

INTRODUCTION.

Introduction.What's it All About, and Why Are We Here?
A Personal Preface.

When I began the research which has eventuated in this dissertation I had two main issues in mind, which I hoped would turn out to be interrelated. One was a concern about mutual misunderstanding within our culture between people in the sciences and those concerned with the arts. For reasons of my own personal history I had come to feel the divide between the so called "two cultures" (1) rather acutely. With some loyalties on either side, I should have liked to have been able to show that at root their methods and aims have more in common than is usually believed; that there are things on either side which the other by its own lights, ought to cherish beyond, respectively, utility and entertainment value. However, I could see no direct way to tackle this issue beyond adding to the rhetoric which it usually brings forth.

The other issue which concerned me was the problem of cultural and historical relativism as it bears on the natural sciences. This, of course, has been a major point of dispute amongst philosophers at least since Kuhn's *The Structure of Scientific Revolutions* was published (2*). After reading that book myself I, like many others, was puzzled as to how to reconcile the persuasive case for relativism which it seems to make with the powerful intuition that contemporary natural science is, for the

most part, telling us the truth about the ways of nature in a sense that, say, Ptolemaic astronomy or Zande witchcraft lore do not give us the truth. This intuition seems to be powerfully confirmed by the fact that we in the contemporary West are surrounded by science-based technologies, from TV sets to penicillin, which manifestly work.

However, there was much in Kuhn's account of science which rang true to me, or certainly more true than other more formal scientific 'methodologies'. In particular I liked his notion that fields of scientific inquiry are centred around "paradigms" or "exemplars", "concrete problem solutions" (3), rather than around 'laws' or 'theories' as these terms are generally understood by philosophers of science. My original scientific education had been in biochemistry, but I had heard precious little about such 'laws' or 'theories' there, although modern biochemistry is about as 'hard' and as 'normal' a science as one could reasonably wish (4). Biochemical knowledge is primarily about metabolic pathways, the chains of reactions by which the chemical substances within organisms are transformed one to the other, and the way in which the enzymes, the proteins which catalyze most of these reactions, do their job. An expert biochemist is not someone who knows a lot of 'laws' and 'theories', but someone who has a relatively detailed knowledge of the pattern of the chemical events which go on inside cells (and a mastery of certain laboratory techniques).

Biochemists, and other scientists, do, of course, frequently talk about "theories", but what they usually seem to mean by this are either very narrow hypotheses, of relevance to one or a few very specific experiments, or else something very loose and general - something pretty much like what Kuhn means by "paradigm" {5}. I shall not distain to use "theory" in this latter, 'ordinary' sense.

Quite apart from this apparent conflict of intuitions about the nature and status of scientific knowledge, I also feel a conflict of moral intuitions about relativism. A major virtue of the doctrine would seem to be that it allows us to pay proper respect to people who are remote either geographically or historically from ourselves (or who used to be remote geographically). When, as often seems to be the case, such people sincerely hold or held beliefs radically at variance with our own, it seems to me to be entirely proper to wish to check the impulse to regard them as ignorant and benighted. Relativism provides us with an intellectual justification for checking this impulse. When the people concerned are (or were once) distant from us geographically, relativism provides a bulwark against racism. Awareness of historical changes in beliefs helps to dispel arrogance and complacency. We know that belief systems have changed radically in the past, and it is salutary to realize that they may do so in the future. However, if we regard this succession of changes as a progress it seems to follow that not only were our ancestors benighted, we are almost certainly benighted too!

Relativism provides an escape from these uncomfortable conclusions.

However, I am sure I am not alone in feeling that strong relativistic premises too readily lend themselves to abuse. It is, regrettably, only too easy to deploy relativistic arguments to undermine the claims to universal validity of any belief system or way of life of which one happens to disapprove, whilst failing to apply the arguments to one's own beliefs and standards, however eccentric these may be. I fear this frequently happens. In fact I suspect it may be unavoidable for the radical relativist. In practice, everyone, whatever their epistemology, must prefer to live in a certain way and to have the people around them behave in a certain sort of way. If the relativist criticizes claims to absolutism of belief and value systems which he does not like, how can he avoid implicitly recommending what epitomizes his own ideal way of life - whether it be the zany "anarchism" of a Feyerabend (6) or the post-prandial "conversation" of a Rorty (7). Deployed critically, relativistic arguments are too powerful; they can debunk anything. It is, I suspect, this excessive power which so often leaves one (well, me at least) with the feeling of being intellectually bullied when reading the work of radical relativists. On the other hand, if the relativist conscientiously manages to abjure criticism of anyone's belief system, how is he going to be able to stand up against, say, Nazism (8*)? Do we really want to embrace a doctrine which faces us with the choice

either of being intellectually dishonest, or else supine in the face of evil?

