably be internally consistent — so what criteria could we have for knowing which l-g we were actually using on a given occasion? And in particular, how could we know when we had changed our l-gs, i.e. how could we know that we had just made a revolutionary change (from CAP₁ to CAP₂)?

A similar problem arises in principle, it should be noted, with regard to the several language-games of the 'teaching science' family of l-gs which were partially explored in Chapter 6: what are the criteria for distinguishing between the various l-gs?

Since the language-games associated with different CA paradigms are ex hypothesi internally consistent (in principle, anyway), there would appear to be only two possibilities for setting up criteria for distinguishing between l-gs from different CA paradigms:

(a) Differing degrees of empirical plausibility, i.e. one l-g accords with observations, with 'the facts', better than another; and/or

(b) What might be designated 'inter-paradigm dialogue', i.e. mixing of l-gs between (or from) different CA paradigms, would be expected, again in principle — and necessarily, somewhere or other — to generate anomalies. These could take different forms, superficially: the assertion and denial of a particular proposition, the assertion of mutually incompatible propositions (e.g. 'The earth is flat', 'The earth is round'), or the failure of adherents of one of the CA Paradigms to perceive any meaning in the statements, or some of the statements, of the proponents of the other paradigm.

It may be noted that the last of the above possibilities places emphasis on the social dimension of science (or other academic discipline or any intellectual activity). 'Mutual incomprehensibility' as a criterion is meaningful only if public discussion takes place, i.e. more than one person is involved. Also it may be remarked that incomprehensibility is likely often to be 'one way', rather than mutual in the strict sense, since the proponents of a new and revolutionary theory (i.e. a new paradigm, using language-games of CAP_2) are likely to have been educated in terms of the previously-established paradigm and therefore will understand its l-gs, those of CAP_1 , as well as the new l-gs of CAP_2 ; whereas adherents of CAP_1 , the 'old' paradigm, may be genuinely unable to understand the CAP_2 l-gs (partly, perhaps, because they are likely to be older, as individuals, than the proponents of CAP_2).

Within science, criterion (a), above, is usually in principle straightforward, though in practice it may not be met for a number of years and may depend upon technological developments unconnected with the relevant paradigms. As Toulmin and Goodfield (1965: 303) remark with regard to the first observations on individual molecular interactions:

"Not for the first not the last time, the crucial scientific observation became possible only at the extreme limit of our senses ... and with the most minute particles."

Kuhn's proviso with regard to the empirical 'proof' of new theories must also be kept in mind. "All historically significant theories have agreed with the facts, but only more or less," he points out (1962: 146). "There is no more precise answer to the question whether or how well an individual theory fits the facts. But ... It makes a great deal of sense to ask which of two actual and competing theories fits the facts better." Also, as with regard to the discovery of the positron in 1932-3 by Dirac, Anderson and Blackett working independently, it is noted by Toulmin and Goodfield (1965: 333) that:

"Here for once three different elements in scientific discovery can be distinguished sharply: recognizing a theoretical possibility (Dirac), recording a novel observation (Anderson), and demonstrating that this observation brings the theoretical possibility to life (Blackett)."

In philosophy, and presumably in any non-empirical discipline, criteria of type (a) above will not be applicable. The language-games of different CA paradigms must therefore be distinguished basically by

the occurrence of 'anomalies of inter-paradigmatic dialogue' (criteria of type (b) above). As was noted earlier, however, these anomalies are unlikely to be perceived in the same way by the several groups of people involved. Adherents of CAP_1 will most likely feel, when they encounter proponents of CAP_2 using language-games from the CAP_2 family, that the latter are just talking nonsense; or, more charitably and more truly, that they themselves just don't understand what the CAP_2 people are talking about. Most of the CAP_2 people, on the other hand, will readily understand the CAP_1 l-gs (since their own earlier training in the discipline in question, whatever it may be, would have been in terms of these language-games) — but they will recognize the limitations of the CAP_1 l-gs, and will choose to use l-gs from the CAP_2 family, on the basis of their understanding of the l-gs of both families (not necessarily an exhaustive understanding, indeed with regard to CAP_2 it will ex hypothesi be necessarily non-exhaustive since CAP_2 is assumed to be a new paradigm whose full implications have not as yet been explored). The CAP_2 people will be able to recognize the l-gs in a direct way: they will know, not necessarily from only one or two sentences, but certainly after an adequate sample of discourse has been 'obtained' (i.e. by listening or by reading), which family of l-gs is in use, just from the statements that are made, and those that are not made. Some illustrations clarify this. The famous " $E = mc^2$ " equation, for example, immediately identifies the discourse of physicists as being post-Einsteinian — but to be able to say this, we have to know at least something of the various language-games that may or may not be in question. We have to know that this particular equation was not used prior to Einstein's publications on 'Special Relativity', and that it was used subsequently. Presumably it is at least desirable — though this is not a necessary condition — that we should know, at least roughly, what it means; but it would be possible for us to assign it to the correct historical context, the correct family of l-gs, even without a full understanding of its meaning. An important implication of this is that it is possible for people who could collectively be designated as 'spectators' to assign particular utterances to the correct l-g families, purely on the basis of a 'descriptive/historical' knowledge of which l-gs were used when and by whom, and without actually understanding the language-games to any significant extent. Thus a large number of inscriptions of a particular sort can be, and are, classified as 'Linear A', and collectively contrasted with another set of inscriptions classified as 'Linear B', even though nobody as yet has

worked out what the Linear A inscriptions actually mean; whereas the meaning of Linear B has been worked out (see Chadwick 1958).

Two hypothetical families of language-games, respectively designated CAP_1 and CAP_2 on the basis of their relationship to different underlying paradigms, are therefore distinguished from each other, in practice, by different criteria depending on the educational background of the different groups of people who may be dealing with the l-gs. At one extreme we have the 'spectators' who do not understand either the CAP_1 or the CAP_2 families, though they can distinguish between them at a 'linguistically descriptive' or 'historically descriptive' level. Then come the CAP_1 people, the adherents of the 'old' paradigm, who understand the CAP_1 l-gs but not CAP_2 . Finally we come to the group designated as the ' CAP_2 people' who understand the CAP_2 l-gs — because they have actually formulated them, or are in process of doing so — and also the CAP_1 language-games, because they were originally trained in them. It needs to be kept in mind that though this last group of people have in some senses rejected the CAP_1 l-gs in favour of CAP_2 this does not entail that they cease to understand the l-gs of the CAP_1 family.

A further and most important proviso needs, perhaps, to be reiterated: that in all the foregoing, 'understand' does not necessarily mean 'understand completely/exhaustively'. It is doubtful whether any family of language-games is ever completely/exhaustively understood, if this is taken as meaning that some one person has actually explored all of the actual, let alone the possible, language-games that can be constructed in relation to a particular subject-matter or a particular Common Assumptions Paradigm. The same point could be made in terms of 'knowing all the rules' of a particular l-g or family of l-gs. But I suggest that, while 'understand' can be taken as related to a spectrum or range of levels of understanding, from 'having some idea of ...' at one end, to 'having understanding that approaches comprehensiveness' at the other, there is a broadly-defined 'threshold level' of understanding or knowing the rules, below which we would want to qualify an assertion that the person in question understood or knew the rules, and above which we would not feel that any such qualification was desirable (except perhaps with regard to special usages, special rules). This 'threshold' notion is meant to be similar to that of 'quorum characteristics' in defining the use of a word or concept. (See Hospers 1967: 69-74 for an accessible discussion, and references.)

This question of the relationship between 'understanding a language-game' and 'knowing its rules' deserves some further discussion. On this topic, linguistic philosophy links up with the views of important scientific researchers on linguistics like Chomsky (see Lyons 1970, also papers in Minnis (ed.) 1971). Chomsky's position has been — if one can hazard a minute summary of very extensive and complicated argument — that universal similarities in the 'deep structures' of all known languages, plus the fact of the rapidity of language acquisition by children (also the fact that they can readily extrapolate from limited already-known usages, and get the extrapolations usually fairly correct), collectively imply that the 'deep structures' are constituted by innate factors of the human mind. This view has been criticized on a number of grounds — as well as being attacked as 'a return to Cartesian innate ideas', 'biological determinism' and so on: all the catch-phrases currently used by people trying to discredit what they do not understand — and Chomsky himself would no doubt be loath to have such a simplistic view wished upon him (some further discussion is offered in a later Chapter). But the limited point being made here seems to be generally accepted: that the 'rules' of a particular language or language-game are picked up extremely rapidly, and from very limited experience of their instantiation; and this enables extrapolation from already-experienced usages without (usually) too many mistakes being made. This does not of course imply that all the rules are known, or that they are known perfectly — but if they were not known at all, the fact that novel utterances, extrapolations from previous actual experience, are not usually random nonsense, would be difficult to explain. This would also account for the 'creative' aspect in the use even of ordinary speech which has been remarked upon e.g. by Hockett 1967.

Returning more specifically to the question of how it is to be known whether particular language-games of, say, philosophers, are part of the 'family' of l-gs of one CA paradigm or of another, it may be appropriate to recapitulate at this point that the criteria for deciding on this must vary with the 'background' of the persons attempting to decide. It all depends, we might say, on the type and depth of knowledge which people possess, of the language-games in question. It depends on what sort of education they have had in philosophy (and similarly with regard to any other discipline). Recognition of this enables us to understand various phenomena which would otherwise remain enigmatic.

We are now in a position to appreciate the connection between the characterization of philosophy, by Peters, Hirst and others (for refs see earlier Chapters) as 'second-order' and especially 'spectatorial', and the 'exclusivist' character which it has acquired increasingly over the last century or more. If the education of those who become professional philosophers has included little or no science — or little or none of a particular science — then with regard to that science, or science in general, the philosopher is necessarily limited to the spectatorial role which has been outlined. If he does not understand the language-games of a particular science, or of any science, then it is quite natural that what he can say about these language-games is limited. It is not the case, of course, that he can say nothing about the l-gs of science: like the student of ancient languages who can classify some inscriptions as Linear A and others as Linear B even while having no understanding of the former, the exclusivist philosopher can say something, even something significant and valuable, about language-games which he does not properly understand. In this light, it can be seen as no accident that there has been an increasing formalism in general philosophy, that Philosophy of Science concentrated for so long on attempting to formalize science and construct symbolic calculi which were intended to represent the processes of science, and that philosophers of education, having so far avoided the lure of constructing symbol-systems to represent the actualities of educational interactions (it is mainly the Educational Sociologists, interestingly, who have fallen into this particular trap, also some branches of Educational Psychology), have by a peculiar partial insight that stopped half-way, labelled their discipline 'spectatorial' and 'second-order'. Could it be yet another sign of their conservatism that, having had the insight to perceive their own position as mere spectators, they have been content to leave themselves in that position? Could they not explore further, and try to find out why they are merely spectatorial? Or do they not wish to be participants, rather than be spectators? There is the adage which one has heard quoted by prominent educationalists (e.g. the late Sir Fred Schonell, when Vice-Chancellor of the University of Queensland), that "The spectator sees most of the game". We might well ask whether merely seeing the game is enough, and whether one can really understand what is going on in the game if one has never actually played (or if one has played to only a limited extent many years ago). Even if we were willing to grant that the spectator does see and understand more of the game than any one of the players, the

moral question still remains (since 'game' is only an analogy, and what we are really talking about is education), whether one can be justified in remaining a mere spectator, especially if the 'game' seems to be deteriorating. In a real game, of course, the spectators ought not to be encouraged to jump into the arena when their team seems to be losing — very much to the contrary, in fact — but in education, which as a societal activity is intimately involved with most other aspects of societal activity, especially perhaps in the political and economic spheres, it is at least questionable whether a purely spectatorial role is morally justified. It might also be questioned whether it is even possible. In the political dimension the operational principle 'Whoever is not for us is against us' (cf Matt. 12: 30) is normally used in converse, 'Whoever is not conspicuously and consistently against us, is strongly and approvingly on our side'; i.e. any Establishment, educational or political, usually finds it convenient to assume that everyone is in enthusiastic support of their policies and persons: and that opponents are either not really opponents (that is why opposition to be recognized as such, has to be both outspoken and consistent), or are misinformed, irrational or, in the extreme of Establishment paranoia, traitorous. In the absence of definite statements of position on education and/or political issues, educational philosophers are and will be taken as supporting the Establishment and the status quo. This is not to suggest that current policies and practices are always to be rejected — far from it — but we do need to examine them, decide on their merits and demerits, and then make our views known unequivocally.

Educational philosophers are at some advantage as compared to many philosophers of science, in that while it has been possible, and still is, for an individual to have little or no first-hand experience of science, nobody (in Westernized countries anyway) can be totally lacking in first-hand experience of education — if only on the receiving end.

It is at this point that we can begin the simultaneous resolution of a number of distinguishable but interrelated difficulties.

If everyone has had experience of education, in one form or another, we can argue, then, that in one dimension at least they should be capable of breaking through the 'spectator boundary/barrier' and participating in discourse about education which is in at least one respect 'first-order' and non-spectatorial. Those who have had experience of teaching in addition to being taught are thereby able to operate on a first-order basis in

an additional dimension. (We do not commit ourselves to saying how well they, or anyone, can operate.) We have not been mere spectators in our own education; neither are we who teach philosophy mere spectators of the activity of teaching (or of doing philosophy, if in fact our teaching consists in actively arguing ... See Chap. 1.).

From the fact of our own experiential involvement in education can come not only a repudiation of a merely spectatorial role in discourse about education, but also the possible genesis of 'revolutionary' statements with regard to educational policy and practice, educational theory and even educational philosophy. To be revolutionary, as was argued earlier, a statement would have to involve, either explicitly or by implication, a negation/rejection of at least a part of the previously-accepted Common Assumptions Paradigm, and the supplanting of what was rejected by some new assumption (this again could be either explicit or implicit). Since more or less everyone in Western societies has received some formal education, and since many of them have not liked it very much, it seems at least a possibility worth exploring, that a good many people may wish to reject one or more of the assumptions of the CA Paradigm(s) upon which their own formal education was based. Some of them, of course, wish to reject the whole business outright, emphatically, and even, alas! violently. Total rejection of all education is certainly in one sense 'revolutionary', but not really very constructive or in the long run adaptive. Formal education is literally and totally rejected by some individuals, it is true — but the important question is, to find the grounds for their rejection. Once we find out why people reject particular features of an education system (or the system itself in toto), we may be in a position to formulate statements of a truly 'revolutionary' nature, in the Kuhnian sense, about education. Some of the individuals who do in fact reject education may themselves formulate such statements. Unless they have sufficient background in formal education, however, they are perhaps rather unlikely to do so effectively — also, their formulations may be distorted by emotional reaction — so it is more likely that useful 'revolutionary' statements will be made by persons who have not themselves reacted too strongly against the education which they have experienced.

Thus the requisite background that seems to emerge from the foregoing is that of a person who has been able to remain within the processes of formal education of his society, and who has been able to gain

whatever benefits such education can offer, but who has been disillusioned by his experiences so that he has sympathy for and understanding of the views of those who have perhaps been even less fortunate than himself. Given a widespread dissatisfaction with formal education, then, and given that there must be some individuals with the right background both to understand and to give expression to this dissatisfaction, it becomes likely that new and 'revolutionary' theories about education will emerge.

As examples to show that this sort of thing is in fact happening at the present time, we may cite Ivan Illich's (1972) Deschooling Society which is known, it seems, throughout the Western world, Koerner's (1964) The Miseducation of American Teachers, and the volume Counter Course (Pateman 1972) from which the paper by Adelstein has already been discussed. With regard to theoretical statements such as these, two questions arise:

(a) How are they, or can they be, related to the Common Assumptions Paradigms which underlie them? And are these CA paradigms the same as, or different from, the currently-accepted paradigm(s) of the society in question? This latter issue is intimately related to the second question:

(b) What is or is going to be the reaction of the society involved, or of the relevant sub-section of the society, to the new theory and its underlying paradigm(s)?

It may be appropriate to begin with a few remarks on the latter question first.

In a general and abstract way, we could say that the 'criticisms' being made against current education are either justified, or they are unjustified. It must be remembered, here and always, that any suggestion for improvement is, implicitly, a criticism. To suggest that something should or could be done better is, necessarily, to suggest that it is not at present being done as well as possible. On a different tack, we can say that from the viewpoint of those being criticized, there is no such thing as 'destructive criticism': there is only the question of whether or not the criticisms are real, are well-based. (The motivation and intent of those making the criticisms is, of course, another matter, and one which is important in its own right. People do make criticisms, suggest improvements, etc., for all sorts of reasons, and these may not

be disinterested. A critic may be seeking ~~more~~ notoriety, or to 'put down' a personal enemy, or to get a better job ... But these are different issues from those we are considering now.) On this matter of the response of those being criticized, again we can set up a simple heuristic dichotomy: either they are prepared to examine the criticisms and suggestions for improvement 'with an open mind', or they are not. If they are not (and in practice this may not be easy to detect), then presumably they should relinquish whatever responsibilities for education they currently hold, and hand over to persons who are prepared to give a fair hearing to criticism and suggestions for improvement. But they are unlikely to do this. Instead, they are more likely to try to hang onto whatever position and power they possess, and try to brush aside, suppress and/or discredit their 'critics'. If this is their reaction, then by it they constitute themselves an 'Establishment' in what seems the most basic definition of that term, viz. a group within a society, which has been charged by it with a responsibility, but which is more concerned to retain for itself power and status, etc., than it is with actually discharging the responsibility in a proper and effective way. It may be reiterated, that often in practice an Establishment is difficult to detect as such: dissimulation and deceit, 'conning' the outsiders, fooling most of the people most of the time — these are parts of the repertoire of any Establishment. No member of an Establishment is likely openly to refuse consideration to suggestions and criticism. Camouflage is essential.

From the point of view of the society, losing the possibility of improvement by rejecting or ignoring suggestions for improvement even if these are expressed in the form of criticism is always in the long run dangerous. Probably we could go further, and say that it is not only dangerous, but damaging. Any society which fails to improve itself, no matter what the 'dimension' of the improvement, renders itself less competitive in relation to other societies. It is likely, if suggestions for improvement are consistently ignored, that the society in question will sooner or later lose its autonomy and be supplanted by another, more progressive society. But societal 'weakening' is a complex process; and part of it is likely to involve the alienation of its own creative minorities who see the possibilities for improvement, and see them rejected. The creative individuals who see their ideas being rejected — and who see conforming mediocrity elevated to the highest honours — are likely to switch their creative efforts away from dimensions beneficial to the society; or else merely

abandon the arduous business of creative thinking. When the dimension in question is education, when education of the young is allowed to remain sub-optimal, the society's most vitally important resource — its people — is left with its potential not fully developed, and/or mis-directed. No society can for long afford to do this.

These foregoing matters have already been widely debated, and the general importance of improvement in education is recognized (even though there is no agreement on what should be counted as improvement). What is not yet widely understood is the real nature of some of the difficulties of communicating a new theory, a proposed improvement, from those who have thought of it to those who have to decide whether and how to implement it. The latter persons are those in positions of power and responsibility in the society. They may or may not constitute an Establishment as defined above — but for various reasons they are likely to find the reception of new ideas both uncongenial and difficult. For one thing, they are likely to be relatively old: it takes time to work one's way up the pyramids of power. Age not only lessens a person's ability to assimilate new ideas (usually); but also, anyone who has spent a professional lifetime working within one particular frame of reference must inevitably tend to be heavily conditioned by it. It is here that we can begin to see a parallel, or perhaps more truly a convergence, between the processes and interactions of social and societal change, on the one hand, and those of intellectual 'revolutions' in the Kuhnian sense, whether in science or in other fields, on the other. Innovations in a society are in many ways like scientific revolutions. In both cases, two or more groups of people are thinking and talking within frames of reference which are in fact different — though usually at least one of the groups does not realize this. They are working in terms of different Common Assumptions Paradigms. As we have seen in the earlier parts of this Chapter, and in earlier Chapters, adherents of the older and established paradigm (CAP_1) are likely to be, quite genuinely, unable to understand the statements based on a new paradigm (CAP_2). It is not necessary to postulate unusual obtuseness, or malice, on their part, to explain their non-acceptance of the theories or policies offered by CAP_2 (though it is also possible that malice, etc., may be present). The basic problem is that the proponents of CAP_2 are quite literally, though probably only partially, talking a different language from that of CAP_1 ; and though the adherents of the latter cannot understand what the CAP_2 people

are saying, they do not usually realize that this is the case, whereas they do realize that the CAP₂ people are no longer accepting what they, the established CAP₁ people and hitherto the accepted leaders, are saying. Hence to them, the behaviour of the CAP₂ people appears to be simply perverse: they are seen as rejecting CAP₁ but as putting nothing sensible in its place.

Additional to the basic problem as already outlined, there is the further difficulty that many of the people who will eventually (perhaps much later) be recognized as having been the early proponents of CAP₂, do not initially manage to get their revolutionary CAP₂ ideas effectively formulated. This adds to the hazards of communication between the several groups. As one example of this, we may refer back to the criticisms of Peters and the 'London' school of Educational Philosophy which were promulgated by Adelstein. These criticisms were not baseless — but they were to some extent mis-formulated, as we saw in Chapters 3-4. Adelstein may have lost many potential supporters, simply because they were unsatisfied by his case as he had formulated it. For another example, widely diffused, take the reaction of many established scientists to the Drift from Science (see Chapter 2). Many of them are so deeply involved with their own particular 'normal science' paradigm — which has certainly been successful, otherwise it would not have given rise to a sustained deployment of normal science — that they are unable to see that current science education may have its limitations and weaknesses; hence they fall back upon 'laziness and/or stupidity of students' as what is to them the only intelligible explanation for the Drift. In this I believe they are wrong, and I have argued the issue in various other parts of this thesis; the paramount requirement, however, is that everyone involved should understand the real nature of what is going on: the change from CAP₁ to CAP₂. To equip people for such understanding must be the objective and aim of an improved type of education which must be initiated at tertiary level. This initiation — itself almost the diametrical opposite of Peters' notion of 'education as initiation' — must be regarded as an example of the sort of societal improvement which is of similar nature to a 'revolution' in science. We must therefore return to further examination of what is involved in such innovations, revolutions, and/or 'improvements'.

Some of the behavioural propensities relevant to the dissemination of new ideas, especially in relation to social dominance hierarchies,

have been discussed elsewhere (Stenhouse 1974, Chap. 8), but some may usefully be given brief mention in the present context. It has already been indicated, for example, that individuals of seniority and influence are usually older and, more significantly, more deeply imbued with already-orthodox views, than are the individuals likely to come up with new ideas. Even when (and if) the new ideas achieve explicit formulation, then, they are likely to have to contend with opposition from the established and influential people at the upper ends of the social dominance hierarchies. They have to battle against the 'mimetic gradient' — though if they are understood quickly by intelligent individuals at the upper levels of influence, then can be propagated rapidly, with the mimetic gradient in their favour. On the other hand, new ideas may not initially be formulated in a readily comprehensible statement, because the individuals who first think of them, as well as lacking 'standing' in terms of the current orthodoxy (CAP_1), may also be genuinely of less-than-outstanding competence at the usual CAP_1 activities. The 'outsider' phenomenon is well recognized by now: that innovations especially if of 'revolutionary' nature are often made by persons having relatively little background in the discipline in question. (This fits in well with my arguments (Stenhouse 1971, 1974) for 'negation of the current assumptions/' 'knowledge' as an essential prerequisite to creative advance, since individuals who have relatively scanty background have, in a sense, less to negate, hence should find it easier to negate it.) But this consideration goes to some extent against my earlier generalization that the proponents of a new paradigm (CAP_2) are able to operate in terms of the language-games of both CAP_2 and CAP_1 : for the proponent of a revolutionary new theory (CAP_2) may not, in fact, make a very good job of stating or explaining it to people whose understanding has been couched exclusively in terms of the l-gs of CAP_1 , even when he is genuinely trying to explain.

Clearly, this can constitute a powerful argument for increasing the amount and quality of philosophy-type training provided for scientists, indeed for everyone.

At this point we must endeavour to relate our discussions of the hypothetical schema of relationships between 'normal' and 'revolutionary' in both science and philosophy, earlier in this present Chapter, to the educational needs which were outlined in terms of existing disciplines in the latter part of the previous Chapter. We are now in a position to specify in detail the type of philosophical training which is most urgently needed.

The heart of the educational problem can be represented as that of getting people to understand the changeability of language-games, or, to put the issue in its sharpest and most worrying form, the changeability of our knowledge. For it is, or should be, a very worrying thought, that as teachers we are teaching to our pupils material some of which is certain to be proved erroneous. Yet unless the nature of science has changed radically at this very time, some at least of what we are now teaching, some at least of what we are presenting to our students as true, will at some time or other in the future be proved to be false. We are up against the problem of whether the earth is round or flat.

In putting the problem in these terms I am deliberately emphasizing the 'naive realist' approach, because this is in fact the approach adopted by the vast majority of science teachers at the present time. In taking this approach, the 'shape of the earth' problem is dealt with by presenting it as an historical progression from error to truth: people once thought (or to be more accurate, perhaps we should really say they knew — but to say it this way would destroy the plausibility of the story) — people once thought that the Earth was flat, but now we know that it is round (spherical, spheroidal). This is fine. The story is neat and tidy, and the pupils can learn it up and get it right, we hope, in the examinations. But it involves a trap. The trap is, that if science continues in the future as it has in the past, with errors being exposed as such and replaced by truth, then some of what we take to be truth, now, will be exposed as error and will be corrected. The trouble is, we do not know which parts of what we now take to be truth will turn out to be error — since we cannot foresee future developments in science any more than we can in any other field.

The way out of the trap is, of course, to repudiate a naive realism, and develop a more sophisticated approach. We have already been doing this, in earlier Chapters. We have used Kuhn's notion of the 'paradigm', particularized in terms of Common Assumptions Paradigms each having its associated family of language-games, to show how a 'gestalt-switch' of meaning can occur — and does occur, whenever a scientific revolution takes place — simply because a transition has been made, wittingly or unwittingly, from one family of language-games to another. Since the basic problem can be solved only by recognizing the existence and the significance of difference between language-games, the type of philosophy in which it would be most valuable to provide education is that which

takes most notice of differences between language-games. Clearly, the work of Kuhn, Toulmin, Wittgenstein himself, and others will be relevant here — but perhaps of greatest significance, certainly for our purposes at this stage in our argument, may be the works of those who have carried the Wittgenstein tradition to its most extreme limit.

Wittgenstein's successor in the Chair of Philosophy at Cambridge was John Wisdom; and while it is not being suggested that Wisdom's writings form in every respect an 'extreme limit' to the line of development initiated by Moore and Wittgenstein, there are some respects in which I feel it can be said that Wisdom exemplifies in a fairly extreme (and therefore readily perceived) form some of the features of the 'linguistic' approach in philosophy which are most valuable for present purposes. Passmore (1968: 440) says of him:

"His characteristic method consists in first making a distinction — say, the distinction between a 'logical' and a 'conflict' dispute — as if it were a sharp one, and then blunting its edges; or first making an assertion — say, that philosophical paradoxes are verbal recommendations — and then asserting its contradictory. ... all the philosopher can do is to mislead and then — elaborately — to draw attention to the points at which what he has said is misleading — and not misleading."

'First making an assertion ... and then asserting its contradictory' — this would certainly appear to be a fairly extreme case of 'alternative language-games', strikingly similar to the example used several times already of 'The Earth is flat' and 'The Earth is round'. If Wisdom's philosophy consists of trying out various alternative language-games, it would seem that the educational programme for which we have been searching should approximate to Wisdom's manner of doing philosophy, his way of conducting the activity of philosophical investigation.

Two questions may be asked at this point:

(1) Does this mean that a short (or even a longer) course of study of the published works of John Wisdom would provide the corrective for the alleged deficiencies of current education in science (in the sciences)?

(2) What exactly is Wisdom's method of doing philosophy? Or, to make the question more pointed, does the mere enunciation/utterance of an assertion and its contradictory (or a variety of alternatives) constitute Wisdom-type philosophy? (Because if it does, philosophy would appear to be both easy, and utterly trivial.)

The answer to both questions (taking (2) in its second form) is "No" — but what is important is not just a yes-or-no answer, but what lies behind it, the explanations and reasons for giving one or the other, a 'Yes' or a 'No'. These will be offered in the next Chapter.

CHAPTER NINE

PHILOSOPHY IN EDUCATION: KNOWING AND DOING.

There is no question that anyone who wishes to attain to a real understanding of Philosophy and competence at doing it must study it at length and in detail, must study its historical development and the variety of its present manifestations. As Wisdom (1952: 58) pertinently remarks, "The value of a move cannot be judged without knowing what moves have been made and what are intended"; and this applies to judging Wisdom's own place in the history and methodology of philosophy as much as to a move in chess, politics, or evolution. There would seem to be a limitation implicit here, on the feasibility and/or desirability of using Wisdom's own work in the early stages of teaching people how to do philosophy. It has developed out of a particular context in the historical evolution of philosophical methodology, and it seems possible that its significance may be unappreciated by persons who are unfamiliar with its context. But does this mean that we can approach a Wisdom-type philosophy only by first recapitulating the whole history of Western philosophy? (This, it should be realized, might be held to entail studying the history of the various sciences too, as they severally 'budded off' from the philosophical stem.)

I suggest that there are, from the educational point of view, two very different answers which are possible here. They are not mutually exclusive, indeed to some extent they might be regarded as complementary; and either, on its own, might prove adequate for the educational purposes in question. (To some extent this question of adequacy is contingent upon the persons being taught, their type and level of ability and existing background, etc.; also the degree of sophistication being aimed at.)

One strategy which could be used to generate the ability for and indeed the habit of 'cross-disciplinary intercourse' or 'inter l-g flexibility' is simply that of requiring a multi-disciplinary education and then forcing students to attempt cross-disciplinary syntheses in the form of written exercises and/or examinations. They would thus come to be familiar with a number of different language-games; and especially if some of them overlapped and provided alternative ways of saying the same thing — and also if they illustrated that similar forms of words could overlies conceptual relationships that are basically different — the students could

come to appreciate both the diversity and also the 'shiftiness' of language-games. (See earlier Chapters, esp. Chap. 7). This strategy would be facilitated if one of the disciplines studied were Philosophy — and this leads us toward consideration of the second strategy:

Appreciation of the multiplicity of and confluences between language-games could also be attained by a course of study mainly in Philosophy itself. Needless to say, it would be desirable that the philosophy should be taught on an 'inclusivist' rather than an 'exclusivist' basis — nevertheless it is possible that what is taught should be mainly philosophy, and recognizable as traditional philosophy, at that.

Within the spectrum of traditional philosophy, and with classes which have a good general background and some (albeit slight) acquaintance with philosophy, my own practice has been to start them off by reading Locke's Essay concerning Human Understanding. (I hope the reader will excuse this intrusion of personal experience). One of the educative experiences they get from this is that of seeing modern ideas gradually emerging from the archaic language and literary forms of seventeenth-century English. This alone is an eye-opener to most students. They are quite startled to find an account of how we come to have knowledge of physical objects, say, which is similar in many respects when appropriately translated, to what they find in modern textbooks of psychology and sensory physiology. It is educative, again, for them to discover in the writings of a great figure from the past both penetrating insights which seem to leap ahead across the centuries, and also views which seem to be not only incorrect but even quite silly. But then again, some of the latter can seem to have more point when the historical background is sketched in, e.g. that Locke was arguing against the Cartesian doctrine of innate ideas. (It will be apparent from the foregoing that I believe in pushing my students into the primary sources and making them grapple with the original material sometimes without any initial guidance as to historical/methodological perspective or as to what the accepted interpretations may be. They get all that in the second phase of teaching. It seems to me important that students should not merely be given all they need. If they are ever to become 'match tough' intellectually, they have to learn to cope with perplexity, confusion and frustration. As Passmore has emphasized (1972: 429): "One of the educator's tasks is to make his students puzzled. ... unless pupils leave school puzzled ... teachers have failed as educators...".)

In fact I do not normally attempt much critical analysis of Locke. His writings tend to be plausible and commonsensical, at the expense, often, of being not very rigorous in argument. David Hume, on the other hand, came as a matter of historical fact to adopt roughly the same basic position as Locke, but developed it rigorously — so that its flaws came to be perceived, with two results:

- (a) Hume himself gained notoriety as a 'sceptic' (cf Passmore 1952, Chap. 7); and
- (b) Kant was later stimulated by Hume and in answer to him, to develop the 'critical philosophy'!

It is convenient for teaching purposes, then, to allow students to be 'convinced' by Locke for a while ('sucked in' would be a colloquial and somewhat tendentious way of putting it), and then to study Hume and get down to serious criticism. Following and in fact recapitulating the actual historical development of the subject, the exposure of grave difficulties in the Locke-Hume position leads on to detailed examination of Kant's reconstruction of the fundamentals (a 'revolutionary' change in philosophy itself), using his Prolegomena as a more concise and self-consistent exposition of the 'critical philosophy' than the Critique of Pure Reason. Even the Prolegomena are far from easy. Considerable 'translation/interpretation' has to be done even to come to an understanding of what Kant is saying (cf the meaning of 'intuition', see Chap. 8, above). Careful reading and class discussion of the Prolegomena can lead to a number of appreciations which I believe to be important: Kant's emphasis on the need for both empirical realism and transcendental idealism; the function of the latter as providing a limitation to the claims of 'positivism' and reductionism and 'scientism', a function which is especially needed today (see illustration discussed below); and, perhaps above all, Kant's careful regard for the extreme complexity of the issues with which he is dealing and the respect which he accords to the achieving of appropriate formulations and adequate argument. This last point, of 'formulations and argument', deserves some further discussion.

Lucas (1953: xiii) mentions the enormous length of many of Kant's sentences, notes that in some cases this is due merely to careless punctuation, but points out that in many other cases the long and complex sentence-structure "... is a significant consequence of Kant's philosophical method. Kant's characteristic way of thinking and his conception of reason as only corrigible from within itself (i.e. by surveying the relations of

its parts) makes it part of his philosophical impulse to hold an unusually large number of ideas for consideration together and to need simultaneously a large number of qualifications and expressions of relations. It is not easy to break up such sentences without falsifying the thought by giving what is really synopsis the appearance of inference." (Emphasis added). In other words, what Kant presents to the reader is in these cases not a chain of inferences along which the reader is led, step by step — giving his hand, as it were, into that of the author, who decides the nature and the sequence of the steps — but rather a full array of all the relevant considerations plus a statement of the conclusion to which Kant feels they lead. It is, in effect, left to the reader to construct the details of the argument for himself; and though this might be thought to make undue demands upon the reader (as indeed it usually does), it is also a very honest way of proceeding. The 'evidence' is presented, the conclusion Kant draws is presented, and readers are left to make up their own minds. The very first sentence of Kant's own Preface to the Prolegomena has a striking significance in this context (indeed in relation to the central theses of the whole of the present work):

"These Prolegomena are not for the use of pupils but of future teachers, and they are intended to be of service to them not for arranging their exposition of an existing science, but for making their first discovery of this science itself." (p.8).

In other words, Kant sees philosophy itself — or his type of philosophy, anyway — as being both creative and educational, and by implication as being an activity. (The word 'science' in this context would appear to mean no more than that the enquiry should be exact and rigorous; the term's modern connotation of 'necessary empiricism' had not developed in Kant's time.)

Due to limitations of time in the courses I have been teaching since 1966, I have normally decided to jump straight from the rewarding difficulties of Kant to the equally rewarding — but different — difficulties of Wittgenstein and 'linguistic' philosophy. Here again, it has seemed to me to be best to concentrate on a few selected primary works, rather than dissipate attention over a wider area. The Philosophical Investigations demand acquaintance, especially the earlier sections. As a good example of linguistic philosophy in the Wittgenstein style, and as giving an excellent link-up with the main topic of attention throughout the course (viz. our knowledge of physical objects), I have found the

paper by Ziedins (1956) extremely useful. Entitled "Conditions of Observation and States of Observers" (Phil.Rev. 65: 299-323), it tackles some of the issues relating to the question of what it means to say that some conditions of observation are 'standard', some states of observers 'normal' etc.. Most importantly, this paper exemplifies (in some ways perhaps even better than some of Wittgenstein's own writings) not only talk about the avoidance of linguistic confusions and deceptions, but also shows how it can be done. Necessarily, this 'doing' often involves complexity of formulation. Ziedins' investigations are similar in subtlety and complexity to the writings of Cavell (see Cavell 1969) — the latter are of course much more widely known. Simplistic formulations are likely to cause as many problems as they cure. Neither Ziedins nor Cavell goes to Wisdom's lengths in adopting a 'method of extremes', the enunciation of a statement and then of its contradictory; but both take their readers with them on exploratory journeys into 'What it could mean to say ...'. Ziedins' paper I have found especially valuable in the context of the particular teaching course involved (at first-year postgraduate level, but with students who have done very little philosophy beforehand), in that its topic, our knowledge of physical objects, links up with the work done earlier on Locke's 'simple ideas of sense' out of which he and Hume tried to build our knowledge of the external world; also, while the paper itself is complex in its argument, this provides the teacher of the course with an opportunity to offer simplified versions of similar argument, which is useful in various ways. Students are helped towards a better appreciation of Ziedins' own argument. The contrast between the simplifications (which are easier to grasp) and the original complex argument (which avoids the misleading ambiguities of the simplified version — but which is initially rather difficult to understand), points up the basic lesson of the whole 'linguistic turn': that of minimizing the deceptiveness of our verbal formulations while at the same time dealing adequately with the often immense complexities of the relationships with which we have to deal. The 'Law of Requisite Variety' (see Ashby 1964: 206) applies: phenomena of a particular degree of complexity can be 'dealt with', whether in terms of understanding them, controlling them, or whatever, only by means of formulations, devices, etc. which themselves have enough complexity or 'variety' to do the job. This is not to say that the formulations, controls, etc. necessarily have as much variety as the phenomena to which they relate. In general, however, it might be said that complex phenomena demand formulations etc. which are of sufficient complexity and variety — or in

other words, the danger to be avoided is simplicism. In the teaching situation, a simplified initial presentation is often desirable; and it needs to be followed up by further teaching which gives, eventually, the full story. In attempting to teach philosophy as an activity, I have found it useful (some but not all of the time) to reverse the usual order and present complexity first, then show how it can be simplified and thus more readily understood. But after the simplified story has been grasped, the next lesson is to show why the more complex formulation is in fact often superior to the simple. (Not that it is superior merely on account of its complexity. The complexity has to be of the right sort — but still, complexity as such must be seen as sometimes desirable.) This is where the recapitulation of the historical progression of Locke-Hume-Kant-Wittgenstein is valuable: because it starts the students with a relatively simple, seemingly commonsensical and 'scientific' position, shows up first some of the basic limitations of that position, shows how some of these might be surmounted, then starts to show, reflexively, how to go about this 'surmounting the limitations' in terms of conscious and deliberate awareness of and control over the way in which problems are seen and formulated. Wittgenstein, Ziedins and other linguistic philosophers can be seen to be doing something very similar to what Kant was doing, but doing it rather differently, and better — partly, of course, because they have had the benefit of being able to start where Kant and his many successors left off.

The sequence of study in this teaching course recapitulates the historical sequence of the development of a particular strand of philosophy over the last three centuries; and this in itself seems to me to have several benefits. The course shows something of the history of ideas (even though this aspect as such is given little explicit discussion), and thus provides a basis for some historical insight to be developed. Here again, an inclusivist and inter-disciplinary approach is implicit. Links are maintained with history on the one hand, and the psychology and physiology of sensory perception on the other (as mentioned earlier), also through various issues of interpretation a continuing exercise of literary skills is carried on — and all this helps in the mutual activation of interest between different disciplines. Additionally to mere interest, there is also a potential bonus in the possibility of transfer of specific insights and skills between disciplines, especially perhaps from philosophy and the literary disciplines into the various social and so-called 'basic' sciences:

all subjects involve clear, critical thinking, and literacy. (Thus it is no accident that Parkyn 1957 found that the best school-level predictor of university performance was the individual's mark in School Certificate English; somewhat surprisingly, this was found to be a slightly better predictor than either aggregate or average marks in the same set of examinations.)

It is not being claimed that the course as outlined is the only or the best approach. I have devoted some space to outlining and discussing it here because it represents a departure from what I understand to be the usual type of approach to the teaching of Educational Philosophy (or Philosophy of Education), which takes publications specific to this field as the basic sources/texts to be studied. Often the publications of R.S. Peters and others of the London school are the main or the only works to be studied; and this, as I hope to have demonstrated (in the foregoing Chapters, also below), can result in inadequacies and a general superficiality of treatment.

My own teaching is conditioned, as is inevitable, by the context in which it takes place. The course mentioned in the foregoing is given within a Department of Education in a university. The students are studying for an Honours degree (either BA Hons, MA or MPhil) in Education and, as previously mentioned, they have relatively little background in Philosophy, in most cases. I feel that, given the very limited amount of contact, it is best to put maximum time and energy into grappling with some of the most basic problems of philosophy, using primary sources. The question of 'what Knowledge is', of 'what it is to say that we know something, e.g. a material object', is certainly a basic question in Philosophy and also, one would have thought, in Education.

Education is commonly held to involve the transmission, enhancement, etc., of knowledge — and surely people with postgraduate degrees in Education should be expected to handle questions as to 'what knowledge is', and so on. The centrality of the concept of 'knowledge' to education has been attested by Peters (1974) among others, when he writes:

"The writings of Paul Hirst, especially his 'Liberal education and the nature of knowledge', have been one of the most formative influences in the development of philosophy of education during the past ten years. There is a sense in which anyone working in the field has to take up some stand with regard to the 'forms of knowledge'." (Peters, in Hirst 1974: viii).

Hirst himself, in the paper in question (Hirst 1974, originally published in 1965), talks of 'knowledge' as "... a concept central to the discussion of education at any level" (p.30). Few, presumably, would dispute this; but many would wish to know more about what is meant by 'knowledge'. Within the general concept 'knowledge', Hirst distinguishes a number of 'forms of knowledge'. "By these is meant ...", he says (p.38) "not collections of information, but the complex ways of understanding experience which man has achieved, which are publicly specifiable and which are gained through learning." He summarizes the 'forms of knowledge' as follows:

"In summary ... it is suggested that the forms of knowledge as we have them can be classified as follows:

I. Distinct disciplines or forms of knowledge (subdivisible): mathematics, physical sciences, human sciences, history, religion, literature and the fine arts, philosophy.

II. Fields of knowledge: theoretical, practical (these may or not include elements of moral knowledge)." (p.46).

He had earlier stated (p. 43) that "... it is necessary first to try to distinguish the various forms of knowledge and then to relate them in some way to the organization of the school or college curriculum. The first of those is a strictly philosophical task." (my emphasis added). He starts his 'philosophical task' by explaining: "... by a form of knowledge is meant a distinct way in which our experience becomes structured round the use of accepted public symbols." We shall return to detailed discussion of what he can mean by this — but first I must digress, to suggest that this formulation, involving 'the use of public symbols', might be interpreted as hinting at a language-game approach. This suggestion will be developed later. The second (and quite independent) point in my digression is, that while in the quoted summary, above, Hirst seems to include both 'disciplines' and 'fields of knowledge' as 'forms of knowledge' (i.e. both the former are subsumed under the last), in the preceding passage of which the quoted section is a summary, he distinguishes 'fields of knowledge' on the basis that they are constituted by contributions from more than one discipline: they "make use of several forms of knowledge." (p.46). If this were the whole story, we might think that his 'Type I' and 'Type II' forms were to be equated with 'simple forms' and 'complex/compound forms' respectively (similarly in some ways to Locke's 'simple ideas' and 'complex ideas'); in which case some of the other things he says, e.g. that "... the major forms of knowledge, or

disciplines, can each be distinguished by their [sic] dependence on some particular kind of test against experience for their distinctive expressions", would be at variance with this particular doctrine. Either each major 'form' does have its 'distinctive expression', or it does not. What the 'Type I/'Type II' dichotomy suggests is that some 'forms' are distinguishable by their 'distinctive expressions', while others are distinguishable by having mixtures of 'expressions'. Hirst is far from clear on this issue. Furthermore, and neglecting several other points of possible confusion, Hirst also talks of "second order forms of knowledge which are dependent for their existence on the other primary areas." (p.46). It might be thought that these would (or could) fall into the classification as 'Type II forms' — but Hirst explicitly restricts Type II to 'fields of knowledge' defined in terms of 'unity of subject-matter'; and he talks of the 'second order forms' as being distinct from the 'fields'. In his 1963 paper Hirst had emphasized the 'second order' and 'spectatorial' nature of philosophy as he sees it, and he may have felt constrained to be self-consistent and incorporate that characterization in his present work. It is significant from several points of view, I suggest, that his 'second order forms' all have to do, in one way or another, with language. Hirst himself says that "... there are second order forms of knowledge which are dependent for their existence on the other primary areas. On the one hand there are the essentially scientific studies of language and symbolism as in grammar and philosophy. On the other hand there are the logical and philosophical studies of meaning and justification." (p.48). These 'second order forms' do not, in short, fall into either category in his schema of classification — why, then, did he set this schema up?

I shall offer a tentative answer to this last question, below. Before doing so, however, it seems desirable (for educational purposes if for no other) to point out that, as philosophy, Hirst's paper does not set a very good example. If the simple schema of classification does not work out, why give it? If the relationships between the various 'forms of knowledge' are too complex to be embodied in summary form, why not just say so? That would appear to be better than giving a 'summary' which is in fact at variance with what it purports to summarize. It seems to me to be possible to give a summary of what Hirst actually argues, even in tabular or graphic form — but it would have to be a different summary from the one given by Hirst himself.

Several features of Hirst's presentation can be understood, I

suggest, if we expand our range of considerations beyond what many people would take as 'philosophical' in a strict or narrow sense, and think of the personal and social background to his paper.

It is a standard technique of pedagogy to summarize what has been said, at the end of a lesson and/or before moving on to a next major stage of argument. Hirst has been a teacher — and of mathematics, too — and on both counts could be expected to want to provide a neat 'rounding off' to the discussion he has offered; especially when the discussion has in fact been both abstract, and fairly complicated. Also, Hirst like Peters learned his philosophy from the Oxford sources of 'linguistic analysis'; and 'labelling' is, as was mentioned in Chapter 4, one of the characteristics of Oxford-type lingphil. It can be useful as an educational and/or mnemonic device but, as was argued earlier, it is useful as such only when it comes subsequently to, and as an accurate summation of, complicated and competent discussion of the substantive issues. The discussions in question (i.e. Hirst's paper) are hardly complicated enough to need labelling or summary — not, anyway, for anyone having reasonable experience in philosophy — but presumably Hirst assumes a relatively low level of philosophical sophistication in his readers.

Another point which I at least find interesting and possibly significant is his use of the word 'form'. I shall argue, below, that Hirst's case could be made more effectively if put into terms of language-game philosophy. Now, while one can hardly suggest that he ought to have done this, it does seem to be not unreasonable to question his picking on this particular word, 'form'. After all, there are several other words which appear to be at least equally suitable for the function he seems to require. He himself mentions the Kantian 'categories'; and there are various other terms which could have served roughly the same functions, e.g. 'division', 'domain', 'type', 'order', 'class', 'province', and so on. But why 'form'? For its connotations of Platonism and the classical tradition? These connotations could be expected to have a tendency to elicit attention and respect, especially perhaps from persons having only a slight acquaintance with the actual works of Plato. And what about the similarity between 'form' and 'formal' — as in 'formal logic', about which most educated persons know relatively little except that it is excessively difficult and excessively important?

Examination of Hirst's use of the two concepts 'form' (or 'formal') and 'logic' can lead to an enhanced understanding of his methodological position.

