CREATIVITY AS A PSYCHOLOGICAL CONCEPT AND ITS IMPLICATIONS FOR PSYCHOLOGICAL METHODOLOGY

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1980
There is a role for philosophy within psychology, in that the ways in which human beings structure their understanding of the world have direct implications for the sorts of explanation that psychology should be aiming to provide. Consideration is given to what is involved in the ascription of creativity to a person, taking into account what empirical researches have contributed to the enrichment and clarification of this area: attention is drawn to the need for some definite performance which acts as evidence for the ascription; for the performance to be an original effort on the part of the creator; for the performance to be by 'A' in the sense that it is an intentional action and not a random or a rote occurrence; and for the presence of another person 'B' to provide social context for the ascription. These criteria are indispensable to the adequate psychological investigation of creativity, and where they are not sufficiently taken into account, the value of research findings is vitiated. It becomes clear that the investigation of creativity is at odds with a strictly objectivist methodological approach, in that to enquire in a systematic way into creativity is not to analyse facts but to explore judgments; that it is also at odds with a strictly deterministic methodological approach, in that choice of action is a necessary element; and that the use of statistics is also curtailed. As a psychological concept, creativity thus undermines much orthodox psychological methodology; and supports the development of an alternative approach, emphasising the need for shaping and reshaping partial and provisional explanatory patterns which attempt to make human behaviour coherent.
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DECLARATION AND

ACKNOWLEDGEMENTS

The thesis has been composed by myself; I am grateful to the following individuals who have been instrumental in its development:

Dr John Beloff
Ephraim Borowski
Larry Briskman
Dr Oliver Flint
Isabel Nisbet
David Salmond

Isabelle Low
CHAPTER 1

PHILOSOPHY AND

PSYCHOLOGY
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PHILOSOPHY AND PSYCHOLOGY

INTRODUCTION

1.01 This chapter serves as a prolegomenon to the main development of the thesis. It considers various roles that philosophy could be said to perform within the psychological sciences ranging from, at one extreme, the neutral analysis of the logical structure of specialised theories to, at the other extreme, the supersession of empirical by conceptual investigation. I shall argue, after examination of the cases put forward by a selection of theorists dealing with this aspect of the philosophy of psychology, that conceptual analysis linked to empirical research can have something to say about the conduct of psychology. On the basis of the tentative conclusions reached at the end of this first chapter about what status can be accorded to and what use made of conceptual analysis as part of a psychological study, I shall proceed to outline the strategy to be followed in the main body of the thesis.

THE DEVELOPMENT OF PSYCHOLOGY

1.02 It is possible to chart the beginnings of psychology as an experimental study at around the turn of this century. (This sets aside the purely speculative writings on "the science of man" of philosophers of previous years.) At this time psychology was clearly the study of mind: and what first came under experimental scrutiny was the
prevalent associationist theory of mental phenomena. At Wurzburg experimenters asked subjects to report on their mental processes as they compared, judged and discriminated; it was hoped thereby to establish that thought operates in terms of mental images which are copies of sense impressions. Such introspective methodology however produced no clear evidence to validate the associationist theory.

Because the results of such processes cannot be checked against external circumstances, nor be invalidated by rechecking, there are no criteria for deciding whether such descriptions are correct or incorrect. Inevitably disputes about descriptions arose and could not be settled.

1.03 At this point the methodological revolution proposed by Watson was a breath of fresh air. This promised (and achieved) considerable advances for the incipient science, by eliminating all but publicly verifiable behaviours as the data base. This allowed for (in fact, demanded) experimentation that looked very much like experimentation in other sciences. However this strategy undoubtedly also had the effect of limiting the scope of psychological research by its insistence that the science of behaviour must reduce itself in turn to the science of physical movements, i.e. denied the appropriateness of a subjectively oriented discourse and stressed the necessity of purely objective discourse about behaviour.
Psychology is nowadays still considered to be the science of behaviour, but this no longer tends to imply a rejection of anything smacking of a mentalist concept. Rather, inherent in the present day understanding of this definition is the conceptual assumption that "mind" is expressed as behaviour, as well as the practical acceptance that it is only the mind in action that can provide the scientist with data. The rapid technological developments of this century have vastly extended what the psychologist is prepared to call behaviour—attention and expectation (as measured by eye scan patterns), dreaming (as measured by rapid eye movement) and self-control (monitored by biofeedback machinery) can now be accorded the same status of measurable behaviours as can grosser physical movements. Nonetheless, psychology still assumes the necessity of an observer-based objective logic of discourse in which explanations of human activities are to be couched for such explanations to be scientific. Psychology comports itself as an empirical science, seeking to discover causal explanations of human activity. It therefore proceeds via observation of behaviour in controlled situations, and bases its claims to knowledge on what is elicited from these controlled and replicable experimental situations. In what follows we shall see that philosophy on the other hand roots its explorations in the agent-based subjective discourse in which human activity is commonsensically explained, and proceeds via non-experimental reflection. If these languages are incommensurable and non-interactional, then philosophy cannot contribute directly to psychology; if on the other hand it can be shown that the two languages can be of relevance to each other, then it is possible for philosophy to have some function in the psychological sciences.
MODERN VIEWS ON THE ROLE OF PHILOSOPHY