Relativism seems to lead both to virtue and to vice. It is also, on the more personal level, both a promise and a threat. The promise is that of freedom: whether freedom from conventional restraints or freedom from having to worry about difficult problems; freedom to try and build utopia now, or freedom from having to bother at all. The threat is that of anomie, of losing our sense of direction and purpose, of decadence (9), perhaps even of madness. Whether one feels the promise or the threat more acutely is perhaps a matter of temperament. I must confess to being more sensitive to the threat. However, the proper task seems to me to be neither to refute relativism (which is either too easy or impossible (10*)), nor to promote it. What we should hope to do is to construct a viable outlook which combines as much as possible of the virtue and promise of relativism with as little as possible of the vice and threat. When I first read Kuhn I not only began, dimly, to sense these problems about relativism, I also sensed that the notion of "paradigm" or "exemplar" might contain the seeds of a way to resolve them.

Kuhn's idea of how "paradigms" or "exemplars" actually function in science is, as he acknowledges (11), indebted and closely akin to the notion at the centre of Michael Polanyi's philosophy of science (12). This is what he calls "tacit knowledge", knowledge which underlies

scientific understanding and practice, but which cannot be articulated. Polanyi, however, was no relativist but a staunch scientific realist, and his belief in an irreducibly tacit component of scientific knowledge was a central plank of this realism:

There are an infinite number of mathematical formulae which will cover any series of numerical observations. Any additional future observations can still be accounted for by an infinite number of formulae. Moreover, no mathematical function connecting instrument readings can ever constitute a scientific theory. Future instrument readings cannot ever be predicted. But this is merely a symptom of a deeper inadequacy, namely, that the explicit content of a theory fails to account for the guidance it affords to future discoveries. To hold a natural law to be true is to believe that its presence will manifest itself in an indeterminate range of yet unknown and perhaps yet unthinkable consequences. It is to regard the law as a real feature of nature which, as such, exists beyond our control.

We meet here with a new definition of reality. Real is that which is expected to reveal itself indeterminately in the future. Hence an explicit statement can bear on reality only by virtue of the tacit component associated with it. This conception of reality and of the tacit knowing of reality underlies all my writings. {13}

By contrast, the so called "incommensurability", which forms the basis of Kuhn's relativism, is essentially a linguistic matter. It is impossible, we are told, to choose between Einsteinian and Newtonian mechanics on a purely logical and observational basis. Nor can we show that the latter is a special case of the former. The reason for this is that the central terms of each theory have different and incompatible meanings, even when the words used are the same. For Einstein, mass increases with

velocity and can be converted into energy. For Newton it is part of the essence of mass that it is conserved, and, it is suggested, Einstein's conception of mass cannot in any way be expressed in Newtonian terms (or vice-versa) {14}. Likewise, the Einsteinian notion that space may be curved is seen as quite properly incomprehensible for a Newtonian {15}. For such reasons, it is claimed, statements made in the language of two different theories can never be properly compared. They can never really contradict one another and thus no strictly logical choice can ever be made between them. I believe that this argument and other related ones {16} do show that strictly formalizeable methods of theory choice, such as we find in the philosophies of the Logical Empiricists and of Popper, cannot be made to work. However, it does not follow that there can be no reasonable grounds for preferring one theory over another, and, indeed, since he first wrote 'Structures', Kuhn has been at some pains to insist that he did not mean to imply this {17}. He has not, to my knowledge, ever entirely eschewed relativism, but he seems to hold only a very moderate form of it. In fact it has been Paul Feyerabend, the co-originator of the notion of theory incommensurability {18*}, who has really pressed the idea's relativistic consequences - to the extent of seeming to make a virtue of irrationality and even capriciousness in science {19}. But Feyerabend, it should be noted, continues to see science as being primarily about 'theories' - sets of statements which can be expressed in a language.