He states that "... the form has a distinctive logical structure" (p.44). By this he seems to mean that each of the several 'forms' has its own peculiar and unique logical structure. But what can he mean by 'logical structure' in this context? If this were to imply that there are differences in the formal logic as this is presented and discussed in widely-used textbooks of formal logic, e.g. Copi 1972, as between the different 'forms' or disciplines, then his assertion would appear to be false. Insofar as the arguments of different disciplines have been formalized, there appears to be no significant difference between them in the formal logic that is used. There are, of course, a diversity of formal logics, and some, e.g. Aristotelian syllogistic logic, prove to be unhandy in attempting to deal with complex arguments. But the arguments of any discipline, insofar as they are purely formal, can be dealt with by, say, one of the propositional calculi, and can even be cast into symbolic form using the standard 'operators' of the Russellian or Łukasiewicz systems, for example, such as conjunction, disjunction, material implication, and so on. Valid argument is valid argument, irrespective of the discipline in which it occurs; and invalid is invalid. This applies, of course, only to argument once it has been formalized — the big questions arise, and the differences between disciplines occur, at the stage of translation from informal to formal. But to recognize this is to recognize, not that the disciplines are formally different, but that they are formally the same, in the sense that, given argument from any discipline, it can be translated into symbolic form using, say, the Russellian propositional calculus, and tested for validity, using one of the standard techniques for doing so (e.g. Quine's 1964 Full Sweep or Fell Swoop). Differences between disciplines (and I am not committed to saying that each discipline is unique) occur not at the level of the formal logic that they use, (since they can all use the same, though they often use none), but rather at the 'informal' level: the sorts of questions that are felt to be appropriate, the types of criteria that are held to be significant, and so on. This is the sort of level at which Kuhn's concept of the 'paradigm' applies; and it appears to be widely accepted that paradigms change not only within a discipline during its historical development, but also are different between disciplines. If this is what Hirst is really pointing to — and various of his other statements suggest that it is — then one can only remark that his formulations in terms of 'formal structures', 'forms' etc. are mistaken and misleading.

Perhaps his emphasis on 'forms', the formal and the abstract, is an

attempt to preserve the exclusivism and 'second order' character of philosophy yet still make it relevant to the various other academic disciplines, to education and to ordinary real life.

Hirst's view that each 'form' or discipline has its own unique 'logical structure' is difficult to sustain, then, if this is taken to mean that a different kind of formal logic is used in each one. If, however, 'logical structure' refers not to formal logic but rather to the 'logic of language' as this phrase would be understood, in one way or another, by avowed adherents of 'linguistic philosophy' (see, for example, the volumes Logic and Language I and II, ed. Flew 1951, 1953), then what he says can be seen as sensible and perceptive. A number of quotations can be given which support this view, e.g. where he says:

"In a given form of knowledge ... concepts that denote, if perhaps in a very complex way, certain aspects of experience, form a network of possible relationships in which experience can be understood. As a result the form has a distinctive logical structure. For example, the terms and statements of mechanics can be meaningfully related in certain strictly limited ways only, and the same is true of historical explanation."

(p.44). My emphasis added.

"Each form ... has distinctive expressions that are testable against experience in accordance with particular criteria that are peculiar to the form."

(ibid.) Emphasis added.

"The forms have developed particular techniques and skills for exploring experience and testing their distinctive expressions ... The result has been the amassing of all the symbolically expressed knowledge that we now have in the arts and the sciences." (ibid.)

These passages, and others similar, suggest that what Hirst is really talking about in terms of 'logical structure' is not formal logic, but is very close to, if not identical with, what Kuhn, for example, is talking about when he uses phrases like 'the structure of science', 'the structure of scientific revolutions'. Both Hirst and Kuhn can be assumed to have been influenced, directly and/or indirectly, by Wittgenstein; and it seems to be a possibility worth exploring, whether Hirst's position could be more effectively presented if framed in terms of a language-game approach.

Each of the quotations above, short though they are, exemplify Hirst's high-sounding and impressive and delicately vague discourse.

"Each form", he says, "has distinctive expressions" — I am adding emphasis to each word which is significantly ambiguous: presumably by

'expressions' he means 'statements', but he could be referring to the facial demeanor proper to the custodians/practitioners of the 'forms', or to whatever Ayer (1936) meant by his 'expressivism' — "that are testable against experience ...". All of this could perhaps mean something different: but it seems to me to be very much compatible with the language-game approach as exemplified say in Chapter 6 above. We tested "Grass is green" against experience — or rather, we said that sometimes, when in daylight we look (having no reason to believe our eyes to be defective) and see the grass and that it is green, and say "The grass is green", we will not be found to have been mistaken. This l-g approach may seem longer-winded, but it is clearer than Hirst's: readers can end up knowing more precisely what is involved. It relates directly to ordinary life, and to the language-games of ordinary life which we all know, to a fair extent at least, how to cope with. It also relates, intimately and necessarily, to the acquisition of language and of the skills of everyday living (see Chap. 6 again). In this connection, Hirst says:

"A form of knowledge is ... a distinct way in which our experience becomes structured round the use of accepted public symbols. The symbols thus having public meaning, their use is in some way testable against experience ... The various forms of knowledge can be seen in low level developments within the common area of our knowledge of the everyday world. From this there branch out the developed forms which, taking certain elements in our common knowledge as a basis, have grown in distinctive ways." (p.44).

Insofar as Hirst is talking here about the everyday experience of every person — and it appears that he is — it must seem odd, indeed perhaps untrue, to say, as he does, that our experience "becomes structured round the use of accepted public symbols". For most people, the use of what we would commonly call 'symbols' is something we learn, often somewhat traumatically, in our middle and later childhood: mathematics, music and so on. And these symbols are not involved in structuring very much of our experience, if any, for most of us. But what Hirst obviously means is that we learn language or, more accurately, the use of language, or, more accurately still, language-in-use, i.e. language-games. To equate learning language-in-use with 'learning symbols' is misleading in a number of ways, but the details of all this are dealt with in various other parts of this thesis and will not be reiterated at this stage. One positive point which he does appear to make, viz. that our knowledge of the 'forms' develops out of and on the basis of our knowledge of the things of everyday life,

would be exactly the same, if my interpretation is correct, as my point in Chapter 5 above about the 'oceanic' nature of language-games: that they all start with the earliest learnings of the infant and are all dependent for their genesis upon the l-gs of everyday life. Hirst does not take the idea so far, of course, and does not make it explicit in the way I have attempted; but it does seem to be much the same idea. He seems also to recognize the importance of what Wittgenstein called 'ostensive learning', though he presents the notion in an unclear and somewhat misleading way.

"All knowledge involves the use of symbols [= words?] and the making of judgements in ways that cannot be expressed in words and can only be learnt in a tradition. ... all of these activities ... are not in themselves communicable simply by words." (p.45).

Now while it is true that one can learn one's own language, i.e. become a 'native speaker' of it, only from persons (usually parents, siblings and other relatives, and neighbours, in the first instance) who are themselves native speakers, and in that sense one must fit into a 'tradition', Hirst seems to overstate his case, and gives it a bias ^{towards} conformism, when he talks about knowledge being learnt 'in a tradition' and that it "must be learnt from a master on the job" (p.45). Most things have to be learned from a person who knows them or can do them (unless one 'learns for oneself'), but to call everyone who can do or who knows things, everyone from whom one learns, a "master", is to be committed to calling 'master' a very large proportion of one's acquaintances. In particular, everyone who participates in teaching basic language-games to young children would have to be called 'master' — and though some of the elder siblings might be gratified by such a practice, one of the connotations of 'master', that the person concerned should exhibit a high degree of competence, would frequently be violated. The learning of language-games has been sketched elsewhere in this thesis; and one important point has been touched upon, at least by implication: that children/people learn new language-games from lots of different other people, that their learnings are widely varied, and that 'learning' in this context does not mean 'performing an exact replication'. The word 'not' in the previous sentence points to two very important features of language. There is the general 'creative' aspect of language-use, which has been remarked upon by Hockett (1960) and many other students of scientific linguistics. Associated with this, but logically distinct from it, is the possibility and the actual

occurrence of 'non-conformism' in language-games. For example, a small girl I used to know, who already displayed a marked individuality at the age of about one, used to say "Bockle" instead of "Bottle" when referring to her young brother's feeding-bottle. She had never talked 'baby-talk' (and had never used a baby's bottle herself, going straight from her mother's breast to drinking from a cup), and her speech was normally faultless. Yet despite several attempts at correction, and despite the fact that she could say other words like 'battle', she persisted, amiably, in saying "Bockle". Everyone knew perfectly well what she meant; and luckily her parents came to see, at the time and even more clearly in hindsight, that this was simultaneously, for the little girl herself, both a joke and an assertion of individuality. Not that she was sophisticated enough to explain it as such, at the time — but it quickly took its place in a total behaviour pattern that included, on occasion, affectionate teasing of her parents and others. She grew up to conform, in general, to most of what are called the 'social norms', but not because conformity was 'required' but because it was the intelligent thing to do.

This brings us to another flaw in Peters' and Hirst's approach to education as revealed by their Educational Philosophy. Their authoritarianism tends to show. You don't need a 'master' to learn most language-games, nor even most academic 'disciplines' (despite that word): you just need other people. Obviously, if some specialist and esoteric language-game is in question, the persons from whom one is to learn the language-game must themselves know it. But as argued elsewhere in this thesis (especially Chapter 5), very few l-gs can be known completely. Most of them are open-ended, susceptible to extension and change in one way or another. With regard to a particular specialist l-g some people may wish to call a person who can teach that l-g a 'master' of it (why not 'mistress'?) — but this title can connote only competence in, not complete or exhaustive or definitive knowledge of, the language-game.

But Hirst's use of the term 'master' might be taken, not as a tacit claim to 'complete knowledge', but rather as an indication that a person who is to teach the language-games of a 'form' of knowledge must be invested by the society (or the societal sub-group concerned with the discipline in question) with authority to perform such teaching. Insofar as this authority is distinct from the 'intellectual' function of simply knowing the l-g, i.e. insofar as Hirst (like Peters and others) feels that it is necessary for the teacher to have social as well as intellectual authority, it appears that he may be assuming that learning language-games (and perhaps a fortiori

learning a 'form' or discipline) would not take place in the absence of authority, social pressures, etc., or (to put the matter in extreme form) that 'pupils' are not naturally inclined to learn new language-games (or to learn language-games at all) in the absence of imposed 'authority'. In short, Hirst and others sometimes appear to assume, tacitly, that learning is in some way unnatural, that if it is to occur at all it must be made to occur. Any assumption along these lines is unwarranted. All human beings have a natural tendency towards the learning of language and a great variety of other things. (I have discussed some of the evolutionary and adaptive aspects of this in Stenhouse 1974 especially Chapters 11, 12, and Appendix, and 1977a and b.) The fact that much formal education has the effect of inhibiting children's natural tendencies towards learning, co-operation and a variety of other valuable behaviours is certainly a cause for concern (as Passmore 1967 points out). Philosophers of education might well give attention, in co-operation with people qualified in appropriate other disciplines, and with parents and practical teachers, to the question of how to educate in accordance with, rather than at cross-purposes to, the natural inclinations of pupils and teachers. (This is a major question to which Dewey addresses himself. His own writings can still provide useful guidance today, despite that he has been made unpopular as a result of the distortions propagated by his followers; and of course we now have the benefit of a half-century of further research, notably the growing researches in human ethology. We are now in a position to take up some of the Deweyan lines again, and develop them on an immensely extended basis. But success in such a complex venture will demand multi-disciplinary education of those involved, see Chapter 7.)

If Hirst is assuming that education and learning need 'authority' and perhaps some form of coercion to make them happen, one can only remark that his own experience of education and of young children must have been limited and/or unfortunate. The whole question of 'authority' in education and in child-rearing is extremely complex, and I have myself argued a need for 'social authority' in relation to some of the specifics of education (Stenhouse 1977b and in press) — my intention in the present context is the limited one of pointing out what seems to be an assumption behind Hirst's discussion, viz. that children will not learn unless made to, and offering a limited contradiction of this view: my positive assertion is that quite a lot of learning does and will always occur spontaneously and endogenously.

Hirst reveals his basic conservatism fairly explicitly in some of

his statements. In one of the quotations already given, above, he talks of the 'forms' involving the structuring of experience "round the use of accepted public symbols" (my emphasis added). If this were to be taken strictly, it would render problematical the status of e.g. technical innovations in music such as the use of the Moog synthesizer, 'creative' poetry and other forms of writing, and scientific revolutions. All of these could be said to involve the public use of 'symbols' (stretching this term to include language, musical notes, etc.) — but the whole point of distinguishing scientific revolutions, creative writing, and so on, is that the 'symbols' etc. are not as yet accepted. If the revolution is successful, if the innovation 'takes on', then whatever is involved can be said to be accepted (with the various provisos mentioned elsewhere in this ~~thesis~~, and by Kuhn (1962) and others). But when the innovation is first proposed it is, more or less by definition, not accepted. Further, some innovations involve what are completely new symbols in a quite literal sense, e.g. symbols such as that for 'material implication', thus: $\supset$, first used by Peano (1894).^x

Not all innovations involve the use of new symbols (in the strict sense); but if Hirst's use of 'symbols' is taken to include the use of language, as suggested earlier, then again he runs into difficulties if he attempts to reconcile statements like that quoted above, with the occurrence of innovations and 'revolutionary' changes. New individual words may or may not be involved — but new combinations of words, and new meanings for old words, certainly are. And any innovation or revolutionary change is going to run into the problem of exploring the possible meanings of the new formulations (see Chapter 8). Historically, it is quite standard for innovations to be regarded as incomprehensible or even definitely meaningless. As examples, we can cite Kant's Critique of Pure Reason, the early paintings of Whistler and Picasso, various works of Beethoven, and so on. Non-comprehension is, I repeat, quite standard. This is in flat contradiction to Hirst's statement about the use of "accepted" symbols/statements.

One last point of criticism, this time regarding the notion of a 'form' or 'field' of knowledge as being a 'discipline'. "... We refer to them as disciplines", says Hirst, "... because the forms require particular training of this kind [He had earlier said that knowledge of a 'form' "...

^x I am indebted to Professor Max Cresswell, Victoria University of Wellington, for this information.

must be learnt from a master on the job."] in distinct worlds of discourse, because they necessitate the development of high critical standards according to complex criteria, because they involve our coming to look at experience in particular ways "... (p.45). It would appear from the foregoing that the 'discipline' would in some way or other be imposed by the 'master', which would of course be true of the early stages of formal education in a particular subject. But it would be far from the whole story even there. Even in the early stages, the 'master' is subject to constraints of several kinds, and the pupil is subject to similar constraints which are additional to and to some extent independent of the 'discipline' imposed by the master. Hirst gives little clear indication of the nature of these wider constraints which actually constitute the 'discipline' as it affects all its practitioners, 'masters' and independent adult workers within it, as well as pupils. What he does say is not necessarily inconsistent with an adequate account of the nature of academic/intellectual/craft and other disciplines — but he himself does not furnish such an account, and what he does say gives a biased view. While it would be inappropriate to attempt in the present context to give a comprehensive account of what is really involved in the concept of a 'discipline', it is both possible and desirable, I believe, to give brief indication of two major dimensions in which Hirst's presentation is inadequate and/or misleading.

In the first place, Hirst fails to accord due weight to the constraints of what may, for convenience, simply be denoted as 'objective reality'. Such constraints are more readily perceivable in some disciplines than in others, e.g. they are peculiarly obvious and important in the sciences (and are often taken to be more straightforward, in the sciences, than they really are) — but they are also relevant, though in more complex and subtle ways perhaps, in other fields as well. Hirst's appeal to the phenomenalist term 'experience', while it goes some way towards acknowledgement of the complexities of even the most simple transactions with 'objective reality', omits any indication of some of the most basic constraints. Recognition within the last few decades of the ubiquity of what might be called a 'subjective' element in even the most down-to-earth of the sciences does not lead to the abolition of 'objective' constraints. Although Toulmin, Kuhn and others have shown that what we observe in 'objective reality' is heavily conditioned by, indeed to a considerable extent determined by, what we bring to our observing in terms of the Common Assumption Paradigms in which we are used to working, it does not follow

that any observation at all, without constraint, is possible. In the simple example of the 'gestalt switch' between seeing the picture as either two faces in profile, or as a vase, we can see one or the other — but we cannot see the picture as being that of the Statue of Liberty, or a square-rigged ship in full sail, etc.. Kuhn's (1962) remark, cited earlier, that all seriously-considered scientific theories fit the facts, "but only more or less", does not give licence that any theoretical statement that might be made would also fit the facts. Some fit less well than others; and some not at all. (We would not normally think of such statements as being part of science, if they literally had no 'fit with the facts'.) Both 'master' and pupil, teacher and taught, then, are subject to the constraint of 'fitting the facts' — and it is in principle possible that the pupil's statement may fit the facts better than that of the teacher. We would no doubt expect, and hope, that this would be of relatively infrequent occurrence; nevertheless we have to recognize that it is possible. (And we ought to be prepared to set up institutional procedures to cope, practically, with this possibility; see Chapter 7, above.)

A second and different type of constraint, which seems to be apprehended to some extent, but misunderstood and misrepresented, by Hirst, is that of the 'social pressures' exerted upon each other by the practitioners of a discipline. The picture emerging from what Hirst says is that the mistakes of the juniors and the incompetents within a 'form' or discipline will be corrected by the more experienced senior people, the 'masters'. While Hirst does not specify exactly how the correcting will be done, presumably it will be merely a matter of the senior people telling the others when and where they have gone wrong. (Corporal punishment would presumably be reserved only for the recalcitrant.) It scarcely needs to be pointed out that not only does this picture completely omit recognition of the whole dimension of 'revolutionary' change in the Kuhnian sense within the disciplines in question, but the possibility of some of the more junior practitioners making factual discoveries and/or technical innovations, let alone interpretational 'gestalt-switches', is completely ignored. Hirst simply does not touch upon the various questions about how new discoveries, ideas, inventions etc. can be communicated, debated, tested, and so on — yet these questions are central to an understanding of how any discipline 'works'. The two central analogies within the concept of a 'discipline' used relative to academic/intellectual/craft activities, viz. the 'discipline of objective reality' on the one hand, and social discipline on the other, are left almost completely obscure. Perhaps Hirst assumes that these matters

are so well understood by his readers that he need not mention them! But in that case one would expect his own **discussion** to be more in accord with the conceptual framework within which the recent and on-going published discussions have been set. As it is, he appears to be unaware of the relevance to his own argument of the work of various contemporary philosophers of science (to name but one type of 'form' or discipline); or else he is aware of all this work but has decided that it is not relevant. In the latter case we are brought back to the point being made in this present paragraph, that Hirst simply does not deal with some of the most basic issues relevant to the case he himself has presented. The notion of a 'discipline' does even on his own showing involve a social context, and interpersonal communication — it "... cannot be done by solitary study ..." (p.45) — yet he neglects to provide adequate explanations of what does actually happen or what could happen in the social interactions which must, in part, be constitutive of any and every discipline. Without attempting to argue out or illustrate my own case at this stage, I shall merely refer the reader to the argument of previous Chapters (especially Chapters 5-8), and suggest that the most immediate and most basic aspect of the 'discipline' of any 'form' of knowledge, for the more mature practitioner just as for the most junior initiatee (or even the young child learning its earliest and simplest language-games), is the reinforcement, positive or negative, of either being understood, or not being understood, respectively, by his/her fellows.

As was pointed out in Chapter 7, the fact that a particular individual, or a particular statement, is not understood by the person(s) within the discipline to whom it was addressed, does not by itself entail that the individual or the statement is wrong. If we see the discourse of the discipline in question (which could be any discipline) as being, not a circumscribed universe with cut-and-dried rules, but rather a series of partially-interrelated language-games in which at least some are 'open-ended' in the sense that a finite set of rules for them has not (or not as yet) been determined, then we can see that the more experienced and more able practitioners of the discipline are more likely to be right, in case of dispute, than those with less ability and experience — but there is always the chance that the junior person, even the pupil, may perceive a possibility that has escaped the 'master'. In short, the picture is both more complex and less authoritarian than Hirst depicts it.

One last weakness in his account may be mentioned. (I mean that

this will be the last weakness I shall mention now. There are others that could be mentioned — but they will have to be dealt with as and when the totality of the work of the London school is re-done on a more satisfactory basis, as is desirable. No doubt this work will occupy a decade or more.) Hirst in his discussion of knowledge and its 'forms' ignores a distinction made long ago in 'pure' philosophy, between 'knowing that' and 'knowing how' (Ryle 1949, Chap.2). It is true that some of what he says would apply equally to both sorts of knowledge — but the 'how'/'that' distinction is itself now widely known among students of Education, and Hirst's failure to acknowledge it cannot but perplex many of his readers. Also, some of what he does say would apply to one sort of knowledge rather than the other, so his omission does lead to some real confusion. For example, he talks of "a form of knowledge" being "structured round the use of accepted public symbols" (my emphasis added), and this would seem to be a matter of 'knowing how'; but on the same page (p.44) he also talks of "... the amassing of all the symbolically expressed knowledge that we now have in the arts and the sciences." (my emphasis, again), and it is difficult to see how knowledge can be 'amassed' except in the 'knowing that' sense. An individual can, of course, learn the 'how' of a number of activities and in that sense could be said to have 'amassed' a certain amount of knowledge (but the various distinct 'knowings how' would in this case be 'amassed' only in the sense that one particular individual had learned how to do them, and could thereafter do any or all of them — there would appear to be no way in which the activities in themselves could be additive and hence be susceptible to 'amassing': knowing how to do algebra, for example, does not seem to be 'additive to' knowing how to play tennis except in the special sense just mentioned.) When Hirst talks of the 'amassing' of knowledge, however, it is clear that he has something more than and different from this in mind. He talks of the "... knowledge that we now have in the arts and the sciences" (my emphasis added) — and the 'we' and the two uses of 'the' indicate a collective and societal not just an individual 'amassing'. The collective knowledge possessed by a society or social group (e.g. the practitioners of a discipline) is usually regarded as being of the 'that' type, in that it is accumulated mainly in the form of written records: books, learned and other journals, and so on. Even if it were stipulated, for the sake of argument, that this societally 'amassed' knowledge is of the 'knowing that' type, however, it is clear that, when the stored knowledge is put to use, it is often 'decoded' into operational terms, i.e. it is transformed into

'knowing how'. How this can be done, and especially how best it can be done, is one of the central problems of education; and one that Hirst does not appear to recognize, far less attempt to solve.

The nature of knowledge, what 'knowledge' can mean, is as both Hirst and Peters (and many other educationalists and educational philosophers) have shown by their concern about it, absolutely basic to our understanding and practice of education; and the distinction between 'knowing that' and 'knowing how' is as Ryle has shown basic within the concept of 'knowledge' itself. As indicated in Chapter 2 above and elsewhere, many of the limitations and distortions of educational practice can be represented as outcomes of failure to differentiate between 'knowing how' and 'knowing that', and especially of teaching on the basis of the latter when what needs to be taught is the former. Over-emphasis on 'knowing that' and neglect of the 'how' dimension has been the bane of formal education for many centuries. Whitehead's criticism of the prevalence of 'inert ideas' in education can be interpreted as but one of a very large number of independent recognitions of this problem. Ryle's distinction enables us to formulate the problem in categories similar to these of information theory (see Ashby 1964 for explanations and references) but in a terminology which has by now become familiar to most educational philosophers and also a great many practising educationalists. We can represent knowledge as always arising from (i.e. originating in) a 'knowing how' situation or, perhaps more accurately, a 'getting to know' situation. Its accumulation and storage at the societal level, however, is usually in 'knowing that' terms (except for activities which are passed on from one generation to the next purely by means of direct participation, as 'traditions of activity' transmitted by 'ostensive teaching and learning' as Wittgenstein would have called it). In modern formal education a vast amount of 'knowing that' is available, stored in the libraries and textbooks. It seems likely that from many points of view, 'knowing that' in itself is literally worthless. The conversion of inert to 'ert' ideas must be, essentially, a matter of converting 'knowings that' to 'knowing how', of decoding the inert ideas that have been amassed by the society into terms of active understanding and, where appropriate, various forms of overt activity: knowing how, and doing ...

With regard to this central activity of education, that of decoding the 'knowings that' into 'knowing how', the account given by Hirst (notably in the paper which has been under discussion, but also in various other

writings) in terms of 'forms' and 'disciplines' can be seen as not only to some extent mistaken and misleading in detail, but also as being largely beside the point, peripheral to the key issues. Much the same could be said, and perhaps with more disapprobation, of the work of Peters: the backward-looking, status quo orientation is even clearer in work which takes 'the educated man', 'authority' and so on, as central concepts. In the educational process of ^{the} decoding of knowledge into activity, it is a central thesis of the present work that a Wittgensteinian 'language-game' approach can provide the necessary methodological leverage, notably perhaps in that it can readily accommodate not only the 'normal' activities of a 'form' or discipline (this is generalizing from Kuhn's concept of 'normal science' and applying it to Hirst's formulation), but also the vital 'revolutionary' dimension as well — which Peters and Hirst and their followers seem totally to ignore. To reiterate a point that has been made several times already, in one form or another, in the foregoing: our present educational policies are heavily biased, in all disciplines, towards the 'normal' — yet the great names in the history books are all 'revolutionaries'. In short, at the level of basic intellectual productivity our education is counter-productive, or at least tends to be. Formal education tends (to at least a significant extent because of its institutional context) to be a matter of 'learning that'. Higher standards, better education, and so on, tend to be a matter of learning more and more of the 'that' that happens to be involved. Students, as a result, know more but can do less. They are less competent at themselves doing the decoding from 'knowing that' to 'knowing how'. To the extent that some of them have been taught Educational Philosophy of the London type, without this providing much of a corrective to the 'knowing that' trend and the concomitant tendency towards rigidification, a less-than-optimal approach (e.g. to 'knowledge' issues) in the teaching of Educational Philosophy would appear to be a contributory factor.

On what basis could I possibly claim that my own teaching of epistemology might be more effective than, say, Hirst's? Any sweeping and categorical claim along these lines would be presumptuous, and is not in fact being made — students can obtain a great deal that is of value from Hirst, Peters, and other London-influenced people — nevertheless I do claim that my own Wittgenstein-influenced approach has a few merits which are at least less obvious in 'London' work.

Firstly, as indicated earlier, I initiate our discussions of 'knowledge' with a sketch of a theory of knowledge arising out of recent work

in science. This draws upon the knowledge and interests many students have already gained from other courses in e.g. Psychology, Physiology, and so on. The integrational effect of this is clear, and must be greater, one would think, than in a Philosophy course which emphasizes its exclusivism, second-order and spectatorial character. A further bonus which is not insignificant is that some students have usually been latently resentful of the science courses they have taken (whether within the Science or the Education Faculty), and they are delighted to find some of the basic methodological limitations of the science-based picture being exposed and discussed. Other students find this disturbing, of course, and they are sometimes moved to reassert, dogmatically, the various particular findings of science — which are not in question, in fact they form the starting-point for the whole exercise — so we have lively discussion, in the course of which I gradually pose the issues in progressively more abstract and general terms. In effect, we move from psychology/physiology to information theory to the Lockean formulation of the problem — which is a re-tracing, in reverse, of the historical development (or part thereof) of this particular field. This again is integrative. Then, as outlined in the early part of this Chapter, we reverse again, and come forward along the track navigated successively by Locke, Hume, Kant and Wittgenstein. Perhaps one could claim that this might give students some insight into what is often called History of Ideas, and to a slight extent into History itself — integration again, if only on a modest scale. (But sometimes a modest scale is more suited to getting students started into a field than a 'proper' and extensive approach would be. In educating as in some other areas, it seems often to be the case that "Small is Beautiful"!)

Secondly, once we have Wittgenstein and the language-game approach we can start to link our philosophical studies with various other fields which the students are likely to have some knowledge and interest in, e.g. child development and the ontogenetic acquisition of language, how to deal with classroom problems like understanding an inarticulate pupil who doesn't really know what question he is trying to ask, and so on. Some of these 'fringe benefits' of the language-game approach have already been indicated in earlier Chapters — but it is only from a narrowly academic viewpoint that they can be regarded as 'fringe'. Even in an introductory course of Edphil, when one is attempting little more than to reveal to the students the ambiguities and 'shiftiness' of language both written and spoken, and

the "dishonest tricks" of argument as Thouless (1930) calls them, the students are learning to cope with, for example, the manipulative attempts of politicians, bureaucrats, advertizing and other people who make their livings by moulding public opinion not necessarily (or even often) to the advantage of the public. The ability and better still the habit of seeing through such manipulations is not 'fringe': it is the very heart of modern democracy. To the extent that our education systems are failing to teach people to cope with the pressures of 'admass' society (in which almost every aspect of everyone's life is seriously affected by 'mass advertizing' and other forms of indoctrination and opinion-moulding (Packard 1957) — J.B. Priestley's term 'admass' sums it up), then to this extent they are failing in one of their most essential tasks — if it is the intention of our education systems to promote the reality of democracy. Certainly the teaching of Educational Philosophy can be used to promote the complex of skills and habits encapsulated in the phrase 'critical thinking' (see Passmore 1967); and especially so if attention is focussed explicitly upon the use of language, the vehicle of indoctrination and mass (and individual) deception. The 'game' component of the l-g concept can help to alert people to the fact that the rules can be changed, and often are, and that we may think we are playing under one set of rules when in fact the 'opposition' may have changed the rules, surreptitiously, to the heads-I-win-tails-you-lose variety. People who are playing fairly have nothing to fear from exposure and discussion of the rules — those who do object to investigation of the rules are likely to have something to hide.

Consideration of Education Philosophy in this context can give us a new perspective, perhaps, on the approach exemplified in the 'London' school. Do they try to explore all the possibilities of meaning — and the possibility that we may be deceived? If not, why not? Do they wish to prevent the rules from being scrutinized? We have touched upon these issues before, and will return to them again. It is worth keeping in mind, throughout.

Additionally to drawing upon the existing interests of students, interests, both 'pro-' and 'contra-', in the academic courses they have already taken and in their various outside interests, teaching educational philosophy as the exploration of actual and possible language-games can provide 'spin-off' benefits, as mentioned elsewhere, in the form of enhanced ability in teaching and for seeing new possibilities for scientific

research. Thus a language-game approach can link formal education to the rest of life, both retro- and pro-actively, and can assist in the quest for 'relevance' which has been in the forefront of so much discussion in recent years. It can also play a substantial part in integration of different disciplines within formal education, as well as integrating formal education with the rest of life.

The 'London' approach to teaching Educational Philosophy, certainly as this is exemplified in the published writings of the various people involved, lends itself all too easily to a 'learning that' approach on the part of students. I have observed this innumerable times, both in academics whose training has been of 'London' type (even if not carried out actually in London), and in students whose background has been provided by such academics. Also, several academic colleagues whose concerns have been to teach their students how to do philosophy have remarked on the 'dead weight' of those with London-type background: learning Educational Philosophy in a London-type regime has been a matter of learning that Peters has said such-and-such, Hirst has said this and that, and so on. The cause of this is easily found: it lies in the way Peters, Hirst and the others themselves do their philosophy. We have already seen (Chapter 4) how in his discussion of the notion of 'equality' in relation to educational opportunity, Peters does little more than make a series of assertions (disguised to some extent by a fair amount of talking round the point, the asseveration of a few platitudes dressed up in 'learned'-seeming locutions) that some statements are 'right', others are 'wrong', that some conclusions follow from the facts (alleged) of our actual usages and others do not, and so on. Lieberman's attempt to grapple with the diversities of possible meanings, possible interpretations, is dismissed as "a sham" — which it emphatically is not. Since Peters so obviously sees the possibilities only in terms of 'his views versus wrong views', it must be very difficult for students being taught and examined by followers of Peters — and there seem to be many who are in a real sense mere followers, who have jumped on the bandwagon and who teach the gospel according to Richard merely because it is the 'dominant' doctrine — to avoid just learning up the doctrines, and performing on a 'knowing that' basis. Often their only choice is between conforming to the 'London orthodoxy', and failing their degrees. Basically, this is because Peters et al. do not present their philosophy as 'doing', but as doctrine.

This situation cannot, of course, continue indefinitely. Already

there are people teaching Educational Philosophy whose own background has been in pure rather than educational philosophy. (As I hinted in 1968, philosophy can be educational only if it is 'pure' — I would want to add, now, "and active"!.) Some of these have no doubt been appointed more or less by accident, though I know of at least one who was not. It will be interesting to see how institutional policies develop over the next few years. On the one hand, the very rapid expansion of universities and other educational agencies which has been the unprecedented feature of the last few decades is now slowing down, may stop, and may even go into reverse. (See my Evolution of Intelligence 1974, Chap. 10 for further discussion of some of these issues.) This would presumably mean that more 'pure' philosophers would be available to teach Edphil. But on the other hand, some of the institutional Establishments in education (and in Education as an academic discipline) are likely to react defensively, and instinctively 'close ranks', endeavouring to keep outsiders out and the available jobs kept for those who have signified their solidarity with the Establishment. Such a policy will in the long run inevitably be fatal. Education in all its forms is ultimately the concern of the whole society, and if those entrusted with providing education fail to do the job adequately, the educational Establishments will be swept away by political and/or other direct action on the part of the community as a whole. The intelligent and far-sighted action for an educational Establishment is, of course, to spare no effort in attempting to obtain the very best personnel for every aspect of the educational enterprise, and in providing them with every encouragement to educate every individual to the very limits of his/her ability — but an Establishment which does in fact implement this sort of policy is no longer an 'Establishment' in the pejorative sense of the term.

For Education, then, the way in which Educational Philosophy is taught, and the types of persons who teach it, are simultaneously major causal influences upon the effectiveness — which is the same as to say: the value — of education as practised as a societal activity, and symptomatic of what the long-term outcomes of that education are likely to be. In the simplest possible terms, if a society in the persons of its pioneering thinkers is willing to explore any and every possibility within the scope of human capability, then it is in a healthy state and its chance of survival and further progress is good. To the extent that exploration of possibilities is restricted, on the other hand, the actualization of

individual potentialities is needlessly limited and the society's adaptive ability is likely to be seriously impaired. It is against such considerations as these that we must evaluate our methods of teaching, and of doing, philosophy. Peters, in his chapter on "Equality" which we examined in Chapter 4 above, showed not only that he is himself disinclined to look beyond what he thinks are the standard or 'correct' usages, but also that he reacts aggressively against anyone else doing so, e.g. Lieberman. In short, exploring possibilities is what Peters does not want to do. Perhaps we could say — and not merely ad hominem — that Peters wants to take the 'spectatorial' role which he and Hirst and others wish to foist upon philosophers, quite literally. Perhaps what he really hankers for is the spectator's role of god-like omniscience, of being able to see all the mistakes of the players, the referees, the coaches, the other spectators, and to shout admonitions and instructions to everyone. This is common in every spectator-sport. We have all suffered from the know-all who keeps up a running commentary on the game for everyone within earshot. This is bad enough when he gets it right, when his comments are in fact perceptive and illuminating — how much worse when he gets it all wrong! But of course the people who do this sort of thing could never admit the possibility that they might perhaps be wrong. Oh no! The Archie Bunkers in the spectator-stands are never wrong. They could never be convinced of having made a mistake; and anyone who tries to convince them is simply demonstrating his animosity and/or ignorance, and deserves to be shouted down. Everything that happens, no matter what, is bound to reinforce the self-superiority of the Know-all Spectator. Like the paranoid who thinks he is being spied upon, the absence of evidence of spying merely proves how cunning and efficient the spies are. In short, spectatorialism carried to an extreme becomes a form of paranoia.

This sketch of a common spectator-type does not apply to all spectators. Most of us confine our overt vocal utterances to the climactic moments of the game; or at least restrain ourselves to muttering to our friends in the neighbouring seats. Also, some individuals are in fact perceptive with regard to the activities, personalities, tactics and strategy, etc., of the drama being enacted by the players, and their comments can add a valuable dimension to the spectatorial experience — witness the many spectators who plug in to their transistor radios to pick up the broadcast commentary while they watch. (I remember an incident that was instructive and satisfying to me, at the Milton tennis courts, Brisbane, about 1964. Rosewall was quietly demolishing Rod Laver, 'Grand Slam' world amateur

champion the previous year. Half-way through the second set Rosewall gave away an easy point; and from behind me, and a fraction of a second after my own unspoken comment, Ken Fletcher, then ranked about fifth in the world's amateurs, remarked to the girl beside him: "That's his first mistake in this match, and it may be the only one!") But the spectatorial role is a seductive and can be a corrupting one. The spectator is not put to the test of events in the way that the player is. True, his predictions may be proved wrong — but then he can always blame that on the players, the referee, etc., or trot out some other rationalization. It is very easy to drift into the illusion of omniscience and omnipotence — and no doubt that is, subjectively, a happy state to be in. Just so long as the pleasant spectatorial illusion is not imperilled by an impulse to step out onto the court or the playing-field: to take one's place among those who actually play might change one's views, and would certainly destroy any ideas of personal omnicompetence.

Sports and games have become one of the major features of the twentieth century, along with technological expansion, urbanization, the population explosion, large-scale wars, and so on. But while the numbers of people who actively participate in sports and games has increased, a very much greater increase has occurred in the number of people who only watch (and/or listen). The spectator explosion has been very much greater than the player explosion. Hence the instant popularity of the notion of philosophy and Edphil being 'spectatorial'. Peters, Hirst and their followers have cashed in on one aspect of a massive trend of interest. Spectating is something that anyone can do, and everyone does. Not only that, but it is easy. Also, most people find it enjoyable. So, by the association of ideas, but without the connections being made to explicit — for that might puncture some illusions — it comes to be thought that Educational Philosophy as exemplified by the London school is something that can be taught, pretty easily, to almost everyone. Hence the rapid proliferation of London-style textbooks, cashing in on the recently-created bandwagon ... We can emphasize 'rapid' and 'cashing in' in the previous sentence. Many of the books in question contain the same bits of material, re-used time and again. Enormous mileage has been wrung from webs of argument which were pretty thin and threadbare in the first place. We have seen, in other parts of this thesis, just how deceptively shoddy much even of the central arguments really are. Nothing could be more basic to the concept of 'education' as used currently in Western societies than the concept of 'knowledge';

no issue has received greater public attention this century, among the 'developed' nations at least, than that of 'equality' — and we have seen what sort of philosophical treatment the two leaders of the London school of Edphil, Peters and Hirst respectively, have accorded these concepts. I cannot restrain myself from remarking, in this context, on an exchange which took place some years ago in the Australian Journal of Education. John Powell (1965) wrote a paper criticizing much that had been published up to that time under the rubric of Edphil, as being not philosophy. He was replied to the following year by Ivan Snook (1966), who said, in effect, that various of the publications to which Powell had objected could not be excluded from the classification 'philosophy', but would have to be accepted, rather, as being bad philosophy. (It seems doubtful that Snook would have wanted this conclusion to be stated explicitly; nevertheless this was the conclusion to which his argument pointed.) Similarly in the present case, one is tempted to suggest that, on some criteria, various of the 'London'-type publications should not be regarded as philosophy — but taking a line similar to Snook's, perhaps we must limit ourselves to deploring the relatively poor quality of some of the London Edphil.

One proviso, of considerable significance, can be made: that some of the work produced by the less eminent members of the London school — and much work which is not London-influenced at all — is of better quality than that of the 'leaders'.

In this connection, however, it would need to be pointed out that this better quality philosophy has mostly confined itself within the conceptual framework set up by the 'leaders', and is thus 'normal philosophy' as distinct from 'revolutionary philosophy' in the sense discussed earlier (see Chapter 8, above). Thus, although the better Edphil is often more competent in the detail of the arguments offered, if the 'framework' or paradigm (Common Assumptions Paradigm) within which it is set is itself defective or systematically distorted, then the basic distortions are going to limit the value of even the competent 'normal' philosophizing which is done within this paradigm. To mention but a few examples, we have seen competent and fairly exhaustive discussions of the concept of 'needs', of the 'task'/'achievement' distinction, and of the distinctions between 'education', 'training', 'indoctrination' and so on, all published in the last decade (refs in Powell 1974, current journals). The various papers have in general been competent and percipient examples of philosophy — better, in many cases, than some of the publications of the eminenti — but while

many persons who are concerned in one way or another with and about education have felt that all of this work does offer something, some enhancement of understanding, some clarification of issues that are to some extent relevant to what they feel is important, there is nevertheless a widespread conviction that Educational Philosophy under the London influence has drifted away from issues which are central to education as a social and personal activity and which should, therefore, be central to Education as an academic discipline.

Take the complex of 'education'/'training'/'indoctrination', for example. It is undoubtedly of concern to everyone connected in any way with education to have a rough idea of the differentiation of these and other related concepts. But the drawing of finer and finer distinctions, past a certain point (which would be difficult to characterize, but is readily perceivable in actual discussion), has rapidly decreasing marginal utility. The attempt to continue the dissection of the logic of these concepts is then likely to be dismissed as mere virtuosity, and as 'academic' in the pejorative sense. To what extent are Educational Philosophers justified in ignoring such reactions on the part of other educationalists, also teachers, students, parents, and the community generally? Perhaps it could be said that, purely for their own interest (and entertainment?), they are quite entitled to exercise their analytical talents on anything they please; but that to the extent that their efforts are intended to contribute positively to the total enterprise of education, then their efforts would be more usefully employed on a very much wider front than at present. Wider not only in the sense of covering a greater number of concepts but also, notably, in being orientated especially towards relationships between concepts, (e.g. the relationship between 'authority' and 'learning' in a classroom context), and in relating theoretical issues with real-life practicalities.

Behind the three concepts mentioned above there lie related concepts of 'control', 'discipline', questions as to the relationship between the social control exerted by the teacher in the classroom and the cognitive/'learning' outcomes of the teaching, and so on. These issues cannot be settled by abstract discussion. Nor can they be settled — not at the present time, anyway — by appeal to what might be thought to be the appropriate behavioural/social sciences. The empirical-level evidence that is available at present has only limited usefulness. The basic reason for this is that the current paradigms in most branches of the relevant sciences are

couched in terms of behavioural categories which are inappropriate, non-illuminating or at least not illuminating enough. The behavioural/social sciences are at present embarrassed by a multiplicity of paradigms, many of them pretty much at the ad hoc level, or little above. There are plenty of signs that this multi-paradigm situation is reaching crisis-point (see Palermo 1971, Piaget 1973); and presumably a 'revolutionary' unification is in the offing. Some of the functions of 'active philosophy' in such a unification have been touched upon elsewhere in this thesis: pluralities of meanings and criteria demand philosophers' skills for the sorting out, unification demands inter-disciplinary translation, and so on. But achieving the appropriate, the illuminating and true-to-life categorization within, say, the behavioural sciences, cannot be only a matter of abstract explorations of language-games, important though such exploration undoubtedly is. There is a sense in which it is not so much the exploring of the language-game which is in question (though the exploring is in question), but rather the setting up of a language-game (or games) in the first place, de novo. Perhaps we can put it this way: if our present categorizations are inadequate, then we must formulate new ones — but how? Where can we get them from? How can we generate them?

If we continue to think of the behavioural sciences for our examples, it is possible to give a general answer to these questions. The general answer involves relating our conceptual systems to the 'objective systems' within which we live, both types of system being included within the extended concept of the language-game which we have been developing.

Niko Tinbergen, one of the pioneers of the new science of Ethology (who shared one of the 1973 Nobel Prizes in recognition of his work), has always emphasized the importance indeed the necessity of simply observing the behaviour of any species being studied, before any attempt is made to experiment and manipulate the behaviour, quantify it and explain it and so on. But his insistence upon observation has been misinterpreted as being nothing more than a demand for description: the behaviour must be described before it is manipulated. This misses the point of Tinbergen's original (1951) exhortation about merely observing (see his Introduction to the 1969 re-issue of The Study of Instinct — but even this does not, I feel, make the issue as clear as it should be). Consider a researcher going out to investigate the behaviour of a new species, one whose behaviour has never before been studied or described. He carries into the field with him, assuming he has had the proper academic training for research, a knowledge

of the various categorizations which have been found appropriate in the past study of the behaviour of other species. If he sets out to describe the behaviour of the new species, he is likely to do so in terms of the already-known categories. Many of these are quite likely to be appropriate for the behaviour of the new species — but there is no guarantee that all will be; and in fact, unless the habitat, phylogeny and lifestyle of the new species is closely similar to that of species already adequately described, the chances are that at least some of the behavioural categorizations will not be fully appropriate. In any case, it cannot be known in advance that they will be. This being so, if the description of the behaviour which eventually results is to do justice to the realities of the actual behaviour in all the complexity of its causal, functional and adaptive interrelationships within the individual, between individuals, and between individual and environment, the researcher must match his descriptive categorizations to the dimensions that are significant to the species in question. The only way in which he can do this, and avoid forcing the old categories willy nilly into the descriptions of the new behaviour, is to go out into the field with his mind cleared, as far as possible, of the 'established' categories, and simply allow the raw reality of the new behaviour to impinge upon his consciousness, probably for a considerable period of time, until gradually it becomes apparent to him, as human observer, which are the behavioural categories which are significant to the species itself. It may be noted that, while extensive observation of the natural behaviour (or, more properly, extensive experiencing of the behaviour; ^{of categorization which is to be avoided}) ^{should eventuate} 'observation' tends to connote the activity in descriptions that are 'true to life' in terms of the biological significancies actually involved, any new categorizations which result are likely to arouse criticism from other researchers who continue to use only the established categories. The situation is essentially similar to that which occurs in a scientific revolution — and in a limited way, of course, the new categorizations do in fact constitute a 'revolution', a sort of 'mini-revolution', in that a change has been made in the language-games of the science in question. Just as with major or 'maxi-revolutions' (see Chapters 5-8, above), those who continue to play the old l-gs may be genuinely unable to understand the new categorizations and why they are necessary. (To what extent this may absolve them from trying to understand the new developments is another question — it has always seemed to me that intellectual and scientific integrity demand that one should give a sympathetic reception to any new conceptualization,

any new argument, before one moves into a more critical appraisal. But it seems that many people are unwilling or unable to do this.)

The task of the researcher can be represented, then, as that of finding or inventing a language-game which will prove adequate for describing (and then explaining, predicting, etc.) the behaviour of the species that he is studying. It may be apparent from what has already been said that in this endeavour, as in so many of the investigations of language-games that have already been made, there is no guaranteed recipe for success. There is in fact no recipe at all, let alone a 'guaranteed' one, beyond that of "See what you can say, and then see how what you've said works out..." Beyond this, a few suggestions can be made, e.g. a generalization of the point made above about behaviour research, that insofar as the subject-matter of the investigation is new, some at least of the old categorizations are likely to be inappropriate and will need to be discarded. Therefore all must tentatively be negated — or at least we must be prepared to negate any of them that seem in the slightest way dubious (since it is probably impracticable to negate literally all of them.)

The point just argued, above, can be seen as a particularization and specification of what is meant by the general maxim, to be 'open minded'. In one sense what I have just been doing is an attempt to give an 'operational' or a 'behavioural' re-interpretation of the maxim. The need for open-mindedness is asserted almost universally in textbooks of science education, indeed in a great many texts of general education as well (they still tend to be exhortatory) — but while everyone can agree that open-mindedness is a good thing, it has usually been far from clear what we have all been agreeing about. Now that I am trying to particularize it, no doubt there will be many readers who will exemplify the neophobia I mentioned above, and who will reject outright my offered specification. To those who do, I can only say: Do it better, then!

While it is not my intention to attempt a full-scale investigation of the possible meanings of 'open minded', I do feel inclined to suggest that, since the phrase is so widely used in education, it should have received more attention than it has so far, from those who are professional Philosophers of Education.

Reverting now to the question of how new categorizations of reality are arrived at, how new language-games are generated — and bearing in mind that we are not at present concerned with how any particular formulation

comes to be made, let alone justified, but rather with the general question of how we come to innovate within this dimension at all — perhaps we have now reached a point where we can offer an explanation of the genesis of new language-games.

If we accept the Darwinian Revolution (which many within the present social/behavioural sciences seem reluctant to do), and if we take it as a fact that the species Homo sapiens and its behavioural propensities have been produced by the processes of organic evolution (and we are not thereby committed to claiming that we fully and finally understand these processes as yet), then on general grounds of evolutionary theory we would expect to find, in our total behavioural repertoire, both phylogenetically new behaviours some of which might be unique to the human species, and also various of the phylogenetically older behaviours (or behavioural propensities) which have survived perhaps in modified form from the past history of our species. This 'mosaic' picture of evolutionary change is generally accepted within the biological sciences (see Huxley 1974 for discussion and further refs) and applies to behaviour just as to anatomical or physiological features. At the descriptive level it is often difficult to assign a particular behaviour as either 'old' or 'new' — this difficulty is largely the result of the interaction between old and new, and the generally greater plasticity of behaviour as compared to, say, anatomy. But while the assumption that every behaviour should be classifiable on an exclusive either/or basis as either 'old' or 'new' would be mistaken, it is both possible and desirable that we should be able to discern the various elements and tendencies, old and new, within the total adaptive complexes of human behaviour.