1.05 Much of philosophy in Britain into the middle distance has, under the influence of Wittgenstein and before him G.E. Moore, been concerned with the elucidation of ordinary language use. The slogan "don't ask for the meaning, ask for the use" has passed into common parlance, perhaps sometimes without its real force being appreciated. The discovery of within what context of linguistic activities (language games) a concept is wielded enables the rules which govern its use to be exposed. Language games themselves are rooted in wider contexts from which these language games derive their significance, contexts which Wittgenstein calls forms of life. Concern with a language has replaced both metaphysical and anti-metaphysical procedures. The empiricist tradition of epistemology generally predominant in Britain has always played down the contribution that reason makes to the gaining of knowledge in order to stress the dominant and innovative role of experience; it is antipathetic to metaphysics, interpreted as the attempt to gain knowledge about reality (or Reality) by the application of thought. Rather, it is possible to trace through Bacon, Locke, Hume, Mill down to Russell in this century the robust conviction that knowledge comes from experience. Reality is directly investigated by observation, and the role of ordered thought is to maximise the benefit to be gained from this observation. The establishment and refinement of scientific methodology is indicative of a felt need for a rational framework in which knowledge by experience is central and conclusive. On this view metaphysics is a waste of time, a pointless exercise in which the philosopher is prone to indulge himself when he could be doing something useful to assist those truly engaged in exploring
reality. The rigorous methodology of questioning all assumptions is held to be a useful tool: the central skill of philosophy is therefore logic, conceived as a neutral discipline at the service of the empirical sciences. The most obvious flowering of this view of philosophy, rooted as it is in turn in an empirically-oriented epistemology, was the logical positivist movement, which set empirical verification as the criterion of meaningfulness. The school was soon enough discredited; it has nonetheless had a crucial influence on the self-image of philosophy, and the ready adoption of linguistic analysis as a dominant methodology is partly because of this. Philosophical emphasis on the elucidation of the contexts within which concepts of ordinary language are used is an acceptable strategy in place of discredited metaphysical speculations about the nature of thought, while also avoiding the anti-metaphysical dead end.

Language is seen as mapping thought, as offering an accessible and non-private indication of the structure of the mind. The strong version of this thesis would claim that thought and language are identical in form. However, it is not necessary to validate this strict identification in order to postulate an intimate connection between the way we speak about the world and the way we think about the world. Thus to come by way of conceptual analysis to a deeper understanding of the way in which we use a concept is at the same time to arrive at an understanding of how we think. The elucidation of concepts is at the same time an elucidation of the nature of reality as it appears to us. There is a certain cloudiness as to whether such a procedure yields increased knowledge. Certainly more mundane than old-style metaphysics, such a programme of philosophical
investigation does not claim to end by revealing anything that is not known, yet if this were to be taken literally, the investigation would not be worth undertaking at all - a conclusion which has not yet forced itself to a significant degree on philosophers. Strawson makes a defensive distinction between descriptive and revisionary metaphysics, and Ryle summarises his aim as "mapping the logical geography" of psychological concepts. If by chance they end up by revealing what has not been appreciated before, then they have simply rectified temporarily mistaken perceptions, not created new knowledge. Ryle defines the main task of the philosopher as the determination of the logical cross-bearings of concepts which we already know quite well how to apply: and this is to be done in a severely logical way, by showing with what other propositions the propositions in which the key concepts in question are wielded are consistent and inconsistent, and what propositions follow from and are antecedent to them. Ryle limits the function of philosophy to clarifying and correcting such knowledge as we already have available to us.

1.07 While there is little argument today but that the aim of philosophy is to clear up (dissolve rather than solve) problems by treating them as logical confusions, what is crucial for the present exercise is whether this process is seen as concerning contingent or necessary elucidations. In many cases, the endeavour appears to be a quasi-Kantian one, with the aim of establishing, by analysing how we do make reference to the world, how we must conceive of reality.
Strawson for instance aims to "describe the actual structure of our thought about the world". Here the word 'actual' is ambiguous, in a way that is worth noticing in view of the argument to come. The methodology of Individuals proceeds by the conceptual analysis of the most fundamental assumptions of (? all) language, the centrality to any kind of referent language of the continuing existing of material bodies and persons. The argument seems to presume (though this is nowhere explicit) to deal with language, rather than languages, let alone everyday twentieth century BBC English: the grand sweep on the line of reasoning implies that Strawson sees himself as elucidating important corollaries of any possible use of language. Thus he is engaged in extracting a priori presuppositions from the possibility of speech, and using these to establish a priori presuppositions for a person's apprehending the world. Such a conclusion is somewhat at odds with his claim that he is not engaged in innovation; which claim in context seems to have more to do with the pejorative implications attaching to the practice of metaphysics than to be an accurate assessment of his activity.

In a similar way, Ryle also practices a sleight of hand in regard to the status of his conclusions. His analysis of the logical status of the concepts of human activity is intended covertly to delimit the necessary structure of psychological concepts: when he speaks, as he does at regular intervals throughout The Concept of Mind, of ordinary people knowing quite well enough how to understand, appraise, resolve doubts about; and influence actions, he is not drawing on how
people in Oxford use language, but how anyone who exists in a social world/is a person (it is not clear from Ryle's argument which he considers the primary prerequisite, upon which of course his entire argument rests — R.S. Peters, as I shall discuss later, takes a more extended interest in this question) must employ concepts about human action. Again, Hamlyn, in his article on "Behaviour" makes much the same point about the necessary conceptual distinction between movement and behaviour (drawing upon the Aristotelian distinction between ἄρτιτον and ἐνεργεία, the former of which is not intelligible in itself and by itself, whereas the latter has a completeness which is reflected in the sort of explanation appropriate) on the basis of a somewhat cursory appeal to linguistic usage, and proceeds thereby to a general stipulation about the conceptual wrongness of a behaviourist approach to the practice of psychology, a stipulation that can follow logically only from the establishment as necessary of the distinction he has drawn from the analysis of language.