Kuhn's "exemplars" and Polanyi's "tacit knowledge", on the other hand, seem to hold out the promise of being able to get at a deeper sort of knowledge than can be explicitly expressed in words, a knowledge that arises from direct contact with the natural world, rather than being mediated and distorted by the cultural forces embodied in language. As Rorty (20) makes clear, it is precisely the widespread belief amongst contemporary philosophers that this cannot be done, that what we can say is co-extensive with what we can know, that gives relativism much of its plausibility, or even makes it seem inescapable to contemporary philosophers. But if Kuhn and Polanyi seem to hint that there may be such 'infra-linguistic' knowledge, they leave its nature very obscure. It is so obscure, in fact, that at one time Polanyi was tempted by the notion that what he then called "scientific intuition" might be a sort of psychic power, similar to clairvoyance or precognition (21). (I am relieved to say that he later abandoned this idea (22).) However, there are other clues. Both Kuhn and Polanyi express an intellectual debt to Gestalt Psychology (23), and at one time a major part of the present work was intended to be a study of the Gestalt Theory and its influence on these and other related thinkers (such as N.R. Hanson, who also cites them (24)). However, I came to the conclusion that Kuhn and Hanson had attained (or, at least, they displayed) only a very shallow understanding of Gestalt Theory (25), which was, indeed, profoundly anti-relativistic (26). The real influence upon

Kuhn from psychology came, I suspect, from the so called 'New Look' theory of perception, whose leading figure was J.S. Bruner. Kuhn does not mention Bruner, but they were at Harvard at the same time (27) and the one piece of psychological research which Kuhn (28) considers at any length (though without citation) is an experiment, involving anomalous playing cards (red clubs, black diamonds, etc.) done by Bruner & Postman. This, perhaps significantly, was published under the title "On the Perception of Incongruity: a Paradigm" (29). (We shall meet another member of the 'New Look' school, Richard Gregory, in chapter II.D.) Polanyi was, I think, much more genuinely influenced by the Gestalt Theory. But even in his work this influence is difficult to disentangle from other strands of thought. The Gestalt Theory does, I think, give us some insight into the nature and possibility of "tacit knowledge", and we shall discuss it in §I.B.4, but it can hardly be relied upon to provide us with the answers we seek - it is almost certainly empirically false.

Another possible clue as to the nature of exemplars and tacit knowledge seemed to lie in the notion of a theoretical model. Many, and perhaps all, scientific theories (or should we say paradigms) seem to have at their heart an analogy drawn between the more or less hidden processes which are proposed to underlie the phenomena to be explained and some other process, or combination of processes, which is more accessible and familiar. An obvious example is the Kinetic Theory of Gases, in which

the molecules of gas are envisaged as very much like tiny billiard balls moving about and colliding in three dimensions. But the analogy need not be mechanical. The Darwinian theory of Natural Selection is based on a composite analogy between the methods used by the breeders of domesticated animals and plants, and with Malthusian economic theory. Positivistic philosophers of science, most notably Duhem {30}, have tended to regard such analogical models as at best heuristic aids to theory building. These heuristic effects are not insignificant, as Duhem was well aware, and they probably bear very much on the independently interesting issue of scientific creativity. However, they are never, it is suggested, a necessary part of a truly satisfactory and finished theory, which should, rather, be a purely formal mathematical and logical construction. Other philosophers of science, however, notably Campbell {31} and Hesse {32} have cautioned that it may not be wise to, as it were, throw away the model once a logico-mathematical superstructure sufficient to the phenomena under study has been built around it. They argued that it is only the analogical model at a theory's core which makes it possible for theories to be 'predictive' in the sense of suggesting the possibility of new, previously unsuspected phenomena. To be sure, positivist accounts of science are much concerned with prediction, but only in the sense that ^{they} ~~it~~ regard science as a codification of beliefs that event E will (probably, and *ceteris paribus*) follow from situation S, because it has frequently been observed to do so before. Predictions in the first sense are surely

more interesting and important. For one thing, although there are exceptions (such as meteorology and the navigational uses of astronomy), most of the technological benefits of science surely flow from the prediction of new effects.

At this point things begin to look promising. Analogical models seem to be assigned to do much the same job in science as that which Polanyi assigned to "tacit knowledge" in the passage we quoted above. The usual notion of a model will not, perhaps, do everything that Polanyi wants "tacit knowledge" to do - he wants it also to express something about the commitment which a scientist holds towards a theory in which he believes - but let us leave this aside for the moment, for there is a puzzling aspect to the notion of models in science which is seldom remarked upon. Most of the time, in most fields, the models have no material existence. There are exceptions to this - molecular biologists find it helpful to build ball and stick models of molecules, 'cognitive scientists' try to model mental processes as computer programs (33*), and engineers test scale models in wind tunnels and the like - but, mostly, scientific models are embodied, at best, simply as flat and static diagrams in text books. The natural habitat of the scientific model is, surely, in the imagination of the scientist. Indeed, if this were not so then it is not at all clear that they would be at all promising candidates to solve any of our epistemological problems. If scientific models were, in the first place,

actual material models, then most of the same problems as to how we can know the material world would seem to arise in the case of knowing the model as would arise over knowing the original phenomena of interest. To push the model back into the mind at this point may seem merely obscurantist, putting up a smoke-screen instead of solving a problem {34}, but I hope by the end of this work to have blown away most of the smoke and to have revealed how a mental model can fulfil an epistemological rôle which a material one cannot.