Some of the main behavioural tendencies found throughout the infra-human animals, and which could be expected to be present in humans too, are those towards exploration of the environment, and towards learning whatever specific behaviours are required for the perpetuation of the species (where the latter are not provided for by the interlocking of endogenous 'programming' and particular 'releaser' signals from the environment, other individuals, etc. — i.e. 'instinctive behaviour' in the narrow sense, see Tinbergen 1969). Another major propensity, found in the 'higher' animals especially the mammals with their juvenile dependency period associated with the feeding of the young on milk produced by the mother, is that towards play. Both exploration and play have as their consummatory achievement an accomplishment of learning. In ordinary exploratory behaviour what is learned is usually a topography, of home range, territory, or any place in

which the individual animal finds itself. The adaptive value of 'knowing' the territory — for escaping from predators, finding food, guiding offspring, and so on — is fairly obvious. (Note how 'knowledge' comes in right from the earliest stages of evolution.) In play, what is learned, and its adaptive value, are perhaps at first sight less obvious. They can be summed up as a gestalt: 'learning what are one's individual behavioural capacities in relation to the environment, other individuals, other species, and so on'. If exploration could be said to result (very roughly) in 'knowing that', then in a similar way play can be seen as conducive to improvement in 'knowing how'.

If Chomsky, Lenneberg and others are correct in regarding language-acquisition and -use as being endogenous and in a broad sense 'instinctive' (see Minnis (ed.) 1971 for discussion and refs), then it might be presumed that interaction could occur between the instincts of exploration and of language-use; in which case we would expect to find that people would show an endogenous (spontaneous) tendency towards the exploration of language. There would undoubtedly be great individual variation in this, as in every other human characteristic; and in the case of these particular instincts, there would be a tendency for their interaction to be inherently self-limiting (or, as an alternative formulation, for them to be mutually limiting). The grounds for saying this are, that in most species overt exploration tends to decrease with age, while the 'amount' of linguistic competency tends to increase with age. In brief, and simplistically, we could say that the young individual has a greater inclination to explore language (linguistic usages) but has less at his command to explore; whereas the older individual, though he knows more about linguistic usages, has less inclination to explore them. Clearly, this situation is conducive towards stability of language (especially since the older and more conservative individuals are likely to occupy the higher positions in the social dominance scale, hence be more influential). This hypothetical sketch of the interaction of two instincts, for exploration and language-use, while it is at present being argued only on general theoretical grounds, does seem to accord with observations on the ontogeny of language: innovativeness tends to decrease with age. But the total picture is really much more complicated. A third major instinct is involved: play.

Play is not exclusive to the mammals, but it does reach its most extensive development in this group, for reasons which have been mentioned. (For more comprehensive discussion of various aspects, see Meyer-Holzapfel

1955, Lorenz 1965, Stenhouse 1974, 1977 b.) Its main function, as already indicated, seems to be to utilize the relatively long juvenile dependency period to enable the growing individual to practice and perfect various complex skills of dealing with the environment, other individuals of its own and other species, and so on. Lorenz (1965) has suggested that the especial prominence of play in certain groups of predatory mammals (he mentions Carnivora, but similar considerations would apply to dolphins, etc.) may be due to their need to preserve great flexibility in their hunting activities: pursuit of fleeing prey, whose strategy is often to adopt 'randomized' or 'protean' behaviour (Chance and Jolly 1970), demands that the variability of prey-behaviour be matched by the pursuit behaviour of the predator. Since this pursuit must, to be successful, combine variability with purposiveness (see Pittendrigh 1958), a high degree of skill is necessitated and this puts a premium on practice both 'in earnest' and in play. Since practice in the 'in earnest' situation is not to be had for the asking, as it were, and could also be dangerous, the main emphasis during the juvenile dependency phase must be upon simulated hunting and other activities in the form of play.

Within the mammals, the juvenile dependency period reaches its most extensive development in Homo sapiens (see Bruner 1974 for insightful discussion and further refs). In our own species, then, we should expect to find the longest-continued instinctual motivation towards play and, other things being equal, the greatest elaboration of actual play behaviours. But as soon as we start to consider whether or not these theoretical expectations square with our actual observations and experience, we encounter an interesting tangle of semantic/philosophical problems. The outcomes of play are distinguishable as development (or perhaps it would be safer to say, enhanced development) on the one hand, and on the other, a variety of learnings. From the latter point, many people might be tempted to conclude that play itself is a learned activity. This is not so. (Similarly with regard to exploration: its consummation is learning, but the activity itself is not learned, but instinctual (contra Barnett 1958; see Stenhouse 1974). The activities used in play are largely derived from the culture in which the individual is growing up, but the playing itself is instinctual. Normal children play, regardless of their culture. It is of course possible for children to learn not to play, if their cultural milieu enforces strong enough suppression — but almost any instinctual activity can be subjected to distortion or even suppression, given a sufficient regime of negative reinforcement: the only price that is exacted for this is loss of adaptiveness

of behaviour, i.e. madness, insanity. So long as this extreme is avoided, however, children have strong natural (i.e. instinctual) inclinations towards play, it helps their development, and they learn a great deal from it.

What then about possible interactions between the instinctual drives towards play, towards exploration, and towards language-use?

In general, exploration takes precedence over play. An individual, whether human or infra-human, will not play until after an unfamiliar environment has been explored. We do not play unless we feel secure in our environment (physical and social); and security presupposes exploration. It is of course often difficult, at the observational/descriptive level, to distinguish between exploration and play (see Stenhouse 1977b) — but for our purposes here we do not need to. In relation to language-use (and keeping in mind our present intention of offering an account of how new language-games come to be generated), play appears to have the same disadvantage as was found with exploration: it tends to decrease with age; so that again, just at the time when the individual's mastery of language is approaching a maximum, the tendency towards playing with language, which could result in linguistic innovation, is tending to decline. It might seem, at this point in the argument, that linguistic innovation must be so unlikely as to be almost paradoxical in occurring at all.

The keys to resolving the paradox must be sought in concatenations of several different factors.

In the first place, individual variation and diversity are, as Huxley (1963) and others concerned with human evolution have repeatedly emphasized, the most salient characteristics of our species. Our range of variation is many times greater than that of any other species (except those which have been domesticated, and on these we have imposed variations as great as our own — or should we perhaps say, that we ourselves are the first of the 'domesticated animals'?). Thus while the generalizations, above, about the decrease in exploration and play with increasing age are true enough, as generalizations, there are in fact many individual exceptions. Significant minorities within our human populations continue to explore, both physically and, perhaps more importantly, mentally/intellectually, until relatively late in life. Similarly with play. Some individuals retain the ability to play, even in a literally childish way, and with children, throughout their lives — and of course it is notorious that aged

people move into a 'second childhood'. (One reason why grandparents are valued as baby-carers for their grandchildren: both parties can enjoy themselves, playing together). Thus on general biological grounds we could assert that there must be a minority of persons, perhaps a very small minority but a highly significant one, who combine the instinctual drive to use language and to explore it and/or play with it, until relatively late in life. Other behavioural tendencies come in, too. Habit formation, for example, is important in all sorts of ways, and all three of the instinctual tendencies we have been discussing — language-use, play, and exploration — can if indulged in from the formative early years become habitual; persons can form a habit, or it could even be termed an addiction, specifically of language-exploration and/or language-play. Depending upon the particular type of exploration or play, such people might be classified as poets, or philosophers, or revolutionary scientists, or perhaps even lunatics.

On the question of habit formation especially in relation to social context, the problem must be faced, as to how some individuals can manage to resist the social pressures to proceed in the 'normal' way through all the various stages of 'maturation', whereas the majority of their fellows move away from the childishness of play and the youthful 'excesses' of exploration. The majority of people do conform to the generalizations made earlier about the waning of the instinctual drives towards play and exploration. How then do we account for the exceptions?

Some part is likely to be played, probably in a good many cases, by more or less accidental circumstances. Longish periods of isolation in childhood, e.g. during illness, could lead a person into habits of verbal/intellectual play as a substitute for the more usual physical and social play with other children. Then again, physical handicaps, illness, social circumstances in say later adolescence might leave little outlet for interest and energy except intellectual/linguistic explorations. Once a habit is formed, the individual is likely to try to continue to in later life. The conformity pressures of jobs and careers, and notably of higher education, are likely to militate against continuance of habits of play and exploration, however, except in individuals of highly robust personality; but against this, it must be remembered that a relatively secret habit of writing poetry, novels or social criticism can act as a compensatory activity or safety valve in the midst of a life of severe pressure.

One factor which, as I have argued elsewhere (Stenhouse 1974), seems likely to have an important role in the combination of play and exploration

which contributes to creativity and the genesis of novel language-games, is the P-factor of 'evolutionary intelligence'. This is not the place for an extensive discussion of its large variety of possible effects and the already-extant evidence for them (the reader interested in this topic may be referred to various papers and books supplementary to The Evolution of Intelligence itself, e.g. my 1971, 1977 a and b, several works in press or in preparation), but some brief indications of its potential may be useful at this stage. Its most basic function in the earlier stages of its evolution was as a means of postponing, temporarily inhibiting, the standard instinctual response which would hitherto have been elicited by the given situation. This postponement or 'pause' effect was to provide the opportunity for the other developing factors of nascent intelligence to elaborate an alternative, different, and thus potentially though not necessarily a better adapted response (as compared to the previously-standard instinctual response). Thus even in the early stages of phylogeny the essential function of the P-factor was to facilitate the generation of novelty —at this stage, in 'behaviour' understood in a straightforward physical and observable sense. But P-factor functioning need not be seen as limited to physical behaviour. It can operate also at the intellectual level, and upon learned as well as instinctual activities. I have argued (1971, 1974 and in this thesis) that creativity certainly if of revolutionary type is dependent upon the prior negation of the previously accepted orthodoxy (some part or parts of the old Common Assumptions Paradigm must be negated, rejected, at least provisionally, before new assumptions can be tried out) — and it seems that the inhibitory tendencies of the P-factor of intelligence are likely to be implicated in such negation. But in the terms in which the problem of the generation of novelty has been set in this Chapter, viz. of the operation of instinctual drives towards language-use, towards play, and towards exploration, the P-factor as an inhibitor of instinctual tendencies might appear likely to be counterproductive. That is to say, that if we are advancing an explanation of the genesis of novel language-games in terms of the three instinctual drives already mentioned, then any factor which inhibits instinctual activity should be expected, prima facie, to reduce the generation of novelty. But this is not necessarily so. P-factor inhibition appears to be selective. On different occasions it can operate to inhibit different instincts. Certainly we must recognize that in some persons, on some occasions, it may inhibit play with and/or exploration of language-games. On other occasions, however, and perhaps with other individuals, inhibition due to P-factor may operate not

upon the language-using, play or exploratory drives, but rather upon the instinctual tendencies which might in the given circumstances be antagonistic to them. Thus one of the factors which has already been mentioned as being potentially antagonistic to linguistic creativity (indeed to creativity in general) is social conformism. This has not so far, in the present discussion, been characterized as being a matter of instinctual activity — but it is in fact increasingly recognized as being just this; or perhaps I should say, not as necessarily due solely to instinctual tendencies, but certainly as involving the very large numbers of instinctual transactions, the majority of them operative mainly subliminally, which collectively can be designated as 'social dominance behaviour' (see Hinde 1972, Argyle 1969, for discussion and refs). If we generalize and say that it is the 'social instincts' (the mechanisms involved in social dominance and subordination) which cause social conformity, then it becomes obvious that the instinct-inhibiting function of the P-factor of intelligence can operate upon these instincts rather than the instincts leading to linguistic innovation — in short, the P-factor of intelligence can act in favour of instinctual mechanisms conducive to linguistic innovation, as well as against them.

Provisional adoption of this position, viz. that instinctual proclivities are deeply involved, though probably not in simple form, in even such 'high-level' performances as the generation of new language-games, enables us to bring into a more meaningful relationship (though it does not yet enable us to offer a comprehensive explanation of) various established findings on human creativity. One theoretical puzzle is the combination, in many great creatives, of opposite or contradictory personality traits. For example, many have been known to combine considerable aggressiveness with a mild or indeed bland social conformity. Darwin is, as usual, a useful because extreme example here (see Hudson 1964). The solution to this sort of puzzle lies, it would seem, in the existence of an intelligently-directed mechanism for the selective inhibition of instinctual aggressive tendencies. They are inhibited most of the time in general social intercourse, because real or metaphorical brawling is unproductive and time-wasting; but on occasion, and depending on the context, the astonishing power of an instinctual drive can be unleashed, and redirected into 'higher' functions such as language-game innovation instead of the normal and recognizably 'instinctive' activities. Hudson (1968) and others have talked of highly creative individuals having something like a 'killer instinct', and of their 'predatory' approach to ideas (in general, towards the subject-

matter in which their creativity is expressed). I have suggested elsewhere (Stenhouse 1974) that, while talk of 'predation' may make a good analogy — the high creative is like the predator in many ways: continuously watchful, 'eagle eyed' for the slightest movement of a promising notion, relentless and efficient in swooping upon it and pursuing it through all its twists and turns of ambiguity and unproductive leads, thorough in ingesting every scrap of useful nutriment — the correct homology (as distinct from analogy) is not with predation (which from the predator's viewpoint is simply food-getting behaviour), but rather with aggression. It is of course quite true, as the critics of Lorenz have pointed out with great sound and fury (see Montagu 1968), that there is no one specific 'instinct of aggression' — but this does not mean that human beings are not sometimes/often aggressive, nor that their aggression is not basically instinctual in nature. In other species aggression is often associated with the various forms of reproductive behaviour, e.g. as a component in territoriality (see Hinde 1970); and both aggression and sexuality seem to be frequently associated with human creativity (cf Storr 1972), indeed Freud advanced the view that the whole of human culture and civilization was due to 'sublimated' (i.e. redirected) sexuality (see Wollheim 1971 for an accessible discussion and refs) — though this last notion seems definitely hyperbolic.

The possibility that instinctual and unconscious causal factors may be involved in the genesis of new language-games — and the importance of unconscious factors has been touched upon already, in a number of different connections in this thesis e.g. the unconscious assumptions involved in any C.A. Paradigm, the necessarily past-orientated and hence potentially maladaptive nature of unconscious assumptions in a rapidly changing world — makes it necessary for us to devote some attention to the relationships, actual and possible, between the conscious activities of philosophy and the unconscious (or only partly conscious) activities and processes which go on below, prior to and/or additional to the conscious and explicit ones. An investigation of some of these issues will be carried out in the next Chapter which begins, however, with a reflexive look at what we have actually been doing in the last few Chapters.

CHAPTER TEN

PHILOSOPHY, MYTH, LITERACY ... AND THE UNCONSCIOUS.

This thesis has been arguing for 'inclusiveness' rather than 'exclusiveness' in philosophy, and the latter part of the preceding Chapter provides exemplification of what is involved. It shows some of the problems, too. The discussion of Ethology and Evolution is not likely to be fully intelligible to readers who have no academic background in these fields— but, on the other hand, it is not likely to be totally unintelligible, either. As a case in point, the references to my four-factor theory of 'evolutionary intelligence' are not likely to be fully understood except by those who have read my earlier book The Evolution of Intelligence, in which the theory is expounded, or some of the other publications, e.g. Storr 1974, in which it is discussed— nevertheless, I would predict that most people who have waded through Chapter 9, above, would feel that they have gained some understanding, albeit perhaps rather vague, of the functions of the P-factor of evolutionary intelligence. In general, perhaps it would be safe to say that, unless it is extremely esoteric, and opaque with specialized terminology, most of us can get into a new sort of language-game and, provided we have the time and motivation just to keep on reading it, we can gradually start to see what it is all about, what it is saying. This happens, and can happen, because in the majority even of fairly specialized works, most of the words used are those which are familiar to us from ordinary language; and even when the meanings of some of them have been changed a bit, we can pick up the changes (though not necessarily fully or precisely) by relating the different usages of some words to the unaltered usages of others (cf Chapter 5 on the 'oceanic' nature of language-games, also the 'possible meanings' model in Chapter 8). This is, of course, the basis on which we can say of some persons that they are 'self-educated'. In one sense it is logically impossible for anyone to be, literally, self-educated (see Ryle 1967); but what is normally meant is merely that the person in question has not gone through the various processes of formal education (and the associated certifying), but instead has read widely or in special areas and has gradually worked out the meanings of the new words, and the new meanings of the old words, on his/her own, without the help of a teacher. In many subjects it is quite possible to do this. The two main drawbacks are, that self-education usually takes far longer than assisted education, largely

because, secondly, there is nobody to confirm our correct statements or learnings and to correct our mistakes. Immediacy of feedback is very important indeed in education.

(As a self-taught tennis player I am sharply aware of the benefits and limitations of self-education. One can know what one should be doing; one can know that one is not doing it properly; but just precisely what one is doing wrong often takes months, or even years, to work out. A competent tennis coach can diagnose mistakes in (usually) a few minutes. Thus the self-taught tennis-player achieves technical competence just when his physical ability actually to play a good game is beginning to disappear: technical advance is beaten by the erosion due to advancing senility. But I think one's understanding of the game may be better than that of even international-level 'natural' players who have had the benefits of coaching, if only because one has had to do so much more thinking.)

Coming now to attempt some consideration of the relationships between the explicit and essentially conscious activities of philosophy, on the one hand, and the diversity of non-conscious or only partially conscious activities, of whose existence and importance we have been made increasingly aware at least since the time of Freud, perhaps the first question which might be raised is whether philosophy itself is properly to be regarded as a fully explicit and fully conscious activity. This might be regarded as either a patently ridiculous question, or an extremely important one. The 'ridiculous' possibility ties in with what I would ~~take~~ to be a more-or-less standard view of philosophy in English-speaking countries at the present time; and the reader will notice that I have tacitly subscribed to this view in the opening sentence of the present paragraph. Certainly if we take 'philosophy' as referring to the words written on paper (whether as manuscript, typescript, printed and published, or whatever) or spoken aloud and thus publicly available (whether or not anyone is actually listening: think of "He read his paper to the Conference, but someone had drugged the afternoon tea/coffee and within seconds of his commencing everyone was sound asleep ..." — presumably we are not justified in asserting that he did not contribute a philosophy paper to the Conference), then by definition or usage, philosophy is explicit. Is it also necessarily conscious? In the hypothetical example where the audience goes to sleep, they are not conscious, yet we would hardly be justified in withholding the credit of having contributed a paper, to the person who composed and read it. (But what if, instead of a soporific drug, the sleep of the audience had been caused by

the dullness of the paper?) And what if the reader also had partaken of the drugged tea/coffee, and had read the paper 'in his sleep', 'in a trance-like state' — but without getting the words wrong, and managing somehow to get emphasis, expression etc. at all the right places? (In my own experience, literally reading a paper at a conference does induce a trance-like state, and I really have no idea of the structure of the argument while I am reading it. But perhaps this is due to the fact that I spend most of my professional life thinking out the argument while I am lecturing; and doing it the other way is thus unfamiliar.)

An obvious retort to the foregoing is to say "Yes, but even if the reading of the paper was non-conscious or only partly conscious, at least the writing of the paper, the real 'doing' of the philosophy, must have been fully conscious". This too is unexceptionable; but it must be pointed out that it does assume that one is using 'philosophy'/'philosophizing' in an 'achievement' sense, not an 'activity'/'task' sense. That is, it assumes that the important parts of the doing of philosophy are conscious, and thus to some extent begs the question. Certainly much of the doing of philosophy can be conscious, in that one can know some of the various 'standard moves' (just as in chess) and can try them out to see if they work — but the decision that a particular move does or does not work is not in itself so obviously conscious, though obviously one becomes conscious of it. Further, the 'seeing' of the tracks of inference within an argument also seems to result in something becoming conscious but not itself to be conscious — Ryle's paper "On 'inferring'" (1954) appears to establish, not only that inferring is not properly to be regarded as a process, but also that the inferring itself cannot be considered a conscious activity; and it cannot in a straightforward way be considered an unconscious one either.

Having made these preliminary remarks, we must now leave these issues, to be taken up again at a later stage in the argument, and for the present move into some investigations of possible relationships between philosophy and some of the other categories of intellectual activity which may be allied to it. 'Having a philosophy of ...' — life, education, science, and so on — appears to involve, in part at least, having particular views about the nature of life, education, science, etc.; and as we have already seen, the views we hold about a particular topic are conditioned not only by our actual experiences relative to that topic, but also by how we have 'seen' and understood those experiences. Our 'seeing'

and understanding in turn are conditioned by the ideologies, mythologies, etc. of the culture in which we have grown up: these strongly influence the sorts of language-games which are played within a society, within a culture, and hence influence the ways in which people see the world and themselves. These influences in present-day societies are engaging increasing attention in the disciplines of social anthropology, sociology, social psychology, etc. — see Hall 1977 "Culture, the Media, and Ideological Effect", for example. Historically, conscious and explicit philosophy is supposed to have emerged from the putatively more primitive mythic type of thinking in classical Greece (though not necessarily only in that place and at that time). It will be convenient, then, to make a brief examination of some current views as to the nature of that historical (or as we might say, phylogenetic) transition, since an understanding of the transition may help us towards a better understanding of philosophy itself and also, if there is any substance in 'recapitulatory' theories of development (on this, see later), of the ontogenetic development of philosophical competence in the individual.

Kirk (1974: 301) notes that myth in some ways precludes philosophy, notably in that acceptance of a mythic explanation of a phenomenon puts a stop to further inquiry or even speculation about it. For example, if it is accepted that thunder is the sound of the Gods being angry, then not only any attempt at empirical investigation (e.g. Benjamin Franklin flying kites into thunderclouds) but also any speculation at variance with the mythic explanation, is 'logically' dubious, and also impious, heretical. This explains why those who do initiate argument or speculation at variance with the myths of their society are likely to have troubled and possibly short lives. As Kirk remarks, "The organic use of myths has to disappear before philosophy becomes even a remote possibility" (p.279). A problem arises here, however, if we are taking an historical (as well as a logical) view of the transition of myth to philosophy, because, taking societies and cultures as wholes, or even working at the level of the individual, the fact is the myth has not disappeared. Mythic operations still occur, and are important, in advanced 'Western' societies, e.g. the mythic character of the contents of women's 'glossy magazines' (see Phillips, in prep.; also Jung et al. 1964 Man and his Symbols). For some persons and on some topics however, a breakaway has occurred from 'mythic dogmatism'; and the existence of philosophy as an element of culture is attested historically for the past two millenia or more of Hellenistic and Western civilization, indeed Western

philosophy and its offspring Western science and technology are much more accepted as cultural phenomena, of central and massive significance for the societies involved, than is the case with regard to any contemporary mythology. Books entitled "A History of Western Philosophy" are taken for granted, whereas a book on "Present-day American/British/German/Swedish/French/Spanish ... Mythology" would elicit raised eyebrows.

That Kirk should talk of the necessity for myth to 'disappear' before the genesis of philosophy became possible, is supportive of the view argued elsewhere (Stenhouse 1971, 1974) that a 'negating' function is a necessary prerequisite for creativity especially of 'revolutionary' degree. If this 'negating' is indeed a function of the P-factor of intelligence, then it would seem to follow that a high P-factor endowment would be a desideratum for the philosopher — especially, again, if the revolutionary dimension were to be involved.

Vernant (1965) argues that, in looking into the transition from myth to philosophy, we should not be too concerned with the "ancient and irrational residues" still present within early Greek philosophy, but should concentrate, rather, upon what was fundamentally new within the character of philosophy itself. He discerns the new as including (a) the rejection of the supernatural as an explanation of the observable world, (note again the rejecting, negating function), and (b) a search for internal coherence in argument, i.e. logical rigour.

Snell (1960) notes that mythic and logical thought are not co-extensive, and that "... the transition between the two is slow and gradual ... in fact no transition is ever fully completed" — one of the conclusions urged in this thesis is exactly this: that mythic or at least non-philosophical thinking is often/always the accompaniment to or precursor of philosophical thinking. But we must leave that particular issue for the moment, and return to the track we have been following, seeking to characterize the new development, whatever it was, that appeared within early Greek philosophy.

Three general characteristics of philosophy are put forward by Kirk (1974: 289): (a) That it is rational and systematic;
 (b) That it deals with subject-matter of high generality; and
 (c) That the attitude of the philosopher must be that of
 "... unrestricted and wide-ranging enquiry".

Heraclitus is seen by Kirk as having made one of the most crucial

moves in the transition. Prior to his time, thinking had been anthropomorphic, and the family with its central activities of procreation and its blood relationships had served as the fundamental model or paradigm — this is called the 'genetic' model by Kirk., Heraclitus rejected the genetic model. Intellectual unity and cohesion were to be found, not in terms of similarity of subject-matter (i.e. the family or 'genetic model') but rather in terms of unity of 'process': the process of systematic rational thinking, which can apply to any and every aspect of experience, to questions of ethics and morality just as to the physical world. "Heraclitus' innovation consisted in the tacit rejection of the genetic model ... [and] ... substitution of an analytical and synchronic view of the world for a historical and diachronic one", (p.299) — in other words, natural phenomena are to be seen not as 'offspring of the gods' or due to the 'kindness/love, or on the other hand the malevolence, of the gods', as they are in Homer, but rather as the outcome of causal factors operating immediately and on the spot, factors which could in principle be investigated and discovered, and which are to be regarded as 'synchronic' in the sense of acting in the same (and 'impersonal') way irrespective of time and other circumstances. This notion is so basic to modern thinking that it is difficult for us to envisage a different approach. Sympathetic observation of the behaviour of young children reveals, however, that even today many of them go through a phase of anthropomorphic thinking, similar to the family or 'genetic' stage in Kirk's terminology. "Naughty chair!" says the toddler, smacking it; "Mustn't hurt my leg!" This is pretty similar to Homeric-type explanations: "Then grey-eyed Athene, to frustrate his plan, placed a chair in his path, over which he stumbled ...". We need not press this point, though for educational purposes it is worth noting; but reverting to our present concern with the transition from myth to philosophy, it may perhaps be desirable to point out that the abandonment of the 'genetic model' is in no way inconsistent with the 'biologicalism' of Aristotle which was discussed earlier (see Chapter 7). 'Biologicalism' can be contrasted with 'physicalism', both terms being used in a special sense, and both equally 'analytical and synchronic', and in contrast to 'historico-genetic and diachronic' thinking.

By ceasing to think of the interactions of natural phenomena as being of the same type as the personal interactions within a family, the pre-Socratics opened up for themselves a much larger array of possible types of relationship and interaction. Few of them adopted the emphatic process- rather than thing-orientation of Heraclitus — Parmenides' 'the One',

Empedocles' earth-air-fire-water doctrine are still mainly 'thing'- or matter- or substance-orientated, though Parmenides', Anaximander's and others' doctrines were to varying degrees abstract and general — nevertheless the break from mythic thinking is generally accepted as having been made, and Western philosophy and science are generally accepted as having started, with the Milesian pre-Socratics. This does not imply that pre-Socratic or subsequent thinking has been unaffected by mythic elements. Cornford (1947) has emphasized the continuing existence of what Kirk calls "ancient and irrational residues" (Kirk 1974: 277) in pre-Socratic thought. (Both Kirk and Vernant 1965, and probably others, regard Cornford as over-emphasizing this aspect; but one's position on this must depend upon what one sees as being included under the rubric 'philosophy'). Kirk adopts two criteria for the differentiation of philosophy from myth. "Philosophy tries to be consistently reasonable", he says, p. 278, "and to deal with general subjects of universal application; myths do not."

It is perhaps worth noting that Kirk's own practice of scholarship, even though he is not setting out to write a book of philosophy but one on The Nature of the Greek Myths, is itself a good exemplification of linguistic philosophy. In his preliminary clarification of his subject-matter, Kirk remarks that "... one is compelled to take some notice of contemporary usage. Modern philosophers as well as Aristotle have started from popular opinions about indefinite subjects like goodness and the nature of Being. Provided long-standing usage is regarded as indicating no more than a broad area of probable relevance, general opinions on the range of a term and the limits of a corresponding concept can be a valuable starting point." (p.25). The emphasis on ordinary usage is in accord with much Oxford-type philosophy and its derivatives (e.g. London-type educational philosophy); but the emphasis that such usage is only a starting point accords more with the Wittgensteinian approach which is advocated in the present work. Again, Kirk warns against the dangers of premature definition: "It is probably better, and less methodologically constricting at an early stage of the enquiry ..." (p.27) to take a common-usage interpretation as a provisional starting-point — another adumbration of what is here advocated (and has been practised, e.g. Stenhouse 1974, Chapter 1.).

Returning to the three general characteristics of philosophy put forward by Kirk (see above), some remarks can be offered at this point which may help to concentrate our focus upon issues which still need clarification. With regard to his (a), that philosophy is "rational and systematic", we

might accept that 'systematic' is generally clear enough, though 'rational' is not perhaps as fully understood as is desirable (see earlier discussions of Peters' use of this word and its relatives). 'Subject-matter of high generality' seems appropriate enough, though it must be pointed out that both this and the previous criterion apply to science and other modern academic disciplines equally as much as to philosophy. Kirk's purpose is of course to differentiate all of these modern disciplines from myth, rather than from each other: he is not attempting to define modern philosophy as distinct from other modern disciplines, though the criteria he offers can be useful towards this latter function. Philosophy as it developed in ancient Greece can in this context best be regarded as the relatively undifferentiated mother-matrix of which the diversity of modern academic disciplines eventually grew. As contrasted with myth, Kirk's criteria are indeed significant. Myth is not explicitly of high generality: its stories are all of the doings of individuals, whether men or gods (e.g. Ulysses, Athene), and their activities and adventures are all particular (though general lessons may be implicit within them). At first sight it could seem incorrect to say that myths are 'unsystematic'. The adventures which constitute a particular story are normally related in a more-or-less standard order and with appropriate internal coherences and plausibility; and even when variations occur, the two latter criteria would not normally be violated. (With regard to the Odyssey myth, Bradford 1963 has put forward an interesting interpretation, in terms of the specifics of geography, climatology, seamanship, etc. of exactly where Odysseus wandered, and why. For a similar hypothetical explanation of the historical facts behind the 'Atlantis' myth, see Luce 1972.) But of course the significance of a myth lies not in the explicit story, but in the understanding of people and events, natural phenomena, the view of the world which is implicitly offered within the story — and this implicit message is not itself developed systematically.

In a similar way, to the objection that a mythic story is not in itself 'irrational', it can be pointed out that while this is true of the story just as a story, the message embedded in the story which gives it its significance, is not stated clearly and unambiguously. Hearers of the story might well take an entirely different message out of it: the fact that the message is implicit rather than explicit allows this.

Kirk's third criterion of philosophy, that it is "unrestricted and wide-ranging enquiry", clearly cuts off philosophy from both myth on the one hand and the various modern but non-philosophical academic disciplines on the

other. No other academic discipline is unrestricted as to the topics it may deal with: the Common Assumptions Paradigm in force at any given time determines the subject-matter that is acceptable within the discipline. This appears to be as true of nonscientific as of scientific disciplines; though in both there is the possibility of revolutionary-type change. The latter possibility might appear to open the door to unrestricted change, in that the CA Paradigms of various sciences might be able to change (though presumably fairly slowly) to cover virtually any subject-matter — given sufficient time. But this amounts to an argument to limitation. While we cannot categorically deny that science might one day deal with gremlins and astrology, say, it seems rather pointless to claim that this bare possibility justifies including these phenomena, whatever they are, within science at the present time. If we are talking about sciences or other disciplines, we must take them as they are or have been at specified times, as they have been seen by their practitioners or by historians, philosophers, sociologists, etc., who have studied the ways in which the disciplines have been practised. And in general it seems to be the case that any discipline, besides being guided positively by the CA Paradigm of the moment and its emanations in the form of the (possibly various) Public Demonstration Paradigms, is also restricted by them as to both subject-matter and acceptable methodology — hence the investigations within the discipline are not totally unrestricted, hence most academic disciplines do not meet Kirk's criteria for being 'philosophical'.

If we accept, provisionally, Kirk's criteria of "unrestricted and wide-ranging enquiry", and follow the possible applications of these criteria a bit further, some interesting implications begin to emerge. Firstly, we may remark, in relation to the investigations made elsewhere (see Chapters 3 and 4) of the work of Peters and the London school of Educational Philosophy, that insofar as there are substantial limitations in the topics, and the several aspects of topics, that they are prepared to look at, limitations in the possible interpretations of discourse that they are prepared to recognize, their work falls short of the criteria for being philosophical. And it must be kept in mind that Kirk, in drawing up his criteria as he did, had no polemical intentions with regard to Peters or anyone else working in Educational Philosophy. He was not trying to set up criteria which would rule Peters out of court (cf the brief controversy, already mentioned, between Powell 1965 and Snook 1966). His concern was merely to distinguish between mythic and philosophical thinking in ancient Greece.

Secondly, while the argument of the foregoing has pointed to the restrictions inherent in the inquiries which are possible at any one time within any one science (or other academic discipline), a general possibility is opened up, that science in total, taken collectively and over a period of time, should still be regarded as falling within the bounds of the philosophical. This conclusion, speaking in terms of my own personal position (see Chapter 8) as one who has been professionally engaged in philosophy, science, education and Education, I find congenial, and encouraging with regard to an enterprize like this thesis (arguing as it does for an 'inclusivist' approach).

Thirdly, what of Philosophy itself as an academic discipline? If we extend Kirk's 'wide-ranging' criterion so that it becomes co-extensive with his 'unrestricted', then to the extent that Philosophy has in fact become 'exclusivist' in the sense indicated earlier, especially during the present century, it has ceased to meet the criteria for being truly philosophical. Note that this is being put forward very much as an hypothetical. I am not trying to assert that none of the philosophy that has been done in this century is really philosophical. It seems clear that, as a matter of practical expediency, the enormous range of language-games which include say a science and/or technology at one extreme and matters of abstract logic at the other, cannot all be handled competently by one single individual. Limitations of time and educational opportunity make individual omni-competence out of the question. It is inevitable, and also unexceptionable, that some individuals should concentrate upon the more abstract end of the spectrum, and others upon matters of technique or technology. But it is essential to be clear about what is accepted as an unavoidable limitation in terms of practicality, and what is adopted as a deliberate methodological axiom, a matter of principle. There is no need to elevate the former into the latter, indeed a great deal of harm may be done by gratuitous narrowing of vision. It is one thing to recognize and accept that any particular individual is unable due to contingent circumstances to cope with more than a small section of the total range of language-games — it is something quite different to say that such limitation is inherently necessary or desirable. Those who wish to insist upon an 'exclusivist' approach in philosophy, whether they do so in terms of its alleged 'spectatorial' or 'second-order' character or in some other terms, are in effect committed to asserting a separation of philosophical discourse, philosophical language-games, from all other types of discourse and language-games. I have argued that, while there are great differences

within the enormous variety of language-games, there is no particular boundary which separates philosophical ones from the others (even though there may be 'family resemblances' among philosophical l-gs) — and it is interesting that a literary/historical scholar like Kirk should bring forward criteria of philosophy which run counter to 'exclusivist' assumptions (which explicitly contradict such assumptions, in fact).

In a brilliant short paper which has just (27/12/78) come to my notice since the earlier part of this thesis was already in typescript, Goody (1977) discusses various theories which attempt to explain different types of thinking in relation to literacy and other features of human culture. His position seems closely similar to my own, in methodology, and his views, as a social anthropologist/sociologist looking at the same general issues as those we have been examining, are worth citing in some detail.

"Much of our thinking about the development of human thought from its first beginnings, and about differences between traditional and modern cultures today, is set in a binary framework. We still speak in terms of 'primitive' and 'advanced', almost as if human minds differed in their structure like machines of earlier and later design. The emergence of science ... is held to follow a prescientific period, in which magical thought predominated. Philosophers describe this process as the emergence of rationality from irrationality (Wilson 1970) ... others have attempted to get over the difficulties raised by a purely negative definition of the situation (e.g. rational-irrational) by means of more positively phrased dichotomies, the wild and domesticated (or cold and hot) thinking of Lévi-Strauss (1962), and the open and closed situations of Robin Horton (1967, applying Popper).

"I do not find that any such simple design provides an adequate framework for the examination of human interaction and development. Yet neither is it possible to accept the opposing tendency ... cultural relativism ... (and) treat all societies as if their intellectual processes were essentially the same. Similar yes, the same no. And once one allows this case, the specification of difference is not in itself enough; one needs also to point to mechanisms, to causal factors."

(Goody 1977: 227).

The causal factors in which Goody is interested are those associated with different modes of communication as they affect man's "functional cognitive systems" and "the growth of knowledge":

"While this has to do with 'content', it also presupposes certain processes which are related, I argue, to the modes of communication by which man interacts with man and, more especially, transmits his culture, his learned behaviour, from generation to generation" (*ibid.*).

That the findings from this sort of investigation must have highly

significant implications for education as a social process (and Education as an academic study) is clearly apparent. The reader may also note the general correspondence between Goody's 'content' and 'processes' on the one hand, and my own use of 'knowing that' and 'knowing how' (Chapter 9, above, following Ryle 1949) on the other. One of Goody's suggestions is that literacy, by involving semi-permanent records, allowed for and to some extent caused the development "... of critical activity (and hence of rationality, skepticism, and logic ...)." What he is pointing to is apparently roughly the same as my 'repeat loops' model elaborated from the superficial linearity of manuscript or print (see Chapter 8 above). He agrees with Horton (1967) that there is not necessarily so great a difference between modern 'scientific/rational' and 'older' types of thought as is often assumed, and that "Like atoms, molecules, and waves, then, the gods serve to introduce unity into diversity, simplicity into complexity, order into disorder, regularity into anomaly" (Horton 1967: 52). Note that here, instead of emphasizing the survival of "ancient and irrational residues" in modern thought for which Kirk castigates Cornford, Horton and Goody are pointing to 'modern' features in pre-scientific 'primitive' thought — but, either way, the outcome is a denial of any 'great divide' between the various types of operation. This may perhaps be compared to my own suggestion of the 'oceanic' nature of language-games (cf Chapter 5).

In endorsing a dichotomy between 'open' and 'closed' thinking and the societal backgrounds conducive to one or the other, Goody remarks that such a dichotomy "... is often a useful preliminary for descriptive purposes; once we accept it as such, we can go further and attempt to elucidate the possible mechanisms that bring about the differences, a step that usually involves modifying or even rejecting the original dichotomy." This accords very well with the methodological position adopted in this book, following Waismann and Wisdom (refs in Chapter 8 above), that neat and tidy conclusions are usually best to be both affirmed and denied, and that what really matters is the careful and sustained attempt to come to grips with the full complexity of whatever subject-matter may be in question.

Goody quotes Horton's (1967: 155) definitions of the 'open' and 'closed' situations: "in traditional cultures there is no developed awareness of alternatives to the established body of theoretical tenets; whereas in scientifically oriented cultures, such an awareness is highly developed"; it is "the awareness of alternatives which is crucial for the take-off into science". Goody himself says that: "Closure is associated with

lack of awareness of alternatives and anxiety about threats to the system; openness, with the opposite." (p.231). The point about anxiety, and alternatives being seen as threats, may be compared to my discussion of some of the psychosocial aspects of examinations in education (see Chapter 7) — but perhaps the most salient issue that emerges from comparison between what is said about the 'open'/'closed' dichotomy with the other dichotomy to which frequent reference has been made, viz. that between 'normal' and 'revolutionary' science, is that these two dichotomies do not coincide. Both Horton and Goody appear to assume that science is 'open' — whereas the whole trend of Kuhn's arguments for the 'normal'/'revolutionary' distinction in science (and this seems to be accepted by most subsequent discussants of the distinction), is that 'normal' science is not open, or, at least, that there are very substantial constraints on openness in normal science. Revolutionary science is open in the sense which Horton and Goody seem to demand — but while revolutionary science is of the very greatest significance, as discussed earlier, the fact seems to be that, quantitatively, normal science is vastly predominant at any one time and, most important for our present purposes, that education in science is strongly, one might well say almost exclusively, orientated towards normal science.

The most significant issue which Goody's paper is intended to communicate, however, is that while making an illuminating distinction is a good starting point, the really vital questions are those concerning the specific causal mechanisms which give rise to the substance of the distinction. Goody himself seizes upon literacy, and its absence, as providing the ground for various causal mechanisms. In summary, his argument is that traditional oral cultures tend to be 'closed' because the spoken word is ephemeral and does not readily permit the close and sustained examination which might expose ambiguities, logical fallacies, etc., — or, in more general terms, which would allow alternatives of meaning to be revealed. In contrast, the written word allows for both criticism/"skepticism" and the perception of alternative possibilities, i.e. 'openness'. As an example of this he takes Margaret Masterman's (1970) paper in which she demonstrates that Kuhn in The Structure of Scientific Revolutions (1962) uses 'paradigm' in at least twentyone different ways. Goody's point relates to the technique used to establish this. As he puts it, "... the detection of ambiguity or inconsistency leading to a reformulation of the argument was effected by reference to a box of filing cards which kept track of different usages of one key word in the author's [i.e. Kuhn's] argument.

It was effected by a purely literary technique, which permitted a more systematic exploration of a written text than was possible by the more casual techniques of visual inspection usually undertaken by critics ..."
(Goody 1977: 240).

This dimension of relative permanency which is generated by the written word (in contrast to the spoken) can be seen as a causal prerequisite to the resolution of some of the anomalies round which we have skirted, several times, in the course of our investigations so far. I have attempted, in Chapter 8 and elsewhere, to show that the notions of philosophy as being 'spectatorial' and 'second-order' as alleged by Hirst (1963), Peters and others, are mistaken and generate anomalies: philosophical discourse cannot be purely 'second-order' and still do what Hirst and Peters want it to do. We have also found grounds for querying the use of 'rational' and related terms. Goody remarks (p.238): "As with skepticism, rationality is often seen as one of the differentiating features of the 'modern mind', of the scientific view. This is not a debate we find very promising." He goes on to cite Wartofsky (1967) as providing a fruitful line of attack on the notion of 'rationality' in relation to modern and scientific thinking. Wartofsky is quoted as follows: "rational practice entails ... the self-conscious or reflective use of concepts; i.e. the critical attitude towards scientific practice and thought, which constitutes not simply scientific knowledge alone (which is its necessary condition), but the self-knowledge of science, the critical examination of its own conceptual foundations." (Wartofsky 1967: 151). As Goody points out, "Rationality in this sense implies metaphysics, which is "the practice of rationality in its most theoretical form" (p.153); "a rational theoretical science is continuous with the tradition of metaphysical theory-construction" (p.154); metaphysics is a "heuristic for science". [These page references are to Wartofsky 1967 as cited by Goody.] Whether or not we agree with Wartofsky's point, it seems clear that the kind of reflective use of concepts required by his definition of rationality is greatly facilitated by the process of giving speech permanent embodiment and thus creating the conditions for an extension of reflective examination." (Goody 1977: 239).

In the light of this, Peters' view of 'rationality' as being roughly equivalent to 'giving reasons' can be seen as even more clearly defective. We saw in Chapter 4 that merely 'giving a reason', i.e. providing a general statement under which a given action or particular statement is logically

subsumable, allows any action or statement at all to be justifiable and 'rational'. If the general statement also has to be such as to be acceptable to a particular social group, then Peters' 'giving reasons' is tantamount to 'giving a rationalization' in the Freudian sense. Compare this with the view of rationality which emerges from our examination, in this Chapter, of the arguments of Kirk, Goody, Wartofsky, Horton and others. On this view, rationality involves several features:

- (i) A capacity for self-examination — we could perhaps denote this as 'reflexivity';
- (ii) An ability to perceive more than one alternative with regard to objects of attention, meanings/interpretations of words and/or statements, etc.;
- (iii) An ability to at least hold in abeyance the acceptance of any one of the particular possibilities referred to in (ii) above — and on the occasions when this entails withholding assent to something which previously had been accepted, this amounts to a capacity for self-criticism.

Clearly, these three features do not provide anything near a full characterization of rationality; and even on their own, they need further explication to bring out their various significances. A few initiatory remarks may lead us towards an enhanced understanding:

The capacity for what we are calling 'reflexivity' (cf Wartofsky's "reflective use of concepts") is probably what Peters, Hirst and others have in mind when they talk of 'spectatorial' or 'second-order' activities. These latter terms either offer no real increase in understanding ('second-order' need not be regarded as false — but exactly what does it mean?), or else they are positively misleading ('spectatorial' tends to suggest that one watches without taking part). And if Goody is correct in suggesting that one's capacity for 'reflexivity' is enhanced by dealing with the written rather than (or additionally to) the spoken word — and surely he is correct — then the value of education as enhancing a person's general capabilities can become somewhat clearer. To the extent that education deals with the written word and encourages awareness of alternatives of meaning, it can be seen as increasing the effectiveness of peoples' rationality and thus, in a sense, as actually increasing their rationality (as well as increasing what might be regarded as 'components' of rationality, e.g. 'openness', 'self-criticism'). More on this later.

The 'ability to hold in abeyance', (iii) above, seen as an element in

rationality, links up with one of the factors of intelligence elucidated on the basis of evolutionary and ethological theory in my The Evolution of Intelligence (Stenhouse 1974), namely the P- or Postpone-ment-factor. If several distinct lines, of argument, which start from widely different bases, converge on similar or perhaps identical conclusions, it would seem that they should be regarded as reinforcing each others' plausibility — and the plausibility of the conclusions.

We shall return to a more extensive discussion of the relationship between the several media of communication and the effectiveness of intelligence, rationality, etc., especially in relation to education. For the moment I want to take up several other points made by Goody in the paper already referred to.

Firstly, a point on which I think he is quite right: "... it is not so much skepticism itself that distinguishes postscientific thought [i.e. thought which, even though it may not be directed towards subject-matter which is accepted as coming under the heading 'science', still exhibits the type of thinking characteristic of science, e.g. is 'rational' as interpreted above] as the accumulated skepticism that writing makes possible; it is a question of establishing a tradition of critical discussion." (Goody 1977: 287). In my own foregoing discussion I have toned down the notion of 'skepticism' to my (iii), viz. an ability and willingness to withhold assent to any one position until others have been investigated; and taking this less extreme interpretation, it seems to be true that a tradition of moderate scepticism is extremely important. Several related issues have to be kept in mind, however. One is that much of the responsibility for continuing such a tradition has now been taken over by the various institutions of formal education — and it is possible that institutional mechanisms will gradually erode the tradition, and may indeed already have do so to some extent. Another issue is that the pressures and the rates of change of societal/institutional mechanisms have been increasing at an accelerating rate, as is widely recognized — but not all changes take place at the same speed. Some may become extremely rapid, others may even be slowed. Especially with large and complex institutions, as Schon (1973) and others have shown, changes may become irreversible: rigidification may set in. In the practice of many of the so-called 'basic' sciences, 'scepticism' has now become, at the operational level in the everyday thinking of a majority of practitioners, merely a dogmatic unwillingness to consider any proposition that goes in the least bit beyond the observables and what can be wrung out of them using the accepted statistical and

computer-science techniques. The 'Western intellectual tradition' in a broad sense is one thing — the practice of science in the late 1970s is not necessarily the same. Kuhn (1970: 7) is only too realistic when he says "... it is precisely the abandonment of critical discourse that marks the transition to a science".