1.09 What I am concerned to stress is that this kind of philosophy in action, whatever the protestations made by its proponents, sets itself the task of delimiting human understanding through the non-empirical study of linguistic usage. Epistemologically this is linked to the Kantian insistence upon thought-structured knowledge of the world. Though the knowledge thus gained is not about the objective world it is nonetheless synthetic knowledge, creating new understanding about the necessarily subjective way in which persons confront the world. Thus at a stroke the independent status of philosophy as a
discipline in its own right with distinctive methodology and distinctive subjects of concern is re-established. The analysis of language therefore provides philosophy with a procedure by means of which knowledge of a special kind can be arrived at, and therefore with an arena of activity special to itself. The study-bound philosopher has at his disposal a method of scientific enquiry as proper as empirical research, given the nature of the subject matter which philosophy attempts to make more intelligible. The conceptual exegesis of those ideas which are involved in how persons crucially structure their world can positively contribute to the growth of psychological understanding.

Notice however that this fine-sounding defence of the philosophical enterprise trades upon two ambiguities, both of which involve the necessary/contingent distinction highlighted earlier. In the first place, a philosopher's concern with language, while billing itself as involving the analysis of "ordinary" language, often requires for the establishment of its conclusions (and conclusive prescriptions) to be an elucidation of what is necessarily presupposed in the use of any language whatsoever. It is an insufficient ground for a prescription about psychological methodology that the rules governing the use of "X" have implications A and B, if the rules governing the equivalent French phrase (let alone whatever it might be in Serbo-Croat or Chinese) do not share in these implications. The alternative to showing the conceptual necessity of a given usage is to allow that linguistic analysis cannot purely be an a priori exercise, that
empirical researches into, inter alia, particular language structures and general theories of linguistics drawing upon such empirical work are directly or indirectly of relevance to the enterprise.

In the second place, the argument that analysis of the way in which the world is understood gives us insight into what sort of explanations are appropriate to it trades on an ambiguity about human understanding. The insistence that all knowledge of the world is unavoidably saturated with subjectivity (so that the clarification of how we think about a thing contributes directly to our knowledge of it) contains in itself a distinction between the sort of subjective considerations which are common to a specific culture, and the sort of subjective considerations which are necessarily shared by all human beings. It is the distinction between the distorting lenses that I wear as a member of a cultural grouping and those that I wear as a member of the human race. In the first instance, analysis of my subjective ways of apprehending, of the way the relevant rules operate, will yield only culture-bound assessments of what sorts of explanation are appropriate: this will vary a great deal, from Eskimo to Zande. ¹

It is only when the subjective presumptions can be shown to be necessarily shared by all persons that it can be valid to derive general propositions and prescriptions.

A CENTRAL ROLE FOR PHILOSOPHY IN PSYCHOLOGY?

Let us now look at these arguments as they relate more specifically to the practice of psychology. I want to begin by summarising the

¹ See Pritchard, 1951.
argument of Peter Winch for the central role of conceptual analysis in the social sciences (especially, in Winch's treatment, sociology). It goes as follows: individuals mostly have little difficulty in explaining the activities of other people; indeed, there is a sense in which it is odd to use the term 'explaining' in this way, as 'explaining' connotes the making comprehensible of something which is puzzling. In the main, other people's behaviour is not puzzling to us: we are usually able to understand the behaviour that we encounter. It is only when an action is prima facie puzzling that we seek an explanation, and we are content with a re-interpretation of the action that assimilates it to the sort of behaviour that we understand. The sort of behaviour that we find meaningful is purposive, intentional, appropriate to context, rule following. Questions about human activity are therefore requests for rules or reasons, not for physical causes, i.e. they are essentially philosophical and not empirical questions. Thus the social sciences are at heart philosophical enterprises and conceptual analysis is central to their progress.

1.13 It is surprising that this line of argument should be as rife as it has been in relation to the philosophy of psychology, because it rests upon the mistakes about linguistic analysis discussed earlier. If Winch's argument is correct, then empirical research in the social sciences can be severely pruned by a priori considerations. Analysis of linguistic usage, not observation or controlled experiment, will yield information about activities. A trenchant objection to this comes from N.S. Sutherland in his article "Is the Brain a Physical
System?" where (arguing against Geach) he produces what is at base a version of the second argument above. To conclude that a machine cannot be intelligent because a machine is not an animate being and because, conceptually, thinking is one of the attributes of living beings is, he argues, fallacious: to say that in ordinary language the word 'thinking' is only applied to man and higher animals is simply to show that historically this has factually been the case. Times change, and technological advances may change the way people are prepared to use the concept 'thinking'. The way people use language reflects their muddles and mistakes as well as their wisdom (and also, though Sutherland does not make this point, reflects what they have actually experienced — no conclusions about what is necessarily so can be derived from differences in conceptualisation as a result of available resources). A meaning enshrined in concept-use can indeed be shown to be necessary to that specific culture bounded in time and space, but only at the cost of vitiating the general conceptual point that Geach is trying to make. Nonetheless Sutherland draws too strong a conclusion from this, i.e. that questions about whether thinking can be understood and explained in terms of the organisation of matter in the human nervous system, or whether a machine could be built in which matter could be organised in the same way are purely empirical questions; although he is correct in saying that they cannot be solved by an appeal to ordinary language. J.H. Grundy in his comment on this article is surely right to say that a priori considerations of our talk about human behaviour is important to such a programme of empirical research. When would we be able to say that computers are intelligent? The question is
not simply one of what advances in electronic engineering are possible, but also involves the conceptual exploration of what intelligence is (i.e., what intelligence must be, not simply what our present use of 'intelligence' reveals). (I have somewhat amended Grundy's argument in order to bring out this distinction: the lack of which leaves him open to the counter-attack which Sutherland duly delivers.) Indeed, the development of mechanisms which parallel some aspects of what has hitherto been thought of as purely human or higher animal behaviour (e.g. problem-solving programmes, homeostatic systems, goal-directed missiles) has opened up new possibilities in philosophical analysis which did not exist prior to this technological advance. Neither the empirical development of such programmes nor the conceptual analysis of such terms as 'intelligent' or 'purposive' can proceed independent of each other; if the aim is real progress.