We are suggesting then that the primary mode of existence of scientific models is as mental images. They are now beginning to look even more like "tacit knowledge", and perhaps we could say that the main function of Kuhnian "exemplars" is to convey such models, such images, into the scientist's mind, and to sustain them there. Green {35}, it seems to me, is quite right in arguing that Kuhn can fairly be glossed as proposing a major rôle for the imagination in science. Furthermore, there is a respectable tradition of giving mental images a central rôle in scientific understanding. I am not thinking, here, merely of the many anecdotes in which unbidden images are said to have played a crucial part in the genesis of new scientific ideas (Kekulé's famous snake images, which inspired his idea of the benzene ring, for example) {36}. Rather, I mean the notion that mental images play a necessary, ongoing part in the understanding and application of a theory. Perhaps the most famous expression of this view came from Heinrich

Hertz in The Principles of Mechanics:

In endeavouring thus to draw inferences as to the future from the past, we always adopt the following process. We form for ourself images {37*} or symbols of external objects; and the form which we give them is such that the necessary consequents of the images in thought are always the images of the necessary consequents in nature of the things pictured. {38}.

As A.I. Miller {39} has recently shown, Hertz was by no means alone amongst philosophically minded physicists of his time in taking such a view (although many also dissented from it). Differences of opinion over the value or even necessity of imagery in scientific understanding were not confined to physics. For instance, Van't Hoff's explanation of optical isomerism in terms of asymmetric molecules was viciously attacked by Kolbe, on the grounds that it was based on fantasy and "hallucinations", on mental images of molecules. Van't Hoff, of course, is now considered to have been right, and Kolbe's attack drew from him a stirring defence of the importance of imagination in science {40*}. However, the doubts raised by the so called "imageless thought controversy" in psychology {41}, and the problems surrounding the interpretation of quantum mechanics, meant that, in the earlier part of this century, anti-imagistic positivism carried the day, at least amongst philosophers. The Vienna Circle positivists (upon whom Duhem was a very important influence {42}) became the dominant force in philosophy of science - and they had no time for such occult entities as mental images.

Miller, however, argues that quantum mechanics can

be understood in terms of imagery, and, indeed, that Bohr's 'Copenhagen' interpretation, with its paradoxical notion of "complementarity", was devised precisely to preserve visualizability, even at the apparent cost of consistency (43). Perhaps it is no accident that the Copenhagen interpretation has remained dominant amongst physicists who actually have to use the theory. In any case, out of earshot of the philosophers many scientists continued to uphold the opinion that imagery played a vital rôle in their own thought processes. Einstein testified, in a letter solicited by Hadamard, that:

The words of the language, as they are written or spoken, do not seem to play any rôle in my mechanism of thought. The psychical entities which seem to serve as elements in thought are certain signs and more or less clear images which can be 'voluntarily' reproduced and combined. (...)

The above mentioned elements are, in my case, of visual and sometimes of muscular type. Conventional words and other signs have to be sought for laboriously only in a secondary stage, when the mentioned associative play is sufficiently well established and can be reproduced at will. (44).

But we should not restrict imagistic understanding to geniuses or theoreticians. The physicist Martin Deutsch avers:

I have never met a physicist, at least not an experimental physicist, who does not think of the hydrogen atom by evoking a visual image of what he would see if the particular atomic model with which he is working existed literally on a scale accessible to sense impressions. (45).

After describing an experiment in subatomic physics, he continues:

We clearly could not have designed our

original experiment and measured the mass of the particle unless we had started out with an image of the phenomenon involving such a "particle." The very word used implies the image.

But a meson is clearly not an object with the general properties of a ball which we could see if we had a sufficiently good microscope, or feel impinging on our hand if our nerves were a little more sensitive. We are not forced by direct sensory perception to use this image. We have developed it because it allows us to reason from one experiment to the next by analogy; even in a mathematically sophisticated theory we deal with formal thought processes designed to connect sensory impressions. It, too, must proceed by analogy with the connections established between such perceivable events. {46}.