At the societal level, tradition is indeed important. Its analogue at individual level is habit. It is one thing to have the potentiality for Goody's 'skepticism', open-mindedness, withholding of assent, or the Postponement-factor of evolutionary intelligence — but this potentiality is of no effect unless it is actualized in the behaviour of the individual. Social, cultural and institutional factors can operate to facilitate or inhibit the actualization of whatever potentiality may be present. Some of these factors as they affect especially the P-factor of intelligence have been discussed in The Evolution of Intelligence Chapters 5 and 10, and I shall not attempt to repeat those discussions here. A few summary points need to be made, however; notably that for a potentiality to be actualized, even for it to remain in existence as a real potentiality, it must not be continuously suppressed. This appears to be as true of mental/intellectual traits as it is of physical ones. And for mental as for physical abilities, full development depends upon their exercise, their being put to use. Goody differs from Horton in seeing the essential difference between 'scientific Western' and 'traditional' thinking not in the presence or absence of skepticism — as he points out, skepticism does in fact occur in 'traditional' non-literate cultures — but rather in "... the accumulation (or reproduction) of skepticism. Members of oral (i.e. "traditional") societies find it difficult to develop a line of skeptical thinking ... simply because a continuing critical tradition can hardly exist when skeptical thoughts are not written down, not communicated across time and space, not made available for men to contemplate in privacy as well as hear in performance" (p.234). Thus the written word, as against the spoken, is conducive to the development of critico-creative thinking within a society — but does this apply to similar developments within the individual?

Here we come to part of the argument in which Goody first makes a very important point, and then goes on to slightly overstate its implications. He first says that "... by putting speech down onto paper, one creates the possibility of what is almost a different kind of critical examination" (p.240), as compared to listening to the spoken word. This seems to me to be of absolutely vital importance for education. The careful and extensive

exploration of alternatives of meaning does, I believe, necessitate working with the written word — and not only reading the written word, but also writing it. This is absolutely essential in learning how to conduct a critical examination; and if persisted in it can lead to the development of a habit of critical examination. But Goody goes on to suggest that, if (his example) Kuhn's The Structure of Scientific Revolutions were to be presented as an oral discourse, "No listener ... could ever spot the twenty-one different usages of the word "paradigm". The argument would flow from one usage to another without anyone's being able to perceive any discrepancy. Inconsistency, even contradiction, tends to get swallowed up in the flow of speech ... the spate of words, the flood or argument, from which it is virtually impossible for even the most acute mind to make his mental card index of different usages and then compare them one with another." (p.241). It is true that keeping track of differences in usages is more difficult with an oral as compared to a written presentation, and probably true that for the ordinary untrained mind it is "virtually impossible" to sort out the differences, possible inconsistencies, etc.. But Goody cannot have spent much time investigating the ethnomethodology of philosophy congresses where first-class philosophers have been participating. The philosopher learns his trade on the written word, but can practise it almost equally well (in many cases) on the spoken word. Here again individual differences are great. As with all intellectual activities, immediate effectiveness does not necessarily correlate with long-term significance. It has been my good fortune to have observed such men as Ryle, Passmore and Mackie in action, and to have seen how they could, in fact, keep track of the intricacies of oral discourse and bring a speaker back, when necessary, to sort out an ambiguity that had been embedded in statements made through a period of perhaps several hours. Lawyers also can develop this type of skill: it is a necessary part of courtroom effectiveness, to make a running analysis of the evidence being given and to be able, at the right moment, to confront a witness with inconsistencies in his testimony which may have been spread over several days. (It is true, of course, that both philosophers and lawyers, and others who need to perform similar functions with the spoken word, do also use written notes. In many cases, however, these will be found to consist of the results only of their 'running analysis', not the raw material upon which it is based, which is what Goody refers to.)

I suggest that it is of the utmost importance in education that students be trained in the skills of analysing argument, first on the written word, then on both spoken and written, so that they not only develop their

ability for such analysis but acquire the habit of it. This is desirable not only for a variety of functions within a society, e.g. exercising the basic function of participating in a real sense in the public debates of a democratic society, but also for the essentially educational function of understanding what is being said or written. Many of the difficulties and troubles within formal education today would be reduced if emphasis were switched from 'learning' to understanding (using 'understanding' in terms of the analysis offered by Goody, which can be assimilated to the 'understanding language-games' picture presented in this thesis).

The most fundamental point to be made in this connection is that an 'argument' which is fallacious/inconsistent cannot be understood. It cannot be understood, that is, unless it is seen and understood as fallacious — and unfortunately, many pupils (also many members of the general public) do not have the requisite background to pick out the fallacies and see that the argument is in one sense not an argument. Further, those undergoing formal education are likely to be under considerable constraint not to expose fallacies in what is presented to them: not, that is, if they want to gain the qualifications they are seeking. As discussed in Chapter 7, anyone who argues against, or who even calls in question, the subject-matter being promulgated by his teachers, is liable to be penalized, informally if not formally. This is not to say that the teacher will consciously and deliberately impose an obvious punishment (though some are quite likely to do even that, for reasons already mentioned). The 'penalty' imposed is more likely to be what we have seen before: non-comprehension of what the pupil is trying to say. The non-comprehension by the teacher, in the hypothetical situation being considered, is likely to be due to an inflexible approach to the language-games of the subject, on the part of the teacher; and this in turn can be due either to innate lack of capability, or, more likely, to an over-rigid approach having been forced upon the teacher in the course of his/her own training. Either way, the teacher is 'penalized', by being a worse teacher and, in the event that he/she also researches in the subject being taught, in missing out on the new and possibly important insights that might have resulted from an attempt to understand the pupil's difficulty and/or criticism.

(It is important to recognize, however, that in many education systems a teacher who does not understand and perhaps will not even permit questioning of the subject-matter being taught will be, not penalized, but rewarded. This reward may be allocated on a more-or-less negative basis,

or as it were by default: if teachers within the system who encourage questioning and criticism are punished for doing so, e.g. by the withholding of promotion, then the nonquestioning and non-critical ones will be the only teachers that the system is able to reward. Mediocrity rises. Incompetence means success. The inevitable corollary to this is, of course, is that the 'education system' in which such mechanisms operate is in reality not an education system: it is an indoctrination system. Such systems can already be found in many parts of the world, even though 1984 is not quite here. Orwell's other forecasts are already recognized as having come true. We have Ministries of Defence which operate purely for attack. Ministries for the Environment which supervise the destruction of what they are supposed to protect — why not an Education System which systematically miseducates? cf Koerner 1964).

If the subject-material which is being presented in education is in fact not coherent either in terms of meanings/logic or with the observable world, then it is strictly impossible for students to understand it. Their only alternative to 'failure' within the system is therefore merely to learn it up, i.e. learn it more or less by rote.

It is perhaps worth pointing out, in this connection, that what was denoted as 'factualism' in Chapter 2 is a natural outcome of the endeavour to teach subject-matter which the teacher himself does not fully understand as theory, or which is defective in its theoretical structure, i.e. is not logically coherent. If what is presented to students is not a coherent and integrated system in which each element is connected to others by intelligible causal or logical connections, then it cannot in any real sense be understood. About the only thing people can do with it is to treat it as though it were a collection of empirical facts, and simply learn it up. This happens with a great deal of subject-matter which could, but for misapprehensions about scientific method and epistemology in general which are propagated by teachers and textbook writers, be taught as interconnected and intelligible argument. The learning up of seemingly unorganized facts is of course heavily laborious; and teaching of this type therefore exerts informal 'selection pressures' upon those who receive it. The only students who continue with subjects taught in this way are those who are compliant and uncritical; the others go elsewhere.

Our brief excursion into the transition zone between philosophy and myth has, I believe, been illuminating. Among other things, we have gained a clearer view of what can be involved in the notion of 'rationality' — a

concept which seems to be close to the heart of much Western education, and certainly of Western philosophy — and in doing so we have also been able to discern more clearly the exact nature of some of the shortcomings of London-type Educational Philosophy (which is important in being the most widely influential approach in Ed. Phil. at the present time, and also as probably the only contact with philosophy experienced by the majority of schoolteachers in the course of their professional training). Some of the issues already raised lead on naturally to the next topic to be covered, namely, the relationship between the explicit and conscious arguments of philosophy on the one hand, and on the other the various non-explicit and largely non- or pre-conscious transactions which sometimes or perhaps often accompany our explicit and conscious activities and may sometimes supplant them.

The reader will note that much of my discussion is framed upon the assumption that doing philosophy is a public activity, and that it therefore involves communication. There are several reasons for this. In the first place, our starting point and main preoccupation is with education, in science or philosophy (or any other discipline, for that matter), and education insofar as it is a matter of deliberate social activity is essentially public, a matter of inter-personal transactions. This is not to say that there are not also 'private' and not-directly-observable aspects of education, or that they are not important. Even in philosophy itself, while 'thinking up ideas and arguments' seems to be private and non-observable (But if I sit in an easy-chair with my eyes shut, can you be sure that I am just having a rest, that I am not thinking up arguments? What if I am sitting with my eyes shut, but at my typewriter?), presenting the arguments, analysing and criticizing other people's arguments, teaching, and so on, are all public activities, and all involve communication. As suggested earlier, the language-game can be seen as the interface through which all educational transactions must pass; and this includes education in philosophy.

A second major consideration is that even the 'private' thinking involved in philosophy or any other discipline, to the extent that it is determinate, able to be specified even if only by the thinker to him- or herself, can become so only to the extent that it is formulated in language. I am of course referring here to thinking which in principle could be communicated, and am not committing myself to the denial that any thinking can be non-linguistic, i.e. that all our cognitive activity is in the

form of internal dialogue. In a sense, I might appear to be setting up a circular argument, but I suggest that it is circular only to the extent that I am trying to clarify a limited specific precondition: that if I am thinking about philosophy (or some other academic discipline), and someone asks "What are you thinking about?", I am normally able to say something like "Kant's noumena/phenomena distinction", even though I might not be able to give further specification as to what I was thinking about it. Alternatively, of course, I might be able to tell, perhaps in clear detail, exactly what I was thinking. Either way, if I am thinking about something, then I am able to say at least roughly what it is; and if I truly cannot say what I was thinking about, even to myself, then I cannot say that I was thinking about 'something'. (Note that I do not say that I was not thinking.)

We may lead in to the next topic by referring again to the paper by Goody (1977). Continuing his review of effects of type of communication upon the messages communicated, Goody remarks that "... the more deliberate deception of the orator is perhaps less easy to overcome than the unintentional ambiguities of the writer ... By means of rhetoric, through the gift of gab, the "tricks" of the demagogue are able to sway an audience in a more direct way than is the written word. What is at issue here is in part the immediacy of the face-to-face contact, the visual gesture and tones of voice, that marks oral communication." (p.241). Without committing ourselves on the question of whether deception is easier or less easy in the immediate, face-to-face situation, as compared to written communication (one of the assumptions of the juridical procedures of the British tradition is that a jury of twelve persons, by witnessing the statements and cross-questioning of those giving evidence, can come to a better estimate of the reliability of the evidence) — and without considering the issue of intentional as against unintentional deception — we can agree that, in some ways, a great deal more information is transmitted in the direct oral communication than is carried by the same words when written down. The very fact of the richness of the interchange can, however, paradoxically cause a reduction in the amount of significant information that is passed, since, in the language of cybernetics/information theory (see Ashby 1964), the 'signal' may become too difficult to distinguish from the 'noise' within which it is embedded. (Note that these terms, e.g. 'noise', are used in a general sense relating to any and every mode of transmission of information: there can be visual, tactile, etc. 'noise' as well as auditory/acoustic.) Also,

and most significantly, some parts of the information emitted from the sender may be inconsistent with, or contradictory to, other parts; and the receiver may thus become confused as to the nature of the message (or, as to which parts of the total are the message, i.e. 'signal').

The question of human communication is engaging the attention of a very large and increasing number of disciplines these days. Apart from the technological fields, and information theory (cybernetics) itself (see Wiener 1948, Shannon and Weaver 1949; Ashby 1964 provides an accessible introduction), social anthropologists are grappling with the enormous complexity, subtlety and sheer convolutedness of human cultural relationships and communication (see Goody's paper discussed above, which comes from a volume "Culture and its Creators" eds Ben-David and Clark 1977; also Leach 1976 "Culture and Communication", and any issue of Man (N.S.) for further refs), Human Ethology is revealing many of the unsuspected intricacies especially of non-verbal communication (see Hinde ed. 1972, Argyle 1975, Morris 1977 for further refs), and Marshall McLuhan coming from literary studies has had enormous popular impact with his aphoristic "The Medium is the Message" and so on.

But what, it may be asked, does a thesis whose basic intent is to argue for a language-game approach in teaching science and philosophy, have to do with the multitudinous studies on communication mentioned above?

With the emphatic proviso that, in what follows, we can sketch only the barest outlines of what are in fact highly significant sets of relationships, the beginnings of an answer to that question can now be offered.

Any argument relating language-in-general to education must take some account of other dimensions besides those regarded as philosophical (in the strict or narrow sense) and those which are purely verbal. Among the students of language who are not primarily philosophers two names stand out in the recent literature: those of Chomsky and McLuhan. Their approaches are superficially very different. Chomsky is a leading figure (perhaps the leading figure) in scientific linguistics (Lyons 1970). McLuhan on the other hand has approached the study of language and communication from a starting-point which was, initially, purely literary (Miller 1971). Beneath the very real and important dis-similarities, however, lie several important areas of convergence. Both in their different ways give great importance to the 'Whorfian hypothesis', viz. that the language one uses or more generally, the medium of communication one uses, to a con-

siderable extent determines how one views one's world — indeed to a considerable extent it determines what one's world, effectively, is to be.

The basic Chomskyan argument, that the 'deep structures' of language are largely determined by endogenous factors in human nature, that these deep structures are broadly similar throughout all human languages, and that it is only at the relatively superficial levels that one language differs from another and is subject to cultural and other environmental influences, is largely irrelevant to our present purposes except insofar as we recognize that it is at this last and most 'superficial' level that cultural influences including education are actually operative. 'Superficial' is put in quotes in the preceding sentence because, while the level in question is superficial within Chomsky's frame of reference, it is certainly not superficial in the sense of being unimportant in terms of individual and societal adaption. It is at this level that linguistic changes occur, and at which the Whorfian and McLuhan theses apply.

"The medium is the message" is a popular and hyperbolical expression of one of McLuhan's major theses, as mentioned above. In Understanding Media (1964) he advances various analyses of the several media of human communication and the different effects they can have. A comprehensive exposition and critique of his theories would be inappropriate in the present context, but a few points may be made here, which will link up with some of the other arguments being advanced in this thesis.

Firstly, while tribute may be paid to McLuhan's pioneering efforts to open up a wider popular understanding of the media and their effects, some of his emphases are certainly misleading and some of his assertions factually incorrect. His emphasis on the 'linearity' of the written word for example, is true so far as it goes: the writing as such does progress in a linear fashion (from left to right along the lines, and from top to bottom of the page, in European languages; differently but still linearly in other languages e.g. Chinese Mandarin ideograms). But perhaps the most fundamental of all features of the written word, as distinct from other means of communication is, as Goody (1977) and McLuhan himself have emphasized, its permanence. The fact that a book, for example, can be a relatively permanent possession, can be read and re-read and its part read in different orders and in different combinations from what was originally set down, tends to make the 'linearity' of its reading a relatively trivial matter, 'superficial' in the sense of having relatively little real importance. Reading a book or similar document is significantly different from reading, say, the 'credits' at the end of a movie for a TV programme. With the book one can go back and read parts of it

again, one can check one part in detail against others, and so on — whereas when looking at the film credits or the TV one cannot do this (and often one cannot even get the credits properly read, if they are projected too fast!)

Miller (1971: 10) summarizes one part of McLuhan's argument regarding the replacement of handwritten script by printing:

"The brilliant legibility of type made it possible for the eye to race along the 'macadamised' surface of a text, taking in at a careless glance notions which might be more subtly modulated and qualified when issued as an improvised speech. McLuhan also stresses the linear regularity of the printed page and claims that our long standing exposure to such display has trained us to accept ideas only insofar as they conform to certain strict logical patterns. Gutenberg Man [i.e. Western literate man] ... by allowing himself to become over-disciplined by the closely ranked regiments of text ... has closed his mind to wider possibilities of imaginative expression."

Now while the above may be true enough as an account of how we normally read a light novel, detective story, etc., it certainly does not establish that we are constrained to read in this way. One of the main lessons that needs to be taught in 'beginning' courses in philosophy, indeed in any properly academic discipline, is that reading has to be done in more than this linear and cursory fashion. The need to extract an internally self-consistent argument out of the looseness and ambiguities of ordinary usage demands that mere linearity must be abandoned: passages have to be read and re-read, partial-synonyms may have to be substituted for the words actually given, here and there, in order to avoid inconsistency, re-structuring of some parts of the writing may have to be attempted on an exploratory basis, all to see whether a coherent and consistent position can be stated on the basis of what has been written. Only after this has been done can the substantive questions of philosophy, history, science, education or whatever discipline is in question, be tackled. In other words, the use of writing (whether hand-written, typed or printed script) is essential for the serious study of intelligent or 'intellectual' discourse, wherever clearness, accuracy and comprehensiveness may be required. Legal documents and business memoranda are useful

to us — indeed for complex long-term enterprises they are essential — not just because of their permanence, but also because as a result of their permanence they can be studied 'in depth' (i.e. going beyond or behind their mere linearity) and their possibilities of meaning/interpretations can be thoroughly explored. This is particularly important with regard to scientific documents. As Kuhn (1962: 19ff, 140ff, etc.) points out, 'revolutionary' changes in science have often been argued out in book form, rather than as papers in the learned journals, simply because a revolutionary 'gestalt switch' involves changes in meaning which can be made intelligible only by extended explanations and discussions which often need to be read and re-read, first in one order then in another, changing the interpretations of the key terms only a bit at a time. Kant's famous 'spiral' methodology (used in his three great Critiques as well as in other works) amounts to building the linearity of written text into a three-dimensional 'architectural' structure. And to talk, metaphorically, of a piece of writing as being '3-D' may still be understating its true complexity. If Empson (1930) is correct in discerning seven major types of ambiguity in Western literature, it might be more just to say that great writing is not '3-D', but 7-D. As a general methodological assumption, we may do better to assume, provisionally, that writing is N-dimensional, rather than that it is 1-dimensional or linear.

The relevance of this to education and philosophy is, briefly, that the media used do have very substantial influences on the effectiveness and indeed the type of education, no matter what the 'subject' may be; and that in proportion as the 'subject' involves critical thinking approximating to the activity of philosophy, the written word assumes a greater and greater importance. To put what is largely the same issue in different terms, it may be pointed out that while McLuhan is undoubtedly correct in emphasizing the importance of non-verbal communication which can and usually does accompany oral verbal communication (see Argyle 1975, Hinde 1974, (ed.) 1972; papers in Minnis (ed.) 1971), this may result in 'enrichment' of the total communication in only some dimensions. It might result in impoverishment and even distortions in others. For example, the tone of voice may enrich the message "There's a snake crawling through the window". (It will be enriched also by the posture, dilated pupils of the eyes, hair standing on end, and so on). Whereas a flat adenoidal and monotonous tone may seriously impoverish the message of "Romeo, Romeo! Wherefore art thou, Romeo?" (This particular quotation was in fact used in parody, in the manner indicated, in a radio show in the 1940's). The richness of assoc-

iations of intellectual meanings, e.g. literary associations, is likely to be appreciated fully, on the other hand, only when one has time to read and ponder, come back later and reread perhaps many times, and this is facilitated by the use of the written or printed word. Only one sensory modality may be involved in the actual reading, namely the visual — but the 'richness' of great literature is comprised not by the number of sensory channels used at the time of 'intake' (i.e. reading), rather by the number of intellectual associations, images (which may encompass many sensory modalities)evoked by the reading. 'Richness' interpreted in this way is clearly a function both of the prior experience of the writer, and also of the reader. A reader who is deficient in experiential background and sensitivity is unlikely to 'get much out of' even the greatest literature — though if sensitivity, perceptiveness, intelligence and willingness to read are present, a relative paucity of real-life experience can often, it seems, to a considerable extent be offset by the vicarious experience of participation in literature. This is true also of other media, e.g. film and TV. One essential proviso is, of course, that the world as presented in literature, film and so on, must in its relevant respects be true to life and, within the constraints of the medium, be as extensively true as possible: i.e. 'warts and all', where the warts are relevant.

Quite apart from the richness and multi-dimensionality of the connotations of the written word, there is also the question of accuracy. This is the other side of the 'ambiguity' coin. While polyambiguity, properly handled, generates richness, for many purposes the complete absence of ambiguity is desirable. It is in fact the endeavour to reduce ambiguity, if possible to vanishing point, which makes legal documents, professional papers in philosophy and in some types of science, so long-winded and complicated. Their standard pattern is to make a general statement, and then to go over its various possible interpretations and applications and say which are, and which are not, to be taken as acceptable. As everyone who has had to work with such documents knows, their proper understanding often entails numerous re-readings (thus again exposing the limitations of the 'linearity' view); but the outcome may well be the virtual eradication of ambiguity.

Ambiguity can also be limited by a different strategy, namely by the use of very simple and straightforward statement. "No Parking", "Sign here ...", "Do not use near naked flame", "Speed Limit 50km/hr" — these

would normally be taken as unambiguous. This is so, however, only because a particular context, a particular language-game, is usually assumed by everyone involved. The same forms of words could be used in different contexts, in different language-games, to say something quite different. This is done as the basis of some types of jokes, and involves what Koestler calls the principle of 'bisociation' (Koestler 1964). A statement familiar in one context is put into a different one. To the reader's surprise and satisfaction (this combination is taken as equivalent to funniness, humour), the statement still makes sense, but a different sort of sense, in its new context. (Traffic officer writing out parking ticket for elephant — or pilot of crashed aircraft — sitting in "No Parking" zone).

Much scientific publication is simple and straightforward for a similar reason: writers and readers all share the same common assumptions, they all assign the same meanings to the words used. They are playing the same language-game, and in Kuhn's terms they are all doing 'normal science' within a common paradigm. One interesting possibility here is that though a particular group may all be playing the same language-game, and thus are intelligible to each other, the language-game itself may be faulty so that some of what is said, though intelligible, is not true. Carpenter (1973:14) remarks:

"Newsmen long ago discovered that news could be used as a hook from which to hang prejudices. They rarely reviewed current events or films or books; they merely ornamented opinions with them. For them, reality was an irrelevancy, something best avoided; what mattered was opinions about reality."

Something similar can occur in science, and education. It is possible that mistakes and anomalies are exposed as such, more quickly in the former than the latter. Scientific experiments can usually be replicated by other researchers, and if the original results are not obtained, those who first reported them are discredited. In education, on the other hand, the phenomena of human behaviour are so complex and the interacting systems so large and, in particular, the causal interactions are often so long-term, that deleterious practices can be instituted and can be regarded as acceptable and even good for perhaps several decades, before their harmful effects become apparent. This brings us towards an appreciation of the limitations of what might be called 'mere-factualism'. It is inherent in Carpenter's

message that what seems to be a fact is not necessarily a fact in reality. Even when 'fact' is equated with direct sensory observation — what one can actually see — it needs to be appreciated that whatever one is going to say (and/or even think) is going to be something not identical with the mere seeing/observing. One can say "I can see that the car is travelling at 50 km per hour". But what one really sees is a dial with the pointer near the '50' mark; and to go from this seeing to being able to say what the car's speed is, involves knowing and making inferences and assumptions about the workings of speedometers, the relationship between the speedometer and the radius of the tires, etc., etc.. Sometimes when one says "I can see that the car is going 50 km/hr", one is not wrong. This is of course vitally important. But on other occasions one can look and see, and be wrong. It is therefore necessary to be able to give an account of how one can come to make a mistake, be wrong. In terms of educational practices, for example, it is possible to go into a classroom and see that the pupils are learning what is expected of them — and yet to find out later that they were not really doing so at all. One can even test what the pupils are doing, and show by elaborate techniques that the new educational practice (whatever it may be) is achieving the results claimed for it — and yet be wrong. 'Being wrong' in this sort of context does not entail that the test results, for example, were falsified or in some way different from what they were. Rather, the mistake usually lies in the inferences from test results to concluding that the educational practice is achieving what it was hoped to achieve; and usually the mistake is due to taking an insufficient range of factors into consideration. More on this later. (The 'Hawthorne effect' which is almost ubiquitous in educational innovation is a special case of this: the extra motivation inspired by novelty is not allowed for, and when the new practice is no longer new, motivation declines to its normal level; at which time it can be seen that the innovation was not necessarily better than what had previously been normal, and might even be worse).

This one might be tempted to say, contra Carpenter, that far from being able to hold up opinions about reality to the test of reality itself, we are never able to deal in anything other than opinions about reality. But having said this, we would then have to recognise that:

- (a) To put the issue in terms of 'opinions about reality' is to misformulate the situation. When I look at a red book, in daylight, and say "This book is red", it is misleading to suggest that I am expressing an opinion about the colour of the book. Further discussion of this is offered below.

- (b) Some opinions about reality are better than others, at least in the sense that some can be held, acted upon, expressed, and so on, without generating anomalies; whereas others cannot.

This latter point (b), can be reformulated in what may be an illuminating way by saying not that some 'opinions about reality' are better than others, but that some statements can be fitted into language-games that can be played consistently whereas others either (i) cannot be fitted into language-games, or (ii) the language-games cannot be played consistently without generating anomalies.

Putting the matter in this way, in terms of language-games, enables us to avoid categorizing our statements prematurely as being 'statements of opinion', 'statements of fact', and so on. We can just lump them all as 'statements', and concentrate on the much more important question of determining how, and ultimately whether, they fit into language-games; and which language-games they can fit into.

Returning briefly to (a) above, it must be pointed out that in our everyday language-games (i.e. in ordinary usages), to state something as being a matter of opinion is to imply that its status in relation to reality is open to question or is as yet undetermined. We don't normally say "In my opinion ..." or "I think that ..." about matters which are known (either generally, or even by oneself — provided one does know) to be the case. On the contrary, in many language-games we do state what reality is — and provided that we always have in mind that reality is what we say it is only within the language-game in question, no problems will arise. On most occasions when I say, in daylight and looking at the book, table, or whatever, "The book is red" or "The table is blue", I will not and cannot be shown to have made a mistake. I have learned the appropriate language-games and am using them correctly, there is nothing wrong with my colour vision, and on the present occasion I am seeing the colours of the objects clearly and am in no doubt about what they are. (To the person who suggests that I see, not that the book is red, but light waves of a particularly frequency, I can retort that his position is that of fallacious reductionism. I can refer him to the arguments and considerations advanced below, and the admirable summation of reductionism in general which is given by W.H. Thorpe (1969) :) On some, indeed on many occasions we can say what something really is, and not be wrong — but of course to do so we must always be using a language-game, some language-game or another, we cannot say or even think something about reality without using a

language-game — and by the very fact of using a language-game which is (implicitly at least) based on rules, we open up the possibility of our breaking the rules and thus of saying something wrong. But we could not say something wrong unless it was also possible to say something right.

In one sense, then, it can be seen that Carpenter is correct in suggesting that reality is the ultimate test of what we say and think. But he misleads us to the extent that he seems to suggest that the 'reality test' is always simple and straight forward. His contrasting of 'opinions about reality' with 'reality itself' is illicit. We can never deal with reality except through a language-game. But if we understand well enough the language-games we use, we can sometimes be sure that we have said something about reality that is not wrong. Our language-games and our media of communication can cause us to make errors, but they do not necessarily do so. We cannot communicate without using a medium of communication, but the media do not necessarily distort the message — though they can do so, and we need to be aware of the possibilities of deception and distortion.

How about McLuhan and philosophy? To what extent, and in what ways could the medium have determined or affected the message in philosophy?

Firstly, and from what has been said above, it would appear to follow that careful, extensive and accurate explorations of the various alternative interpretations or meanings of particular terms and/or whole arguments must be facilitated by the use of writing (and especially perhaps by print or typescript). It has come to be regarded as desirable, for conferences in philosophy and in many other academic disciplines as well, that the various papers of the participants be circulated in printed form prior to the discussion sessions. This serves a number of purposes. The participants (including, not least, the author) can peruse the paper carefully and at length. They have time to perceive ambiguities, flaws in the logic of the argument, further implications that have not been mentioned, and the unstated assumptions that lie behind what has actually been written. What the author has said is preserved in objective and lasting form: nobody need waste time, as often happens in spoken discussion, in debating whether or not he did say so-and-so. The discussion sessions can concentrate, therefore, on going beyond the written word, improving, correcting, and extending it.

In the teaching of philosophy, too, the written word is conducive to, indeed it could well be said to be virtually essential to, accuracy and precision of expression. In twelve years of attempting (among other things) to teach philosophy by 'remote control' to extramural classes scattered throughout the length and breadth of New Zealand, I have become acutely conscious of the relative merits of the various media of communication. Contact between teacher and extra mural class begins by the students being sent study guides which tell them which parts of the textbooks to read and in which order. Supplementary shorter readings (e.g. extracts from papers in the learned journals) are also sent out along with the study guides written by the lecturer. The students in turn are asked to write a short essay (about 1000 words) on a topic formulated to set them at variance with some of the major textbook reading. In effect, they are asked to dissect out some of the confusions and ambiguities of a textbook argument. Naturally in what is really a 'beginning' course, many of them do not manage very well. However, the course is structured so that within a fortnight of their submitting their first essays, they come into the university for a week-long residential course, and during this time there is opportunity for them all to be shown effective approaches to the topics in question, and to discuss the whole business of philosophy in general. Also, those with special problems can be seen individually, and given encouragement or admonition etc. as may seem to be required. After the residential course the students disperse for the rest of the year, to do further reading and written work, assisted (so one hopes and sometimes believes) by study guides and by a few off-campus one-day courses, the latter being packed into the few weeks immediately preceding the final examinations.

In comparison with 'internal' students whom one sees every week, the extramural students have a harder time of it with their university studies. (This is quite apart from the fact that most of them are part-timers, with a full-time job of teaching, children-and-housekeeping, and so on, to deal with — I have also had medical practitioners, nursing sisters, nuns, priests and Protestant ministers, social workers, convicted criminals, Teachers College lecturers and others, in the class, over the years). They get immediate feedback only during the residential course and the single-day courses later in the year. Detailed written comments are given on all their three major essays during the year — a heavy work-load on the lecturer running the course, as it entails reading each essay usually several times, working out what assumptions the student was making which generated

the strengths and the weaknesses of his essay, and writing out analytic and constructive comments which one hopes will lead him to understand better what is involved. Most of the extramural students spend considerably more time on their studies, reading more books and papers, agonizing over more writings and re-writings of essays, than their 'internal' counterparts. They suffer more stress, but their results tend to be better. A very large proportion of those who complete the course attain grades of 'B' or better — and this superior performance, while to some extent attributable to superior motivation (to keep studying in the face of difficulties and competing interests), is also to a significant extent attributable to the fact that most of the educational and philosophical transactions of the course are in the written medium.

A justification of this last assertion may best be approached through consideration of the alternative and more usual media of education.

In face-to-face teaching, whether one-to-one or one-to-many, the 'message' transmitted may be 'richer' in McLuhan's sense — on that, besides hearing the words spoken, the recipients also hear the tone of voice, modulations of tone and speaking rhythm etc., and can see the changing facial expressions, gestures, and bodily postures of the teacher. These extra dimensions may and indeed should be used by the teacher to enhance the message he wishes to transmit. Histrionic skills can be used to achieve improved educational 'impact'. Emphasis can be obtained by either raising or lowering the voice, by a judiciously timed pause, even by a deliberate change in the timbre of the voice, as well as by gesture, posture, and so on. But 'richness' in this sense, besides being logically distinct from richness in the more intellectual and literary sense, may actually be antagonistic to it and to educational effectiveness. Voice and gesture may simply distract attention from what is being said. They may give an appearance of profundity to what is merely trivial; or the richness and smoothness of the voice may make everyone go to sleep ...

It has been fashionable to decry lecturing. Much lecturing is indeed pointless, dull, and maladaptive — especially when the lecturer is simply paraphrasing one of the textbooks, as is often the case. But a lecturer who understands his job can perform valuable services for his students. Even if he gives them nothing apart from what they could have got themselves from reading, he should be able to give it to them quicker than they could get it themselves. In an hours' lecture can be compressed the essence of twenty hours' reading. And of course the lecture can do

much more than simply transmit in condensed form the information from textbooks. It can compare and contrast the accounts given by different authorities and can provide in itself an exemplification of how the students can go about the business of evaluating the more significant from the less significant issues. Even if the largeness of the student audience makes it impracticable for them to participate actively in dialogue with the lecturer (and in my opinion it is sometimes better to separate the different educational transactions: questions can be asked and dialectical skills exercised by the students during separate small-group tutorials, rather than during the course of a lecture especially if several hundred students are involved), it is still possible for the lecturer to gauge the effectiveness of his performance on a moment-by-moment basis and to modify his presentation according to what he perceives to be its reception by the audience.

Many valuable skills can actually be learned and practised during the course of listening to a lecture, notably that of making a running analysis, precis, and evaluation of what is being said. Would that a larger proportion of people had the habit of doing this when listening to all public utterances, political speeches, etc., in these democracies of ours! But it must also be appreciated that, even when much of the material of a course has been given to students in the form of lectures, a great deal of the real work of the course is still done upon the written rather than the spoken word: viz., the lecture-notes made by the student. Again, it is a matter of their permanence ^{allowing} in-depth exploration of precise alternatives of meaning.

One of the possible strengths of the face-to-face situation is also potentially one of its dangers. The student can ask and receive help and clarification — but a request for help can elicit, not the friendly clarification that is needed, but a oneupmanship put-down from the lecturer (and perhaps public humiliation in front of the other students). 'Oneupmanship' is the term coined and made widely known by Stephen Potter (1948): it refers to what would nowadays be described, in the new and rapidly growing science of human ethology, as an instinctual social-dominance contest (see Eibl-Eibesfeldt 1970, Morris 1977). The basis of social-dominance or 'peck-order' contests is instinctual, not intellectual or intelligent (though intelligence can be used for social-dominance purposes — this involves what Kant would have called 'heteronomy of the will', the 'will' or intelligence being used in the service of the instincts). I have discussed

elsewhere (Stenhouse 1974, 1977) the problem for a teacher of dealing with a social-dominance challenge on the part of the students: in brief, the teacher must make sure of winning the social-dominance contest and retaining the alpha position, but must then use his/her dominance to turn the activities of the class (even any further peck-order challenges) towards the intelligence/intellectual rather than the instinctual dimension. But there is no guarantee that a teacher will even try to do this, let alone that he/she will be able to continue doing it successfully and here lie many dangers. The 'richness' and immediacy which McLuhan discerns in face-to-face situations may be used not to enhance the value of an educational transaction but to degrade and distort it. Sarcasm is an example, often, of intelligence being used in the service of the peck-order instincts, and it can be devastating for the pupil. Quite apart from (and additional to) the possibility that both teachers and pupils may use the characteristics of the 'rich' face-to-face media for purposes which are antithetical to education, there is the general possibility that the mere sub-optimal use of the educational situation — due to its very 'richness' and complexity, which may give teachers and/or pupils too much to cope with — may cause pupils (and perhaps also teachers) to turn away from education altogether. There are signs that this does happen, not too infrequently, alas!; and as Passmore (1967) remarks, the question of whether or not a particular effect is intended is irrelevant, what matters is what actually happens.

One of the most significant issues that emerges, with regard to education, from consideration of McLuhan's general thesis and his classification of media is that, for the 'hot' and 'rich' media especially, the quality of the individual educator becomes paramount. It seems impossible to over-emphasize the importance of the individual teacher. A good teacher can use the multi-dimensionality and the immediacy of feedback (especially subliminal feedback) of the face-to-face situation to both stimulate and satisfy the educational needs of many students simultaneously. But some teachers are unable for various reasons — not necessarily discreditable to them individually — to cope with the richness, complexity and, especially with some forms of it, the immediacy of feedback from students. The educational language-games in which they are asked or allowed to participate may therefore need to be limited. Much greater recognition needs to be given to the question of individual differences between teachers. Individual variation in students has attracted massive attention in educational research for many decades past (whether it has resulted in adequate account

being taken of individual differences in actual practice is another question, one which would probably have to be answered in the negative) — but educators tend to be treated in terms of a 'silk purses out of sow's ears' principle: virtually anyone can be made into a teacher provided he/she receives three years of teacher training. The differences between teachers are therefore recognized only for the purposes of promotion, hardly at all for the purpose of attempting to match individual teacher style to the individual needs of pupils. (A significant though still small research effort is now being directed towards assessment of individual differences among educators, see Hudson 1968, Campbell 1971, Stenhouse 1974, Chap. 10, 1977, as a few samples).

The criteria of 'effectiveness', 'goodness', etc., of educators/teachers depend on the aims of education. These are multiple, as is generally recognized, and so are the criteria of 'good teachers'. One dimension within which criteria vary, and which has not been fully explored as yet, is that of intelligence/instinctuality of educational transactions (see Stenhouse 1977); and another, related to the first in various ways, is that of the media used. This is not the place to attempt the comprehensive exploration of these dimensions which is needed (a more extended discussion is in preparation), but a brief sketch may be useful in amplifying some relevant aspects of the language-game concept.

In general, it can be said that the 'hotter' the medium, i.e. the more senses that are involved simultaneously, the more are instinctual mechanisms involved in the educational transaction. It might be tempting to suggest that this is likely to reduce the amount of learning and intelligence that are needed — but assumptions along those lines are dangerously simplistic. Certainly there are occasions when the overwhelming abundance of instinctual 'releasing' stimuli (Tinbergen, 1951) makes it very difficult to exercise intelligence. Demagogues are notoriously adept at using stimuli for group cohesiveness (the so-called 'herd instincts', 'mob psychology') to swamp any attempt at rational and hence possibly critical appraisal of their programmes: Hitler at the Nuremberg Rally, with uniforms, massed bands, and repeated ritualized gestures, is merely one of the more obvious and better-known (and nowadays 'safe') examples which may be cited. Even in such situations, however, it is not absolutely impossible to exercise individual intelligence — whether it is wise to give any outward indications of having done so is, of course, another matter! In educational settings, the use of group cohesive instinctual mechanisms is

usually less extreme. School assemblies may sometimes be Nuremberg Rallies in miniature, but instinctual social dominance mechanisms need not be antithetical to learning and intelligence, they can also be used (as argued in Stenhouse 1977 and in press) to stimulate learning. This depends on the individual teacher. In lecturing to large groups, effective social dominance is an asset — but it may be exercised in a variety of ways, some better than others. Social dominance cannot be equated with a loud voice, domineering manner, and a habit of 'putting down' the audience either individually or collectively. A quiet manner, demonstration of care for one's students and of competence with regard to one's subject-matter and how to teach it — the latter including keeping control of the social group — these may be more effective, if one's aim is to generate the habit of intelligence in one's students, than all the impressive tricks of a more showy 'dominance' and salesmanship.

Comfort, acceptance, and reassurance are also communicated largely by instinctual means, and often are essential to the process of educating. Here again there is no necessary antagonism between instinct and the more 'intellectual' aspects of education. Some children at some times need just reassurance from their teachers. It is desirable, however, to understand precisely what is and what is not involved in this. Mere permissiveness and softness will not do. The pupil needs to perceive that he/she matters enough to the teacher so that the latter is trying to 'improve' the pupil's conduct and performance ('improve' permits of a variety of interpretations) — permissiveness by contrast can be interpreted, correctly in many cases, as a sign that the teacher 'couldn't care less'.

So the language-games of education do include the unspoken language of affectional/instinctual interactions, and teachers who are good at these non-verbal communications are vitally important especially with young and/or 'disturbed' children. But teachers who are not highly effective at non-verbal communication and social dominance can be of great value at the more strictly 'intellectual' end of the scale — provided that they really are intelligent enough!

CHAPTER ELEVEN

SOME RAMIFICATIONS OF WITTGENSTEIN'S 'LANGUAGE-GAME' CONCEPT.

From the point of view of philosophy, it might seem to some people that discussion of some of the topics touched upon earlier in this thesis, e.g. non-verbal communication, must be totally irrelevant. Certainly traditional philosophy took no account of such matters. But a major part of the twentieth-century revolution in philosophy has involved a swing towards investigation of ordinary language; and it is in some ways merely a continuation of such a swing to consider ordinary language not merely as it is written, but also as it is spoken.

It is true, as was emphasized earlier, that we can do philosophy better, probably, on the written word than the spoken; also that philosophy probably originated in association with the development of writing. It might seem, therefore, that colloquial spoken ordinary language, with all its changing fads and fashions, should not be regarded as appropriate to the study of philosophy until after it had achieved stability by being written down. To the extent that this view would tend towards the lowering of barriers between the study of philosophy and, say, the study of contemporary literature, I would agree with the positive message it seems to contain: let us not keep philosophy and contemporary creative writing in separate compartments. But to the extent that a negative message is also given, e.g. that it is in some way improper or illicit for philosophy to take account of current spoken usages and their associated context of non-verbal communication (indeed their context in total) — this I reject, on a number of grounds.

In the first place, it imposes a backward-looking orientation on philosophy, something like the 'spectatorial' character which the London school have wished upon educational philosophy. Philosophy would have to wait until someone had written down the current usages, before they could be taken into consideration. If philosophy itself is to be kept 'exclusivist' yet still take cognizance of ordinary usages, even if only to provide starting-points for argument ... but even to state this is to expose its absurdity. If philosophy is to appeal to ordinary usages at all, it cannot in any strict sense be exclusivist, philosophical language-games cannot be separated off from ordinary-life language-games (or science l-gs, or teaching l-gs ...). (In this connection, it is interesting to note that there is a tendency for many social scientists (and perhaps others as well) to refuse to accept anything as 'evidence' unless it has been written down and

published. What they see every day is not 'evidence' — in some peculiar way it is not real. For example, I have heard academic colleagues deploring at great length the widespread inability of their students to write legibly, spell, write intelligible English, and do elementary mathematics; then, within minutes, virtually in the next breath, they are attacking the 'Back to Basics' movement and insisting that all is well in the schools, that published research on attainment levels shows no inadequacy of academic standards!

Secondly, while there is much to be gained by lowering the boundaries between philosophy and other fields of study (e.g. one's understanding of moral problems can be enhanced by reading suitable literary works, as well as works in ethics), full understanding of the printed word is in part dependent, as was argued in Chapter 10, upon the extent of the reader's own firsthand experience of real life. This applies to philosophy as to any other field. It is not suggested that there is a simple quantitative relationship between the extent and/or variety of experience, and the 'amount' of understanding of which the reader is capable. Some persons can achieve startling insight on the basis of very little direct experience. But some direct experience is always necessary. And much direct experience is constituted by the language-games of ordinary everyday life, supplemented by the more specialized l-gs of particular occupations, types of experience, etc.. Since most of these language-games, and especially those of everyday social experience, involve elements which are non-verbal, the better we understand the non-verbal elements the better we shall understand the totality of our experience. Not that experience has to be verbalized in order to be understood — but if our understanding of experience is to be communicated to others, discussed, compared with that of other people and understood better as a result of such discussion and comparison, etc., then it does have to be (mostly) verbalized. (Experience can be communicated very powerfully by non-verbal means, e.g. mime; but few can approach the effectiveness of Marcel Marceau in this medium.) Furthermore, verbal statement can bring to our attention aspects of non-verbal behaviour of which we would otherwise have been quite unaware, and can thus increase the range of phenomena encompassed by our awareness. This can, therefore, increase the richness and breadth of our experience itself. (All this can be said without violating the distinction between 'talking a good game' on the one hand, and 'playing a good game' on the other. It is true that for many people, talk seems to be a substitute for experience, indeed some people use talk as a way of avoiding experience: in ethological terms, talk can be a

substitute or displacement activity. It is also true that talk about an activity can reduce one's competence in and perhaps also one's enjoyment of the activity, e.g. tennis, sex; but still, the contrary remains true, and it is difficult to see how the range of human experience and achievement could have reached their present scope in the absence^{of talk} to direct, focus, and sometimes transform our attention.)

This brings us to the third point, that better understanding of all aspects of our language-games, both the verbal and the non-verbal components, must be helpful with regard to the teaching of philosophy. Non-verbal communication does not come, presumably, into philosophical activities conducted entirely in writing, but it certainly comes into most teaching, and also into all face-to-face oral philosophizing. It should be helpful to both students and teachers of philosophy, then, for them all to have a better understanding of the multiplicity of interactions between verbal and non-verbal communication. For example, it is quite common at philosophy congresses for some individuals to 'put down' others by the use of non-verbal social dominance displays. A common gambit is for a member of the audience to ask, with suitable 'down putting' display (an amused and condescending tone of voice, drawl, slowly removing pipe from mouth, or other display according to habitual role): "Your paper is really most interesting, but what real difference does it all make ...?" A large number of different messages may be included in this. An obvious one is that the paper was not interesting at all, otherwise why both assert that it was interesting and query what its point was? Another is that the person who presented the paper is not as intelligent, or not as good at philosophy, as the questioner — otherwise how could the question be asked? — but this message is left tacit, so that the rules of good manners will preclude an openly agonistic response (i.e. a counter-attack). Third and most basic, the question as it stands is committing the fallacy of 'many questions': compare it with "Answer Yes or No: have you given up the habit of shoplifting?". It is not so specific, hence not so clearly a trick, as the shoplifting question; nevertheless its logic is similar, in that by being vague and generalized it is both asking any number of questions, and not really asking a question at all. It only appears to ask a question. If the person to whom the question was directed hesitates in trying to sort out what the question might mean, he can easily seem to be at a loss for an answer. He can seem to have been exposed as incompetent; and if he stumbles around trying to look for an answer to one of the questions he thinks may have been intended, the questioner can cut in and chide him for

not answering one of the other questions. So the poor fellow looks evasive as well as stupid. The alternative, that of trying to reveal the true nature of the question that was asked, can easily make the speaker seem both evasive and aggressive — this is, however, the better horn of the dilemma to take, in my opinion, provided one can 'keep one's cool' and appear friendly and relaxed while quickly exposing several of the different issues which have been conflated, i.e. exposing the question as 'many questions'. Depending on the company and the status of the several individuals, it could be feasible actually to ask a 'many questions' question back to the questioner, facetiously, to bring the logical issue sharply to people's attention. This could be hazardous, though. Some elementary logic texts give as an example of the 'many questions' fallacy: "Have you left off beating your wife yet?" The trouble about using this one in the situation outlined above is that men who specialize in 'putting down' their colleagues (and usually also their students) often do beat their wives, or get beaten by them, and they tend to get nasty if this is alluded to ...

The sort of 'putting down' referred to above is less likely to occur, or to be effective, if everyone present knows the ethology involved. They are thus likely to be able to distinguish between instinctual social-dominance transactions on the one hand, and those of intelligence on the other, no matter how thoroughly the two are mixed. Thus an understanding of human ethology can be useful in philosophy, especially in the teaching thereof. Two provisos must be kept in mind, however. One is that knowing about human ethology, oneupmanship tricks and 'the games people play' does not deter people, necessarily, from continuing to play them. The other is, that good philosophers and good teachers have always been able to distinguish between intelligence, no matter how inept-seeming, and the window-dressing of confidence in a social-dominance display; and they have known how to encourage the one and discourage the other, even when they would not have been able to give precisely these ethological labels, these words, to what they were dealing with.

This last point might seem to give an opening for someone to query the usefulness of trying to educate people in human ethology, teaching methods, and so on, at all. If good teaching has been done in the past, before any of these new behavioural sciences were developed, it might be argued, then knowledge of them cannot be indispensable for good teaching now. This argument can certainly be accepted, up to a point — but it cannot be taken as applying generally, for several good reasons. It is true that there have been

untrained but nevertheless gifted and effective teachers in the past. Such teachers can still be found today, doing just as good a job as any in the past. But what has to be recognized is that today, as distinct from all but the very recent past, a very much larger proportion of the adult work-force has to be employed in teaching jobs of one type or another, simply because a larger and increasing proportion of people are in need of being educated; and we can no longer afford to rely on the small minority of gifted 'natural' teachers, but must endeavour to increase the teaching effectiveness of the large numbers of persons who are going to be involved in teaching, willy nilly, and whose teaching skills can be improved if they are given the right sort of education and training. This leads us to the second main point, a particularization from what has already been mentioned in the earlier part of this Chapter: that teaching people the language-games of a new science (e.g. human ethology) is likely to bring to their notice aspects of reality of which they would otherwise have remained unaware; and from the mere fact of being aware of a wider range of phenomena, e.g. hitherto unnoticed aspects of behaviour,

→ they are in a better position to improve the adaptiveness of their own behaviour, irrespective of whether or not they accept or even understand the full theoretical background of the science or other discipline in question. It is true that, as the proverb says, a little knowledge is a dangerous thing. But the safeguard against this danger is not to remain in ignorance, or to remain in the 'little knowledge' state: the safeguard is, to increase one's knowledge.