1.14 This is essentially the same point as Louch makes in different circumstances against Winch: that the exploration of the use of a concept (pace Wittgenstein) is as much an empirical as an a priori activity, as it usually requires an exploration of context. Louch objects that Winch denies the relevance of empirical strategies in the elucidation of rules: "social customs are rather less clearly governed by codebooks than Winch can allow", (Louch, 1963, p.281). Although Louch, like Winch, is here talking specifically about sociology, with perhaps a sideways glance at social anthropology, the general point that, because truth in the social sciences is not established by observation alone, observation is not therefore unnecessary can be carried over directly into psychology.
Sutherland's second attack upon a priori solutions in psychology is to the effect that, while analysis of the words 'voluntary' and 'involuntary' can show there is a real distinction between the actions to which they are applied, it may turn out after further (empirical) research that voluntary acts are simply acts which are determined in much more complicated ways.

"There is a strong temptation to believe that because such words are not normally applied to anything except man, human behaviour must be determined by principles quite different from those which operate in any other physical systems whether actual or possible. This conclusion clearly does not follow: there must be some differences between man and inanimate objects or we would not consistently classify them apart. What these differences are however is a matter for empirical investigation ... [and] ... cannot be settled by fiat, and it cannot be settled by appeal to any distinctions built into ordinary language." (Borger and Cioffi, 1970, p.105)

Now this argument seems to me to be entirely right: and it should be read as pretty fair-mindedly outlining what philosophy can do for psychology - providing insights and guidelines for the conduct of empirical research. It is interesting to note that Sutherland is here willing to grant that conceptual analysis can be positively helpful to the conduct of the empirical process. On occasion however the anti-apriorism of his attack betrays him into a less balanced empiricist stance. For instance, he produces a third argument in his reply to Grundy against the possibility of a priori conceptual delimitation of the science of psychology as follows:

"I am sceptical of Grundy's claim... That 'a priori consideration of our talk about human behaviour' is important to a programme of empirical research. A consideration of animistic thinking about the weather in some cultures conceived in ordinary language, would scarcely have helped the science of meteorology." (ibid, p.134)
This is striking but, although it points out the distinction I have previously noted between culturebound concepts and those reflecting universally necessary assumptions, it fails to make contact with the force of the claim of conceptual analysis to be relevant to psychology. The moral to be drawn from Sutherland's dictum is presumably that the young Zande would-be empiricist has nothing to gain from considering how the animistically minded elders of the tribe employ the concepts 'rain' and 'wind', and everything to gain from the detailed observation of phenomena, the construction of functional hypotheses about the interaction of these phenomena and the attempt to test these hypotheses by prediction and confirmation. In the same way, Galileo was right (in the popular version of the story) to ignore the attempts of the Church to proscribe his empirical research. Sutherland's concluding remarks that:

"the attitude of some philosophers toward experimental psychology is reminiscent of the attitude of the Roman Catholic Church towards physics and genetics at certain stages of history. It is fortunate indeed that such philosophers wield spiritual but not temporal power."

(ibid, p.138)

tend to bear out this construction. But not all proscriptions are interferences with the free investigative spirit: there may be occasions where a little prior consideration of what may be involved in a piece of research will save wasted effort by concentrating the mind more clearly on what looks to be relevant. Our Zande meteorologist may be right to discard the teachings of his elders, but is in fact not likely to do so unless these teachings have proved in some respect unsatisfactory to him. Perhaps he finds that they
have low predictive power, or inherent contradictions that he cannot tolerate; at all events, it is by thinking as well as by looking that he will come to try to establish some alternative kind of explanation. Thinking can be an aid as well as a snare. Sutherland's eagerness to shut the door on conceptual analysis leads him injudiciously to separate out the research programme from the context in which the need for such a programme arises. It is not the case that consideration of animistic thinking about the weather "would scarcely have helped" the science of meteorology: on the contrary, a science of meteorology could scarcely arise in such a culture unless thinking about its animistic conceptualisations had turned up doubts and problems. In stressing the value of empirical research, Sutherland in this argument does less than justice to the value of reflective thinking.

1.16 So far I have been arguing against what I see as the imposition of conceptual analysis upon psychology by sleight of hand. I have dealt fully with the objections raised by Sutherland because he seems to me to offer cogent and pressing arguments against a central role for philosophy in psychology, and these objections have to be met squarely before we can go further. Consideration of the implications of the way ordinary language is employed, as far as the philosopher is aware without empirical study, does not automatically yield truths about what sort of explanation ought to be produced by psychologists, far less by itself producing such explanations. If philosophers believe that this is their role in psychology, then they have fallen victim yet again to the craving for metaphysics. But the strategy
is by itself a constructive one, and the fact of its misuse on occasion should not lead to its condemnation out of hand. If it can be shown that a concept to do with human behaviour has certain concomitant implications, and not simply in a culture-bound sense, then this can have general prescriptive relevance to what would count as an adequate explanation in psychology. It is important however that this is not seen as prescribing empirical research, but rather as providing criteria in terms of which to assess it.