Gerald Holton {47} has remarked on the way that Robert Millikan seems to have visualized the behaviour of the electrons involved in his famous oil drop experiment, and it seems doubtful, on Holton's account, that he would have correctly interpreted the experiment if he had not thus visualized them. A similar visualization, this time of atoms, seems also have underlain the long (and successful) efforts of Jean Perrin to establish the atom's reality {48}. It should be possible to multiply such examples indefinitely.

On the basis of these arguments and this sort of testimony, then, it appeared to me that imagination and mental imagery might well provide a good starting point for an investigation into the "tacit component" of scientific knowledge {49*}. This had a strong additional appeal to me because "imagination" is, of course, a key term in the self-image of the arts. It seemed that an investigation of

imagination might throw at least a sidelight on the first of the issues with which I began this introduction, as well as on the second. At least since the Romantic movement the arts have claimed the imagination as their own (50). If I could show just how it was also deeply involved in science then that might enlarge the basis for mutual understanding. It might seem, however, that this is not very promising. Our modern concept of imagination is often seen as an invention of the Romantics, specifically designed to express some level of reality to which artists have access where 'reason' and the sciences do not (51). Where scientists or philosophers of science try to claim it, it may be felt, they are taking something which does not belong to them, and robbing it of its magic. This does not seem to be entirely without foundation. Consider the following remark of Popper:

Even theories, products of our intellect, result from the criticism of myths, which are products of our imagination. (52).

No self respecting Romantic would consider myth as ripe for criticism, and certainly not as being improved by being turned into falsifiable theory. However, our idea of imagination did **not** originate with the Romantics. It originated with Aristotle, and Aristotle does not apply it to questions of the arts at all (the relevant terms, "phantasia" and its cognates, do not appear in the *Poetics* at all (53)), but to cognition. Furthermore, I shall argue below that the concept is introduced by Aristotle precisely to enable him to maintain, against Plato, that worthwhile empirical knowledge of the natural world is possible. The

Romantic conception of the imagination as giving a special access to reality descended to them from Aristotle. Their main contribution was to make it into something obscure yet peculiarly virtuous.

To be fair, the obscurity of "imagination" cannot be entirely blamed on Romanticism. Aristotle delineated a function for the imagination (in *De Anima*), but he also gave some hints as to a mechanism (in the *Parva Naturalia*). The trouble is that the mechanism, the formation of mental pictures in perception by a process akin to the stamping of a seal into wax (54*), has long proved sorely inadequate to the function. Empiricist philosophers accepted the mechanism and tended to down-play the function (hence for Berkeley, for example, there is no material reality to get in touch with). The Romantics, taking their cue from Kant, exalted the function and pushed the mechanism into obscurity. Although they contributed much to the theory of imagination - of what the faculty could be said to do for us - they contributed little or nothing to what I have called, below, the theory of imagery, the theory of the mechanism by which the faculty operates. It is all too easy to conclude that the term as it has descended from them is no more than windy mystification. However, if we could delineate a mechanism adequate to the Aristotelian function, or at least show that such a mechanism is possible, then we might be able to salvage some of the Romantic insights as well as giving a non-relativist, non-positivist account of science.

If there is to be any hope of finding such an account then the place to look must be modern, scientific psychology, and it was to this that I turned. I found that there was, in fact, an extensive recent literature on the cognitive functions and mechanisms of mental imagery, but I also found that the whole matter was highly contentious. It soon became apparent to me that there were essentially three types of theory currently in play as to the mechanism of image formation. I have called these theory types respectively the "quasi-pictorial", the "descriptive" and the "perceptual activity" theories of imagery. I was encouraged to find, later, that a very similar taxonomy is also given by Morris & Hampson (55) in their recent text on imagery. The term "quasi-pictorial" is that used by this type of theory's chief contemporary advocate, Stephen Kosslyn (although the term "analogue" is also sometimes used). What I have called "descriptive" theories are more usually referred to as "propositional" theories by contemporary psychologists, but this usage implies a sense of "proposition" which differs significantly from that current amongst philosophers (from whom it has, nevertheless, clearly been borrowed). To avoid confusion I have preferred to use a different, but still entirely appropriate, term. Morris & Hampson (56) similarly refer to "description theories" here. The third type of theory has no generally accepted name, so I have invented my own. Morris & Hampson (57) call them "rôle playing" theories, but I think "perceptual activity" theories is more

appropriate.