Earlier in this Chapter we have skimmed past the question of just how important verbal communication is, as compared to non-verbal. One of our only partly explicit assumptions was that, irrespective of whether it was spoken or written, philosophy must certainly be verbal. This brings us to one of the most intriguing issues in modern philosophy:

Wittgenstein is generally credited with leading two revolutions in philosophy during his lifetime. The Tractatus Logico-Philosophicus (1922) became a classis of Logical Positivism (see Ayer 1959); while the posthumous Philosophical Investigations (1953) is certainly the most influential work in 'linguistic philosophy'. The latter is generally taken to have involved the repudiation, at least in part, of the former. A paper by Toulmin (1969), however, has raised important questions and doubts about the accepted view of Wittgenstein. Some of these appear to have been taken up by the authors of several subsequent books (e.g. Pears 1971, Kermey 1973); and seem

to me to be very much worth exploring for their implications for philosophy in general and for education, as much or more than for the new light they may shed on Wittgenstein.

To give a very brief resume of what appears to be the accepted view of Wittgenstein's life and philosophical work, we might say that, after an early foray into physics and applied mathematics, a developing interest in the fundamental bases of mathematics drew him to study under Bertrand Russell at Cambridge. This developed into work in formal and symbolic logic (Whitehead and Russell had recently published their Principia Mathematica) and investigation of the philosophical background to mathematics. Seeing the symbol systems of mathematics and formal logic as examples of how the ambiguities, deceptiveness and general 'messiness' of ordinary language could be clarified and purified, he then set out to provide a general and programmatic guidebook which would allow for similar clarification and purification of all language. This was eventually published as the Tractatus. Wittgenstein then abandoned philosophy for a number of years, and instead worked as a schoolteacher and then as an architect. Late in the 1920s he was lured back into philosophy by various friends, went to Cambridge where the Tractatus earned him a PhD, taught philosophy at Cambridge, succeeded G.E. Moore in the Chair of Philosophy in 1939, resigned the Chair in 1947, and died of cancer in 1951 having published virtually nothing since 1922. Von Wright (1958: 14) notes that: "While he was a schoolmaster he published a German glossary for elementary schools ..." in 1926. In 1929 he wrote a paper "Some remarks on logical form", published in Copi and Beard (1966). In his Biographical Sketch (1958) von Wright states:

"In about 1933 a radical change took place in Wittgenstein's thinking. There came to him at this time those basic ideas whose development and clarification absorbed him for the rest of his life. ... Wittgenstein's 'new' philosophy entails the rejection of some of the fundamental thoughts of the Tractatus. He abandoned the picture-theory of language, the doctrine that all significant propositions are truth-functions of elementary propositions, and the doctrine of the unspeakable. One may say that some of these thoughts had anyway been superseded by philosophical developments after the publication of the Tractatus. But whereas the changes in other quarters had largely consisted in a further development of existing philosophic themes, provided not least of all by Wittgenstein's own early work, the alterations in Wittgenstein's thinking signaled a radical departure from the then existing paths of philosophy." (pp 14-15).

The products of this second phase in Wittgenstein's philosophical

development were not published until after his death, the earliest, best-known, and most important of these being the Philosophical Investigations (1953).

Perhaps the most significant statement Wittgenstein ever made was the last sentence of the Tractatus:

"Wovon man nicht sprechen kann, darüber muss man schweigen."

"About that of which we cannot speak, we must keep silent."

Anscombe (1957: 18) gives "What we cannot speak of, we must be silent about" as the translation of the last of the seven "main propositions" being argued in the Tractatus. Later, p.173, she quotes Wittgenstein's own Introduction: "The whole meaning of the book could perhaps be summed up as follows: What can be said at all can be said clearly, and what cannot be spoken of we must be silent about."

These and other statements were seized upon by logical positivists, and later by positivistic scientists and a wider public, and taken as a complete rejection of all metaphysics (which for many would also have included a great deal, possibly the bulk, of traditional epistemology). This positivistic trend is strong now in the anti-theoretical attitudes of many professional scientists, and perhaps could be regarded as reaching its extreme expression in the straightout reductionistic position taken by many scientists especially in their presentations of science to the lay public. More on this later.

Returning to the question of positivism in relation to Wittgenstein, one can see how statements of the sort quoted could be used to bolster positivistic doctrines such as the Principle of Verification (according to which every proposition was either 'empirically verifiable', or logically tautologous, or nonsense — 'empirically verifiable' has been put in quotes, as standing for a much more complex final formulation evolved from the original 'knowing the meaning of a proposition is knowing its method of verification'). As a generalization, it could be said that the intent of the positivists was to preserve science and mathematics and the formal logic on which these seemed to depend, and to throw out metaphysics and speculative philosophy in general. Their devotion to science led gradually to the substitution of 'logical empiricism' as a label for the movement, in place of 'logical positivism'. (It may be noted how closely intertwined, in this line of historical development which is still going on, are the twin themes of this thesis: philosophy and science.)

Associated with the 'doctrine of the unsayable' were two other Wittgensteinian doctrines of logic-and-language which were taken as assimilable to the positivistic position, but which on closer examination throw doubt on the whole question of a positivistic interpretation of the Tractatus and certainly on such an interpretation of Wittgenstein's overall position. These doctrines are:

- (a) That propositions in some way 'picture' or 'mirror' the reality to which they refer; and
- (b) That some things cannot be said ("gesagt") but only 'indicated', 'pointed to' ("gezeigt").

If we assume, with the logical empiricists/positivists, that the only things that are important, significant, are those that can be stated, and in principle if not always in practice stated clearly, then it would appear to follow that what cannot be said (stated) cannot be important. The similarity of this to the view of 'scientific evidence' mentioned earlier ("If it hasn't been published then it isn't evidence" — even though it may in fact be a matter of common knowledge and everyday observation) is no doubt fairly obvious. And it is only a small step to the 'scientism' which is akin to magic, according to which, if it cannot be observed and measured, it doesn't really exist. (Apparently the Pythagorean doctrine, "All things are numbers", was used as a magical formula, or as a basis for magic, by the later Pythagoreans.)

In working our way towards an assessment of the proper interpretation of the 'doctrine of the unsayable', one step can be taken with confidence: the 'picture' theory of the proposition (a) above, to whatever extent Wittgenstein had ever really held it, was definitely repudiated by him. In Chapter 6 we showed grounds for rejecting a 'picture' theory of meaning as applied to words and/or concepts. As Stenius (1964: 137) emphasizes, in the Tractatus Wittgenstein argued a 'picture theory' as applied to propositions not to single words on their own — nevertheless we can, I suggest, accept Toulmin's view of the definite repudiation of the 'picture theory' however applied: "... his earlier account (in the Tractatus) of the scope and limits of language ... had been given in terms of a "picturing" relation which (as he saw now all too clearly) had been at best a helpful metaphor." (Toulmin 1969: 67 see also Anscombe 1967: 14.). As Wittgenstein himself said in the Philosophical Investigations, section 23:

"It is interesting to compare the multiplicity of the tools in language and of the ways they are used, the multiplicity of kinds of word and sentence, with what logicians have said about the structure of language. (Including the author of the Tractatus Logico-Philosophicus.)"

That some things cannot be said but only 'indicated', 'pointed to' is itself, when affirmed as a positive doctrine, (b) above, tantamount to a denial that what cannot be said is valueless, non-significant. If valueless, why point to it? (Going further, if 'valueless/non-significant' tends towards 'non-existent', we cannot point to what is non-existent.) The Tractatus treatment of logical negation in terms of 'logical form' or 'logical space', using Frege's symbols of quantification to make some of its arguments, is most important (see Anscombe 1967, esp. Chapters 3 and 4) — but this is really an explanation of how a statement like 'Such-and-such is not the case' can be meaningful, and can be understood by a hearer or reader who has never observed or experienced 'such-and-such' (for the good reason that there is no such-and-such), and can be known to be true. A statement like "Unicorns do not live in Africa" could be a useful example here. Miss Anscombe herself quotes (p.25) a lengthy passage from Popper (1957), to reject the empiricist/positivistic interpretation of the Tractatus which Popper gives and which, as she notes, is "... the most common view of the Tractatus".

What we are left with, then, as regards the 'saying' as against 'pointing to' doctrine, is that, while some things can be 'said', i.e. stated to be the case, and described, etc. so that the statements, descriptions, and so on are at least sometimes true (though they can also be false), there are also other 'things' which cannot be 'said' (stated, described, etc.) but which are not less real or less important on this account. They can be indicated, pointed to, alluded to ... This doctrine is developed fully in the Investigations, indeed in some ways it is fundamental to the 'new' Wittgenstein — but even with regard to the Tractatus, Anscombe says (p.162): "But an important part is played in the Tractatus by the things which, though they cannot be 'said', are yet 'shown' or 'displayed'." What is from some points of view the most important example, or class of examples, of 'things that cannot be said but can only be shown', is in the Investigations clearly the "ostensive learning" of the meanings of words for objects, actions, etc., (as discussed earlier, see Chapter 6). Although I feel that it is doubtful that Miss Anscombe would agree with Toulmin's contention (1969: 60) that "There were not "two Wittgensteins", having different philosophical questions and concerns — the author of the Tractatus, and the author of the Investigations", she does nevertheless show that at least the basic presuppositions of the Tractatus are very much compatible with the doctrines of the Investigations, probably very much more compatible than most people would have realized. For example, she says:

"Of all the things that are unsayably 'shown', the most prominent in the Tractatus is this 'logic of the world' or 'of the facts'. 'My most fundamental thought is this: logical constants are not proxies for anything. The logic of the facts cannot have anything going proxy for it' (4.0342). (The reference is to the Tractatus.) Here he is contrasting logical constants with names, which 'go proxy' for their objects: 'The possibility of sentences', he has just said, 'rests upon the principle of signs as going proxy for objects' — and what this principle in turn amounts to is the possibility of logical picturing through one fact's having the same logical form as another — for only in the context of the proposition will a sign go proxy for an object.

"... if asked to explain the composition of the simplest statement, we say that this word means, or refers to, such-and-such, and this one means such-and-such, and together they mean that ... (or: someone who puts them together makes the statement that ...) —and there follows just such another composition of signs as we were trying to explain." (pp163-4).

These passages on their own may still be somewhat obscure; nevertheless it seems fairly clear that, especially in the second paragraph, what is being objected to is the ultimate and unavoidable circularity of verbal definitions/explanations on their own. Synge (1951, Chapter 1) makes the same point in a rather different way. The only way out of the circularity is to leave at least some words undefined, i.e. not given verbal definition or explanation; and this means that they can be learned only ostensively, i.e. by being 'shown' or 'indicated'. (Again see Chapter 6, above.) Part I of the Investigations deals at great length with many issues most of them relating in one way or another to the basic division between what can be said and what has to be shown: the concept of the 'language-game' as outlined earlier may be taken as a summation of (and to some extent perhaps an explication of, and even an extrapolation from) the doctrine of the Philosophical Investigations. That 'what cannot be said but can only be shown, pointed to' is understressed in the Tractatus, even though its importance was, it seems, fully recognized, can probably be best explained in two distinct but related stages:

- (i) That Wittgenstein himself worked from assumptions about what was in need of explanation, and what on the other hand was obvious enough not to need much explaining, which were different from those of many of his readers — thus the first stage must be the explication of what his assumptions really were.
- (ii) That Wittgenstein, in writing the Tractatus, failed to appreciate the assumptions which people were likely to bring to the reading of it.

It appears that there are very substantial differences in what is seen as Wittgenstein's philosophical background. Anscombe (1967: 12) mentions only Russell, Frege, and Schopenhauer. Toulmin (1969) argues for strong though not always explicitly acknowledged influences from Hertz, Belzmann, Tolstoy, Kant and Kierkegaard, in addition to those mentioned by Anscombe. Toulmin's case appears to me to be a convincing one, even if not conclusive on all the issues touched upon (he himself remarks on the need for more extensive explorations of the intellectual history especially of Vienna in the later nineteenth and early twentieth centuries). His summary of Wittgenstein's career is as follows:

"Ludwig Wittgenstein entered philosophy with both intellectual and ethico-religious preoccupations: the former derived from the transcendental enquiries of Kant and Schopenhauer, the latter inherited from Kierkegaard and kept alive by Tolstoy. The two groups of preoccupations together focused his attention on the scope and limits of linguistic expression; and his concern with this problem took four successive forms.

"First, as a young student of applied mathematics, he hoped to solve this "transcendental" problem by generalizing the ideas of Herz and Boltzmann. Next, he found in the new logic of Frege and Russell an instrument — and a symbolism — with the help of which (he believed) one could demonstrate the scope and limits of language-in-general. The outcome of this attempt was his Tractatus Logico-Philosophicus.

"Returning to philosophy after a break of some years, Wittgenstein had second thoughts about the Tractatus. Even in mathematics (he now saw) the deeper problems require one to consider not the inner articulations of mathematical calculi, but rather the rule-conforming behaviour by which such calculi acquire some external relevance. ... Finally, at Cambridge, in a philosophical situation dominated by G.E. Moore, he generalized his analysis yet again, with the aim of demonstrating how the meaning, scope, and limits of any symbolic representation — linguistic as much as mathematical — depend on the relations by which men link it to a wider behavioural context.

"The "meaning" of any utterance was thus determined, for the later Wittgenstein, by the rule-conforming symbolic activities ("language-games") within which the expressions in question are conventionally used; and these symbolic activities in turn drew their significance from the broader patterns of activities (or "forms of life") of which they were a constituent element. The final solution of Wittgenstein's initial, transcendental problem then consisted in coming to recognize all the multifarious ways in which "forms of life" create a context for "language-games", and how these in turn delimit the scope and boundaries of the sayable." (p.69).

It would appear reasonable to suggest, on the basis of the above, that even in the Tractatus, Wittgenstein was bringing to his philosophical

work much more of a 'Continental' background, especially a broadly Kantian background as modified by Schopenhauer, than his English readers (at least) would expect. And most significant among his English readers, as it appears, was Russell. Russell's interpretation of the Tractatus, especially as this was embodied in his Introduction to it, seems to have been crucial. Before we move to consider this in more detail, however, it may be desirable to re-emphasize the differentness between (i) and (ii) above, viz. the unstated assumptions which Wittgenstein brought to the writing of the Tractatus, and the assumptions which his readers brought to the reading of it. As Toulmin says, "... both in Cambridge and in Vienna, the Tractatus Logico-Philosophicus was initially read as elaborating the very same theory of "logical atomism" as had been stated in Russell's Our Knowledge of the External World." (p.61). The British tradition of empiricism on the one hand, and the recent and continuing successes of mathematical physics, mathematics, formal/symbolic logic, and indeed science in general, on the other, all conspired to encourage a 'logical empiricist' interpretation of the Tractatus — once the initial impetus in this direction had been given.

Exactly this sort of impetus was provided by the Introduction to the Tractatus, which was written by Bertrand Russell.

It might at first seem difficult to accept that the basic intent and argument of a book could be completely misunderstood by a close friend and sometime tutor of the author (even granted that they had been out of touch with each other during World War I), and then misrepresented to the reading public who in this case were mainly professional philosophers, or professional scientists with philosophical interests. Yet there seems to be no real doubt that this is what happened. Even von Wright (1958: 11) in his Biographical Sketch wrote that "The problem of finding a publisher caused difficulties, and the matter was further complicated by Wittgenstein's strong disapproval of Russell's introduction to the book. Finally, Wittgenstein turned his back on the whole undertaking."

As Toulmin (1969: 60) points out, "Once Wittgenstein had been labelled as a positivist, men found it hard to see him in any other light. So when, from 1929 on, he returned to philosophy and moved gradually into his second, contrasted phase of philosophising, his new style was not regarded as a rejection of positivism: rather, it was seen as a reconstruction of his earlier positivistic position on new and deeper foundations."

Since Wittgenstein has, from say 1960 onward, become very widely

influential, and since his influence appears to be increasing at an accelerating rate, it becomes more and more desirable that his true position should be very clearly understood. It is evident that Russell's Introduction to the Tractatus had significant effects on how that work, and through it even Wittgenstein's later philosophy, has been interpreted.

Without going into questions of detailed interpretation at this point, I feel it is worthwhile to look at circumstantial evidence in Russell's (1975) autobiography for their (admittedly indirect) relevance.

In the first place, if in the Introduction to the Tractatus Russell had unwittingly misrepresented the views of an associate, it would not have been for the first time. He and A.N. Whitehead had collaborated, in the early years of the century, in writing the monumental Principia Mathematica (1910-13). They remained close friends until 1917. The following excerpt from Russell's autobiography (1975: 306) seems self-explanatory:

"From A.N. Whitehead ... Jan. 8th, 17.

Dear Bertie,

I am awfully sorry, but you do not seem to appreciate my point.

I don't want my ideas propagated at present either under my name or anybody else's — that is to say, as far as they are at present on paper. The result will be a completely misleading exposition which will inevitably queer the pitch for the final exposition when I want to put it out.

My ideas and methods grow in a different way to yours and the period of incubation is long and the result attains its intelligible form in the final stage, — I do not want you to have my notes which in chapters are lucid, to precipitate them into what I should consider as a series of half-truths. I have worked at these ideas off and on for all my life, and should be left quite bare on one side of my speculative existence if I handed them over to some one else to elaborate. Now that I begin to see daylight, I do not feel justified or necessitated by any view of scientific advantage in so doing.

I am sorry that you do not feel able to get to work except by the help of these notes — but I am sure that you must be mistaken in this, and that there must be the whole of the remaining field of thought for you to get to work on — though naturally it would be easier for you to get into harness with some formed notes to go on. But my reasons are conclusive. I will send the work around to you naturally, when I have got it into the form which expresses my ideas.

Yours affectly

Alfred N. Whitehead"

"Before the war started [says Russell] Whitehead had made some notes on our knowledge of the external world and I had written a book on this subject in which I made use with due acknowledgment of ideas that Whitehead had passed on to me. The above letter shows that this had vexed him. In fact, it put an end to our collaboration." (Russell, 1975: 307).

If Russell had misrepresented Whitehead, as the latter seems to have felt so strongly, it seems even more likely that he could have misunderstood and hence misrepresented Wittgenstein. The extent and degree of his association with Wittgenstein was much less; and in fact what was later published as the Tractatus Logico-Philosophicus was written during World War I while communication between the author and Russell was quite cut off. Both Russell and Wittgenstein give reiterated emphasis to the difficulties of understanding the material which was to form the Tractatus.

On 13.3.19 Wittgenstein wrote to Russell:

"I've written a book called Logisch-Philosophische Abhandlung containing all my work for the last 6 years ... you would not understand it without a previous explanation as it's written in quite short remarks. (This of cours^x means that nobody will understand it; although I believe it's all as clear as crystall. But it upsets all our theory of truth, of classes, of numbers and all the rest" (Russell, 1975: 348)

A little later (12.6.19) Wittgenstein wrote from Italy:

"... your book has arrived here safely and I now feel a great need to write you a number of things. — I should never have believed that what I dictated to Moore in Norway six years ago would pass over you so completely without trace. In short, I am afraid it might be very difficult for me to reach an understanding with you. And my small remaining hope that my manuscript would convey something to you has now quite vanished ... Kindest regards, and don't suppose that everything that you wont be able to understand is a piece of stupidity!

Yours ever

Ludwig Wittgenstein."

(Emphasis as in Russell 1975: 349).

Then on 19.8.1919 in a letter to Russell he stated:

"... now I'm affraid you haven't realy got hold of my contention, to which the whole business of logical props is only a corolary. The main point is the theory of what can be expressed (gesagt) by props [i.e. propositions] — i.e. by language — (and, which comes to the same, what can be thought) and what can not be expressed by props, but only shown (gezeigt); which, I believe is the cardinal problem of philosopgy. —

I also sent my M.S. to Frege. He wrote to me a week ago and I gather that he doesn't understand a word of it all. So my only hope is to see you soon and explain all to you, for it is very hard not to be understood by a single sole!" [sic.] (Russell 1975:350)

Now the question is, did seeing Russell and "explaining all" to him result in proper comprehension, by Russell, of the material of the Tractatus?

^x Spelling etc. given exactly as published in Russell 1975.

Russell's own general account of his relationships with Wittgenstein is, briefly, as follows:

"I knew Wittgenstein first at Cambridge before the War ... He was perhaps the most perfect example I have ever known of genius as traditionally conceived, passionate, profound, intense, and dominating. ... At the end of his first term at Trinity, he came to be and said: 'Do you think I am an absolute idiot?' I said: 'Why do you want to know?' He replied: 'Because if I am I shall become an aeronaut, but if I am not I shall become a philosopher.' I said to him: 'My dear fellow, I don't know whether you are an absolute idiot or not, but if you will write me an essay during the vacation upon any philosophical topic that interests you, I will read it and tell you.' He did so, and brought it to me at the beginning of the next term. As soon as I read the first sentence, I became persuaded that he was a man of genius, and assured him that on no account should he become an aeronaut. In early days I once asked G.E. Moore what he thought of Wittgenstein. 'I think very well of him,' he said. I asked why, and he replied: 'Because at my lectures he looks puzzled, and nobody else ever looks puzzled'".
(Russell 1975: 330).

Perhaps Wittgenstein had a greater capacity for being puzzled than did Russell; perhaps this was what goaded him, eventually, into effecting (either one or two, depending on one's interpretation) major revolutions in philosophy. But be that as it may, it is curious that Russell, in the chapter which covers the period when he was reading, arguing about, and finally writing an Introduction for and helping to publish the Tractatus, should say, with regard to the 'explaining all' issue, only "... we spent a week arguing his book line by line" Just that: they spent a week arguing. No indication of how his views were affected by the argument, whether he remained doubtful as to Wittgenstein's meaning, whether he was converted to W's views, or whether he merely understood what they were but disagreed with them. The absence of any definite statement on this issue would appear to rule out this last possibility — surely Russell would have stated his disagreement, both in his Introduction to the Tractatus and also in his autobiography, if he had found that he did in fact disagree. He never lacked the power of forthright statement, indeed his courage in expressing unpopular opinions was one of his most notable and praiseworthy characteristics. Later, when Wittgenstein had returned to Cambridge and G.E. Moore asked Russell to provide a report on the work upon which W. was at that time (i.e. 1930) engaged, Russell gave a very fair — and perhaps more revealing than he realized — account of W's then philosophical position in relation to his own.

"My report to the Council of Trinity on Wittgenstein's work:

..... 8th May 1930.

Owing to illness I have been prevented from studying Wittgen-

stein's recent work as thoroughly as I had intended to do. I spent five days in discussion with him, while he explained his ideas, and he left with me a bulky typescript, Philosophische Bemerkungen, of which I have read about a third. The typescript, which consists merely of rough notes, would have been very difficult to understand without the help of the conversations. As it is, however, I believe that the following represents at least a part of the ideas which are new since the time of his Tractatus:..."

There follows a brief account of views of 'grammar' which have some similarities to those published later in Wittgenstein's Investigations, but which appear to be even closer to the doctrine of 'categories' put forward later by Gilbert Ryle (1937, see also his Philosophical Arguments 1949 and The Concept of Mind 1949). Wittgenstein's views on the nature of mathematics are also mentioned. Russell's concluding paragraph reads as follows:

"The theories contained in this new work of Wittgenstein's are novel, very original, and indubitably important. Whether they are true, I do not know. Emphasis added As a logician who likes simplicity, I should wish to think that they are not, but from what I have read of them I am quite sure that he ought to have an opportunity to work them out, since when completed they may easily prove to constitute a whole new philosophy.

Bertrand Russell."

(Russell 1975: 439-440).

Several comments may be offered at this point. I shall attempt, later, to expose some further implications of the underlined (italicized) sentence regarding the 'truth' of Wittgenstein's views (cf Chapter 8, above) — for the moment it may be suggested, merely, that it does indicate Russell's own uncertainty regarding Wittgenstein's position. Secondly, a question could be said to emerge as to the extent to which Russell's support of the work depended upon his assimilating it to his own position (in general — it is obvious that the particulars of Wittgenstein's argument are quite different from Russell's, e.g. the implications of the remark about simplicity). Thirdly, the final remark about the possibility of "a whole new philosophy" does not rule out that Russell might be enmeshed in a fundamental misinterpretation of W's position, as just indicated, since a 'new philosophy' need not be at variance with Russell's own, it might just as easily be a further development from it. On Toulmin's (1969) interpretation, of course, Wittgenstein's position was fundamentally antithetical to Russell's — and it is clear that Wittgenstein's own view was that Russell misunderstood his — but while Russell admitted having difficulty in understanding W., it seems

at least an open possibility that he may have been attempting to understand W. in the wrong terms — namely, in his terms rather than Wittgenstein's. (More on this below.)

A final comment on Russell's report: in the light of all the foregoing, one cannot but admire — and would hold up for universal emulation among academic referees and examiners — Russell's support for work which he avowedly did not fully understand. Too often the benefit of the doubt goes the other way. Hudson (1964) has illustrated, hypothetically, the judgemental processes which are all too likely to nullify the career prospects of great creatives of 'revolutionary' type (in Kuhn's 1962 sense); his examples are Darwin and Einstein, whose actual careers were, of course, in flat contradiction to the negative predictions which were likely to have been made on the basis of their early performance. Russell, on the other hand and despite all the provisos and his own equivocations, was supremely correct in his positive evaluation of Wittgenstein: "a whole new philosophy" did indeed emerge. It did not achieve widespread acceptance, of course, for over thirty years.

The nature of the problem of interpretation can be illuminated, in philosophy just as in science, by Kuhn's (1972) distinction between 'normal' and 'revolutionary' developments. Russell's difficulties, it may be suggested, arose because a 'gestalt switch' was needed in moving between his philosophical position and Wittgenstein's. Just as in perceptual (visual) paradoxes, e.g. the drawing that looks either like a vase, or like two faces in profile confronting each other, the reader tends to see one interpretation or the other, but cannot perceive (understand) both simultaneously. The simply reason for this is that, though the same words may occur in the several arguments of the two different positions, their meanings are not necessarily the same. (This point has been argued earlier, see Chapter 8.). Philosopher A reads B's work, but (as it were) in terms of A's meanings rather than B's. Similarly vice versa. Hence the phenomenon, almost standard in the history of philosophy, of the epoch-making books "falling stillborn from the press" (e.g. Hume's Treatise, Kant's Critique of Pure Reason, see Kant ed. Lucas (1953: ix)). People read, but they cannot understand, because in their set of meanings, their language-game, the new writings really don't make much sense — or, don't make enough sense.

(Here, we may note some significant differences and similarities between science and philosophy. In science, the revolutionary gestalt-

switch from the old language-game to the new one is achieved largely through the appearance of a new 'paradigm' in the sense of a public and relatively spectacular resolution of anomalies which had proved refractory under the old way of looking at things. A prediction is made, and is later fulfilled; an experiment, conceived in terms of the new language-game, the new way of seeing the world, turns out to be successful. This is the Public Demonstration Paradigm (PDP). Thus scientists, though not at the present time trained to look for alternatives of meaning, can be led by practicalities and by actual demonstration to take seriously the possibility of an alternative approach (though many of them, as Kuhn points out, never in fact do escape from the basic assumptions of the old paradigm in which they had been educated). In philosophy, on the other hand, though the appeal to practicality is not available, practitioners are (supposedly at least) trained to recognize the possibility of alternative interpretations, alternative language-games, and are (again supposedly) more sensitive to nuances of resulting increased cohesiveness etc. at the theoretical level).

Turning again to Russell and Wittgenstein, as late as 1961 in a letter to A.J. Ayer, Russell remarks that "... Wittgenstein and all his followers hated my introduction [to the Tractatus] and ... Wittgenstein only consented to its inclusion because the publishers made it a condition of their publishing the Tractatus ..." (Russell 1975: 625.)

All in all, and whatever the underlying rights and wrongs of the case, it is quite clear that Wittgenstein himself felt that Russell had misunderstood him and (though this is not so readily established) that Russell's Introduction to the Tractatus was substantially misleading.

I have felt that the foregoing exercise in interpretative biography/history has been worthwhile for its intrinsic interest, because of the light it sheds on two of the leading philosophers of this century, and also — particularly significantly in relation to Education Philosophy and Education — because it illustrates the peculiar difficulties of communicating new and unusual ideas even between minds of such outstanding calibre. If Russell and Wittgenstein had such difficulties, how much worse trouble may be generated in communication at less exalted levels! (Or does it work the other way? Are persons of less education in a particular field more, rather than less, likely to understand and appreciate revolutionary new developments within it?)

Finally, the importance of Kuhn's revolutionary/normal distinction

has been shown to be helpful in understanding the history of philosophy, as it is in the history of science; and this distinction has been characterized as involving changes in the meanings of words, changes in language-games. The 'normal' worker, whether in science or philosophy, leaves meanings constant (more or less) — the 'revolutionary' changes them.

One of Wittgenstein's major and revolutionary contributions to philosophy (which has already been taken up in other academic disciplines, e.g. social anthropology, see Leach 1976), was to show that the nature of meaning is not a matter of mere words, of language on its own. This is the point of Toulmin's paradoxical-seeming assertion that "Wittgenstein was not a 'linguistic philosopher'." Toulmin (1969: 62) explains this as follows:

"...Wittgenstein was concerned with language, and with the manner in which language operates within our lives. Yet he never saw language as the self-sufficient subject-matter of philosophy. [Emphasis added.] The philosopher's task was not, in Wittgenstein's view, to instruct the ordinary man, by "analysing the meaning of words" ... Nor was it the philosopher's business to classify the different ways of "doing things with words", by a kind of linguistic taxonomy. ... such linguistic investigations had implications for philosophy only when they were placed in a broader intellectual context. And, when we recognize the nature of Wittgenstein's deeper philosophical aims — to which his theories of language were subordinate — we shall find that he was no more of a "linguistic philosopher" than (say) Plato, or Kant, or Schopenhauer. ..."

While I would suggest that Toulmin overstates the case he himself presents in the rest of the paper in question, when he says that W. was 'no more a linguistic philosopher than Kant', it appears to be true that there were many more similarities between Kant and Wittgenstein than has been generally appreciated. Pears (1971: 45) says that Wittgenstein "... took much of the framework of the Tractatus from Kant through Schopenhauer, whom he had read and admired, and, though he modified this framework in his second period [i.e. after about 1933], he never destroyed it. ... The task which Kant set himself was the demarcation of the limits of thought, and the parallel task which Wittgenstein set himself was the demarcation of the limits of language." The situation could best be described, perhaps, by saying that while Kant worked in terms of 'thought' and Wittgenstein in terms of 'language' (or, more truly, in terms of 'language-games' as this concept has been exemplified in the Philosophical Investigations and explained in this thesis), the basic concerns of both were, as Toulmin emphasizes, with the 'transcendental' problem of how logic could — or

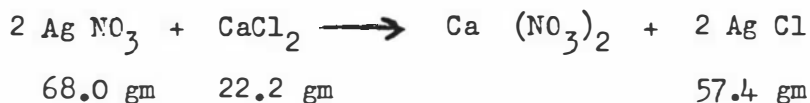
rather, perhaps, with 'why logic does' — 'match up with' objective reality.

We are, in our everyday lives and in our experience of science, accustomed to arguing from premises which are known to be true, to conclusions which are often not known to be true at the time when the argument was first formulated, but which turn out, later, to be confirmed as true by direct observation. This is indeed a commonplace of science. Since it seems to work so well, usually, in practice, we are apt to take for granted the concurrence (more, the exact coincidence) between what we may call the 'laws of logic' on the one hand, and what are commonly called the 'laws of nature' on the other. But if we stop to think about it, surely there must seem to be something almost miraculous in the fact (if it is indeed a fact) that 'logical necessitation' should run exactly parallel to the 'causal necessitation' of the objective physical world?

Perhaps an educational example (which happens also to be autobiographical) will help to clarify the issue.

In the first year in which I studied physical chemistry at university, it seemed to me to be literally miraculous that the various numerical calculations (of reactions between substances in solution, dissociation constants, etc.) should turn out results which were the same (usually) as what one could get by direct measurement, weighings, etc., of the actual outcome of an experiment. How astonishing, that unless one had made a mistake, the observed results should turn out to be the same as the calculated results! For example, from dissolving 68.0 gm silver nitrate in water, adding it to a solution containing 22.2 gm calcium chloride, one could calculate that a white precipitate weighing 57.4 gm could be filtered off — and if one performed the actual experiment (properly), sure enough, one obtained exactly that weight of silver chloride precipitate!

The equation for the reaction is as follows:



(It may be worth mentioning, for whatever educational implications the information might yield, that while I switched to a full science course only in my third year as an undergraduate, having studied mainly philosophy and history for my first two years, I had been able to combine both mathematics and some science through the kaleidoscopic patterns of my secondary

schooling; and had usually been in the top quartile of the various classes. I mention this to show that I had been a relatively 'successful' pupil in science and maths, according to the criteria and standards of the time, despite the fact that there was very little in these disciplines that I felt I really understood. With regard to mathematics, it was not until I studied symbolic logic at postgraduate (MA Hons) level that I suddenly began to see the 'why' of mathematical operations.)

The key to the mystery, so far as chemical reactions are concerned, is that all the units of measurement, the constants and so on, have been set up so that the calculations will work out. If the experiments are carried out using the unit of the 'mole' (the molecular weight of each substance, expressed in grams), then the outcomes of the experiments can be calculated in advance, and will be found to be correct after the experiments have been completed. Why? Simply because the concepts of 'molecular weight', 'mole', and so on, had been formulated because it had been found, in the past history of chemistry, that substances do in fact react with each other in particular ratios by weight which correspond to the ratios of their molecular weights. So, the fact that the sums would work out right had really been built into the units that were used; and this had been done on the basis of past discoveries as to how chemical reactions actually take place.

The explanation of quantified chemical reactions is thus fairly simple — but the apparent correspondence between 'the laws of thought, or of logic', and the causal 'laws of nature' which control the objective physical world, would seem to be very much more difficult to explain. Nevertheless, the 'transcendental' problem which, in effect, Wittgenstein posed to himself in these terms, can be solved in a similar though vastly more complicated way. The solution to the transcendental problem is essentially an evolutionary one. Putting it in a summary and hence probably simplistic form, we can say that our language-games have evolved, as part of our overall ways of life (or 'forms of life' as Wittgenstein put it), precisely because those that have been found to be successful at performing particular functions have been retained, i.e. have survived, while those that have been unsuccessful have been abandoned.

Toulmin (1969: 67) in discussing the solution of the transcendental problem does not specify an evolutionary context. What he does say is perfectly compatible with such a context, however; and indeed it seems to me that an adequate account demands the inclusion of evolutionary

mechanisms — not those of biological evolution per se, of course, but their analogues in cultural evolution. "In the last resort," says Toulmin, "our language acquires its meaning from the procedures by which we give our utterances definite uses in our dealings with the world: not from their inner articulation alone, nor from any essentially "pictorial" character in the utterances themselves." (p.67). The point which Toulmin does not make, and which I believe needs to be made, is that we do not normally start from scratch in our language-games. We gradually learn the l-gs of the society into which we are born and in which we grow up. This learning has been touched upon in Chapter 6, above, and elsewhere. It is important to keep in mind several features and/or implications of the 'language-game' concept, most of which have already been mentioned. We are indeed 'initiated' into their use (cf Peters' notion of education as initiation); but the 'games' into which we are initiated do not have rules which are absolutely fixed and unalterable. Within various constraints which have been mentioned, they can be changed: and the criteria for the survival of changes can be summed up as 'adaptiveness'. Again, from the 'ordinary life' l-gs we can move into learning various specialist l-gs such as those of the various sciences and other academic disciplines, politics, music, religion, and so on — but our being able to learn these l-gs is dependent upon our already knowing the ordinary life l-gs, since we can learn the rules of the new l-gs only (in part, at least) by being told about them in ordinary language. In addition to being told, there is also, in most l-gs, a considerable amount of 'ostensive learning' as Wittgenstein came to call it in the Investigations: dealing with the things that cannot be said, but only pointed to, indicated.

That Wittgenstein regarded 'what is unsayable but can be shown' as important from even an early stage in his philosophical development, as is argued by Toulmin, is supported by a passage from Russell's autobiography which can be read in conjunction with Anscombe 1967 esp. Chapter 4. Taking the latter first:

"In the course of his researches prior to writing the Tractatus, Wittgenstein invented what he called the a-b notation. He proposed to write a proposition like this:

a p b

the a and b being what he called the 'two poles' of the proposition. This notation has survived in the Tractatus at 6.1203, except that he writes T and F instead of a and b." (Anscombe 1967: 70).

It is apparent from the above that his use of this notation ante-dated the actual publication of the Tractatus; and this helps to support a pre-1922 dating for the following anecdote:

"Whitehead described to me the first time that Wittgenstein came to see him. He was shown into the drawing-room during afternoon tea. He appeared scarcely aware of the presence of Mrs. Whitehead, but marched up and down the room for some time in silence, and at last said explosively: 'A proposition has two poles. It is apb.' Whitehead, in telling me, said: 'I naturally asked what are a and b, but I found that I had said quite the wrong thing. "a and b are indefinable," Wittgenstein answered in a voice of thunder.'" (Russell 1975: 331-2.)

As Miss Anscombe, Toulmin and others have suggested, it seems that what was 'indefinable' in this context, to Wittgenstein at that time and in his subsequent development, was equivalent to 'what was not sayable but could be pointed to', i.e. what could be understood through 'ostensive learning' (as was finally explained in the Investigations). This interpretation is supported by Anscombe 1967: 71:

"This rendering of the picture become proposition would stress the fact that it has acquired two 'poles', or senses in which it can be thought, by having the drawn figures correlated with actual men." (Emphasis added.)

In other words, the actual men are the referents of the terms of the proposition, indicated ostensively.

In contrasting his own with the positivistic interpretation of Wittgenstein, Toulmin (1969: 61) says:

Far from equating the important with the verifiable, and dismissing the unverifiable as "unimportant because unsayable, Wittgenstein took exactly the opposite stand. In the concluding section of the Tractatus, and repeatedly thereafter, he kept insisting — though to deaf ears — that the unsayable alone has genuine value. We can (he tells us) discover "the higher" only in that which the propositions of our language are unfitted to capture; since no "fact" ... has any intrinsic claim either on our moral submission, or on our aesthetic approval. So Wittgenstein's silence in the face of the "unutterable" was not a mocking but a respectful silence. Having decided that only "value-neutral" facts can be expressed in regular propositional form, he exhorted his readers to turn their eyes away from factual propositions to the things of true value — which cannot be gesagt but only gezeigt" i.e. not said but only indicated.

On this sort of interpretation, Wittgenstein's work has affinities,

as Toulmin (1969: 65) suggests, with that of some of the Existentialist philosophers: "Post-Kantian Existentialism" is the tag which Toulmin puts up for consideration, noting that this sort of affiliation of Wittgenstein would "come as a surprise" to English-speaking philosophers, and also that labelling him thus, without qualification, "would be as grotesque a caricature as labelling him — without further qualification — a mathematical logician, a logical positivist or a linguistic analyst." (p.68). In short, with Wittgenstein as with most other great philosophers, most of the standard tags and labels are applicable, and none on them tells the full story. (See Passmore 1952 for the several major strands discernable within the work of Hume. Perhaps I might remark at this point that one of the most useful educational functions that can be achieved in teaching philosophy, closely in line with the desiderata mentioned at the end of Chapter 7, above, is to take students in some detail through the work of several philosophers, showing how most of the standard labels, most of the '-isms', can be applied to everyone — hence the limited though not completely negligible significance of such labels. I have already outlined, in Chapter 8, some of the strategies used in my Educational Philosophy teaching at MA Hons level; and I draw a little personal satisfaction from having suggested to the MA class, as early as 1967, that Wittgenstein's approach could usefully be compared — on the grounds of having illuminating similarities, as well as differences — with the Existentialists on the one hand, and the Pragmatists on the other.)

A proper comparison between Wittgensteinian linguistic philosophy, ^mpragmatism which has been largely an American philosophy, and existentialism which has been Continental (i.e. European) in origin though now increasingly influential on a worldwide basis, would be an enormous task and will not be attempted here. There are, however, some good reasons why a very brief look at some of the similarities and differences should be attempted. ^mPragmatism, having originated through the personal influence of C.S. Peirce (his writings were almost totally unpublished in his lifetime: he died in 1914, and his Collected Papers were not published until 1931-5), became widely known through the works of William James and John Dewey; and especially but not exclusively through these last two, has exerted a considerable though by now fairly diffuse influence upon the academic disciplines of Psychology and Education respectively. As we have found it necessary to discuss various aspects of these disciplines, an understanding of some of the impingement of pragmatist thinking upon them cannot but be helpful — similarly with

regard to the various existentialist approaches which have been taken up, though often in 'popularized' form, by many young people in recent decades.

The basic assumption of pragmatism may perhaps be most readily appreciated from James's 1902 explanation: "the whole meaning of a conception [i.e. concept] expresses itself in practical consequences, either in the shape of conduct to be recommended or in that of experience to be expected if the conception is true, which consequences would be different if it were untrue ...". The similarity will readily be noted, between the Pragmatists' notion of 'experience which constitute the meaning of a concept' (we may correct this, along Philosophical Investigations lines, by substituting '... meaning of a word'), and the positivists' 'meaning as method of verification'. In both cases the emphasis on direct experience (with a large though not necessarily total sensory component) can be therapeutic against loose and pretentious theorizing (cf Farrell 1946), and this can be beneficial (cf also Andreski 1974). If taken literally, strictly and universally, however, such doctrines would be intolerably limiting on our use of language and our thinking (in ways which have been indicated elsewhere in this thesis, and paradigmatically in the Philosophical Investigations), and they cannot be accepted, therefore, as constitutive with regard to philosophy or to intellectual activity in general. Taken in a broader and/or looser way, however, and not tied to positivistic emphasis on sensory/physical experience, the affinities between pragmatism and Wittgensteinian language-game philosophy can be illuminating, especially perhaps with regard to education. In attempting to teach introductory courses in Educational Philosophy, for example, I have found it most effective to use a 'reflexivity' approach, compatible with pragmatism and existentialism and Wittgenstein's doctrine of 'what can be shown but not said', in contrast to the common indeed apparently quite standard practice of offering verbal definitions of the subject as an introduction. For students who have had no prior experience of educational philosophy, verbal definitions and explanations can have little real meaning. They have to be led into the experience of doing educational philosophy (cf Dewey's slogan, "Learn by doing"); and after they have had a few experiences of this sort, preferably including written analysis etc. of written (published) argument, they can then be told "Educational Philosophy? If you want to know what it is, look back at what we have been doing over the past few weeks.". The teacher must try to select material for the exercise of nascent philosophical skills which is appropriate to the interests, abilities, etc., of the

particular students involved, and must provide for them the needed commentary of evaluation and criticism which speeds up improvement in their performance — but talk about educational philosophy, or philosophy in general, must be largely unintelligible to those who have had little or no experience of doing it. This is one of the major implications, especially for education, of the 'revolutionary' recognition of philosophy as an activity (rather than a body of ideas/knowledge to be learned up). And the above reflexible 'showing' of what philosophy is, also exemplifies Wittgenstein's doctrine (as explained by Toulmin) that what cannot be said but can only be shown, is the very opposite of unimportant.

If it is Wittgenstein's view of philosophy, that what is most important cannot be said but can only be shown, this brings his view of the activity of philosophy closely into accord with one of the most widely-accepted and basic maxims of education, that showing by example is more effective than merely 'telling about'. Especially in philosophy as activity, the example of the teacher in doing philosophy, performing the activities, must be very important. And considerations along this line, if we follow them up from their start in questions of educational effectiveness, will lead us to a position where the significance of Existentialism (in a general sense) can be seen to have great relevance.

It has been mentioned earlier, that a critical or 'philosophical' approach can be interpreted in terms of merely finding fault with everything that other people say, and this can be combined with 'putting down' other people (especially students, or colleagues) in instinctual social-dominance contests. Individuals who have had some training in philosophy can easily pick up techniques and intellectual 'tricks and dodges' which can be used very effectively for this type of thing. If a Department of Philosophy in a university gets too many people of this type on its staff, and if they use their talents as talons to rip into their students (perhaps as a way of ego-building, in default of gaining satisfaction from substantial scholarly achievement, and/or from effective teaching), then philosophy itself is burdened with a reputation for negativism and destructiveness. Students and the general public see nothing useful, nothing illuminating, nothing positive and life-enhancing, in what is done. So the students stay away from philosophy, the 'subject' as well as the Department gets a bad name, and the ill-repute may spread undeservedly to the whole of Humanities Faculty. Alternatively (or possibly even in addition to the above), the 'distance' and detachment from topics with heavy emotional loadings, which is certainly

to some extent desirable at least in the earlier stages of teaching philosophy (everyone finds it difficult to think clearly about things which violently arouse their emotions, pro- or contra-), may be carried to an extreme in which it appears that the philosophers don't really care about anything, have no concern for the problems and worries (or even the joys) of the everyday lives of ordinary people, or for the wellbeing of the society in which they live.

In contrast to these particular faults (and perhaps others which could be mentioned) which are not uncommon in the world of academic philosophy, not only the writings and teachings but also the very lives of many Existentialists show up in heroic prominence. They not only talk commitment and involvement, they embody and practice it, even to the point of personal hazard. Sartre, for example, whatever one may think of his writings, must be acknowledged to have put himself in the firing-line of active politics, at times quite literally (or perhaps one should say, not 'firing-line', but 'being-fired-at-line', or 'being-clubbed-and-beaten-up-line' — see Simone de Beauvoir's autobiographical volumes, which are also part-biographies of Sartre). It is this personal unity, the absence of dissociation between ideas, theorizing and real-life, which has attracted so many young people to the Existentialists. Also, even in their writings, many Existentialists acknowledge the issues which perplex the young, and which 'worldly and cynical-seeming elder writers either ignore or misrepresent. Their solutions may or may not be adequate, but at least they can be seen to be aware of and in a serious way concerned about the problems.

Talking of Sartre, de Beauvoir (1962: 30) says he "... wanted to grapple with ... living reality, and despised any analysis which limited its dissecting to corpses." She contrasts Virginia Woolf's desire as a literary artist to abolish or reduce the gap between words and reality, with Sartre's desire, not to abolish it but to "... turn it to good advantage": "... he kept himself detached enough from the event to try to catch it in words." (p.37). She goes on (p.39): "... in order to make discoveries, the essential thing is not merely to observe a gleam of light here and there that other people have missed, but to drive straight for your goal, and damn everything else. I often accused Sartre of careless inaccuracy, but nevertheless I observed that his exaggerations tended to be more fruitful than my own scrupulous precision." Talking of her own academic training in philosophy (she placed second to Sartre in the nationwide university examinations in philosophy), she says: "... if a theory convinced me, it

did not remain an external, alien phenomenon; it altered my relationship with the world, and coloured all my experience ... philosophy was for me a living reality, which gave me never-failing satisfaction." (p.178). On teaching philosophy (as against 'living' it, becoming involved by it 'existentially'): "When I talked philosophy with Sartre, and took the full measure of his patience and audacity, the idea of a philosophical career seemed wildly exciting — but only if one was bitten by a theory of one's own. Expounding other people's beliefs, developing, judging, collating, and criticizing them — no, I failed to see the attraction of this." (p.178). This latter type of teaching can have its merits, if well done — but it would seem to be true that Sartre and de Beauvoir, by combining teaching (in the narrower, professional sense) with journalism, writing plays and novels, and being often deeply and conspicuously involved in politics, have done more than most for both education and philosophy. By using the term 'philosophy' in this context I am, of course, breaking the (linguistic — or could they be philosophical?) rules for its use, as those rules are drawn up to confine it to activities of clarification of what has already been said. (If it were in truth confined to such activities, then a dilemma pokes out its horns:

- either: (a) The 'what has been said' which is to be clarified, has been said by other people — in which case the philosopher is cast in a very secondary; dependent, and in a sense a parasitical role;
- or (b) "What has been said" was said by the philosopher himself — in which case he must be pretty incompetent at communication (speaking or writing), if he could not get his message clear in the first place.