One writer who tends to take this line is Richard Peters, who has said that psychology is an empirical science in an unsatisfactory condition because it is operating in the midst of conceptual muddle. He attempts throughout his writings on the social sciences to provide a general exegesis of what an adequate explanation would look like in terms of a purposive, rule-following model, which he seeks to show is conceptually central to the understanding of human action. With this model in the background he proceeds in The Concept of Motivation to reveal the confusions, inconsistencies and inadequacies of various theories of motivation, notably the various drive reduction theories in the American behaviourist tradition. Typically he sees these as overstretching models of explanation appropriate to a limited kind of situation to meet situations significantly different in kind. Such reckless theory building would, he thinks, be avoided by a greater sensitivity on the part of psychologists to the different kinds of "why?" different behaviours evoke. What leads psychologists astray is their fondness for
technical rather than ordinary language, a predilection which, far from allowing greater precision, blunts the sense. This is an approach which, while claiming a significant role for philosophy in psychology, does not lead to the belittlement of empirical research. In his Chairman's Remarks at a symposium on "Psychology as the Study of Human Knowledge" he argues explicitly for the undertaking of many more specialised piecemeal research projects, opining that it has been the predominance of the ambitious classical theories in psychology that has inhibited progress, and that a yearning for scientific respectability has stunted true experimentation in psychology. More sensitive than Ryle to the chasm between ordinary language usage and psychological knowledge, Peters offers a rationale for the proposition that the way in which language is used is relevant to the practice of psychology by emphasising the shared stock of commonsense knowledge as the context in which action is comprehended. We understand behaviour "because we have been initiated from childhood onwards into the various rules and purposes that are constitutive of human life ... In learning to behave as human beings we at the same time take into ourselves the structure of concepts and categories without which we could make no sense of human life." (Brown, 1974, p.139)

On this thesis, philosophers can get at this structure of concepts and categories which 'explain' human behaviour through careful study of commonsensical language. Now this is very far from what Kant has in mind when he talks about categories structuring reality for people, and it is questionable whether Peters gives sufficient consideration to this necessary/contingent distinction to which
attention has already been drawn. Certainly Peters does obfuscate
the issue by referring to clearly culture-bound examples of rules
governing concepts; and remarks like

"... commonsense knowledge ... is to a large extent
culture-bound, depending on the level of differentiation
and complexity that a society has attained. May vary
from group to group within a society ..."

(ibid, pp.137-8)

"All these presuppose elaborate conventions worked out
over centuries, which are incorporated in our
language. If we understood nothing of these elaborate
conventions, human behaviour would be as unintelligible
to us as to small children and animals."

(ibid, p.139)

alert one uneasily to a slide between contingent and necessary rules
governing the use of concepts. The truncated argument however is
most favourably read as a summary of Chapter 2 of Ethics and Education
which contains what Peters intends as a transcendental argument to
establish the necessity of the rule following, purposive model.
This primary conceptual scheme, derived from 'common sense' is thus
indispensable for the explanation of psychological phenomena.

1.18 That analysis of the way in which language is used reveals a
rule following model of human action which in turn has prescriptive
consequences for the adequacy of psychological explanation is often
argued in present writings by philosophers on psychology. Such a
procedure is potentially open to the objections already made to the
too easy elision between analysis of ordinary ways of speaking to
prescriptions about necessary ways of understanding. But such
objections can be avoided in various ways - for example, Peters'
'transcendental argument', or as essayed by Toulmin, by the involvement
of empirical research as well as linguistic analysis in the
provisional exegesis of the rule-governed nature of behaviour.
Experimental investigation of the developing relationship between
linguistic and practical activities which are involved in the learning
of concepts can reveal structure in a way that linguistic analysis
of the finished product cannot: the researches of Vygotsky and his
successor Luria into the interaction of speech-acts and manipulation
in the child's development of concepts, internalised as rules learned
by experience and expressed as discriminatory behavioural competences
are indicative of the kind of support necessary and available for
the development of conceptual analysis within psychology. Such a
view of the interaction between philosophy and psychology would,
I take it, assuage Piaget who in his article "From Developmental
Psychology to Epistemology" argues that the theory of knowledge should
be seen as the theory of the adaptation of thought to reality, and
that it can be distinguished from sterile metaphysics by an interest
in the developmental process, which opens up possibilities for
experimental investigation, and thus progress: Empirical psychological
research thus would allow for empirical psychological research to
promote advances in epistemological analysis, as well as vice versa.
Out of the interaction, it may be possible to extend and develop our
understanding of how we understand the world.

A LIMITED ROLE FOR PHILOSOPHY IN PSYCHOLOGY?

While many philosophers would in varying fashions endorse this view
of the central involvement of psychology and epistemology, psychologists
themselves, even when sympathetic to the implications of an intentional
model of action, tend to resist, not simply because they see such a development as a threat to their own procedures, but because they are unhappy about what appears to them to endanger the independent status of psychology. Robert Borger for example, who has a sensitive grasp of the sort of argument philosophers draw upon to establish a rule-governed or purposive model of human action, does not think that such analysis should have prescriptive power over the direction of psychological research. While wholly conceding the primitive nature of commonsense explanations of human action, in terms of intentions et cetera, he sees no reason in principle to abandon the search for mechanistic explanations of actions: that these have hitherto proved unsatisfactory reflects upon the inadequacy of the particular models, not upon the mode of explanation itself. Whether or not such explanations are possible, he says (along with Sutherland), is an empirical and not a conceptual question. Borger's concern is that psychology should remain an empirical science, rather than as an adjunct to the humanities, its fate if it locks itself inside a language rooted in our experience as agents; whereas a purely physical language, rooted in our experience as observers, and therefore consisting of concepts "symmetrical with respect to their users" makes scientific enquiry possible. He argues that both subjective and objective descriptions are fundamental, and while incommensurable need not be reserved for separate entities. Borger is opposed to philosophical analysis exercising a normative influence on psychology e.g. proscribing the continued search for mechanical explanation.
Again, Sigmund Koch, himself a noted critic of much of psychological theorising's failure to provide adequate explanations of human behaviour, in his "Epilog" to Volume 3 of the massive *Psychology: A Study of A Science* argues for more sensitive experimental techniques and theory construction in the light of analysis of logical weaknesses and inconsistencies in current practice, but at the same time issues a strong warning about philosophy in psychology:

"What psychology needs to know about its goals and stratagems is far too subtly embedded in the tortuous quiddities of enquiring action for the philosopher to be of appreciable help. It would of course be as callow to maintain that philosophical analysis is of no relevance at all as it would be to entrust philosophy with total responsibility for the mapping of our future. But the need for testing, culling, transforming, supplementing and adapting philosophical insights within a context utterly controlled by responsiveness to the indigenous is absolute. And indeed, it is probable that in the long run psychology will have more to contribute to central problems of epistemology, and others traditionally associated with philosophy, than vice versa."  

(pp.787-8)

In an article in 1974, Koch again stresses that the problems of psychology must be solved by its own practitioners, not by "visiting mandarins". Like Piaget he thinks that progress in psychology will come through the abandonment of the chimaera of synoptic breakthroughs, and the substitution of a variety of flexible, contextually relevant methods; and like Piaget his dislike of claims of philosophy to a significant role in the enterprise is based on philosophy's tendency to general revelation:
"The philosopher can be recognised by the fact that he talks about everything at once - while the scientist does his best to deal with only one thing at a time."

(Piaget, 1973, p. 94)

What follows from this attitude is a prescription for the role of philosophy in psychology which is most clearly articulated by Rozeboom of the Center for Advanced Study in Theoretical Psychology of the University of Alberta. He focusses upon logic rather than epistemology as the central discipline of philosophy, and envisages a role for philosophy as an adjunct and neutral discipline to be drawn upon by psychology to evaluate theory construction in much the same way as inferential statistical methodology is used to evaluate obtained data. On this view, philosophy can best be employed to lay bare the logical structure of a particular theory, thus revealing the impeccability or otherwise of the coherence and the deductive relations of its constitutive propositions, and demonstrating whether parsimony in postulates and logical machinery (in accordance with Occam's law) has been achieved.

"Once the raw theory is palpably before us, we need to pound, grind and wring it metatheoretically in order to squeeze out just what it actually accomplishes; to make clear the respects in which it is or is not merely a restatement of notions already familiar in previous work on this problem; to recognise what is tenable in it and what is gratuitous; to extract from it what, tenable or not, is conceptually solid and what is too vague and ambitious to do more than emote - to make, in short, a technically detailed appraisal of what the theory is and does, in as many respects relevant to the cognitive goals of psychology as metatheory has learned to receive."

(Royce, 1970, pp. 74-5)

What Rozeboom proposes is clearly a highly specialised role for philosophy, with status likely to be concomitant to its specialisation.
It differs from the previously outlined role for philosophy in emphasising the philosopher's skill at logical rather than conceptual analysis. Another marked difference arises from Rozeboom's insistence that metapsychology should be a methodology employed neutrally, "in the indicative, not the imperative, mood of speech". This means that the exercise of the philosopher's technical skills need not, indeed should not, result in recommendations for the revision of the theory under consideration. It is this insistence upon the neutrality of philosophy in psychology that critically marks this view out from the alternative, that philosophy can on the basis of a priori considerations have something to say about the way psychology should proceed. Such neutrality is necessary, Rozeboom thinks, because metascience simply doesn't know enough as yet to make useful recommendations for the conduct of research.

AN ATTEMPTED SYNTHESIS

1.22 This, the 'underlabourer' theory of philosophy, takes the attitude that philosophy offers technique but not insight. It is opposed by the thesis that philosophical elucidation of the rules governing certain vital concepts holds the key to a certain kind of knowledge. The fact that we have seen how such moves to validate philosophically obtained knowledge may be criticised does not mean that valid moves are not possible; nor does such possibility imply that empirical research is not also crucial to psychology. This is Winch's mistake, for which he is properly taken to task by Louch. By stressing the complementarity of empirical and conceptual investigation in the social sciences, we are able to meet the substantial
part of Borger's argument. Psychology remains within the sciences: but this does not mean that an objective mode of explanation alone is appropriate. A mode of explanation which remains objective can only be objective; i.e. about objects and as such unobjectionable. There can be no reason not to keep striving for better objective explanations about the physical world and how physical systems work. But this kind of answer does not satisfy questions about persons, which require the systemative empirical and conceptual probing of the world of persons.