Quasi-pictorial theories can be seen as the direct inheritors of the philosophical and popular tradition of treating mental imagery as the 'seeing' of 'pictures in the head' with the 'mind's eye'. This tradition, as we shall see in chapter II.A, descends from the Ancients, and has been conveyed to us through Descartes, Hobbes, Locke and others. As such, despite the greater sophistication and detail which it takes on in Kosslyn's modern version, it soon became apparent to me this type of theory could no more solve the epistemological problems in which I was ultimately interested than could the philosophers just mentioned. However, in the light of its antiquity, its popularity, its historical resilience, and the mass of empirical evidence which Kosslyn (58) and others have recently marshalled in its support, it is not to be lightly dismissed.

"Descriptive" theories of imagery have grown out of modern research aimed at programming so called 'Artificial Intelligence' into computers, and their viability is bound up with the viability of that project. These theories treat mental imagery (and, indeed, all mental contents) as being embodied in the brain in the form of a set of 'sentences' (or "propositions") in an unconscious "language of thought" (59), which is envisaged as the brain's equivalent of the internal representational system found in (appropriately programmed) computers. This

type of theory has been vigorously promoted, largely on metatheoretical grounds, by Zenon Pylyshyn {60} and others, in direct and conscious opposition to quasi-pictorial theories. Despite its modern and philosophically sophisticated flavour I was soon forced to the reluctant conclusion that this type of theory would be of no help with my epistemological problems either. If a mental image were really a set of descriptive sentences expressed in "mentalese" {61*} then it could, in principle, be translated without residue into a set of descriptive sentences in English (including, if need be, mathematical English). "Tacit knowledge" (and the expression is used in this way by Pylyshyn) thus turns out to have exactly the same sort of logical form as does explicit knowledge. Any imaginary model within a theory would be potentially eliminable in favour of further sentences. Thus theories conceived of as containing theoretical models which are like this would be in no different epistemological situation from that of theories made up entirely of explicit sentences. In effect I am in agreement with Churchland {62} that if we are to progress decisively beyond the sort of philosophy of science we find in the Logical Empiricists and in Popper (and which has found its *reductio* in the work of Feyerabend {63}), then we must develop a viable notion of "non-sentential" mental representation. This rôle has traditionally, since Aristotle, been filled by mental images, but pictorial images have proved seriously inadequate to the task.

When I came across what I call "perceptual activity" theories of imagery, particularly that of Neisser {64}, which is linked with the 'direct realist' perceptual theory of Gibson {65}, I sensed at once that these might provide the sort of account I had been seeking. Unfortunately, however, for a number of reasons these theories have had relatively little attention paid to them by most psychologists, and the kinship between the various versions of them seemed little recognised even by those who propounded one or other of them. Although the fierce disputes between quasi-pictorial and 'descriptive' theorists were widely perceived to have reached a stalemate by the late 1970s {66}, 'perceptual activity' theories continued to be mostly ignored. This lack of criticism has inevitably led to a lack of development amongst theories of this class.

Here I found myself in a difficult position. The contemporary debates about imagery within psychology (and to a lesser extent within philosophy) had become extremely complex, and the empirical evidence, though extensive, had failed to resolve the matter. Furthermore, the sort of theory which looked to be by far the most promising for my purposes was not widely known, and even less widely believed. If I were to illuminate the epistemological problems which I cared about, and along the generally promising lines that I had been pursuing (and I still felt that this was possible), then it would be necessary to enter into the fray of psychological argument and to try to

show that 'perceptual activity' theories have distinct advantages in psychological as well as epistemological terms. I must show that they were at least as coherent, and at least as successful at dealing with the empirical data as the other extant types of imagery theory. This was a tall order, but I thought it a course worth embarking on. It was already widely recognised that the disputes about imagery turned, in large part, on metatheoretical issues. It thus seemed appropriate to treat the matter in the historical context of philosophical ideas about the imagination and of the rather chequered career of the study of imagery within empirical psychology itself. The philosophical context of philosophy of mind also seemed of relevance. This explains why the body of this dissertation, despite the epistemological motivation behind it, is almost entirely taken up by history of philosophy and psychology, a little philosophy of mind, and a good deal of straightforward theoretical discussion of psychology. Part II is mainly taken up with a critique of the three extant types of imagery theory, and with some development of 'perceptual activity' theory to meet certain objections that have or might be made to it. Part I, in preparation for this, deals with the origins and coherence of our conceptions of the imagination, and details how the study of imagery within psychology arrived at its present state. That, then, is why we are starting from here.