A third possibility arises from (b), above: that if only clarification of what has been said already is philosophy, then 'primary' or 'revolutionary' or 'original' philosophy is not philosophy!)

As a result of their active involvement, often at risk not only of their careers but also their personal safety, both Sartre and de Beauvoir have achieved a position where their teaching reaches a wider public than does that of any other living philosopher. Bertrand Russell perhaps reached a similar position in English-speaking countries during his lifetime, and for similar reasons: intelligence, and the courage to take a public and sometimes dangerous stand on issues of wide importance to humanity. One may not agree in detail with the positions that any of these persons adopt —

and it is no doubt apparent that I do not agree, myself, with much that they have, severally, said — but the positions they have taken up deserve respectful attention, as being 'philosophical positions' in the sense outlined in Chapter 8, above, also in view of the personal commitment (to the point of hazard) of their protagonists.

Two points call for additional discussion in this context: the question of personal involvement, commitment, of educators, and the question of 'rationality'.

If a teacher is going to use the most powerful means at his command, and teach/educate by example — and if he/she is an intelligent, perceptive, and realistic person, able to see the world as a whole and the long-term trends of the changes that are occurring — then if the society in which the teacher is working is being run by what is to a substantial extent an Establishment (i.e. a group which is more interested in preserving its own power than in discharging the functions required of it by the society, which nominally it is discharging), the teacher/educator is almost necessarily bound to become a critic of the Establishment. This is not to say that he/she should necessarily indulge in public denunciation of official Establishment policies. Neither should polemics, denunciation, or fault-picking be substituted for true education. But on the other hand, neither should educators shut their eyes to the realities of the situations in which they and the people they are educating find themselves. Failure to perceive what is there is almost as damaging as telling lies about it. So is suppression/repression of what one has seen but may not want to talk about. The younger one's pupils are, the more one can get away, for a time, with presenting a distorted picture of the world, especially the social, political, economic, etc. aspects of it — but eventually a 'credibility gap', if one exists, will cause one's own teaching, and/or education in general, to be discredited. Once this has happened on a large scale, not only the individuals but the society as a whole is in trouble. It is seldom utterly impossible, presumably, for the situation to be retrieved — but drastic and painful (perhaps seemingly destructive, in the short run) restructurings may have to be accomplished, to get an education system, and through it the society of which it is part, back on the right tracks again.

The concept of 'adaptation', as used in the biological sciences, is usually interpreted mainly as adaptation in a physical sense, to the physical environment. Such adaptation is ultimately essential: recent years have seen an increasing awareness that even the all-powerful human species

may render the planet Earth unfit for the support of life. But the more immediate aspects of adaptation are mainly behavioural, and to the societal rather than the physical environment — though the societal must be regarded as including its physical manifestations, e.g. urban slums and ghettos, suburban isolation, the commuter society, general overcrowding, pollution, exhaustion of natural resources, and so on. These physical phenomena tend, in fact, to be over-emphasized. Not enough attention is focussed upon the behavioural processes — of political and economic organization, manipulation of the news media and of the educational processes themselves — which produce the material and hence more readily observable results. It has been in large part by the highlighting of questionable societal practices that Sartre, Russell, de Beauvoir and others, have achieved their standing as educators and, in a broad sense, as philosophers. (If we take 'philosophy' in an approximation of its original Greek meaning, as 'love and pursuit of wisdom' — surely it is wise to try to ensure the survival and wellbeing of our own species?)

It is against this sort of background, of the personal commitment of the teacher to exploring all the possible interpretations, whether congenial or uncongenial to himself, his pupils, or anyone else, and whether the 'interpretation' be of the written or spoken word or of states of affairs within a society, that the present state of Educational Philosophy and of education in the sciences must be judged. Professor Peters and the 'London' school of Educational Philosophy, it could be said, are either unaware of many of the really urgent problems afflicting the practice of education today (whether from the teachers' or the students' points of view) — in which case they must be adjudged imperceptive, and on that ground undeserving of attention — or else they do perceive what the problems are, but do not, for whatever reasons, wish to acknowledge them or attempt to give them proper discussion. In the latter case they are disingenuous, and in effect (again it is immaterial whether this is intentional or not) they are attempting to mislead their students, by tacitly denying that certain problems exist or are important. In a later section of this thesis I shall offer argument to suggest that Peters himself does seem to be aware of things which are important but to which he gives little explicit acknowledgement and which he makes no attempt to explore — he shies off them, in fact — and this seems to suggest that what Peters and others are doing is not failing to see the difficult issues which need to be dealt with, but rather suppressing or repressing their own awareness of these matters. Bertrand Russell would probably have called the unrealistic dignification of

Establishment or 'official' practices 'humbug'; Sartre would presumably designate it 'bad faith'; and Davies (1975) has drawn attention to various contemporary variations on this theme.

If an educator is to keep 'good faith' with those he is attempting to educate, he must hazard himself with them at least to the point of making a serious attempt to explore all the possible meanings of statements, all the possible implications of situations being considered. It is not always practicable, of course, to try to cover literally every possibility; but at least every possibility that the teacher and students think significant can be looked into. And so it should. The educator is, after all, supposed to be educating his students for the future, so that they can cope with the eventualities that may arise even several decades ahead. A similar argument as was used at the beginning of Chapter 7, with regard to the optimization of education in science, applies here also: unless all possibilities have been covered, any decision as to which ones are significant or probable etc., must be to some extent precarious. Such a decision will be precarious, in fact, in proportion to the number of possibilities which have not been explored. We have to recognize that it is in principle impossible to be sure that literally every possibility has been covered — on what basis could we ever be justified in saying that no further possibilities exist? (Except when we are dealing with abstract generalizations, e.g. 'Everything is either A or not-A'. But such abstractions, while having their own usefulness and value once we have learned to deal with them, are helpful with regard to the problems of real life only when we can give real-life interpretations of 'A' and 'not-A' — just as algebraic formulae are useful to the engineer, for instance (and they can be fantastically useful), only when the appropriate numerical values can be substituted for the several variables, 'a', 'b', 'c', etc..)

To repeat: education is for the future — and it is the pupils/students, rather than the teachers/educators, who are likely to have most future to live through. This follows from the fact that, from the very nature and circumstances of education, teachers are usually older than pupils. Most of the problems and hazards will be coming the students' way. They have most at stake, therefore, in the educational transaction. They will suffer, if their education has not been good enough (inappropriate, or badly done, or whatever ...). Their educators will probably suffer too, to some extent; and those of them with sharper moral perceptiveness will suffer, in some cases are undoubtedly already suffering in

the knowledge that they have fallen short as educators. (But can we ever be fully adequate?) If an educator has real concern for his pupils as persons, if he sees himself as teaching children not subjects, surely he will question what is done in terms of what 'subjects' are taught as against what needs to be taught? To put the issue aphoristically, if a teacher teaches children not subjects, then he cannot just teach subjects. We need not indulge in erudite futurology to start an assessment of the adequacy of present-day school subjects and syllabi: all we need to do is to look at the problems which young people, at school and in the years immediately after their formal education has been completed, are not coping with (or are coping with sub-optimally). There are plenty of such problems, and I do not intend to document their existence here; I shall merely indicate the areas of personal and family finance (deficiencies here are implicated in a large proportion of divorces and family break-ups), sex and social relationships in general (Davies 1975 in commenting on suggestions that premarital counselling be mandatory for second marriages, argues that if this is thought desirable, why not mandatory counselling/education prior to first marriages?), politics and the organization of society (are suicidal conflicts between labour and management really due to sheer stupidity in everyone involved? Or is the more charitable explanation also the correct one, that conflicts and gross inefficiency are due rather to lack of understanding of the complex relationships and processes involved, i.e. do these people simply not understand that they are precipitating societal suicide?) — many more problem areas could be specified, but I suggest that the above are sufficiently well recognized, and important, to support the point being made. This point is, that a large proportion of young people today are not being educated adequately in various important areas; and following from this, educators who do not attempt to remedy the situation are either obtuse (if they genuinely do not perceive the problems — to be fair, they themselves may have been so conditioned/indoctrinated that they cannot easily see such problems), or else they are guilty of bad faith (if they see the faults and do nothing to remedy them — a fortiori if they continue to perpetuate misleading and unrealistic doctrines).

As a minimal step in the direction of good faith, it appears desirable that educators should at least be willing to look at the possibility of educational shortfall or misorientation in a serious way. Very few if any even of the most rabidly discontented students (in my experience) persist in their 'anti-everything' attitudes if they are given a fair hearing

and taken seriously (which does not mean giving in to all their demands and allegations). However this may be as a matter of fact — an adequate analysis of the full complexities of relationships between students teachers institutions society has not yet, to my knowledge, been produced — the issue of principle seems to be clear: educators should be prepared to look at any and every possibility (see Chapter 7 above for some specific applications of this), as part of keeping faith with their students. And 'keeping faith' or 'good faith' understood in those terms, links up with the question of 'rationality' to which we have addressed ourselves in various contexts.

'Rationality' for Peters, as we found earlier, seemed to boil down to 'being prepared to give reasons for ...' (one's actions and/or one's statements). This we found to be an ambiguous and problematical formulation, with a tendency to slide into Freudian-type 'rationalization' in a very real sense: this latter coupled with a strong tendency to built-in conservatism and probably also authoritarianism. In contrast to Peters' view of rationality, we found in Chapter 10 that a scholar from another discipline, Kirk (1974), while not strictly equating it with 'rationality', gave as a feature characteristic of philosophy that it is an "... unrestricted and wide-ranging enquiry". Following on from this, we found that Horton's (1967) distinction between 'open' and 'closed' societal structures and their associated types of thinking, was further developed by Goody (1977) along lines congruent with what has been argued by others (e.g. Waismann, *Wisdom* — see refs given earlier — various arguments of my own might perhaps also be cited in this context), to the effect that:

- (a) Simple dichotomies are appropriate for initiatory descriptive categorization, but must usually give place to complexified views involving 'spectra' or 'continua' of characteristics, as phenomena are more intimately understood;
- (b) The characteristics within the continua need causal explanations; and
- (c) The 'awareness of alternatives' is basic to the advance from 'mythic' or 'magical' thinking to 'modern'-type thinking however designated. ('Scientific' if strictly interpreted is too limiting, 'philosophical' could be either too limiting or too loose, 'rational' also, as Goody notes, can be too limiting, too loose, or question-begging — and this concept is in fact the one in question in the present discussion.)

Goody also put forward the notion of being 'reflexively self-critical' as characteristic of 'modern' thinking — and if we take this along with

'unrestricted and wide-ranging' (i.e. unrestricted except by its own perception of self-contradiction or anomaly, reflexively), and make these necessary though not sufficient characteristics of rationality at least as this operates in philosophical enquiries then I suggest that much of Peters' work, and perhaps a good deal of other 'London' work in educational philosophy, may fall short of being truly rational and certainly falls short of the criteria for being philosophical.

This may need further explanation and discussion.

It would probably be difficult to sheet home a charge that Peters (and other educational philosophers too) are not self-critical. One line of defence against such a charge relates to the medium of communication: it can be said that one cannot give all one's explorations of argument, all one's false casts and unfruitful notions, because that would involve writing enormously long books which publishers would be loth to publish (and which would cost a frightful amount ...). And this would be true enough, up to a point. To some extent every book that achieves publication must incorporate the results of self-criticism, without actually showing how those results were arrived at — more especially, without showing the lines of argument that proved to be mistaken or pointless — thus the published work shows only the outcomes of self-criticism, not the process. In this case, however, one would expect that the outcomes would be pretty sound: if the author has criticized what he wrote, thoroughly, then what finally gets published should surely be difficult to fault. In the case of R.S. Peters' own writings, however, the only real difficulty in criticizing them is in working one's way through the shifting sands of ambiguity. From a distance they look firm enough. It is only when one searches closely for definite and self-consistent doctrine that one finds the apparent firmness dissolving into ambiguity, qualifications of meaning that qualify each other away until the reader is baffled and sinking, nothing solid under his feet and handholds that turn to water as soon as he attempts to grasp them.

Furthermore, if philosophy is to include 'unrestricted and wide-ranging exploration of the possibilities' relevant to whatever topic may be in question, it must be said that Peters' work does not seem to be exploring possibilities at all. Rather, Peters seems usually to stop looking (if indeed he is looking, exploring, at all, before he writes) as soon as he has found something which fits his purposes. Then he tells the reader what to think, what the correct view is. Perhaps he could retort to this criticism (along the lines sketched above, regarding self-criticism),

by saying that he himself has done the exploring, and that what he writes down in his books and papers is just the results of the explorations, not an account of how they were achieved. A defence along this line would run counter, however, to one of the doctrines which Peters has explicitly espoused, viz. that philosophy is an activity (whether of 'clarification of concepts' or 'exploration of possible meanings/intepretations/language-games' does not matter in this context). It must be difficult to teach people an activity, teach them how to do something, by not showing them how one goes about doing it, but only showing them the results, the outcomes. "I shall now show you how to bake a cake. See, here is a cake." Can the students, by studying the cake, find out how it was made? Not utterly impossible, perhaps, but certainly problematical; and a teacher of cooking who worked in this way would not remain long in business.

On the question of exploration, it may be illuminating to look briefly at an issue which had puzzled me, off and on over the years, until the time when I sat down and thought it out properly. In geographical exploration, the name of Captain James Cook stands in the first rank — yet when one asks what he discovered, it turns out to be not a great deal that had not been known to at least some extent before. He made the first major explorations, by a European, of New Zealand, eastern Australia, and several island groups in the Pacific — but most of these countries were known of, if not extensively known, before. New Zealand, for example, was first discovered by Tasman in 1642. In one sense Cook's achievement was a negative one: he disproved the existence of the fable Great Southern Continent, Terra Incognita Australis. Herein lies the apparent paradox. Why should he be so highly regarded, for what was mainly a negative achievement? The solution is, that he was a great explorer, not so much for what he discovered, positively or negatively (and we must recognize that many significant discoveries were made more or less by accident), but rather because he was never lost. He always knew exactly where he was, exactly where he wanted to go, and exactly how to get there. He was the 'Compleat Explorer'. He could look after his men and his ships, he could judge exactly when to run a risk and when not. To anyone who knows the seas he sailed, it remains amazing that he managed to do all that he did — with perhaps only two significant mistakes, viz. one of his ships grounding on the coral of the Great Barrier Reef off the Queensland coast near what is now Cooktown; and the fatal misjudgement of a social situation in Kealakekua Bay, Hawaii. But over and above the fact that he planned, organized, navigated and led his men superlatively, a very significant part

of his greatness as an explorer was due to the fact that he could tell other people exactly where he had been, and how, so that in due course they could go where he had first been, in safety and with sureness. His media of communication were his charts, and his Journals (see Beaglehole 1974).

Cook was like a good philosopher, in that he did not conceal his actual methods (and his mistakes if he made any) behind oracular pronouncements about what he had found, or how other people could find the same. The heroes of myth, legend and saga, by contrast, have always been more inclined to puff up their own standing by issuing (usually enigmatic) imperatives to those lesser beings who wished to follow in their wake: "Go to the black cape on the furthest side of the Summer Isles, set your course nor'west, and sail for forty days ..." (This is the sort of thing that can be found in the Odyssey and the Norse sagas, for example; and for the difficulties in interpreting the instructions, see Bradford 1963, Anderson 1967, respectively.) It is, of course, possible to give people directions in the form of a set of directives. A slightly more elaborate modern version of this is the 'strip maps' which motoring organizations prepare for their members, showing the best way to get from A to B. These allow a little more latitude to the recipient, to vary the route to be taken, to make a few side excursions, and so on — but essentially, the recipient of such a map, more so the recipient of a set of instructions, is limited to travelling from A to B. There is no provision for him to change his mind half-way, and decide to go to C or D instead of B — in fact, C and D are not likely even to be shown, unless they are very close to the A-to-B route that is given. Thus the traveller is not able to alter his plans to cope with changing circumstances: the strip map allows only minor variations, sets of instructions none at all (usually).

Contrast the strip map (or even worse, the instructions) with being given a complete map of the whole country, plus perhaps a book of information about the various places. You are not limited to going from A to B. You can look at all the places, from A to Z. You can decide which places you want to visit, in which order, and so on. In one sense this may be more difficult than just following directions or a strip map, in that you have to decide where to go and you have to work out for yourself the best way of getting there (i.e. you have to know how to read maps, and navigate) — but you are given plenty of information, in the map and guidebooks; and if circumstances should demand a change of plans and the abandonment of the

original destination, 'B', you have the information necessary to work out an alternative plan, an alternative destination and a way of getting to it. This could be of the very greatest importance. We have all had the experience of setting a goal for ourselves which we were quite convinced was utterly right, just exactly what we wanted — then, as we approached or reached it, found that it did not suit us nearly as well as expected. Either that, or we found that the goal entailed some concomitant cost or penalty which we had not earlier suspected, and which we found to be unacceptable. One way or another, the complexity of the human situation makes prediction hazardous and precarious, and things often turn out differently from our expectations: and one mark of good planning is to make provision for this, and allow for changes in both factual predictions and in what people will find they really want.

Turning to the philosophy side of the analogy again, we have already dealt with the 'unexpected costs of an intended goal' issue. This came up in Chapter 4. We were examining Peters' discussion of 'equality in education', and came to the point where he mentioned a paper on this topic by Lieberman 1961. The latter had said, in effect, that people have equality of educational opportunity when the removal of advantages due to wealth cannot proceed any further without compromising other important principles (i.e. principles other than that of equality). Peters' rejected this interpretation of 'equality' as "a sham"; and I offered some comments on his reaction, when discussing the issue in Chapter 4. But at this stage I want to point out that while Lieberman does recognize the possibility of there being 'costs' in the implementation of any policy, and that some of these 'costs' are likely to take the form of limitation of the extent to which principles can be translated into practice, Peters seems not to recognize this as a real issue at all. Certainly in the context mentioned he seems to be unable to see that there is a possibility that the pursuit of equality — a worthy goal in itself, with which Lieberman obviously agrees, and so do many people including the writer — could come into conflict with policies based on other principles no less worthy than that of equality of educational opportunity. For Peters, it seems that in education, no other principle is to be allowed to compete with that of equality of opportunity, if we take seriously his rejection of Lieberman's outline, his characterization of Lieberman's "meaning B" as "a sham". (I pointed out in Chapter 4 how Peters does in fact equivocate, and comes out, in another part of his discussion, with views less clear than but seemingly fairly similar to those

for which he berates Lieberman.) But conflict between principles each worthy in itself — or perhaps we should say, competition between principles — is one of the most fundamental problems of ethics. This problem arises almost inevitably whenever policies have to be decided upon; and it very frequently arises again as policies are implemented and unsuspected conflicts or competitions between principles are revealed. This is what we are focussing on at present. And it is toward the resolution of such conflicts that the assistance of philosophy is especially needed: for the many conflicts which arise in education, surely Educational Philosophers can be expected to offer worthwhile help?

"Moral principles and ethical theories do not stand alone: they affect and are affected by beliefs and assumptions which belong to other fields ..." says Mackie (1977: 203): and even within a particular system of morality, it is common for several moral principles all to be relevant to a decision or judgement which has to be made, and for these principles to affect and mutually modify each other. On a point which is relevant to the topic which Peters and Lieberman are discussing, Mackie says that though he would take it to be good to aim at "... the non-existence of extreme unfairness in the distribution of advantages among persons ...", it must also be considered that "... the fairness or unfairness of a distribution cannot be completely distinguished from the fairness or unfairness of the procedures and actions that have led to that distribution, and yet cannot be completely identified with these either." (pp 149-50). In short, Mackie recognizes, as Peters does not, that conflict or competition between moral principles is not something so unusual as to be not worth bothering about, but rather that conflict, competition, and even confusion are so common as to be standard; and that philosophical discourse which fails to recognize this, and take adequate account of it, is operating in mere false abstraction. For example, in arguing a need for 'secondary principles' (pp.154-157), Mackie points out:

"In general any calculation of the consequences of an action beyond the most immediate and obvious ones, even if it were possible, would be absurdly wasteful of time and effort. Besides, even where a choice is serious enough to warrant careful consideration, the question about all the differential consequences of this or that alternative is almost always intractable. Even after the event, and even if all the facts were known, there would be serious theoretical problems about what to assign as consequences of my having done this rather than that, particularly when this act is overlain by many others, and what has happened can be traced causally not only to my choice but also to many independent or partly independent choices of other agents."

Thus both Mackie and Lieberman see the multiplicity of and possible conflicts between moral principles as major problems for ethics; and both of them see their own function as philosophers as (in part at least) that of showing this multiplicity of principles, their interpretation and application, to their readers. Lieberman may appear to emphasize more that there are alternatives of interpretation/meaning, when he distinguishes between his 'meaning 1' and 'meaning 2' — and for educationalists this is indeed important. Professor Mackie, whose teaching has been mainly confined to a highly select sub-group within the population (viz. students of Philosophy at several universities), may perhaps be less aware than those of us who have been extensively concerned with trying to teach philosophy (often under the guise of some other 'subject') to students who have no background in the subject nor, initially at least, inclination to acquire any, of the strong resistance in many minds to the very idea that a word or a sentence could ever have more than one possible meaning. It is at this level of teaching that a simple type of Oxford-style 'labelling' is useful. One establishes a single clearly-distinguishable interpretation of the word or phrase in question and, like Lieberman, calls it 'Meaning 1' (or 'Meaning A'). Then one goes on to show, slowly, carefully, and in detail, that there is another, different meaning; and one labels that 'Meaning 2' (or 'Meaning B'). Then one gives the students a number of exercises of gradually increasing difficulty, in which they have to 'find the hidden meanings' for themselves ... In fact, with my own 'Introductory Ed Phil' class I don't do it quite so slowly and carefully — or so gently — but the point being made here is not how one teaches Ed Phil, but the more limited one of suggesting that although professional philosophers like Professor Mackie do not always say that they are exposing a plurality of interpretations of given words or doctrines, this is part, and an important part, of what they actually do. They usually go on, as each different possibility of interpretation is revealed, to explore and discuss the various implications and consequences of each interpretation; and to show how some interpretations clash with others, while some sets of interpretations may be mutually compatible.

Doing all this is, naturally, usually a lengthy job. I shall not attempt to offer any short and simple examples of philosophical map-making, 'mapping the logical geography of a concept', as some have called it, 'exploring the presuppositional geology of a language-game' as I have called it myself (referring to something a little different). Short and simple examples would necessarily give a misleading impression of what is involved. We can

refer instead to various works of good standard in pure philosophy, e.g. the book by Mackie (1977) to which we have been referring, other works in ethics such as Hare (1952) The Language of Morals or Nowell Smith's (1954) Ethics — the latter two are part of the recent tradition in ethics, influenced by Moore and Wittgenstein proximately through Stevenson (1944), which forms the background to the more recent work by Mackie, Phillipa Foot, and others. The reader will find, I suggest that these works, and others in different areas of philosophy e.g. epistemology, do in fact offer systematic explorations of various possibilities of interpretation of particular statements, e.g. 'This is good', 'This is a material object', and so on. In some ways it is perhaps unfortunate that they often do not explain that they are working in terms of alternatives of meaning/interpretation. It would make things easier, especially for beginning students and the lay reader, if they did. But then, these are works written by professional philosophers for others within the profession (mainly; even though some attempt is often made to render them intelligible to the layman); and the authors tend to assume, probably only partly consciously, that they are writing for persons who share the same assumptions as themselves. That is, these are works of 'normal philosophy', which share in the same 'Common Assumptions Paradigm' (CAP). In addition to this, it is possible that the profession of philosophy, like many others, uses special language, and usages peculiar to itself, as a way of setting itself off from other professions and professional groups and enhancing its own identity and the group-cohesiveness of its own members (see Andreski 1974). Thus special language-games can be used as part of a 'display' conducive to social dominance and group-cohesiveness, also as part of the 'rites of passage' by which students are admitted as members of the profession.

Reverting for a moment to the questions of 'second order' and 'exclusivist' characteristics of philosophy (discussed earlier, see especially Chapters 3, 5 and 8), it is interesting that Mackie (1977) makes a contrast between first and second order discourse with regard to ethics: "... a second order statement would say what is going on when someone makes a first order statement, in particular, whether such a statement expresses a discovery or a decision, or it may make some point about how we think and reason about moral matters, or put forward a view about the meanings of various ethical terms." (p. 9). These are contrasted with first order statements, which "... express first order ethical judgements of different degrees of generality." The latter "... may assert that some particular action is

right or wrong; or that actions of certain kinds are so; ... or it may propound some broad principle from which many more detailed judgements of these sorts might be inferred ...". The mere fact that he uses the same terminology, 'first order', 'second order', might lead us to conclude that Mackie's example supports the position adopted by Peters, Hirst and other educational philosophers in this usage. Mackie makes it quite clear, however, that his investigations are not and cannot be restricted solely to the second-order level (as Peters and others seem to assume); neither can ethics as a branch of philosophy be studied in isolation from first-order ethical discussion (i.e. it cannot be studied on what I have called an 'exclusivist' basis). In his Preface, Mackie says:

"I am concerned in this book with both first and second order topics, with both the content and the status [i.e. logical status] of ethics. In our ordinary experience we first encounter first order statements about particular actions; in discussing these, we may go on to frame, or dispute, more general first order principles; and only after that are we likely to reflect on second order issues." (p. 9).

Thus writings on ethics, while they may not explicitly mention or instantiate first-order ethical statements, always presuppose them as forming the primary subject-matter to which second-order arguments necessarily refer. They do not assume that second-order discourse can be self-sufficient; indeed some of the more significant recent developments in ethics have brought it, in the complexity of the issues and the time-span considered, into affiliation with the novel rather than the proverb or the simplistic little problems found in texts of elementary ethics. (These last are fine as introductory material ...)

CHAPTER TWELVE

WITTGENSTEIN'S DOCTRINE OF 'THE UNSAYABLE'.

It might be asked why, in a thesis devoted to Wittgenstein's concept of the language-game and arguing its potential value for education, I should have incorporated fairly extensive discussion of the (at least superficially) heterodox interpretations offered by Toulmin (1969). It is true that some recent books on Wittgenstein (e.g. Pears 1971, Kenny 1973) allow much more continuity between the 'two Wittgensteins' than was once popular (though neither, so far as I can discover, refers to Toulmin's paper; and Pears starts by saying, p. 11, that Wittgenstein "... produced two different philosophies"). Nobody, however, seems to have taken up and extensively developed the implications of one aspect of Wittgenstein's work that is emphasized by Toulmin, viz. his doctrine of 'the unsayable'. It is my wishing to give considerable attention to this and related aspects of Wittgenstein's philosophy that I feel that an appearance almost of paradox may be generated, especially in a work dealing with Education: for education is generally taken to be very much a matter of 'the sayable', whether spoken, written or printed. We need not see teaching as being a matter of mere 'talk and chalk' to recognize that language is central to it, or, as I have said elsewhere in this thesis, that "The language-game is the interface through which pass all educational transactions". Why then should we concern ourselves with 'the unsayable'?

In fact one important aspect of the doctrine of 'the unsayable' has been touched upon repeatedly already, in many contexts, though usually without being identified in quite these terms. This is the emphasis on what Wittgenstein himself called "ostensive learning". It seems unnecessary to offer further elaboration on this aspect, here, beyond reiterating that unless the learner is able to have experience of blocks, beams, and so on, he cannot know what the words 'block', 'beam', etc., refer to. Besides the actual physical presence of blocks, beams, and so on, at some stage in the learning process, there is also the need (again, this has been discussed more fully in earlier parts of the thesis) for the 'linking up' between word and object to be indicated to the learner, by pointing, showing, and so on — hence language-learning as an essentially social process, and Wittgenstein's preoccupation in parts of the Investigations with the impossibility of a real 'private language'. This need for showing, pointing, etc., has been argued, in earlier Chapters, as leading us to an appreciation of the very

great importance within all language-games but especially those of education, of the non-verbal components in language, the 'language' of gesture, tone of voice, postural signals and, in general, of the types of behaviour which forms the subject-matter of the science of Human Ethology (see discussion and refs given in earlier Chapters). With regard to this extensive and important subject-matter, in the present context I propose to follow Wittgenstein as depicted by Toulmin: I shall refrain from trying to say it all, but will merely indicate its existence!

(Perhaps I might also indicate that I have already completed parts of another book which will offer extended discussion of the role of non-verbal communication specifically in education and other social sciences fields. The provisional title is The 'Ethology-and-Intelligence' Paradigm in the Behavioural and Social Sciences.)

A second aspect of the doctrine of 'the unsayable' which has also been touched upon in a previous Chapter is the connection between Wittgensteinian language-game philosophy and many Existentialist writings-and-doings. This has obvious connections with the 'ostensive dimension' just mentioned, and also, and importantly in the present context, with the widely held belief in educational circles that 'The best teaching is that done by example'. A further and related point is that the better Existentialist writers have recognized that many matters cannot be communicated (said) simply and clearly, and that this does not entail that they cannot be communicated or said at all: rather, the message is, that communication is possible, but it often has to be complicated, circuitous, subtle, and lengthy. It is thus not a matter of personal idiosyncrasy but rather an outcome of this particular philosophical position, that many Existentialists (Sartre, Marcel, de Beauvoir, and others) have extended their recognizably philosophical messages into the forms of drama, the novel, and so on. Here again is something which perceptive teachers have understood, and used in their teaching, for a long time. As an instance from my own experience, I recall coming into the Senior Lab in the Zoology Department at Otago, on a bleak day with the rain driving horizontally before a gale (Dunedin is well down in the 'Roaring Forties' of the old sailing-ship days), to face up to an afternoon's dissecting — and instead, having our august Professor read to us several fascinating chapters of Gerald Durrell's The Bafut Beagles. We 'got stuck into the dissecting', later, and with our enthusiasm, breadth of perception, etc., much enhanced. The later careers of the people who listened to Brian Marples on that afternoon are in process, I believe, of vindicating his

judgement, in many dimensions including specifically the use of teaching techniques which might not be approved by some educational 'authorities', in this case the use of 'real life' accounts of the worrying, amusing and sometimes downright hilarious 'incidentals' to zoological field-work. (One of the points is, of course, that these are not really mere 'incidentals' at all: they are the realities of particular types of research, as I found by direct experience in later years.) Durrell's books, and others of diverse kinds which we were encouraged to read, often had little apparent relevance (to some people, anyway) to the serious, not to say grim, study of the subject-matter (i.e. the findings) of the science of Zoology — but they had a great deal of relevance to the understanding of and enthusiasm for the activities of actually doing this type of science. Of course we also had direct experience of microscope work, dissecting, etc., in the laboratory, seeing museum specimens, going on field trips and to various institutions to gain experience of a diversity of techniques and activities. But there are limits to what any one person can experience directly; and it is valuable to supplement and extend this direct experience by means of surrogate experience in the form of narratives and discussions of the experiences of other people. If such narrative and discussion is presented in lively and stimulating form, and so that it chimes in with such direct experience as students already have, so much the better.

What is indicated in the foregoing is, I suggest, very much more to the point of 'knowing how to behave like scientists', 'knowing about the processes of science', and so on, than are the misleading abstractions offered in Science Education textbooks such as those discussed in Chapter 6, above. It may be objected, and quite fairly, that the example just given of my own experience was of undergraduate education at a university, whereas the textbooks in question relate to the teaching of science to children at school, mainly at primary/elementary level. It is true that there must be differences. The question is, are the present differences always of the right sort? I would reiterate the suggestions made earlier in this thesis, especially in Chapters 2 and 6, that a significant amount of teaching at all levels is misorientated and misleading. Specifically with regard to the material from textbooks discussed in Chapter 6, while the picture presented to younger children naturally has to be simplified, there is no need for it to be falsified in the way that seems often to be done. For example, where one would point out to university students that the application of a particular concept (word) was "inferential", one could get across the same basic idea to primary/elementary-level schoolchildren by saying something like "You have

to work it out in your head that ...". (It is apparent that the 'Teaching Children Science' language-games have to be progressively modified as the pupils advance in age, intellectual maturity, amount of already-acquired knowledge, and so on; and that a language-game approach in teaching science or any other discipline lends itself particularly readily to adaptation in the light of, for example, research findings on development and mechanisms of reading in children (see Clay 1972), as well as, perhaps most significant of all, allowing without disruption for the accommodation of revised findings of scientific research and even major alterations in techniques and/or methodology.)

Thus the reading of witty 'literary' books, also philosophy, biography and even novels — currently regarded as at best peripheral, if justifiable at all in science courses worthy of the name — can be seen to have very substantial importance when seen in the light of 'the unsayable' not taken literally but rather in the manner of the Existentialists, as signifying a need to deal with some matters in a complicated and lengthy fashion so as to show how they are, rather than merely saying/describing. The difference might be put by contrasting the plot of Hamlet written in three sentences, with reading the play in full. The play itself might again be contrasted, in a different direction, with reading a complete description of everything that can be seen and heard by someone watching a performance of the play on stage (or screen); and in a different direction again with a book giving a psychological analysis of the play ... All of these other things give descriptions of one sort or another — only the play itself shows how the characters react and interact. In science education, though I have just been affirming the value of biography and autobiography in coming to understand how science is practised, how scientists behave, we must recognize that such works do tend to describe rather than show, also they tend to be selective in one way or another, and present a story that may be somewhat different from the reality as it was. Lytton Strachey's Eminent Victorians (1967) achieved both notoriety and fame, as a break-away from the eulogistic tradition in biography; and though 'warts-and-all' biography can err in the opposite direction from eulogy, a tradition of realism and balance has certainly grown up in the present century. Nevertheless we can never be sure that the picture we get is a true one, as biographers can never know the full story of a person's life, and autobiographers are unlikely to tell it (and may even, so far as conscious recollection goes, be genuinely unable to tell it). Watson's (1968) The Double Helix has been

noted (Goodfield 1977) as giving an inside view of the rivalries, jealousies, etc., that lie behind the facade of serene objectivity of science; and as Goodfield says, we need more of this. But in some ways even very candid and open autobiography (e.g. Huxley 1970-3) is not so enlightening as, say, some of the novels about scientists and academics by C.P. Snow. The fictional characters have fewer inhibitions than real persons: they can tell all the details. If the writer is a good one, knows people and knows science and institutions, and so on, the reader can gain an insight, drawing upon his knowledge of the life he knows from the inside: namely, his own — of what the life of a scientist is like from the inside. Quite apart from the understanding of scientists in particular, this is how we can all get a better understanding of people; and 'people' includes ourselves. We understand ourselves by reading about others. This is why good literature is educative. (For the same sort of reason it is especially good for teachers.)

Relating to this Existentialist-type interpretation of the doctrine of 'the unsayable', and also to the first aspect touched upon, above, viz. the importance of 'ostensive learning', a third point which has already been discussed in Chapter 9 can be mentioned again here. This is the desirability of having extensive experience of a phenomenon, especially if it is new and/or complex and/or 'difficult' to experience, before one attempts to give a description or categorization of it (and a fortiori before one attempts to investigate its causality, etc.). The example discussed in Chapter 9 related to the investigation of the hitherto-unknown behaviour of a species of animal; the specific point made was that the already-established descriptive/classificatory categories might not be fully appropriate to the new species. The issue can be generalized, so that we can say, perhaps, that we would do well to be aware of the possibility that the purely verbal components of our language-games may come to be inadequate in some way or another; and that we ought therefore to be on the lookout for inadequacies, misleading formulations and, above all, that we should be prepared to try out alternatives, even when the standard verbalizations seem adequate, in case improvements can be found. It is here that the naive formulations of children and even of older students can provide valuable hints (as was mentioned in Chapter 7 in relation to academic examinations) as to changes from the standard language-games which may in some cases prove to be illuminating. Here also we get inter-disciplinary considerations inserting themselves again: for the arts of the poet, the novelist, the painter, musician, and others, are often concerned specifically with innovation; and the

habits of innovation, once acquired say in an artistic field, may carry over into other types of activity, e.g. science, or education.

A fourth and exceedingly important point with regard to 'the unsayable' relates especially to education in the sciences. To understand the issues here we have to take up the suggestion of Toulmin (1969) and others that there is a basic similarity of intent between Wittgenstein and Kant in the development of their several philosophies. Toulmin (1969: 62) says of Wittgenstein: "He was above all ... a "transcendental" philosopher, whose central question could be posed in the Kantian form "How is a meaningful language possible at all?"." The latter question we shall leave with Wittgenstein — his answer, briefly, lies on the concept of the language-game, and the fact that we give meaning to our utterances by the rules which we use for them — but the question of the 'transcendental' is what must engage our attention for the present.

Pears (1971: 46) puts the issue thus:

"The task which Kant set himself was the demarcation of the limits of thought, and the parallel task which Wittgenstein set himself was the demarcation of the limits of language. Wittgenstein's task may seem to have nothing to do with an investigation of the foundations of logic. But he saw a close connection between the two undertakings because he thought that logic covers everything that is necessarily true, and so can be said in advance of experience; or, to put this in the old terminology, everything that is a priori. It is, for example, a contingent fact that the moon is smaller than the earth, and experience was needed to establish it: but it is an a priori or necessary truth that it either is or is not smaller than the earth, and that could have been said in advance. Now the limits of language, like the limits of thought were supposed to be necessary limits. So, given Wittgenstein's broad conception of logic, it would be logic that plots them. In this way his investigation of the foundations of logic came to include an inquiry into the limits of language."

In this passage, Pears is talking about the Wittgenstein of the Tractatus: and as we have seen, he subscribes to the view that Wittgenstein led two revolutions (though he does acknowledge the existence of continuities between the Tractatus and the Investigations.) But Toulmin argues (p. 62) that "... the Tractatus and the Investigations have the same subject-matter". He goes on:

"At first sight, two books could hardly be less alike than the Tractatus and the Philosophical Investigations. On the surface, the Tractatus is a contribution to symbolic logic, in the tradition of Wittgenstein's immediate phil-

osophical teachers, Frege and Russell. By contrast, the Investigations presents an empirical-looking argument, designed to demonstrate the "prodigious diversity" of ways in which language is put to use in human life; and it continually seems to be verging, not into mathematical logic, but rather into anthropology and psychology. This surface contrast is, however, misleading. Waismann has recorded a conversation ... in which Wittgenstein talked about his disillusionment with logical symbolism, as an instrument for explaining the significance and scope of actual linguistic behaviour. Seven months later (1 July 1932) he was telling Waismann:

In the Tractatus I was unclear about "logical analysis" and the clarification it suggests. At that time I thought that it provided a "connexion between Language and Reality".

From these and other passages, we can now demonstrate how — despite the obvious differences between them — Wittgenstein could see his two books as successive attacks, using different methods, on one and the same group of problems."

These problems appear to be, basically, of a 'transcendent' nature; or more specifically, of dealing with the demarcation between what can be said as against what cannot be said ('the unsayable') but can only be shown.

Perhaps the nature of these problems can effectively be brought out by asking a question set in terms of the 'said/shown' dichotomy:

Can the demarcation-line, between what can be said and what cannot be said but only shown, itself be described or stated (i.e. said)? Or can it only be shown?

It seems clear that an affirmative answer to the first question would generate a paradox. If the boundary or edge of the sayable is itself sayable, then the edge or limit of the sayable has not been reached. Anything that is sayable is within the bounds of the sayable. But if the limit (or edge or boundary) of the sayable cannot be said (i.e. stated), then in the framework set up by Wittgenstein himself, it can only be shown.

This may seem straightforward enough, if we are thinking in terms of 'showing' in a context of 'ostensive learning' such as Wittgenstein himself discusses in the early sections of the Investigations; and if we are thinking in these terms, we must be thinking of the 'limits' which are in question within the implicit physical analogy. Physical limits or boundaries can indeed be shown, in a perfectly straightforward way — but they can also quite readily be described; and in fact the 'limits' or 'boundaries' which are in question here are not physical boundaries at all. Thus if we think that

'showing' these limits is going to be a straightforward matter, we have simply misled ourselves by our use of an implicit metaphor: that of the 'boundary' as being something like a fence, or a line of surveyor's pegs, or ... But the 'boundaries' or 'limits' of language cannot be of this nature; and they cannot be 'shown' (e.g. by pointing) in the way that 'showing' is done in ostensive learning. How then can they be shown?

Toulmin suggests the following approach:

"The nature of these problems is best indicated by following up the analogies between Wittgenstein and Kant. For Immanuel Kant, the central tasks of philosophy were (i) exploring the scope — and the intrinsic limits — of the reason; and (ii) demonstrating the consequences of our irrepressible tendency to run up against, and attempt to overleap, those unavoidable limits ... The ideas of Schopenhauer were profoundly influential in the circles within which Ludwig Wittgenstein grew up; and one can regard Wittgenstein's own philosophical pre-occupations as carrying further the variations on Kant already initiated by Schopenhauer. As a result, Kant's philosophical tasks were restated yet again: (i) exploring the scope — and the intrinsic limits — of language; and (ii) demonstrating the consequences of our irrepressible tendency to run up against, and attempt to overleap, those unavoidable limits. In composing the Tractatus, indeed, Wittgenstein still thought of language in very much the same way as Schopenhauer — i.e. as essentially "representational" — and he set out to analyse the resulting Verbindung der Sprache mit der Wirklichkeit in the new logical notation of Frege and Russell. Putting Schopenhauer's ideas on a formal basis: "unit propositions" represented "atomic facts", and the limits of the "sayable" were defined by exhausting the available "logical transformations" of these representations." (Toulmin 1969: 62-3).

Discussion of the Tractatus as a contribution to formal logic in the terms indicated in the last part of the above paragraph would take us far from the main line of the present argument, and is a vast topic in itself (see Anscombe 1967, Stenius 1964) — it may be remarked in passing that one of the problems touched upon is that of the intelligibility, on a representational theory of meaning, of a proposition which asserts "... the non-existence of a configuration of things" (my emphasis added) which, as Miss Anscombe points out, is usually "... a clear and intelligible idea" (Anscombe 1967: 71) despite the fact that in such a case there is nothing to be represented. Thus a representational theory presents severe difficulties (i.e. if put forward as a general or universal theory of meaning), and in his later work, of course, Wittgenstein abandoned the attempt to habilitate a representationalist general theory, and gradually built up the complex language-game theory which was eventually published in the Investigations.

Disregarding as secondary the question of why we should have an "irrepressible tendency" to push against and attempt to surmount the 'limits of language and/or reason' (some hints towards a possible answer to this one have already been given in Chapter.9, above), the primary question facing us at the moment is that regarding the nature of these 'limits'. Associated with that is the question of how we are to know the limits: that they should be stated (i.e. described) has already been ruled out, and we have accepted (if only for the sake of argument) that they have to be (and can only be) 'shown' — but we come again to the question of how they are to be 'shown'.

At this point it becomes evident that, in following up Toulmin's suggestion of analogies between Wittgenstein and Kant, we have to go beyond, or at least amplify very considerably, what Toulmin himself was able to say — or more properly in Wittgensteinian terms, show — within the bounds of a journal article.

One of the most basic features of the Kantian position, the paradigmatic shift which Kant effected as a way of coping with the problems exposed in Hume's philosophy, was the distinction drawn between 'the thing as known by us' and 'the thing in itself'. In the simplest possible terms, and putting the issue in modern and linguistic terms, Kant's doctrine is that it makes no sense to talk of anything (e.g. a material object) except as we do or could know it. Hume's mistake, he suggests, is that he treated 'things in themselves' as though they were known; whereas in fact 'things in themselves' cannot be known, they can only be argued for, or, putting it weakly, postulated. The sort of mistake that Hume made, in confusing the 'thing in itself' with the 'thing as known or knowable by us', is an example of what Kant in the Critique of Pure Reason called an 'antinomy': a confusion or in a broad sense even a selfcontradiction generated by basing argument upon false or misleading (though unrecognized) assumptions. Among the examples of antinomies are the two propositions "The universe has a beginning in time" and "The universe does not have a beginning in time", for both of which apparently-flawless arguments can be given, but which cannot both be true. Such antinomies can arise, Kant suggests, only because of suppressed premisses as to what can be known. The arguments on each side of the antinomy presuppose that it makes sense to talk about 'the beginning of the universe', as though it were something that could be known; whereas it cannot be known — for if a knower were present, i.e. existed, then 'the universe' would already have started to exist, notably in the 'person' (not necessarily to be equated

with a human person) who does the knowing. (The issue could be formulated in terms of 'observing' and an 'observer'.) Thus any talk about 'the beginning of the universe', if by this is meant 'the beginning of all that we know and can know about', is bound to be self-defeating and paradoxical. One of Kant's general messages is that we can talk sense only if we keep in mind that 'things' are intelligible only as 'things known or knowable' and not as 'things (as they may be) in themselves'. (Kant's German phrase is "Ding an sich", which translates literally as 'thing to itself', to me a more illuminating formulation.) Körner (1955: 92) talks of "The doctrine that the objects of experience are not things in themselves and that things in themselves are unknowable ..." as being part of that side of Kant's philosophy known as (and called by Kant himself) 'transcendental idealism', in contrast to the other and necessarily related 'empirical realism'. 'Things as known or knowable' constitute for Kant the realm of 'phenomena' in relation to which we operate as empirical realists; in contrast to this realm is that of the 'noumenal', for which we can only argue, whose existence we can reach only by inference.

In relation to the noumena/phenomena distinction, 'antinomies' can be understood as arising when noumena are treated as though they were phenomena, notably as though they could be known. Does the noumena/phenomena distinction therefore allow us to specify or define the conditions under which antinomies are likely to occur? And conversely, can we characterize such conditions and thus be able to prescribe how antinomies can be avoided?

In one sense, within the Kantian framework it is possible to state the conditions for the avoidance of antinomies, and I have in fact just done so: avoid treating noumena as though they were phenomena. The drawback to this, however (and investigation here will reveal the advance in efficacy of formulation represented by the Wittgenstein of the Philosophical Investigations as compared to Kant) — the drawback is, that on the grounds of his own basic argument, Kant is able to characterize the noumenal realm only negatively, only as being 'non-phenomenal'. Körner (1955) puts the relevant issues very clearly, I feel, and is worth quoting extensively at this point:

"Kant calls the things in themselves 'noumena' because they are entities of the understanding to which no objects of experience can ever correspond, and contrasts them with 'phenomena' which are or can be objects of experience. The conception of a noumenon is self-consistent and formed in an entirely straightforward manner by means of the rules which govern negation of concepts. If we know the rules which govern a concept, say 'coloured', and the rules governing the auxiliary term 'not', then we know ipso facto the rules which govern the negation of the concept, namely, 'not-coloured'. It

is, in particular, not necessary to the negation of the concept that we should know instances of it, even if the concept itself cannot be forced without knowledge of instances.

"Similarly if we have formed the concept of an object of experience or phenomenon ... and if we know the rules governing the auxiliary term 'not', then we have ipso facto formed the concept of a not-phenomenon or 'noumenon'. The difference between 'not-coloured' on the one hand, and 'not-phenomenon' ('noumenon') on the other, lies mainly in that while we may, and in fact do, experience not-coloured entities, e.g. pains, noumena are defined as something which we cannot experience. The concept of a noumenon is, as Kant puts it, a limiting concept.

"... In this sense the concept of a noumenon carries no metaphysical commitments with it further than the concept of a phenomenon. ...

"It is, as Kant points out, very easy to replace the negative concept of a noumenon, surreptitiously and illegitimately, by a positive one."

(Körner 1955: 94-5)

In other words, 'noumena' as strictly defined have no positive characteristics or attributes. Körner notes (p. 95) that Kant does not stick consistently to his own negative characterization of noumena, and goes on:

"It is, I believe, possible to reconstruct the Critique [of Pure Reason] in such a way that the concept of a noumenon is in fact used only as a negative concept. But it would be a mistake to regard such reconstruction as a mere interpretation of Kant's philosophy." (p. 95).