Psychologists have objected to the stronger thesis (see 1.06 - 1.12) regarding the role of philosophy in psychology because of its implications for their own activity. If the centrality of philosophy in psychology is conceded, this is thought to pose the question of whether all psychologists should trade in their laboratories for book-lined studies, in order to pursue the explanation of behaviour. For those who wish to retain psychology as an empirical science rather than part of the humanities, such a conclusion must be avoided. And this is clearly proper. We can learn, and have learned, a great deal about the morphogenesis and structural archaeology of behaviour by meticulous observation and imaginative experimentation. The weaker reading of the role of philosophy in psychology (1.19-1.20) places empirical research clearly in the centre of psychology, and allows that philosophy can help improve the quality of individual pieces of research through analysis of structure. The most likely change that would result for research would be the scaling down of hypotheses and consequently of
investigative strategies, the proliferation of more modest, piecemeal bits of research aimed at the testing of highly specific and low-level theories. Nevertheless the inherent relationship between psychology and epistemology cannot be ignored; and for this reason the role of philosophy in psychology is in certain respects central and prescriptive. The ways in which human beings must structure their understanding of the world have direct implications for the sorts of explanation that psychology should be aiming to provide. We can investigate the nature of human thoughts both experimentally and by linguistic analysis. The resultant insights can be used to prescribe the sort of explanation that will be appropriate in some psychological studies. The interaction between epistemology and psychology must be a two-way process. Much of the work in philosophical psychology has turned on the exegesis of agency, often with reference to questions of moral responsibility. My attempt will be to approach the problem of what general prescriptions can be put upon the way psychology should progress by considering the concept of creativity. My strategy will be to examine what is implied in the ascription of creativity to a person, taking into account what empirical researches may have contributed to the enrichment and clarification of this notion; and then to consider what such an analysis entails as to how psychological research should be conducted, with regard to test construction, hypothesis construction; data assessment and the eventual arrival at adequate explanations.
CHAPTER 2

CREATIVITY AND

PSYCHOLOGICAL

RESEARCH
J.P. Guilford's presidential address to the American Psychological Society at Pennsylvania State College in 1950 is generally credited with heralding the onset of the "creativity boom" in psychology. In this address, Guilford announced the beginning of a research programme (significantly, supported financially by the Office of Naval Research) into the aptitudes of high level personnel: after reviewing the field of creativity and its problems, he described his plans for research. Guilford powerfully denounced the appalling neglect of creativity by psychology, and in his short but trenchant address neatly pinned down three features of creativity which were to shape the development of the research programmes into creativity that burgeoned thereafter.

(a) Firstly, he said, creativity is not at all the same thing as intelligence.

(b) Secondly, creativity has much closer links with insightful behaviour, and a comprehensive learning theory must have room for both insight and creativity.

(c) Thirdly, he put his finger squarely on what was to prove the major stumbling block for psychological research into creativity: the difficulty of choosing a practical criterion of creativity. The extreme rarity and inherent fluctuation in performance of acts of creativity which are "of unquestioned order of excellence" made it unlikely that even the most well constructed test of creativity would ever show consistently replicable results, and would therefore have low reliability. More crucially, the sort of objective tests analogous to those used in
intelligence testing, which could be readily and unambiguously scored and which would yield data capable of being freely manipulated statistically, would not do on grounds of validity: what would be most appropriate to the measurement of creativity in action would be open ended non-objective tests. In short, objective measures would be invalid: valid measures would be unreliable.

2.02 In a later article, Guilford suggests that the failure of psychology hitherto to study creativity may be due to the prevalence of the stimulus-response model, which, while a valuable conceptual tool in the development of psychological investigations, has severe limitations in the study of the higher thought processes, where it is necessary to draw inferences about events happening within the organism to augment the observations of stimulations and response. Guilford proposed an alternative approach to the study of the higher thought processes, based upon a theoretical background involving the conception of groupings of primary abilities which are related to each other in multiple second order factors defining wider ranging abilities of greater generality. Within this theoretical framework, Guilford and his team developed batteries of tests to tap and differentiate between the various abilities hypothesised as making up intelligence; they devised methods of factor analysis to sort out relationships amongst the data obtained so as to reveal traits, defined operationally as "relatively enduring ways in which persons differ from each other"; not just properties which individuals possess in common but to different degrees, but also ways in which individuals function. Trait theory deploys explanatory constructs which on any given occasion may (but, depending on circumstances, need not) result in certain characteristic behaviours. The
incidence of such characteristic behaviours serves as evidence for
the postulation of the trait under consideration. By the
development of factors analytic techniques, clusters of facts could
be identified as related causally or otherwise. In this way a
model of the relationships between the separate traits of intellect
identified could be constructed. Guilford's aim was to deploy this
theoretical approach and empirical methodology so as to isolate the
pattern of traits characteristic of the creative personality.
Thus he carried over to the investigation of creativity a theoretical
approach and a highly developed methodological apparatus which
assumed that creativity was explicable in terms of a theory of
intellect. He took it as axiomatic that

"creative abilities determine whether the individual
has the power to exhibit creative behaviour to a
noteworthy degree"

(Guilford, 1950, p.444)

The actual production of creative objects or ideas however depended
also on motivational and temperamental traits. The research
planned was designed to investigate the nature of such creative
abilities or traits: tools to isolate hypothesised factors (such
as word fluency, associational fluency, expressional fluency,
idea and fluency etc.) plus statistical manipulation to reveal
the patterning of these factors. While this methodology did not
entirely yield results confirming Guilford's theoretical jumping-off
point, nonetheless the outcomes arrived at must be viewed within
the context of the theoretical framework which shaped the method-
ological approach. Given the influence of Guilford in the field
of creativity research generally, and the prevalence in creativity
research programmes of tests either developed within Guilford's
laboratory or closely modelled on Guilford's, the aetiology of the kinds of tests that he developed must not be lost sight of.