The position that emerges is that 'noumena' can be verbally defined, but only negatively, viz. as 'not-phenomena', 'not-experienceable'. But the question then arises as to how useful such a definition can be, in enabling us to avoid overleaping the boundaries of reason (and/or language) or even in enabling us to know where they are. Without arguing this out in detail, it seems clear that the attempt to use 'reason and/or language' to state and specify the limits of 'reason and/or language' is likely to involve exactly the same sort of antinomy as we have just seen generated with regard to the question of the beginning of the universe: if a rational account can be given of the limit, then the limit of reason cannot have been reached; if the limit can be stated (described, etc.) then the limit of language cannot have been reached. This is of course exactly in line with Wittgenstein's doctrine, that the limit cannot be stated but can only be shown. With regard to what 'showing' can mean in this context, however, we are now perhaps in a better position to specify what must be involved. Perhaps we can put it this way, that as the limit of reason is approached and then reached, reason turns to

unreason, i.e. nonsense. Similarly, as the limits of language are approached and then reached, language in the sense of 'intelligible and coherent language', turns to unintelligible and incoherent language.

An interesting and significant parallel emerges between this sort of account — which is really only indicating the sort of thing which we can expect to find as we approach the limits, rather than describing or specifying definitively the nature of the limits — and Kuhn's (1962) account of the signs of crisis in normal science, or, more generally, of the signs which herald the onset of 'revolutionary' change. These signs can be characterized generally as 'anomalies', and they have been discussed in several parts of earlier Chapters. Of them, Kuhn himself remarks (1962: 52-3):

"Discovery commences with the awareness of anomaly, i.e., with the recognition that nature has somehow violated the paradigm — induced expectations that govern normal science. It then continues with a more or less extended exploration of the area of anomaly. And it closes only when the paradigm theory has been adjusted so that the anomalous has become the expected."

If we relate this back to the model presented in Chapter 8, above, of families of language-games each family resting upon a Common Assumptions Paradigm (each of the CAPs in turn having come to be accepted or taken up as the outcome of successful Public Demonstration Paradigms), and if we continue the device of referring to successive paradigms within a particular field as CAP₁, CAP₂, and so on, we find that the limits of 'the sayable' (in Wittgensteinian terms) and perhaps also of 'reason' are in fact shown by the occurrence of anomalies within (or perhaps better, at the edge of) the language-games associated with a particular CA Paradigm.

It would appear to follow, from the earlier discussions of this model, that anomalies are determined or constituted by the particular nature of the underlying CAP; and that they are necessarily, therefore, to some extent relativistic, in that what counts as an anomaly in the language-games of CAP₁, may not be an anomaly within those of CAP₂. Exactly this is the case within the language-games of science. Innumerable instances could be given. To re-use one which has been mentioned in previous Chapters, what is seen and described by contemporary physicists as electrostatic repulsion was seen and described by earlier physicists (whom we can represent as having been adherents of paradigms which we can collectively designate as those of CAP₁ generation, in contrast to the modern paradigm which can be designated CAP₂) as being either mechanical rebounding of the chaff particles from the

larger (and electrically charged) body, or else as simple 'falling off' under the influence of gravity; see Kuhn 1962: 116. Within the 'mechanical rebounding' paradigm, it would be anomalous if the particles were observed to move faster on the 'rebound' than they did on the 'approach'; and a statement describing such observations would have been anomalous within the l-gs of the 'mechanical rebounding' paradigm. But of course it is important to realize that, to the users of the 'mechanical rebounding' l-gs, it would have been impossible to give a satisfactory account of exactly what was wrong with this particular part of their language-games, it would have been impossible to characterize the exact nature of the anomaly, and it would very likely have been difficult even to state with confidence the existence of an anomaly, i.e. to tie it down, as it were, to a particular range of observations. Once the new paradigm (CAP_2) has been established — we may refer to it in limited form as the 'electrostatic repulsion' paradigm — then the users of the CAP_2 language-games, who because they know something of the history of their subject and of its other branches, can also use the l-gs of 'mechanical rebounding' and of 'gravitation', can specify what the anomaly really was: that the phenomena of electrostatic repulsion were mistakenly seen in terms of the other paradigms and described (wrongly) in terms of the other language-games.

But we are justified in saying, categorically, that the descriptions in terms of the other language-games were just wrong? Following Kuhn (1962, especially his Chapter 10) I think we have to say, not (or not only) that they were wrong, but rather that they failed to capture the most important or most significant feature of the situation. The particles which rebounded were also, we must presume, subject to the influence of gravitation; and mechanical rebounding might also have occurred, though probably only to such a slight extent that it would have been very difficult if not impossible to notice (bearing in mind the apparatus available at the time). — So what could have been said in the terms of these other paradigms was not utterly wrong, it was not ridiculous still less nonsensical: it merely missed what turned out, eventually, to be the most significant of the phenomena involved in the situation.

From the considerations adduced above it becomes apparent, I suggest, that while the approach to the limits of 'the sayable' may be characterized as involving an increase in the number (and perhaps also the 'severity') of the anomalies that are generated, Wittgenstein was absolutely correct in his insight that these anomalies themselves are not 'sayable' — and we are

now in a position to give an account, using the Kuhnian concept of the 'paradigm' in the special sense of the Common Assumptions Paradigm, of the conditions under which the anomalies which are symptomatic of the limits may and ~~may~~ not be specifiable (sayable), may and may not be even 'showable'.

Taking any family of language-games constituted by their common association with and dependence upon a particular Common Assumptions Paradigm CAP_1 , the limits of 'the sayable' within this family of l-gs are signalled ('shown') by the occurrence of anomalies which are not resolvable or even statable ('sayable') within this family of l-gs, because of the nature of the underlying CAP many of whose 'components' (assumptions, etc.) are not consciously known to the adherents of the CAP.

The 'limits of the sayable' (or in Kantian terms 'the limits of reason') are not, however, absolute limits, but are relative to the particular CAP which underlies the language-games in question. Thus if a 'revolution' or 'paradigm switch' occurs, with a transition from CAP_1 to CAP_2 , what was previously 'unsayable' but of which people could become aware (being 'shown' perhaps by being taken through the language-games and becoming conscious of some 'oddity' or anomaly within them), could in the language-games of the new paradigm CAP_2 actually be stated. This is not to say that all anomalies are necessarily 'sayable' after a particular paradigm-shift from CAP_1 to CAP_2 — some of the anomalies within the CAP_1 family of l-gs may not become statable, hence resolvable, until a further revolution has brought in a third paradigm, CAP_3 . Clearly, the use of our 'CAP model' does not adequately represent the full complexity and subtlety of actual revolutionary changes, paradigm shifts, in science or any other field of intellectual activity. It does, however, help towards an understanding of problems which, if left in their complicated 'real life' form, would perhaps be too difficult readily to be dealt with. In particular, I suggest that it does bring out the relativistic nature of the distinction between 'what can be said' and 'what cannot be said but can only be shown'; and if this distinction is accepted as being relative always to a Common Assumptions Paradigm, the way is open for a better understanding of how progress can occur in other intellectual disciplines — notably philosophy itself — as well as in the sciences. Wittgenstein's closing statement in the Tractatus, to the effect that we must keep silent about that of which we cannot speak, can if taken as an absolute and categorical pronouncement become either a slogan for positivistic limitation (see Toulmin 1969: 60), which can lead in turn to reductionism and related fallacies (see Chapter 2, above); or else it

can provide grounds for misrepresenting Wittgenstein as putting forward a static and in a sense reactionary doctrine (which would be sharply at variance with what is implicit in his own development and use of the 'language-game' concept).

An important proviso must be made with regard to the foregoing, notably the suggestion that the 'limits of the sayable' are 'shown' by the occurrence of anomalies. It is implicit in the account given above — and it must now be made explicit — that what is shown is nevertheless not necessarily clearly and distinctly perceived. It is not necessarily not clearly perceived, either; and this indeterminacy with regard to the clarity or otherwise of what is pointed to, shown, or indicated, accords with our actual experience of approaching the limits of a language-game. The researches of the last two decades on the general topic of creativity have shown that, especially with major or 'revolutionary' creativity, the shift to a new paradigm is usually accomplished only gropingly and unsurely: the title of Koestler's study of the persons who actuated the 'Copernican revolution' brings this out: The Sleepwalkers! But acceptance of the idea that what is shown is not thereby clearly perceived or understood will not come hard to anyone who has been involved in the practicalities of education: it is commonplace that not many see what is shown to them, not for a while, anyway. And again, in the practice of critical philosophy, when one is examining an extensive argument, say in a journal article or a book, it is quite standard to have a 'feeling', a hunch, that 'something is not quite right', long before one is able to work out precisely where the fault lies and what its nature is. This sort of 'feeling' would appear to be the subjective concomitant of being 'shown what is unsayable' at the limits of a language-game — and in fact, in such a case, one is working at the limits of a language-game: the limits of one's own personal language-game dealing with the topic in question, whatever that may be. If one comes to be able to specify the fault in the argument, expose it explicitly and perhaps restructure the argument to avoid it, one has extended the limits of one's own language-games and has said what was previously unsayable. It is also in principle possible that one may extend, not only one's own l-gs, but even the totality of language-games.

Wittgenstein's doctrine of 'the unsayable', if interpreted as in the foregoing, thus opens up the possibility of extension of 'the sayable', i.e. of language-games in total, and, interpreted in Kantian terms, allows for extension in the scope of reason. This possibility is of great importance

in several areas.

In the first place, it allows us to see history (in general, or histories of particular disciplines e.g. the sciences) as involving not only the replacement of one set of language-games by another, and that by yet others, and so on — which if left in those terms could tend to trivialize both the history, and also the language-game approach to its interpretation — but the genesis of new language-games out of what was hitherto 'the unsayable', i.e. genuine and in a sense 'absolute' advances in understanding.

It is important to emphasize this, since Kuhn and others, for example, with regard to the historical development of science have quite rightly been concerned to argue against the simplistic view (still widely propagated, e.g. in textbooks of science and of science education), that what is involved is essentially a mere accumulative process of factual discovery, and that new theories are mere inductive generalizations from the newly-discovered facts. Against that sort of 'additive' view (cf Stenhouse 1971), it is highly desirable to stress the 'transformative' view, that old science was not simply mistaken and has now been replaced by 'true' science, 'true' theories, but rather that the older and now abandoned theories did make sense in relation to the evidence and the assumptions etc. (the CAPs) of their own times. There is a danger, however, that as this new relativistic and 'transformative' approach is taken up more and more widely, and perhaps in some cases by persons who do not fully understand it and the background against which it was developed, we shall see it distorted into a simplistic 'anything goes' approach in which any one language-game is assumed to be as good as any other. In the social sciences we have in recent years seen a simple-minded 'social relativism' become widespread (see discussion of examples in Andreski 1974, also Goody 1977 for penetrating discussion of basic methodological issues); and a 'phenomenalistic' approach has been invoked in an attempt to justify 'research' programmes in some fields which have amounted to little more than uninformed meddling. It would indeed be a pity if the valuable work of 'relativistic' people (like Kuhn, Toulmin and others in relation to the history and philosophy of science) were to be debased as they spread within the institutions of formal education.

A second and extremely important consequence of a proper understanding of the relationships between 'the sayable' and 'the unsayable' is prospective rather than retrospective as in the case of history. In terms of present-day education — upon which depends, to a great extent, our individual and

collective capacity for adaptation to the future — understanding of the 'sayable-unsayable' relationship furnishes us with a powerful weapon against dogmatism and conservatism and the backward-facing orientation which has been increasingly affecting our intellectual life. What is involved here may perhaps best be explained by means of an example.

Consider, say, an antique table made perhaps in the year 1700. We can make various statements about it which we would accept as being true. Among such statements would be, that it is made up of atoms and molecules, and that its atoms are made up of electrons and protons and so on. Now consider the people who had experience of the table soon after it was made, say in the year 1701. They would not have been in a position to say that it was made up of atoms, electrons, protons, etc.; and even if someone had uttered the words "It is made up of electrons and protons ...", nobody would be able to understand what was being said, and it would not be possible for the statement to be explained to them (not, certainly, in the way that it could now be explained by anyone with the requisite knowledge of physics). Yet, looking back, we feel that it was made up of electrons and protons, etc., even back in 1701 (or 1701 BC, or 170001 BC). In other words, the electrons, protons, and so on were not invented by Rutherford, Bohr and others in the early years of this twentieth century: they were discovered. They have been there all the time, even when nobody knew about them. Putting the issue in yet another way, we can say that the electrons and protons etc. have in our century moved out of 'the unsayable' into 'the sayable'. (This is not to suggest that everything that is said about them is true, or will not be modified in the light of further research.) Now we can talk about electrons and protons etc., and provided we have learned the relevant language-games properly, what we say will make sense to others who know these language-games, and what we say can be true (but of course could always be false, if we break the rules of the l-g).

If we now detach ourselves from the present, and try to see our own time in the longer perspective of history, we can ask: "What properties might the table have now, which we did not suspect (just as the people in 1701 did not suspect the presence of electrons etc.), but which may be discovered by new sorts of research which may be developed during the next few centuries? Or even the next few decades?"

We cannot, of course, give a positive answer to such a question — but we can give a negative answer. We can say, indeed I would suggest that we must say, that it is impossible for us to assert that no more properties

can ever be discovered in the table. It might in fact turn out to be the case that no more properties are discovered; but we are in no position to know this at the present time, and we would therefore be unjustified in asserting it.

The issue can be put differently, in the form of a dilemma:

Either (a), the progress of science has just at this moment reached its culmination, we know everything there is to know and continuation of scientific research is futile because nothing further can be discovered (though it may be a few weeks or months before practising scientists are forced by circumstances, and reluctantly, to accept that this is the case);

Or (b), science is going to continue much as it has in the past, at least in the sense that progress will continue (by alternating periods of 'normal' and 'revolutionary' science, if we accept the Kuhnian paradigm), new discoveries will be made and new theories developed — in which case, clearly, we cannot rule out that new properties may be discovered in our table.

I suggest that we do have to accept one or the other horn of this dilemma — either science is going to continue, or it is not — and though horn (a) might be forced upon us, for example by the extinction of the human species in a nuclear holocaust, it does not seem to me to be reasonable to choose horn (a). If 'reason', 'rationality' and related concepts include within themselves any notion of 'keeping open the possibilities' (as we have seen in earlier Chapters many people feel they do, e.g. Kirk, Horton, Goody, and others), then anything which closes down the possibilities — to zero, in the case of nuclear holocaust and human extinction — is neither reasonable nor rational.

Consideration of Horn (a) of the dilemma has its obvious importance; but on the assumption that the human species and human societies are going to continue, and notably that formal education is going to continue though not necessarily (and perhaps not desirably) in exactly the same form as it takes at the present time, then Horn (b) takes on a particular significance.

Within science education (and mutatis mutandis within education in any other discipline), consideration of the implications of Horn (b) especially with regard to future developments, may be expected to reduce complacency and self-satisfaction with regard to what has already been achieved and with what is now being passed on to the rising generations. To put one particular issue in simplified and perhaps extreme form, if the future of

science is going to be like its past in the relevant respects, then some of what we are now teaching our pupils will at some time in the future be revealed to have been erroneous. This should be a disturbing thought to all educators. What can we do about it?

One thing which could be done, and I urge should be done, is for all teaching to be made adequately sophisticated, along lines indicated (though not extensively explored) in the preceding Chapters. (There are limits to what can be attempted in even a fairly sizeable thesis; this one has been concerned with methodology rather than technique; too lengthy and/or difficult a message will merely actuate people's 'switching-off' mechanisms.)

In summary, while 'naive realism' (as philosophers would call it) has to be avoided in one dimension, i.e. as regards the subject-matter of science, in another dimension — that regarding the so-called 'processes' and activities of science — what is needed is a great deal more realism, of the sort revealed by Kuhn, Toulmin, Goodfield, N.R. Hanson, and others who have attempted to depict the real activities and thinking of scientists; and a great deal less in the way of 'fairy stories' of the kind revealed in some of the textbooks on science education, as discussed in Chapter 6. And one extremely important dimension of sophistication and realism with regard to the methodology of science (and therefore at one remove of science education) is recognition of the significance of 'the unsayable' in all of its several interpretations.

Several of its interpretations have been discussed already in this Chapter. Others remain to be mentioned (though we cannot attempt a comprehensive survey of all possibly-significant interpretations). Some of those which have significance especially for education must at least be touched upon, even though extensive discussion of them cannot be attempted.

On the question of values, Toulmin (1969: 61) argues as follows:

"Far from equating the important with the verifiable, and dismissing the unverifiable as "unimportant because un-sayable", Wittgenstein took exactly the opposite stand. In the concluding section of the Tractatus, and repeatedly thereafter, he kept insisting — though to deaf ears — that the unsayable alone has genuine value. We can, he tells us, discover "the higher" only in that which the propositions of our language are unfitted to capture; since no "fact", such as can be "pictured" by a "proposition", has any intrinsic claim either upon our moral submission, or on our aesthetic approval. So Wittgenstein's silence in the face of the "unutterable" was not a mocking but a respectful silence. Having decided that only "value-neutral" facts

can be expressed in regular propositional form, he exhorted his readers to turn their eyes away from factual propositions to the things of true value — which cannot be gesagt but only gezeigt."

Three significant dimensions of life are mentioned in the above: the moral, the aesthetic, and "the higher" by which it appears Wittgenstein meant what most people would call the religious. All three, but especially perhaps the first and the last, are of major and increasing concern to educationalists at the present time. What implications can be discerned in Wittgenstein's life and philosophy for our understanding of these dimensions and of education relevant to them?

Of the aesthetic dimension I propose to say virtually nothing, beyond that it is generally recognized as being important, that many education systems already devote substantial efforts towards it (and most at least acknowledge that such efforts are desirable), and that in this dimension what can be shown and done is fairly obviously more basic than what can be said. Not that talk about music, art, literature, and so on, is of little value — far from it — but clearly, the composing and playing of music, the painting of pictures, etc., come before the discussion of them. Literature might appear to be an exception or at least a difficult case, in that it does use words and in one sense is certainly gesagt (said — whether in oral or written form); but as hinted earlier in discussing the methods of Existentialist writers, we could argue that 'what is said', i.e. the words, the descriptions of what the characters do and say, etc., is in a real sense only the medium through which the significant message is transmitted; and that this real message is shown, rather than said directly, by the words.

Questions of morality and moral education have been assuming great and increasing prominence in recent decades, in part due to greater contact between major cultures (which in turn has been due to technological achievements, e.g. in transportation and travel), and in part due to doubts and questionings of traditional Western values from within the Western society itself. Whatever the causes (and it would be beyond our scope to attempt to examine them here), I shall simply stipulate that a widespread concern about morality and moral education is a fact of present-day life; and that there is considerable diversity of views as to what is meant by 'morality', 'moral education', and other related terms.

One symptom of the general concern about morality has been the proliferation of books about morality and moral education (see Wilson 1965, Wilson et al. 1967 as early examples of the current 'wave', Lindley et al. 1978 as a

recent one). Many of these books are extremely good, e.g. that by Mackie (1977) which has been mentioned earlier. The sub-title of Mackie's book "... Inventing Right and Wrong" seems to me particularly valuable at the present time, and especially for those concerned with education, in that it emphasizes by implication that we all, collectively, as people, and each one of us separately as an individual, have responsibility for the moral decisions, the moral attitudes and so on, which affect our own lives and the lives of everyone. Again, in their Introduction, Lindley et al. deplore the fact that philosophy is often presented in such a way that "... it can seem very remote from people's ordinary lives." (1978:vii). They go on: "We think this is unfortunate, because philosophy can be of use and interest to a much wider group of people than select members of the academic community". As will readily be appreciated by readers of this thesis, nothing could be more in accord with my own views and intentions!

Granted the importance of clear thinking in the ethico-moral and related domains (e.g. politics, and social issues generally), and stipulating that in democratic societies clear thinking in these domains is the concern of everyone, it would appear to follow that public (formal) education should devote substantial efforts towards enhancing general perceptiveness and effective intelligence in these areas. The question then is how best such education is to be accomplished.

There can be no doubt, I suggest, that great improvements can be effected by means of rigorous and percipient discussion of moral and related issues. In this connection, I would reiterate the suggestions made earlier, that the initiation of teaching in any area, and particularly this one, should start with the existing interests and concerns of the pupils. Lindley et al. follow this strategy in their book mentioned above, where they discuss issues like abortion, contraception, etc., which are of very immediate practical as well as theoretical importance to adolescents and young adults. But while it is possible to do a great deal in teaching 'critical and rational thinking' at this level, i.e. teaching students to cope with tendentious and fallacious 'argument' in these areas (where it abounds) and to distinguish between education (argument and discussion) and indoctrination (which are often confused), it is in my own experience difficult for many students to break free from their own particular emotional commitments in this area sufficiently for them to be able to give proper consideration to positions alternative to their own — it is often difficult for them even to perceive, in any real sense, any range of alternatives (which, as we have argued earlier,

seems to be a substantial component in 'rationality'). In this connection, Toulmin's 1969 paper reveals that my own practice in attempting to teach philosophy (specifically from 1966 onward) seems to have unwittingly paralleled that of Wittgenstein himself. Toulmin says (p.64) that "... the two sides of Wittgenstein's thought — philosophical and ethical — were evidently related. As a philosopher, he did what could be done to show men the boundary between the sayable and the unsayable, and to delimit what Peter Strawson calls "the bounds of sense". This was best done, in practice, by using examples concerned with comparatively "unimportant" topics: for instance, from philosophical psychology and the foundations of mathematics. But, in his "At Homes", Wittgenstein would let the discussion go much further afield ..." In other words, he seems to have tried to induce increased flexibility, enhanced awareness of wider ranges of possibility at the level of 'the sayable' (and 'the thinkable'), by assisting the students' powers of abstraction from the immediate and 'concrete' and emotively-loaded (see discussion of this in relation to the P-factor of 'evolutionary intelligence', and to the work of Piaget, in Stenhouse 1974), by deliberately giving them philosophical discussion which was chosen specifically to be 'light' in all these dimensions, hence easier for them to deal with in 'rational and philosophical' terms, i.e. in being able to find wide ranges of possible alternatives, i.e. in being able to construct and/or explore a wide range of alternative language-games.

But 'the sayable' is only one dimension, and in Toulmin's (and others') view the less important, for Wittgenstein. Toulmin goes on (ibid.):

"Perhaps the "ethical", though inexpressible in "direct discourse", could after all be hinted at in some less "representational" way. At any rate, he never gave up the attempt. Though, even in his last years, he still seemed to believe what he had said as early as the Tractatus — namely, that whatever belongs to the realm of "the higher", whatever has "value", lies outside the boundaries of the utterable — he continued, like Kierkegaard, to seek some alternative way of conveying what could not be stated."

In the concept of the 'language-game' which I have been developing to expressly include all the non-verbal elements of our activities, as well as the purely verbal, there is in principle no difficulty, I suggest, in recognizing and giving emphasis to 'teaching by example' in moral education and in education generally. Thus if a 'language-game approach' in education is understood as necessarily including the totality of behaviour as intrinsic to the very concept, the scope of education must in principle also be total; and

with regard to moral education, the total behaviour of those who set out to teach it is necessarily to be regarded as (ultimately at least) a part of their teaching. (A particularization of this was discussed in my 1969 "Good persons, good teachers and language-games".) And a corollary to the general position is, that all teaching, whatever the nominal 'subject', is part of moral education; in fact all living, especially that of adults vis-a-vis children and younger and/or subordinate people, is part of moral education.

The obvious question to be posed at this point — but to which I am emphatically not going to attempt to give a comprehensive or even an adequate answer — is that of the types of behaviour which it may be desirable to 'show' in the course of attempting to provide moral education as a 'totality of language-games in relation to forms of life'. Some negative answers may be offered. It is not suggested that mere conformity to 'conventional morality' is desirable (though rigid conformity to an extreme of such morality was demanded of schoolteachers, as a relict of the Victorian outlook, well into the present century; and is still demanded today in some places). At a different extreme, unthinking compliance with all the fads and fashions — of dress, mannerisms, intellectual assumptions, educational practices and so on — seems unlikely to achieve much for the moral education, or any other sort of education, of young people. What is desirable, I suggest, is that teachers should exemplify intelligent and rational behaviour, and concern for their pupils' welfare — the latter not implying mere permissiveness, the former definitely implying rigorous thinking in the usual sense but also including (what has hitherto been understressed) the ability and willingness to look for and at a wide range of alternatives (as the leading scholars from various disciplines have all, in one way or another argued for, in their works cited in our earlier Chapters). Perhaps we could paraphrase Wittgenstein's plea to Russell, in a letter quoted earlier: "Don't assume that everything that you cannot understand (perhaps we should qualify: "at first glance/hearing") is nonsense!"

Various other suggestions can be put forward, to show at least a few of the more positive possibilities. The implications of "Inventing Right and Wrong" (Mackie 1977) are important, as noted already; and it can be useful to read works on ethics, not to be told how to think or act, but to be shown how one can work out the presuppositions, implications, alternative interpretations, and so on, of ethical/moral utterances. (I might perhaps point out that, if we take philosophy as an activity, then philosophy books should

be interpreted as in an important sense showing rather than saying ...) Also, the totality of 'teaching' language-games in which the individual teacher participates, while they may well show some inconsistency between what is said (prescribed) and what is done — it seems good to have ideals, but it is also good to recognize that we seldom live fully up to them — should not exhibit too gross a discontinuity between prescription and action, between what is said and what is shown. This is probably even more important at institutional level, since there are probably more individuals who live up to their ideals than there are institutions which do so; also, institutions are more apt to proclaim their ideals, and endeavour to enforce them on all who will submit, than are individuals.

Lastly as regards moral education, the distinction mentioned by Davies (1975) between 'moralist' and 'causalist' approaches appears useful, and with potential for further development and finer distinctions to be made. The 'causalist' approach is very much compatible with that of the 'language-game', in that both emphasize the need both to take a wide view and also give careful examination to details; and both put a premium upon percipience and intelligence.

Turning briefly to the question of religion — again, brevity is not to be understood as implying lack of importance, but rather as an exemplification of Wittgenstein's view that little of significance is readily 'sayable' in this area — it is clear from what Malcolm and von Wright (1958), Toulmin (1969), Pears (1971), Kenny (1973) and others have written (the latter two authors both give extensive references), that Wittgenstein himself endeavoured to practice his teaching in the actualities of his own life. He was certainly not religious in any conventional sense. On the other hand, von Wright (1958: 10-11), writing of the later 1920s, says: "In this period, Wittgenstein contemplated entering a monastery. The same thought occurred to him at other times in his life too. That it never came true was, partly at least, because for him the inner conditions of monastic life were not satisfied." The nature of the 'inner condition' that was never satisfied is hinted at, later in the Biographical Sketch: "His life was a constant journey, and doubt was the moving force within him." (p.20; my emphasis added; of my analysis of the role of doubt and 'negating' in creativity, Stenhouse 1971, 1974). Presumably it was doubt which precluded his entering the monastery. Later (p.22), von Wright writes:

"In Wittgenstein many contrasts meet. It has been said that he was at once a logician and a mystic. Neither term is appropriate, but each hints at something true."

Malcolm (1958: 27), while he is not at this point dealing with any question of Wittgenstein's religious position, does give us a glimpse of what may have been associated with or an emanation of a basically religious orientation:

"Primarily, what made him an awesome and even terrible person, both as a teacher and in personal relationships, was his ruthless integrity, which did not spare himself or anyone else."

Later, Malcom devotes several pages (pp 70-72) to "... the difficult subject of Wittgenstein's attitude to religion". I shall not attempt to discuss all the points raised, or even mention them; but will pick on a few that seem to me significant in the present context.

"Wittgenstein did once say that he thought that he could understand the conception of God, in so far as it is involved in one's awareness of one's own sin and guilt." In talking of 'sin and guilt' he is of course emphasizing the negative side, and a retrospective view, of one's actions as a person — one's living — and he seems to imply that he could accept that God was, or could be, in some way involved in this living. Malcolm goes on (p. 71): "He added that he could not understand the conception of a Creator" — this seems to imply that the concept of a God operating outside the Universe was unintelligible to him, and probably (I feel) that such a notion would generate logical 'antinomies' similar to Kant's, discussed earlier, about the universe having and not having a beginning in time; also, I feel that possibly Wittgenstein might have regarded speculations about 'a God over and above One who is involved (immanent) in human affairs' as being not only unintelligible but very much beside the point. Malcolm remarks (ibid.) that "He was impatient with 'proofs' of the existence of God, and with attempts to give religion a rational foundation". This statement on its own raises questions about what Wittgenstein meant by 'rational' in this context — presumably it would be roughly equatable with 'logical proof' rather than with the sort of interpretation we have adduced in earlier parts of this book, i.e. something along the lines of 'exploring the possibilities of interpretation and the various (stronger or weaker) lines of implication that flow severally from them' — but if religion were regarded as 'irrational' in the sense of 'being unsusceptible of proof', it does not seem that Wittgenstein regarded it as being irrational as implying absurd or necessarily unreal. In support of this sort of view, several further points made by Malcolm (ibid.) are relevant. "When I once quoted to him a remark of Kierkegaard's to this effect: 'How can it be that Christ does not exist,

since I know that He has saved me?', Wittgenstein exclaimed: 'You see! it isn't a question of proving anything!'... He referred to (Kierkegaard), with something of awe in his expression, as a 'really religious' man. ... The Journal of George Fox, the English Quaker, he read with admiration — and presented me with a copy of it."

The foregoing tends to support the view put forward by Toulmin (1969: 63), as follows:

"For though, as a philosopher, Wittgenstein was concerned to delimit the boundaries of language — i.e. of what could literally be "stated" — his deeper ethical reason for drawing this boundary was not to confine men inside it ...; but rather to show that all the really important issues — about God and freedom, value and immortality — overlap the boundary and lie very largely in the realm of the unsayable."

Toulmin also quotes Englemann (1967) on this general issue:

"A whole generation of disciples was able to take Wittgenstein for a positivist because he has something of enormous importance in common with the positivists: he draws the line between what we can speak about and what we must be silent about just as they do. The difference is that they have nothing to be silent about. Positivism holds -- and this is its essence — that what we can speak about is all that matters in life. Whereas Wittgenstein passionately believes that all that really matters in human life is precisely what, in his view, we must be silent about. When he nevertheless takes immense pains to delimit the unimportant, it is not the coastline of that island which he is bent on surveying with such meticulous accuracy, but the boundary of the ocean." (p. 61).

Any statement about Wittgenstein's religious position must be tentative in the extreme; but from the foregoing, it appears that the 'religious dimension' (as we might call it) was of absolutely fundamental importance to Wittgenstein, and that this dimension was at the very heart of 'the unsayable'. Did he then regard religion as a complete mystery, the existence of which could be recognized but about which nothing could be done? Was 'religion' an enclave of mystery, sealed off from ordinary life, to be saluted respectfully and then passed by? Quite the opposite, it seems: probably it was this attitude which angered him in persons of 'conventional religion'. He himself appears to have taken the view that while little of significance could be said about religion, what one does about it is of the ultimate significance. He seems indeed to have taken a rather extreme position with regard to his own life: any shortfall from perfection seems to have generated agonies of guilt. It seems never to have occurred to him that

perfectionism might tend to be counterproductive, i.e. that one ought to accept one's own shortcomings, one's own sins, as being inevitable in the conditions — God-made conditions — of our existence, and that while complacency about them is never justifiable, an extreme of guilt may also be unjustifiable and may indeed be an indication of egotism rather than religious devotion. It must also be recognized, however, that we can very easily use this sort of consideration to give ourselves excuses and rationalizations; and probably Wittgenstein was right if, perceiving the dangers here, he decided to err on the side of demanding from himself too much rather than too little.

It is clear that there was no major discontinuity between Wittgenstein's theoretical position with regard to religion, and his own practice in everyday life. If he regarded religion as part of 'the unsayable', it is certain that he said very little about it (so far as can be judged from published reports). An interesting point emerges, which is probably of considerable importance: if we accept the view that religion should not or cannot properly be talked about (gesagt), then we might assume that it must be 'shown' (gezeigt) or demonstrated, and that this can be done by each individual only in terms of his/her own life. So far so good. But we might be tempted then to ask: "What should be shown?", meaning that if it is the effect of religion that I am attempting in my own life to demonstrate, shouldn't I say that this is what I am doing (so that other people will know what it is that I am attempting to demonstrate)? In other words, should not the relationship between 'showing' and 'saying' which holds in all the other 'ostensive learnings' of our lives — namely, that we both show and say — hold also in this one? Wittgenstein's answer is both given negatively, and is in the negative: he made no claim, ever, so far as has been reported, to have been living or attempting to live as a 'religious'. Yet his life shows many similarities with the lives of persons who have been recognized as religious and who, while not necessarily making any overt claims to be living a religious life, have nevertheless, by the nature of their discourse spoken or written, allowed their concern with religion to become obvious to the people around them. Wittgenstein gave away the considerable fortune he inherited from his father, and lived a life of Spartan simplicity (or perhaps I should say, monastic simplicity). On the outbreak of World War II he left his Chair at Cambridge and worked as a hospital porter. His "ruthless integrity", as noted by Malcolm, above, was a salient characteristic throughout his life. Clearly he organized his own life on principles which to him had an overriding compulsion; and it appears, even from the fragmentary clues available

to us, some of which have been cited above, that these principles were for Wittgenstein of a religious nature. Yet he appears to have maintained virtually a total silence about them and about religion in general. So it seems that in the religious dimension, 'the most basic, the most important of all, he did not follow his own practice with regard to other aspects of 'the unsayable', and combine both showing and saying: he followed literally his own dictum in the concluding sentence of the Tractatus, and was silent.

The question we are left with might be put in this way: if the religious dimension is the most important in life, and if we feel that we have a duty to our children (to young people in general) to educate them about the things in life which are important, how can we educate them about religion — no, this would have to be re-ph²ased to 'how can we educate them in the religious dimension' — if we are not able to talk about it?

It would be pointless, I feel, to pretend to work out, even on the basis of things we know he said and did, what Wittgenstein's own answer to this question might have been. One can only endeavour to work out, on the basis of what we know, what an answer along Wittgensteinian lines might be. Some points can be made with fair confidence. The religious dimension cannot be described — but this does not preclude that it may be 'indicated', in words and in other ways. Before anything can be indicated, however, it has to be there, it has to be present and able to be perceived in some way or other, able to be experienced. So the religious dimension has to be instantiated before it can be indicated, shown; and this means, 'operationally', that there must be a person present who instantiates a 'religious life' to at least some extent. With regard to any given individual, A, who is concerned to provide education in religion to his/her children (or any children), A can do either or both of:

- (i) Rely on some other person(s), B, C ... to provide religious instantiation which can be shown to the children; or
- (ii) Endeavour to provide the instantiation of religion in his/her (A's) own life.

For various reasons which I feel need hardly be enumerated, alternative (ii) would appear, on both moral and 'operational' grounds, to be preferable.

The implications for religious education — if we interpret this in Wittgensteinian terms, and not just as 'teaching people about various religions' — are fairly obvious. The 'teacher' is not, perhaps, prohibited

from talking about religion, especially perhaps in answer to specific queries put up by the young people — most of the 'teachings' of the notable religious leaders can be seen, when the reports of them are examined carefully, to have been in response to questions (see the Christian Gospels, for example) — but preaching would appear to be 'out', and also (probably) theology. (It may be that the latter could be acceptable if framed very emphatically as a hypothetical enquiry, but its status within a Wittgensteinian frame of reference seems rather dubious.) The main task and objective of anyone who would attempt to be a 'religious educator' in the Wittgensteinian frame of reference is to live a religious life and in so doing to show (by example) what cannot be said.

How then can we know what it is to live a religious life, how can we know what to do and what not to do?

True to his own position, Wittgenstein says nothing about this. But if we try to find out what he has shown in his own life, perhaps we can gain some notions, at least as to general strategy.

In the first place, for him as for another great philosopher who published nothing, Socrates, 'the unexamined life is not worth living', i.e. intelligent self-criticism is a necessary pre-requisite. Self-criticism we found, in our investigation of the concept of 'rationality' in Chapter 10, above, to be an integral component of rationality. Other components revealed by our analysis were an ability to withhold assent to any particular proposition being put forward for consideration until it had been properly examined; and the ability to perceive more than one alternative (of meaning and/or action — assuming always that in the situation in question there is more than one alternative). Withholding assent might seem to pose problems for the religious life, in that 'faith' appears to involve giving assent, and would usually be taken, moreover, as implying a giving of assent beyond the evidence available. But I feel that there need be no insuperable difficulties here. In the broad field covered by 'religion' we have an enormous diversity of experiences, practices and beliefs. It is not possible to give assent to all of them, for many would be mutually contradictory, and others would not accord with experience; so withholding of assent is necessary, at least until the variety of possibilities has been sorted through. (In this connection the discussion of how one finds one's 'personal position', see Chapter 5 above, would be relevant.) Even when the possibilities have been sorted through, however, it seems likely that, in religion as in philosophy, the adoption of a particular position is not a matter of logical or any

other kind of necessitation, therefore the adoption of a position, especially when it involves personal commitment and action which may sometimes be hazardous, must involve a decision or judgement — and this might well be called an act of faith. It seems likely, too, that the decision to commit oneself to any reasonably long-term policy or programme, in other dimensions besides the religious, must often involve going beyond the evidence immediately available. This is an aspect which has been stressed by various Existentialist writers, and it seems to me to be generally true (with the proviso that often we are not conscious, at the time we make a decision or commitment, of the fact that our evidence is limited or even seriously inadequate).

If we take it that Wittgenstein was showing us, in his own life, an example however imperfect of the religious life, one of its components was certainly rationality (as we have come to characterize this concept in the foregoing Chapters). Some other concepts of 'rationality', e.g. Peters' view that it entails being willing to talk about and 'give reasons for' whatever one does or says, would it seems be repudiated by Wittgenstein: as we have seen, 'the unsayable', includes a great deal, especially of the basically important things of life, and these we cannot 'give reasons' for.

In the second place, Wittgenstein in his own life showed us that self-criticism is not just a matter of 'picking faults' in oneself, of self-recrimination which can lead into a habit of indecisiveness. He was never indecisive (though no doubt he would put off a decision, when possible, until alternatives could be explored). His many changes of life — engineering, aeronautics, mathematics, formal logic, philosophy, the army during World War I, schoolteaching, house design, gardening, philosophy again, working in hospitals, philosophy ... — all these changes might perhaps be taken as indicators of a 'butterfly mind', but I think are better seen as the actions of a man who was prepared drastically to tailor his life to his own interpretation of external circumstances (e.g. his hospital work during WW2), and to make 'tactical' changes spontaneously, as his own overall strategy became clearer to him. This last point could be put another way, by saying that he was 'prepared to learn from his own mistakes'. But to call them 'mistakes' is seriously to misrepresent the nature of our lives (not only Wittgenstein's). Few of us can know in advance what the optimum 'match up' between our own strengths and weaknesses, and the opportunities 'available' to us in the world, will be; and the only intelligent strategy must be, to try something, but be prepared to change in the light of how it all turns out. This it

seems is what Wittgenstein did. His life exemplifies the evolution of 'tactics' within the context of a developing but essentially self-consistent strategy. I am convinced by Toulmin's (1969) general thesis on this point. In his philosophical work, for example, it seems to me that we must say that Wittgenstein both did and did not 'lead two revolutions'. There can be no doubt that, as they were taken up by others, the Tractatus and the Philosophical Investigations gave rise to two quite different and to a considerable extent opposed 'schools' of philosophy. But there is also no doubt, in my mind anyway, that Toulmin is correct when he says that

"... Wittgenstein could see his two books as successive attacks, using different methods, on one and the same group of problems." (p. 62).

Thus Wittgenstein's life and work exemplifies a basic consistency, within which he was quite prepared to 'change tack', when necessary, in the most radical manner.

Thirdly in relation to Wittgenstein's exemplification of religious life, it seems we must agree with his own estimate that he did not, in fact, succeed in adequately exemplifying 'the Higher'. Towards the end of the first part of his discussion of Wittgenstein's life and work, Toulmin remarks

"... whether or no Wittgenstein could have offered any further justification in principle for dissociating the realm of facts from that of values, he did not succeed in his own life in creating any effective correspondence between them. In his letters to Engelmann, he several times reports thoughts of suicide. He writes repeatedly in tones of self-disgust about his own "lack of decency" ... and he hints at emotional pressures which it was equally difficult for him to suppress or to sublimate ... even in 1937, he is writing from Trinity College, Cambridge, "God knows what will become of me."." (pp 64-5).

So perhaps the final verdict on his life, as an attempt to 'show' what could not in his own view be 'said, namely the reality of religious experience, must be that it fell short of the ideal. Nevertheless, it was a very noble failure, to the extent that it was a failure; and if we take 'showing' in a 'task' rather than an 'achievement' sense, if we take Wittgenstein to have been 'pointing the way' rather than 'exemplifying an ideal', perhaps it was not really a failure at all. It is certain that a great many people take notice of him, and that their numbers are increasing and that they will undoubtedly extend the range of application of his paradigmatic demonstration, in life and works, to many fields beyond those in which Wittgenstein himself was active.

CHAPTER THIRTEEN

.THE GENESIS OF INTELLIGENCE OF 'RATIONALITY' IN EDUCATION.

Working back now towards our starting points, to take up some of the implications which have not yet been made explicit:

The tentative suggestion was made in Chapter 4, that some guidance towards a useful approach could be provided by taking the faults revealed in the work of Peters and his followers, and simply negating them. It may now be suggested that the Wittgensteinian approach which has been developed in the foregoing Chapters has been doing this, implicitly, and that it is now time to say explicitly what has so far only been shown.

A fundamental concept for Peters, as we have seen, is that of 'rationality', 'reason', in the sense of 'being prepared to give reasons, talk about what has been said or done ...'. While we have not found ourselves in total contradiction to this, we have, following Wittgenstein, found this sort of formulation both inadequate and misleading. Being prepared to talk, discuss, etc., is good as far as it goes; but:

(a) There are important limitations on what talk will do for us: some things do have to be shown rather than said; and showing is an essential part even of learning what to say and how to say it. (This point would presumably not be denied by most educationalists, but perhaps it needs more emphasis, e.g. in relation to curriculum theory, teacher training, etc., than it often now receives.)

(b) The reasons given, for Peters, and even what are to count as reasons, are largely determined by the prevailing, assumptions, values, etc., of the social group. To have given reasons, in the 'achievement' sense, can it seems, for Peters mean only that one has given reasons which are accepted by that particular social group. In contrast to this conformist stance, the concept of rationality to which we have argued would demand a free-ranging exploration and exposure of all reasons, all considerations (or at least all that could be thought of, all that could feasibly be canvassed in the circumstances).

(c) It is rational to recognize the need for limits to talk, not only in the sense of (a) above, but also in terms of pragmatics and individual psychology: talk can be carried to the length of being a maladaptive substitute for action, a 'displacement activity' which can interfere with getting on with the job ...

(d) Following on from this, and from (b) above, it may be suggested that Peters and his followers fall short of adequate criteria of rationality in that they accept too readily the various assumptions and values of their own social group: they lack self-criticism (taking 'self' in both individual and societal senses), and by implication self-awareness. This tendency towards uncritical acceptance (or, acceptance which is not critical enough) is evident with regard to both their general societal context (Peters sees his task, for example, as that of 'justifying education' — with 'as practised' as a suppressed premiss), and also with regard to their own methodology of philosophy. The latter can perhaps be discussed as a separate point on its own!

(e) Peters and the 'London' school of Educational Philosophy seem to have taken up some of the philosophical techniques of Oxford-style 'linguistic analysis' without, in general, achieving the strengths of the latter approach but (unfortunately) with its limitations exaggerated. Perhaps because they have felt a need to 'keep close' (in some ways) to issues which can be recognized as educational, the London people seem to have been unwilling or unable to embark on the 'flights of abstraction' which have enabled the better Oxford people to achieve sometimes penetrating insights into the nature of linguistic usages and of particular philosophical problems. Oxford philosophers have shown what can be done through "unrestricted and wide-ranging enquiry" (Kirk 1974, as quoted in Chapter 10 above) and by having the courage (as well as the intellectual ability) to get away from (abstract from) the 'gravitational pull' of familiar usages and the accepted views. The philological and classics-orientated background mentioned by Passmore (1968) as being characteristic of Oxford philosophers may have been a help to them in this, in that familiarity with the languages and literatures of the Hellenic-Roman civilization may have been conducive to their achieving 'distancing' from those of their (and our) Western civilization. It is no accident, I suggest, that Ryle in the opening stages of the argument of The Concept of Mind (1949) should have used the illustration of a foreigner being shown around Oxford or Cambridge, seeing the Colleges, libraries, laboratories, Museum, etc., and then making the category-mistake of asking: "But where is the University?". I suggest that Ryle (stimulated and assisted, probably by Wittgenstein) was able to achieve for himself something of the standpoint of a 'foreigner' with regard to the generally-accepted Cartesian dualism (reinforced by science) of our time, and that only thus was he able to see as category-mistakes a great many of our ordinary (and

professional and 'learned') linguistic usages with regard to what had been termed the 'bodymind' problem; and once he had seen them, he was able to expose them in his book. In contrast to this 'distancing', this critical and transformative exploration of new possibilities, much that has been useful in the work of the London school of Educational Philosophy has been derivative, drawn from the work of (mainly) Oxford 'pure' philosophy — in many cases its derivation and new applications have not been very percipiently carried out — and the London people themselves have done little that has been genuinely and substantially critical or creative, whether in terms of theory or of practicalities. Luckily, there are signs that some of the younger workers are prepared to break away from the hegemony of the 'old guard' of the London school; and independent and original work has already been done, mainly by non-'London' philosophers, notably, in North America and Australasia. There is no lack of important and difficult problems in the total range of education, where conceptual-level sorting out is urgently required.

It is possible to show why social and/or intellectual conformism is bad both as philosophy and as education if we refer back to some of the features which we have elucidated in the concept of 'rationality', and relate these to the notion of the Common Assumptions Paradigm and its associated language-games.

The conformism intrinsic to activities within a 'normal' period (whether of a science or other academic discipline, or of a period of societal stability) holds no dangers but on the contrary, positive advantages, while conditions remain stable and the general 'environment' is unchanging. Once anomalies start to obtrude, however, and change sets in, the conformist education appropriate to 'normal' conditions becomes, as Kuhn remarked in a quotation given earlier, virtually useless and indeed may be definitely maladaptive. In the language of 'systems theory', (see Ashby 1964), a small system set within the context of a big and changing system must, if it is to survive in the long term, adapt to changes in the big system, and it must do so at a rate as fast, or faster, than the rate of change of the big system. Preferably, it must be able to predict changes in the big system, and initiate its own changes in advance, so as to be pre-adapted. Putting this issue in general terms, the range of possible changes must be scanned in advance, and adaptive strategies worked out for coping with the major contingencies revealed. Here again we meet this need for 'free-ranging exploration of all feasible possibilities' as a component in rationality.

Still working in completely general terms, we can represent the current assumptions, values, etc., of any field (an academic discipline, a particular society) as being collectively designated as CAP_1 — and the general problem of education is, to prepare people to change, as changing circumstances may demand, to any of a new and different set of possible paradigms CAP_2 , CAP_3 , CAP_4 , ... By definition, the new paradigms are not yet extant; their exploration, then, can be conducted only in terms of hypothetical possibilities; and while some of the possibilities to be canvassed may involve considerations of practicality, to the extent that the exploration of possibilities CAP_2 , CAP_3 , etc., is necessarily also both abstract and dependent upon the ability to 'hold off', 'provisionally withhold assent from' the current assumptions, values, etc., denoted as CAP_1 , this exploration equates with the activities typical of philosophy. Education in philosophy is to this extent and in this way, then, a prerequisite to all education which is in any substantial and serious way to be regarded as future-orientated.

The position we arrive at, in the light of the foregoing, is essentially similar to that to which we have been led by other lines of argument, in earlier Chapters. Education which restricts itself entirely within the current CAP_1 , irrespective of the field to which the latter refers, is thereby necessarily inadequate as a preparation for the future. This abstract and general conclusion applies in fact, I suggest, to much present-day education in science, and also to the London approach in Educational Philosophy. In both of these cases (and similarly, mutatis mutandis, in other academic disciplines) the remedy is, to sit looser to existing assumptions, practices, values, etc., and radically to extend the methodological basis of the educational approach: in other words, adopt as a standing precept of the discipline and of education in it, that possibilities should continually be looked for which are beyond those that are 'standard' for the time; thus self-criticism within the discipline will be both practised and also in a sense transcended, in that 'criticism' will be more than a matter of 'picking faults', it will be fused with the creative search for new avenues of advance, new infiltrations from the language-games of CAP_1 (whatever these may be) into language-games which may turn out to be those of a CAP_2 generation. Whether the lift-off from CAP_1 to CAP_2 ... can be achieved, and what is the nature of the new CAP_2 , CAP_3 ... and other families of language-games, can be known, of course, only retrospectively — but we can at least try to generate the habit of looking, if only as 'sleepwalkers', for alternative or wholly new language-games. If educating is really for the future, and if Mackie (1977) is right about 'inventing right and wrong', then it appears that this is what we ought, as educationalists, to attempt to do.

EPILOGUE

Is it possible to give an outline of the main implications for education of Wittgenstein's language-game in philosophy? We must keep in mind various provisos about 'educational implications' and their limitations which have been argued e.g. by Burns 1962, Guttchen 1966. Perhaps the main point is that 'implication' cannot be limited to the strict 'logical necessitation' of formal logic, nor to the 'logically necessary statements' of linguistic philosophy — rather, 'educational implications' must be understood as standing in a much looser relationship to particular positions or methodologies of philosophy, a relationship which could be said to parallel that between, say, a particular scientific theory and the Common Assumptions Paradigm on which it rests. Bearing in mind the looseness of the relationship, it may nevertheless be useful to attempt a brief summary of what appears to be some of the main implications of a Wittgensteinian approach, as they have emerged in the foregoing Chapters.

1. Emphasis on the 'language-game' concept — which would need to be explained and discussed explicitly and probably at some length for those not previously acquainted with it, e.g. students at teachers college or university, practising teachers, and even parents — can bring out two important and complementary features of both language and social living, viz.:

- (a) The necessity of having rules; and
- (b) The fact that the rules can, within various objective constraints, and subject (desirably) to the agreement of the persons involved, be altered. (They can also be altered by some groups within a Society, to deceive others, Steiner 1975: 34)

2. The 'game' analogy can help to make the features mentioned in (1) above, intelligible and acceptable to even fairly young children, those capable of seeing rules as 'constitutive' rather than 'constituted' in Piaget's (1932) sense, i.e. from about middle childhood.

3. The possibility of alteration and change in the 'rules' of language-games can help in the accommodation of progress, both 'normal' and 'revolutionary' in Kuhn's (1962) sense, in science and other disciplines. In particular, it can help towards an escape from the simplicism of 'naive realism' and the paradoxes it generates, e.g. that some of what we teach is mere falsehood.

4. The extended concept of the 'language-game' as here developed from Wittgenstein's own usage, notably in incorporating not only the words used but also, and in some respects more importantly, the 'forms of life', i.e. the totality of people and objects, activities and ideas and values, etc., within which and for which language is used, can enable young people (and older ones too, for that matter) to see that rules are not necessarily to be regarded as mere arbitrary impositions of the older generations or of the 'Establishment'. The rules of language and of society can be seen as, to at least a considerable extent, the natural outgrowths of what might be collectively (and rather loosely) designated as 'the laws of nature'. It must be kept in mind that, when we talk of such 'laws', we do not necessarily know exactly what they are as yet, nor can we formulate more than a few of them with any precision.
5. Some of the 'laws' or constraints which are involved in direct person-to-person l-gs are those now being investigated by the science of Human Ethology, notably those of non-verbal communication; and we can expect to attain greater understanding of these within the next few decades.
6. Within Science Education, it seems that a massive effort must be made to bring to the notice and understanding of all concerned, the 'total intellectual environment' or context within which science education is carried on. This could be said to involve a confluence between what are at present the separate disciplines of Philosophy of Science and Philosophy of Education. It is suggested that the 'flowing together' of these and other disciplines will be facilitated if all are seen as being comprised of language-games which are collectively 'oceanic' in nature, all arising from and in some senses dependent upon the l-gs of ordinary life.
7. This last point (of (6) above) is not to imply that an 'appeal to normal/ordinary/standard usage' is to be regarded as decisive or definitive: ordinary usage can furnish only starting-points and tentative guidelines.
8. Relating again to (6) above, it has been argued that various unstated and often unconscious assumptions always underlie and affect the nature of all educational, intellectual and social enterprises, that great influence is likely to be exerted especially by assumptions of which we are unconscious (cf Toulmin's 1961 'deals of natural order'), that we are not conscious of such assumptions precisely because they derive from the past — and because of their derivation from the past they are likely to have sub-optimal or even positively maladaptive effects in the present and especially the future.

9. It is therefore desirable, for individual and societal adaptiveness, that the underlying assumptions of our various enterprises should be brought up for conscious scrutiny; this can be done by means of philosophical investigations based on what is stated and ~~what~~ is actually done (Passmore 1967).

10. While training in any good-quality 'traditional' types of philosophy will help people towards competence in the kinds of investigations required, the twentieth-century emphasis on philosophy as activity, the 'linguistic' movement's focus on the precise usages of language, and Wittgenstein's concept of the 'language-game' especially used in its extended sense as in the foregoing, can be particularly helpful with regard to education in philosophy ('knowing how') and in relating philosophy to other intellectual disciplines, e.g. the various sciences, and to ordinary life.

11. Utilizing the 'extended l-g' concept, various faults which are postulated as occurring in current science education (and which appear to be exemplified to varying but generally significant extents in actual practice), viz. 'technicism', 'factualism', 'numeralism' and, in general, reductionism, can be shown to be varieties of the fallacy of composition: taking what is true of the part to be true of the whole. Techniques, facts and quantified methods all have their place, and an extremely important place, in the total activities of science — but they are parts only of the total, and harmful distortions occur when their place is magnified until they are made to seem to occupy the whole stage. If we see science in terms of 'extended language-games', however, and especially if we emphasize the developmental perspective (the development of science l-gs out of ordinary life l-gs), it becomes increasingly difficult to be unaware of the fact that particular quantified ('numeralized') methods, for example, have had to be decided upon, that this decision must have been based on certain criteria and intentions, etc., and that these were not necessarily the best and certainly were not the only possible ones. Similar considerations apply to the use of particular techniques, and the selection of particular facts.

12. To state in general terms what is involved in (11) above, the 'game' notion emphasizes the choice factor in our scientific and other activities — particular techniques, quantifications, etc. are not simply there, 'Objectively presented' or 'God given', as it were, (as seems often to be assumed) — and the 'language' side of the 'language game' notion gives emphasis to the fact that, in deciding upon which techniques to use, which facts to select, etc., we have to discuss (if only in our own minds) what sort of decision should be made.

13. The last point of (12) above implies that the technique that is used, the facts that have been selected, the quantifications that are employed, are but particular possibilities selected from a range of others, many of the others presumably 'worse' (since they were not in fact selected) but some of them perhaps being 'better'. Thus prospective as well as retrospective continuity is implied, i.e. the possibility of progress.
14. Thus complacency and 'temporocentrism' (the arrogance of assuming that final answers have been achieved in one's own time) can be combatted.
15. The game analogy with its implicit recognition that the rules of any one game can in principle be subject to considerable alteration, also that there is usually a wide choice available of different games that can be played even in fairly circumscribed given circumstances, accords with some interpretations of 'rationality' which have been elucidated, notably that it involves free and wide-ranging exploration of possibilities.
16. This view of 'rationality' has been found to be at variance with the view espoused by R.S. Peters and others, that it consists in 'giving reasons ...'.
17. It was argued that rationality as 'exploration of possibilities' must presuppose the ability to withhold 'closure' on any one possibility (especially the first one encountered); and that this ability appears to be identical with one of the factors of intelligence argued on the basis of evolutionary considerations (Stenhouse 1974).
18. A similar ability to 'withhold assent' or at least 'reduce self-commitment' appears to be a pre-requisite for another component of 'rationality' as seen by some investigators, viz. that of self-criticism.
19. The problematic nature of Kant's formulation of the 'transcendental' tendency for 'reason to go beyond its limits' and thus generate anomalies/antinomies, can be resolved so as to allow for absolute increases in understanding, if the 'language-game' concept is combined with Kuhn's (1962) concepts of 'normal' and 'revolutionary' activities and the underlying concept of 'paradigm' (the latter understood in two major senses, as Common Assumptions Paradigm, CAP, and Public Demonstration Paradigm, PDP). What cannot be stated (and thus cannot be subject to reason) in the language-games of one CA Paradigm (CAP_1), may in principle become statable in the l-gs of a new paradigm CAP_2 . The statement and resolution of what were anomalies within the CAP_1 l-gs is seen as constituting the Public Demonstration (PDP) of CAP_2 . Thus increases in understanding, while relative to the underlying

CAPs, can in another sense yield absolute increases in what is 'subject to reason'.

20. If 'rationality' is taken as including as components:

- (i) The tendency to explore as many possibilities as is feasible;
- (ii) A tendency towards (reflexive) self-criticism;

and, as a pre-requisite for both the above:

- (iii) An ability and tendency to withhold assent/commitment to any particular possibility (whether of action or proposition, etc.).

then education to enhance/increase individual rationality, it is argued, should include:

- (a) Substantial multi-disciplinarity;
- (b) Philosophy as an activity of exploration of possibilities (i.e. 'clarification' as accepted in current philosophy); and
- (c) The repeated forming and breaking of conceptual structurings, to enhance the three components of rationality mentioned above, especially component (iii). Multi-disciplinary education would be conducive to this, as would an historical approach to philosophy (and other disciplines too, to a lesser extent) especially if resolved into terms of language-games relative to Common Assumptions Paradigms.

21. In relation to the concept of philosophy developed in the foregoing, the contributions of R.S. Peters and others of the 'London' school of Educational Philosophy, though representing an advance on much that had gone before, can be seen as substantially deficient; and it is argued that Educational Philosophy needs to be 'philosophical' and 'rational', as contextually defined above, to be educational.

APPENDIX

CRITERIA FOR DISTINGUISHING BETWEEN LANGUAGE-GAMES.

One issue which may have become salient in the reader's mind, from the discussions in previous Chapters about various different language-games, is this:

How do we distinguish between them? Can we do so? And on the basis of what criteria?

Some relatively simple part-answers can be offered quite readily; and on the basis of these we can work our way into a deeper understanding of the issues involved, and in doing this, achieve whatever answers are possible.

In the first place, perhaps we should stipulate that, given what we might call a 'simple sample' of discourse, i.e. of some sort of language-game, it may not in fact be practicable to identify precisely (or even broadly) what sort of a language-game it is, or even what 'family' of l-gs it comes from.

Let us explore some examples. Take the phrase: "... those bees which are out ..." — what sort of a language-game does or could this come from? Clearly, more of the context must be given before we can have any hope of deciding. So, what if the quotation continues: "... I can kill with a clout ..."? A couple of obvious possibilities are, that the quotation comes from a treatise on beekeeping, or one on the control of insect pests. But then the rhyme between 'out' and 'clout' might suggest that instead of arranging the two part-quotations in a single line, they might be arranged thus:

"Those bees which are out
I can kill with a clout",

and this might give the idea that they might form the penultimate couplet of a limerick. This hypothesis would be confirmed if one found that the given quotation fitted into the following:

Asked a fearful new bee-farmer, Finagin:
"I've capsized the hive — how beginagin?
Those bees which are out
I can kill with a clout;
But much better to chase them all inagin."

(Perhaps I might remark, in passing, that limericks are very much a matter of language-games.)

In fact all of the above hypotheses are mistaken. The immediate context of the phrase "... those bees which are out ..." is as follows:

"This perceptual synthesis can be accomplished by many of the higher Hymenoptera but particularly the hunting wasps and the colony-forming bees. Every experienced beekeeper knows that if you move the hive from one site to another, those bees which are out foraging at the time that the hive is moved of course return to the old place and are very unlikely to be able to find the hive in its new location unless it is very near. If however you move the hive at night or before opening it in the morning while all the bees are inside, one notices that the foragers, instead of leaping out of the entrance and flying straight off as they normally do, will now hesitate and perhaps circle round the hive for a few seconds or minutes, clearly learning the new landmarks, by what one may call a "survey flight" before they go off."

The full quotation is from the book Animal Nature and Human Nature (Methuen, London, 1974), by W.H. Thorpe, Sc.D., FRS, Emeritus Professor of Animal Ethology in the University of Cambridge, England; and in the light of this information, we would have to say that our original quotation came from a 'learned treatise on comparative behaviour' language-game. (There are, it seems obvious many alternatives for what one can call any particular language-game; but the question of what name to call it by is a different question from the question of how and on what criteria we can distinguish it from other language-games.)

The intention of the foregoing was to show that, until enough of a language-game has been sampled/examined, any decision as to what sort of a language-game it is must be precarious. But how much is 'enough'?

It is tempting to think that, as one approaches what one might call the 'limits', one definitely could not, and at the other extreme definitely could, assign a quotation or 'sample' to a specifiable language-game. Single words, we might be tempted to say, are not assignable; whereas given a whole book, e.g. the one by Thorpe (1974) cited above, there should be no problem in saying what sort of language-game it is. But the matter is not always so straightforward. Most single words, it is true, would appear to be non-assignable: 'the', 'and', 'door', 'runs', 'small', and so on, can indeed fit into an indefinitely large array of l-gs. But what about 'entropy', 'prow', 'kundalini'? Those who know them at all, know the type of language-game that each belongs to: 'entropy' to theoretical physics, 'prow' to shipbuilding, 'kundalini' to the language-games of certain forms of Yogic and Hindu religion. At the other end of the scale, given a textbook on, say, electrostatics, then we would appear to be safe in deciding that this could be classified as 'electrostatics l-gs'. And if we were content to leave our classification as

loose as this, we would no doubt be correct. But sometimes it is important not to be content with a loose classification: what if the book on electrostatics is in fact of 'revolutionary' nature in the Kuhnian sense? If this were the case, then in terms of the model we set up in earlier Chapters, the revolutionary book is part of the CAP_2 family of language-games, indeed it may well be the very book which initiates these l-gs; and it is of the greatest importance to recognize that the CAP_2 l-gs are significantly different from the CAP_1 language-games, whatever the latter may happen to be.

Since this present thesis is, in part, an attempt at a synthesis between the work of Thomas Kuhn and that of Wittgenstein, can we look to Kuhn's major concepts (of 'revolution', 'normality', and perhaps most basic, that of the 'paradigm') and his applications of them, for hints as to the distinguishing characteristics of different language-games? We have in fact been using what I have suggested are two of the major senses of 'paradigm' (out of the twentyone listed by Masterman 1970), namely the Common Assumptions Paradigm (CAP) and Public Demonstration Paradigm (PDP), in previous Chapters; and it seems possible that a 'paradigmatic test' might be formulated in terms of these concepts.

Recapitulating and summarizing what has been covered in extenso in earlier Chapters, to provide a concise basis for further argument, we can say that the 'paradigm shift' which constitutes the revolutionary change between CAP_1 and CAP_2 must involve one or more of the following changes in the language-games associated with the respective Common Assumptions Paradigms:

- Type A: Changes in the words used, i.e. the use of some 'old' words associated with CAP_1 is discontinued, and new words are brought in to provide for the articulation of CAP_2 .
- Type B: The same words (i.e. phonemes and/or graphemes, etc.) are used, but their meanings change between CAP_1 and CAP_2 . This sort of change was illustrated earlier with regard to the two propositions 'The Earth is round' and 'The Earth is flat'.

Further typological categories could fairly readily be drawn up, e.g. one based upon relative frequencies of uses of words instead of the simple dichotomy between 'used' and 'not used' which constitutes Type A above; or a typology based, say, upon the relative frequencies of words within a continuum ranging from 'the concrete and observable' to 'highly abstract and inferential' (logical positivism/empiricism could be regarded as having attempted

to do the latter) — but I suggest that it may be profitable to concentrate for the present upon the two simple and basic categories presented above. The essential points can best be appreciated, I think, in terms of Type A and B above; and elaborations can be made later, as may be found requisite.

As an initiating generalization, it might be remarked that the 'spectatorial' differentiation between language-games of CAP_1 and those of CAP_2 can be accomplished upon only the basis of Type A differences, the occurrence and/or the non-occurrence of particular words. This follows from the interpretation given to 'spectatorial' in Chapter 4, above, in which it was stipulated that the spectator does not understand (fully, to an appreciable extent, ...) the language-games with which he is dealing. The example cited earlier was the differentiation between the Linear A and Linear B scripts of Late Bronze Age Greece (see Kirk 1974): the former is still not understood at all, so far as I know, though the latter was deciphered by Ventris and Chadwick (see Chadwick 1958); and one of the important points to be kept in mind is that differentiation between l-gs can be made in principle, even when neither of them is properly understood.

One drawback of the 'spectatorial' investigation of language-games is that, since it is being done, by definition, 'from the outside', it must be allowed as a possibility that particular sets of utterances, particular sets of symbols, might come to be classified as a language-game without actually being so, i.e. without making sense or, effectively, saying anything.

It might be objected, against this view of the extreme limitation of 'spectatorial' investigation, that a great deal of research has been done in the last three decades, into the 'meanings' of the 'languages' of many species of animals, and that meanings have in fact been worked out quite successfully, and furthermore have been tested and proved by experiment to be correct. If this can be done, it might be said — and it has been done (indeed I have myself conducted several years' research on the 'languages' of the Family Grallinidae (Aves), and hope soon to have an opportunity to publish my findings) — then it should be much easier for a human 'spectator' of human language-games to work out their meanings, since humans have successfully 'deciphered' non-human languages.

This objection would have some force, perhaps, if the notion of infra-human 'languages' were more than an analogy. But as Marler (1963) among others, has conclusively shown, there is indeed a difference in kind between

human (true) language and the so-called 'languages' (which are better described as signal-systems) of the infra-human animals. I propose to accept that a signal system (such as, for example, the systems of vocal signals used by birds to communicate a 'considerable amount of information, see Marler 1963, Hinde (ed.) 1969 and refs therein) must be differentiated from a language hence also from any language-game (see Minnis (ed.) 1971 for a number of papers giving different viewpoints regarding language, also extensive references). One of the important bases of the distinction between them is that, while a signal system can incorporate symbols (and many animal signal-systems do; thus disposing of the view that the use of symbols in itself is an adequate differentium of true language), and while human language (speech) includes some 'representational' symbols i.e. those in which the symbol and what it symbolizes have something structurally in common (e.g. words which represent sounds, such as 'hiss'), the bulk of human language, as Russell and Russell (1971) point out, is made up of non-representational or 'arbitrary' symbols. These can be combined to provide an indefinitely large potential vocabulary; and the use of arbitrary symbols by humans is not much tied to particular emotional/motivational states. As Pumphrey (1951) originally pointed out, infrahuman animals can use arbitrary symbols, and do — but their use of them is tied to relatively immediate environmental stimuli and/or motivational states, and thus is limited to the temporal present. An animal can give an alarm signal indicating, say, the presence of a lion. We could say that such a signal would be the equivalent of the human alarm-shout "Lion!". But the word 'lion' as distinct from the alarm-shout can be uttered, read or written, and in whatever form can be intelligible to everyone who has learned what it means, quite without the concurrent presence of an actual lion; and as Pumphrey puts it, "Whereas an emotive lion is necessarily in the present, an intelligible lion could be discussed in the future or in the past; and so tradition and forward planning about lions becomes possible". The enormous adaptive advantage of this, and hence the enormous adaptive advantage of speech and language in human evolution, is sufficiently obvious.

Language is thus 'context free' or, perhaps more accurately, 'context loose', in a way in which the signal systems of infra-human animals are not; and by its extensive use of 'arbitrary' symbols it provides an indefinitely large potential vocabulary, by the use of which enhanced communication, and as Goody (1977) has also argued, improved thinking, both become possible. This infinitely large vocabulary available in true language is to be contrasted with the vocabulary-size available in signal-systems, ranging from

seven to about forty 'items' in the survey by Russell and Russell (1971: 163).

Returning now to direct discussion of the question of the criteria for distinguishing between language-games:

If general criteria (i.e. that would be universally applicable to all l-gs) are to be given, then it would appear to be logically necessary that either:

I. These criteria are stated in terms which presuppose an understanding of some of the particular language-games to which the criteria apply; or

II. Stating (and understanding) the criteria does not depend upon prior understanding of any particular l-gs to which the criteria apply.

If one is thinking in terms of formal logic, it might appear that Alternative II should be preferred, on the grounds that the criteria in terms of which one l-g is to be differentiated from another must exist prior to the l-gs which they enable to be differentiated. Adherence to this view would be mistaken, however, as it rests upon a confusion between the criteria 'existing', but in implicit form only, within the language-games, and being able to discern and state — and understand the statement — what the criteria are. To do the latter, a new and distinct language-game has to be set up (or: we have to initiate a new language-game ...), which is, specifically, a language-game about language-games (and more specifically still, a language-game about the criteria used in language-games); and for that sort of l-g to make any sense at all, we must be familiar with, we must already 'have', some other l-gs for the differentiation of which the criteria in question are criteria. Otherwise, the new 'criteria of language-games' language-game would have no subject-matter, nothing that it could be about.

In other words, Alternative II, above, must be contradicted: stating the criteria does depend upon prior understanding of at least some (though not necessarily all, indeed considering what is involved one might well say 'necessarily not all' — bearing in mind that this last 'necessarily' may be somewhat different from the 'necessarily' of formal logic) of the language-games to which the criteria apply. And if a merely 'spectatorial' differentiation, as contextually defined in the foregoing, is to be avoided — as it must be avoided if we are to avoid also the possibility on the one hand of classifying as a language-game what is a mere congeries of words lacking in meaning and sense, and if we are also to avoid the opposite possibility, of failing to differentiate between language-games which have different meanings

even though they use the same sets of words ('words' here meaning phonemes or graphemes: in the sense in which 'bow', for example, is the same word irrespective of whether it is noun or verb or ...) — then we have to work in terms of our understanding of the language-games we are dealing with. We must be able to cope with changes or differences between language-games of both Type A and Type B, changes in the words used, and changes in the meanings of words. If we accept Wittgenstein's view that knowing the meaning of a word is to know the rules for its use, then we have to know both the words used in our language-games, and the rules for their use.

As we have seen earlier, 'knowing the rules for the use of the words', 'knowing how to play the several language-games', cannot mean 'being able to give a list of the rules', because in fact there are no such lists of rules, and we do not learn how to play language-games by learning up lists of rules (though sometimes we do learn lists of rules, as part of learning a particular l-g — but the full set of rules for that l-g cannot be included within the list, for we can learn the rule 'Learn this (given) list of rules' only from outside the list itself; this is true even though it may also be stated within the list 'Learn all the rules in this list', because we have to know, before we start reading the list, that we have to read and learn it, etc).

Reverting to the questions of general criteria for the differentiation between language-games, it seems clear that we cannot know all the criteria for all possible l-gs, for this would involve, on what we have argued so far, literally knowing and understanding not only all existing language-games, but also all possible l-gs. One is tempted to assert, categorically, that this is impossible — all that I shall actually suggest, however, is that it is simply not feasible, it is not a 'practical proposition'. It seems to me that to suggest otherwise is, in effect, to suggest the practicability of one's being (or someone's being) omniscient. It is perhaps this possibility that is being rejected by modern philosophers (e.g. O'Connor 1957, Wittgenstein himself) when they insist that 'Philosophers cannot tell other people what to do', e.g. philosophers cannot tell scientists how to do their jobs, how to play science language-games. If they are saying that no one person can know, understand and actually 'play' or practise all language-games, (and being able to play/practise a language-game is a necessary part of knowing and understanding it — this is what the 'revolution in philosophy' is all about, this is what 'philosophy as an activity not a body of knowledge' entails), then they are certainly correct. But we cannot leave the matter there, as if this were the whole story. On the one hand we need, I suggest,

to recognize what is to me the obvious desirability that some people — a much larger number than at present — should become proficient at particular combinations of language-games, notably l-gs of both philosophy and some of the sciences. It seems to me to be both feasible and desirable that this sort of multi-disciplinarity should be increased. Educational and institutional policies will need to be modified in various ways, if it is to be encouraged: the individual cannot be expected to pay all of the opportunity costs (and straightforward financial costs) that are involved. But there appears to be no difficulty in principle about people being able to achieve competence in various desirable 'mixes' of language-games, without in any way having to aim at omni-competence. What is to be aimed at, and can in principle be achieved, I suggest, is not that 'philosophers should tell other people what to do', but that many people, by themselves studying philosophy and becoming competent (so far forth) as philosophers, can also become more competent, as a direct consequence of their competence in philosophical language-games, in the various other fields of activity e.g. science, education and so on. Once they know and understand several families of language-games (again the proviso: they needn't know and understand them exhaustively), they will know the differentiating criteria from the inside. Normally, such criteria are seldom actually stated. While they do not all come into the category of 'the unsayable', some types of criteria do: notably those criteria and rules which are learned 'ostensively'. Other criteria, including many of those that would normally be learned ostensively or tacitly, can be stated in words if need be — but if stated, the statement can be made by and would be intelligible to only persons who already knew the language-game, who had learned the language-game de novo in terms of ostensive or tacit learning. The only alternative to this would be the vicious infinite regress (which in practice turns into a vicious circularity) of an endless series of verbal explanations/definitions, such as was indicated in Chapter 11.

It is of course possible to give general features/criteria of language-games, such as 'They all communicate information of some sort, or are capable of being used for such communication'. The second clause of this statement is needed to cover the possibility that some intelligible utterance (whether spoken or written) might not in fact be 'received' by anyone; thus there would be no actual communication, but only potential communication; yet we would feel disinclined, in considering such an hypothetical case, to say that the criteria for the proper use of l-gs had not been met by the utterer.

We would feel this, I suggest, unless we had adopted as a criterion for l-gs something like: 'No utterance is to be made except when it is known in advance that it is going to be received by someone'. It might be felt that such a criterion or rule would be helpful in reducing wasteful proliferation of utterances; or that misunderstandings could be reduced if utterers designed their utterances with a particular set of receivers in mind, tailoring the utterance so that it would be certain to be understood by the receiver(s). But it seems unlikely that we would accept such a criterion or rule as universal (though we often do tailor our utterances to their receivers), for the simple reason that a good many utterances must be made without the utterer knowing in advance what sort of receivers they might reach. Messages in bottles released by shipwrecked mariners, calls of "Help!", and so on, are but one end of a spectrum of possible messages/utterances. Our general strategy seems to be — and when we think about it, it seems to be the most effective general strategy we could adopt — to make an utterance in a form which, so far as we can judge in the circumstances, will be widely or generally intelligible, and then be prepared to give further explication of any aspect of it which may be queried. We can operate, it seems clear, only in terms of the language-games in which we have already become proficient. Though we are sometimes in a position to know, in advance of making a particular utterance, exactly who will be receiving it, hence can tailor the utterance for optimum intelligibility (so far as we can judge) by the receiver, most of the time we are not in a position to know in advance exactly who will be receiving a particular message/utterance, and/or we are not in a position to know what the receivers' capabilities will be. So we just have to go ahead as best we can.

Mention of the communicatory function of language-games leads us to a consideration of the relationship between what might be grouped as 'formalist approaches (formal logic, formal semantics) on the one hand, and on the other (following Strawson 1971) theories of 'communication-intention'. It will be apparent that my own inclination is towards the latter position. The issue of 'formalism' vs 'communication-intention' relates to the interpretation of Wittgenstein's work, in that (as Strawson indicates, p. 92) the 'earlier Wittgenstein' might be seen as being on the side of the formalists, while 'later Wittgenstein' is certainly on the side of the communication-intentionalists. According to the latter (as characterized by Strawson, p. 91):

"... it is impossible to give an adequate account of the concept of meaning without reference to the possession by speakers of audience-directed intentions of a certain complex kind. The

particular meanings of words and sentences are, no doubt, largely a matter of rule and convention; but the general nature of such rules and conventions can be ultimately understood only by reference to the concept of communication-intention."

By contrast, the 'formalist' position is characterized as follows:

"... the system of semantic and syntactic rules, in the mastery of which knowledge of a language consists — the rules which determine the meanings of sentences — is not a system of rules for communicating at all. The rules can be exploited for this purpose; but this is incidental to their essential character. It would be perfectly possible for someone to understand a language completely ... without having even the implicit thought of the function of communication ..." (p. 92)

If an 'inclusivist' approach in philosophy allows us to carry an evolutionary position, adopted from biology, into our consideration of the matters now in question, it would have to be remarked that, if we assume language to have been produced by evolutionary processes, a 'communication-alist' view of it seems almost to be forced upon us. Although the distinction between signal-systems and language is logically clear enough, and although human language seems in fact to be pretty widely separated from the signal-systems of the infra-human animals, the various sciences which touch upon human nature all seem agreed that language itself has been the product of sociobiological evolution (see Wilson 1973; 'Psycho-social evolution' was Huxley's (1964) term for the same thing, see also Stenhouse 1974, Chapter 7 and refs therein). It has many and powerful adaptive advantages (Thorpe 1974), most of which, especially in the earlier stages of its evolution, would have stemmed from its communicatory functions. It is difficult to see that its origin could have been in anything other than communication. This is not to say that its main function today must necessarily be one of communication (though it would appear to be simply false to deny that communication is extremely important) — but if its original and primary function were communication, it seems reasonable to assume that this function would largely have shaped the structure, especially the 'deep structures' (Chomsky 1957, 1968; see also Lyons 1970), of language in general and of any particular language in detail. We might keep in mind, then, that the 'formalist' position may be based upon false abstraction; and certainly on evolutionary grounds, the assumption must be that language was produced as a means of communication and that the onus of proof must be on anyone who argues otherwise.

It would be inappropriate to attempt to reproduce or summarize

Strawson's detailed arguments in the paper in question — close argument just does not summarize, so the reader is referred to the paper itself — but I shall give an outline of what I take to be some of the main points, and the conclusion.

To give the latter first, Strawson after an elaborate attempt to provide a justification of the 'formalist' position which he takes as hinging upon a 'truth-value' theory of meaning, finally says:

"I hope it is clear that any such story [as the one he has just given, in attempting to justify the 'formalist' position] is going to be too perverse and arbitrary to satisfy the requirements of an acceptable theory. If this is the way the game has to be played, then the communication-theorist must be allowed to have won it." (p. 109)

Strawson initiates his argument by outlining how one can elaborate a 'communication-intention' position:

"... first, present and elucidate a primitive concept of communication (or communication-intention) in terms which do not presuppose the concept of linguistic meaning; then show that the latter concept can be, and is to be, explained in terms of the former." (p. 92).

Against this, there is the 'formalist' position:

"The general idea ... is that the syntactic and semantic rules together determine the meanings of all the sentences of a language and do this, precisely, by determining their truth-conditions." (p. 97).

After dealing with several subsidiary issues, Strawson goes on:

"I hope it is now clear what the central issue is. It consists in nothing other than the simple-seeming question whether the notion of truth-conditions can itself be explained or understood without reference to the function of communication." (p. 99; emphasis added).

Without attempting to follow the fascinating intricacies of Strawson's argument, perhaps it could be said that for him it comes down to an issue of 'expressivism': the formalists, in endeavouring to give an account of how utterances can come to have 'truth-conditions' while not being communicatory, are forced to say something along these lines:

"When I say 'The table is brown', and if I am not communicating some 'fact' of external objective reality, about all that I can be doing is expressing something from within myself. Such an 'expression' has no necessary communicatory function (though it can acquire such a function; but this would be secondary). How then can questions of truth/falsity arise about my

'expression'? Only if my 'expression' (= utterance) is an expression of my belief that the table is brown. Thus the expression may be a true expression of my belief, or it may be false.

The problem for the formalists, on this account, lies in how they can explain the 'lawfulness' or 'rule-keeping' which utterances must exhibit if the very notions of 'truth' and 'falsity', hence a fortiori the notion of 'truth-conditions', are to have any meaning. Can they have a 'meaning' which, so to say, is entirely 'internal' to the person who utters the expression of belief? Would this provide for the needed regularities?

One objection to this view is given by Strawson (p. 107). If the regularity of association between belief and utterance is to be established, the utterer must have some way of recording what the association was on occasion T_1 so that, on occasion T_2 some time later, he can get the association right, i.e. he can express the same belief by making the same utterance. But, irrespective of how this 'association' is recorded (obviously the recording need not be physical), the utterer is still involved in communication: "the earlier man communicates with his later self". (p. 107). Thus the endeavour to ensure regularity, lawfulness, seems necessarily to involve at least this minimal communication-intent.

A further problem is given as follows:

"... suppose a man had a practice of vocalizing in a certain way whenever he saw the sun rise and in another, partly different, partly similar way whenever he saw it set. Then this practice would be regularly related to certain beliefs, i.e. that the sun was rising or that it was setting. But this description gives us no reason at all for saying that when the man indulged in this practice he was expressing the belief that the sun was rising or setting, in accordance with a rule for doing so. We really have not enough of a description to how what to say. As far as we could tell, we might say, he just seems to have this ritual of saluting the rising or the setting sun in this way. What need of his it satisfied we don't know." (pp 106-7).

This brings out a problem about Strawson's paper, regarding how he goes about doing philosophy, how he goes about the activity of philosophy. Surely, we might say, in bringing up the objection he does in the above-quoted passage, he is presupposing a communicatory function in language? Granted that the man who vocalized in different ways at sunrise and sunset did not, in terms of the example, have the intention of communicating anything to other people — but is not Strawson setting himself up as an 'audience' of the man's?

utterances, and thereby wishing upon him a communicatory function? Why not just leave the man doing nothing more than express ... something or other, we neither know nor care what. We would not be able, if mere expressivism were universal, to talk intelligibly about whatever the man was doing — or about anything else, presumably — but if so, why not just say this, i.e. what has just been said, to the effect (in general) that unless language has a communicatory function we would not be able to wonder why or even if it had such a function? Now it seems to me that something along these lines does need to be said, if the issue is to be intelligible to readers who are not professional philosophers. Strawson, while apparently aware that he is lecturing/writing for lay persons (his paper was originally given as one of a series of lectures given at the Institute of Contemporary Arts, London, in 1969-70), appears to make little concession to the nature of his audience, beyond sparing them the details of more extensive arguments to which they are referred (in the published works of various philosophers). One wonders whether this is the only concession that needs to be made. I suggest that more needs to be done in the way of providing 'navigational landmarks' (or we could call them 'direction indicators') to give the audience/readers an idea of what all the argument is about, what sort of conclusion the argument is heading for. The reader will note that I have done this, in my attempt to *précis* Strawson's paper, above.

But if one can deal with the issues simply by pointing out that we cannot have a philosophical discussion without communication, why do we need to bother with so much detailed argument as Strawson gives? Do we need to bother ...? My own position regarding this is quite clear in principle. Strawson is doing what I have suggested in earlier Chapters the good philosopher and the good educator must do: they must endeavour to go into all the possibilities, all the conceivable twists and turns of argument, in order that they can, not merely say that a certain conclusion seems to follow from the considerations given, but in a real sense show the reader/hearer that no other conclusion is as reasonable as the one offered. This is what Strawson is doing when he puts the story of the man vocalizing at sunrise and sunset: he is leaning over backwards to give the formalists as much as he possibly can (and I have suggested that this is more than they are strictly entitled to). Further, he puts his conclusion in a cautiously minimal formulation; e.g. on pp 105-6 he argues as follows:

"... we may be tempted ... by a kind of bogus arithmetic of concepts. Given the concept of Audience Directed Belief Expression (ADBE), we can indeed think of Belief Expression (BE) without Audience Direction (AD), and find cases of this. But it does not follow that the concept

of ADBE is a kind of logical compound of the two simpler concepts of AD and BE and hence that BE is conceptually independent of ADBE.

"Of course these remarks do not show that there is no such thing as an independent concept of belief expression which will meet the needs of the anti-communication-theorist. They are only remarks directed against a too simple argument to the effect that there is such a concept."

This is admirably clear and restrained. There is no obvious attempt by Strawson to 'put down' his readers (though the sheer ingenuity of his argument may actually have this effect!), no attempt to rule any potential opposition 'out of court' by made-to-measure definitions of what is 'rational' and what isn't. Thus he does not attempt to build much argument on the fact that having a philosophical discussion presupposes communication. Instead, he suggests that the fact that we use language for communication might be merely contingent and secondary, and that we should explore the possibility whether a formalist 'truth-conditions' basis of language might possibly be the primary and only logically necessary condition for the existence of language as we know it. This is put forward implicitly on an hypothetical basis: 'If we assume that the communication-intention position is not necessarily true, can we build a formalist position without generating anomalies?'. This is the essence of Strawson's methodology. I would also suggest that his methodology is not only 'internally' hypothetical, as it were, but also that the caution and limitation of his claims is a tacit recognition of the real possibility of 'revolutionary' change in the Kuhnian sense within philosophy itself, such that changes in the meanings of words now used could conceivably make invalid arguments which are now valid, and vice versa.

The crucial issue with regard to any language, for both philosophers and educators, is: Could it conceivably be learned, if in the form suggested? Could anyone learn it?

Strawson appears to take a similar view of the importance of this question of language learning, for he touches upon it in the latter part of the paper in question:

"We may reasonably suppose that the learners, to begin with, do not quite appreciate what they will ultimately be doing with language [it is being supposed that they are learning language in order to make true and/or false statements, in accordance with formalist views, not in order to communicate]; that it is for them, to begin with, a matter of learning to do the right thing rather than

learning to say the true thing; i.e. a matter of responding vocally to situations in a way which will earn them reward or avoid punishment rather than a matter of expressing their beliefs. But later they come to realize that they have mastered a system which enables them to perform this (still unexplained) activity whenever they wish; and then they are speaking a language. (p. 108; original emphasis).

Similarities between the above and the accounts given earlier (e.g. Chapter 6) will be noted — notably perhaps that 'learning to do the right thing' is more basic than 'telling the truth' and other functions — but there are significant dissimilarities too. Strawson like Peters, Hirst and others seems to see language-learning as needing to be 'imposed' in some strong sense, witness his talk of 'reward' and 'punishment'. Here as in so many other places we see the unfortunate effects of the 'learning theory' paradigm of the past half-century: it is implicitly assumed that nobody will learn anything unless they are made to. How different, how much more realistic (certainly with regard to very young children in their first language-learnings, before they have been to school and learned to dislike learning), if we can see learning, especially language-learning, as being a 'natural', inner- and automatically-motivated or endogenous activity, if we can see children as wanting to learn ...! If (I should prefer to say when) we make the gestalt-switch to a new paradigm in the behavioural and social sciences — my The Ethology-and-Intelligence Paradigm in the Social and Behavioural Sciences, in preparation, argues for such a gestalt-switch — we shall still see learning as being of great importance, but we shall understand its behavioural and causal context much better. We will not have to talk of 'reward' and 'punishment' in connection with language-learning, but instead will see 'being understood' and 'not being understood' as providing sufficient reinforcement one way or the other; and we will see the 'reward-and-punishment' way of talking as being, not simply false, but as misleading and inadequate. However, I must not divert into a long argument about behaviour theory — the limited point I was making was that I suggest that my formulation of how children learn language is in some respects better than Strawson's (I'm not claiming to be more perspicuous than he, far from it — but my own background of nine years university lecturing and research in zoology and ethology could be expected, perhaps, to yield a few insights); and this can lead us into an examination of the methodological position adopted by Strawson as compared to that adopted in this present work.

I have felt it desirable to make explicit mention of Wittgenstein's notion of the 'language-game' and have endeavoured to explain it and to

exemplify the language-game approach in my own arguments. (One does not need, presumably, to say explicitly that one is using this approach on every instance of one's using it). Strawson does not mention the phrase 'language-game' at all. Yet he is widely recognized as one of the leaders of Oxford linguistic philosophy (Passmore 1968, Mehta 1963, Ellison 1975). Does this mean that the 'language-game' notion is out of date, or that Strawson has rejected it for some reason or other?

It appears that there may be other reasons, besides rejection, why Strawson does not make explicit appeal to 'language-game' philosophy. He has criticized formal logicians for concentrating all their attention upon 'context-free sentences' (Passmore 1968: 461), so it seems that he does want to insist that context be taken adequately into account. But the question, perhaps, is which context? Ellison (1975: 149) reports as follows from an interview:

"As for the suggestion that philosophers should be concerned to make their ideas available to a wider audience, Strawson dismisses it as "absolute nonsense". Philosophy, he says, is not really as remote as people think. If they were really interested most educated persons would find it accessible. "But the thing is, you have to work at it. Philosophy has never been easy..."."

It appears from this that Strawson may be unwilling to assume anything other than a fully professional context when he is doing philosophy, hence may assume that his audience is completely familiar with the language-game approach and that he himself is under no obligation to mention it. If this is the case, I must register some disagreement. While one assumes that, in doing 'normal philosophy' in a professional context, it is unnecessary to make explicit the shared assumptions of the relevant CAP (in this case presumably a 'linguistic philosophy' CAP — or should it be more specific, say an 'Oxford linguistic philosophy' CAP?), if one is addressing one's arguments to people who may not be fully aware of the Common Assumptions Paradigm within which one has been working, it seems to be good educational practice, and in general just good sense, to tell them enough about one's assumptions to enable them to understand what is being done.

We may accept, I think, that "Philosophy has never been easy ...", without going along with Strawson in his assumption that the initiative in getting philosophy to a wider audience must be left entirely to the audience. It is true that a total lack of interest precludes any possibility of teaching and learning — but there must be many people with some interest in philosophy who would be keen to develop their interest if the activities of

philosophy were presented to them in an intelligible and stimulating way. This is where educational philosophers may have an advantage over 'pure' philosophers like Strawson. Educationalists, in these days of compulsory universal education, are used to encountering some 'consumer resistance'. Even persons like myself who have taught always in universities, where the students are not compelled to attend, are used to a 'gradualist' approach in our teaching — or perhaps I should say, we are used to dealing with students whose motivation for the study of philosophy is not great, initially at least, and whose understanding of it is slight or non-existent — so we take it as a matter of course that we have to both explain clearly and simply, and try to elicit motivation. For this, as I have said many times in the foregoing Chapters, an approach made explicitly in terms of 'language-games' seems to have a number of methodological advantages, and also to have the merit of actually working. Students do in my experience 'get motivated', not necessarily to embark on careers in philosophy or other academic disciplines, but at least to try to think more clearly. One of my own rewards has been to see the names of quite a few former students in the 'Letters to the Editor' columns of both professional and ordinary periodicals, subscribed to intelligent comments and/or suggestions. This may not mean much to Philosophy as an academic discipline, but I do feel that it is good for a society, and for education.

Strawson himself appears to recognize a need for effective communication. "For Strawson," says Ellison (1975: 149), "Style is an important factor in presenting philosophical arguments. Often clumsiness of expression will obscure the impact of views which are roughly correct. A philosopher must be able to present a truth with great vividness, and this calls for great literary and imaginative power." I suspect that some of the apparent problems of methodology in Strawson's paper are generated by the fact that it was originally given as a lecture. Many of his locutions are those of spoken rather than written English. Presumably also his audience, on the occasion of the lecture, was generally acquainted with the background of Oxford linguistic philosophy (i.e. the audience and Strawson shared the same CAP). Now it is obviously a good thing that a group of people should know and care enough about a lecture by a leading philosopher on the topic "Meaning, Truth and Communication" to go and listen to it. Undoubtedly most of them found it illuminating and enjoyable (as I did, on reading it years later). But it appears, e.g. from Strawson's reported "absolute nonsense" response to a query about philosophy being brought to wider audiences

that he may see only two possibilities with regard to the activities of philosophy: that they should be carried out by professionals for professionals; and that they should be carried out by professional^{als} for lay persons. I argue that we must take a more complicated view of the possibilities. We must see them in more than binary terms. To the two groups, 'professional philosophers' and 'lay persons' we must add an indefinitely large number of other groups, of people who will study philosophy (in combination with various constellations of other subjects) as part of their education for other professional activities besides philosophy.

One of the implications of this is that the size and diversity of the array of 'Philosophers' Language-Games' must be increased. In addition to the l-gs of Philosophers talking to Philosophers', 'Philosophers giving entertainment and enlightenment to lay audiences', and 'Philosophers teaching Philosophy Students', we shall need to recognize — and to actualize — language-games of 'Teaching Philosophy to Science Students', 'Teaching Philosophy to Education Students and/or Teacher Trainees', 'Teaching Philosophy to Social Sciences Students', and so on. No doubt there will be family resemblances between all these language-games; but there will be differences too. Students who actually enrol for courses specified as Philosophy can be assumed to have certain types and levels of motivation and background, and some approaches to teaching philosophy will therefore be feasible with them which might not be effective with, say, Social Sciences students. History of Philosophy might be effective with the former, for example, though less so with the latter. An approach exploring the similarities between, say, 'ideology' and the Common Assumptions Paradigm, might be effective for building social scientists; alternatively, books like Andreski's (1974) The Social Sciences as Sorcery or Polsky's (1971) Hustlers, Beats, and Others could furnish a useful lead-in. For students in Education, an approach through the deceptions and confusions of language used in and about education can be effective, e.g. exposure of the logical/linguistic pitfalls of 'selfdetermination as the aim of education' (cf Broudy 1961). An educational approach explicitly in terms of language-games can usefully be brought in at an early stage of teaching, with students whose major commitments are not with Philosophy as such, for reasons which have been mentioned many times in the foregoing Chapters and which will not be repeated here; while for those committed to long-term study of Philosophy itself, it is probably better that the sequence of the student's contact with the subject should (roughly) recapitulate its historical development, at least within the 'modern' period say from

Descartes to the present, thus providing an appropriate context of intellectual history. (In this connection, Toulmin's (1969) plea for greater attention to be given to the intellectual/cultural history of Vienna in the 1890s and early 1900s assumes considerable importance.)

Finally on the question of the relationship between formal logic and ordinary language, George Steiner (1975: 472) remarks:

"Whether attempts at a comprehensive anatomy of language by formal and logical means are more than an intellectual exercise, often illuminating on the level of the ideal, remains a moot question. ... For the most recent attempt to apply formal logic to vagueness, context dependence, and polysemy in natural language, see M.J. Cresswell, Logics and Languages (London, 1973). Nothing in this acute treatment seems to overcome Wittgenstein's admonition against the derivation of systematic logic from ordinary language or Tarski's theorem that 'there can be no general criterion of truth for sufficiently rich languages' — all natural languages being 'sufficiently rich'."

In one sense, of course, formal logic, mathematics, etc., are derived from ordinary language, and necessarily so, as Wittgenstein was at pains in the Philosophical Investigations and subsequently-published works to insist — for what other basis could there be, on which and by which they could be set up? — but what Steiner refers to is undoubtedly the notion that formal logic etc. can in some way simply be extracted from ordinary language, having been intrinsic to it in the first place, and forming, so to speak, its natural skeleton. The true state of affairs seems rather to be (as Steiner argues, citing recent Chomsky papers, Dell Hymes 1973 and others in support), that while any particular system of formal logic can in a sense be extracted as a skeleton from ordinary language, no such system can in any sense be regarded as the skeleton. In other words, formal systems are dependent upon informal 'ordinary language', not vice versa, even though anomalies and confusions in ordinary language often are revealed, diagnosed and corrected by the use of formal systems abstracted from ordinary language. The key question to ask, with regard to any proffered language-game, is: How could it — or, more basically, could it — be learned?

It is no accident, I suggest, that Wittgenstein commenced the Philosophical Investigations with consideration of how a primitive language-game could be learned and taught: here, though he did not say it, he showed us philosophy as education, as coming to understand ...

Perhaps the final point to be made in the present work is this: that the usefulness of the extended language-game approach must be apparent to

every educator, every teacher: for the need to match manner and type of approach to the particular audience on the particular occasion — i.e. to keep the total context continuously in mind — is one of the 'constants' of educating, which is presupposed by Bruner's famous dictum about being able to teach anything to anybody provided that the appropriate 'matching' is achieved.

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