2.03 The attraction of Guilford's proposed research programme to the Office of Naval Research lay in its objective of identifying the pattern of traits characteristic of the creative personality. The discovery of a formula for identifying persons with high creative potential, and hopefully the development of tests that would throw up these individuals was recognised to be of great practical significance, offering the possibility of a great leap forward in the identification and fostering of talent. Remember that the early 1950s were the time of the Cold War between West and East: the launching of the Soviet Sputniks had US politicians, military personnel and scientists tripping over each other to catch up and overtake. Creativity research promised results which would boost scientific progress, by making it easier to identify those most likely to come up with productive ideas and by indicating how highly creative individuals could be nurtured to maximum potential. The following is typical of this view:

"Since 1950, those willing to recognise the true situation have known that our nation [i.e. the USA] has been faced with manpower problems. Even so long ago, it could be seen that we lacked sheer quantity of available manpower in comparison with our potential international competition. Considering manpower needs in only the basic and applied sciences, it was apparent that we were becoming outnumbered in high-quality personnel in sciences and engineering. Another feature of this situation was that the scientists in the competing nations represented a large youth movement - a great many students and recent graduates and a comparatively young faculty. In other words, the long-range scientific effort of these other nations was strengthened by the long careers ahead of a large percentage of their scientists."
If we are to survive in international competition, our most promising solution to this vital manpower problem is for this nation to encourage and support the identification and development of various types of important personnel. One such type is the highly creative person, for even a few such persons in science can keep our scientific movement vigorously in front — recall the World War II race for the development of the atomic bomb. The creative scientist can also vastly improve upon current thinking and practices and materials.

Since creativity can underlie progress in any field of activity, the current awakening of interest and the recent burst of research on creativity promise new developments on many fronts. There is great potential for long-term gains from comparatively small investments of funds and manpower, centered on the problem of identifying and nurturing creative talent. In the future, our nation cannot depend on sheer quantity of manpower, but must strive to find high-quality personnel, especially creative persons, to deal with its vital problems. In fact, an approach utilizing sheer quantity of men and facilities can be unduly expensive, so much so that we would probably find on careful analysis that we cannot afford such wasteful and inefficient approaches. Since scientists are basic to scientific progress, there may be no potentially greater payoff per unit of expenditure than from continuing to investigate the nature of creative talent and creative performance, preferably more energetically than in the last few years."

(Taylor, 1964, pp.2-3)

2.04 I have quoted extensively from this because this passage handily encapsulates a number of points of relevance. The first is that the special circumstances prevailing in this period tended to lead psychologists studying creativity to take a particular interest in inventiveness and problem-solving, that is to the scientific rather than the artistic manifestations of creativity; and within the scientific field to stress the solving of problems rather than meet unfettered theoretical speculation. This bias was by no means to
the exclusion of the artistic side of things, and few researchers went so far as to identify creativity with problem-solving; but the tide clearly ran in this direction. Secondly, there was an analogous bias (for analogous reasons) toward the identification of the characteristics of persons of high creativity as the aim of creativity research rather than other facets which might be explored, such as how creativity occurs or indeed why it occurs. Thirdly, there was a clear incentive to produce practical tools for the identification of such persons: the funding authorities were looking for something as valuable and as easy to use as the intelligence tests of forty years earlier. All of these have had their effect on the way in which researchers into creativity have tended to go about the enterprise: and, because of this, on the sorts of results obtained.

2.05 To systematise what has been said so far:

(i) the social climate of the time led to an emphasis upon the study of scientific creativity, and in particular of creativity as problem-solving;

(ii) it led also to an emphasis on identification of individuals of high creative potential as primary goal;

(iii) Guilford's previous interests led him to view creativity as a mode or modes of intellectual functions (and given Guilford's seminal influence on research, this was pervasive);

(iv) a common aim was to produce a test or battery of tests which could be easily administered, scored and evaluated, which would unambiguously identify individuals with creative potential;
(v) However, Guilford had pointed out that there were severe practical difficulties in developing a simple objective test of creativity.

These somewhat conflicting considerations impart a general shape to subsequent psychological research into creativity: the shape has its effect on how research was constructed, and on what was discovered.

THE SELECTION OF SUBJECTS

2.06 As an example of the shaping effect of this general weltanschauung let us consider the ways in which researchers have set about collecting subjects for study. Empirical studies to date have adopted one of these approaches:

(a) the historiometric approach, according to which famous creative figures from the past whose creativity nowadays is unquestioned are selected, and writings by and/or about them are adduced as empirical evidence;

(b) the "eminent practitioners" approach, whereby the most eminent practitioners in various fields where creativity is felt to be relevant to success are selected by means of ratings by judges who are themselves eminent in the relevant field, and asked to submit to tests of various kinds; or

(c) the "paper test" approach, according to which populations (usually, for practical reasons, schoolchildren or students) perform various tests designed to measure divergent responses from normal responses, and highly divergent groups obtained by this procedure are analysed against various other factors.
The most celebrated example of the historiometric approach is by Catherine Cox. As part of Lewis Terman's vast study of giftedness, it was attempted via systematic coding of biographical material to estimate the IQ and other "personal and moral" characteristics of historical geniuses. An analysis was made of the differences between scientists and artists, and the results seem to suggest that such differences as there are reflect differences between the scientific and artistic frames of reference: the scientist characteristically is driven by a need to restore order by explaining discrepancies or replacing inadequate theories, whereas the artist has less commitment to orderliness and is chiefly characterised by the singularity and intensity of his vision. Other collections of biographical material are less systematic, and less analysed: Ghiselin's eclectic, anecdotal approach is typical. His selection of creative individuals (the majority are artists - in the broadest sense - not scientists) sets out descriptions with varying degrees of revelation and perception of how the process occurs to them. Ghiselin's preface draws together certain common threads that seem to him to run through the various accounts: that every creative act overpasses the established order in some way, in a way that is neither chaotic nor determinate. Hadamard, except that his work relates exclusively to mathematicians, takes a similar approach. Two other works which draw heavily on biographical material are Anthony Storr's *The Dynamics of Creation* and Arthur Koestler's *The Act of Creation*: in both, biographical material is deployed to substantiate the general theorising, so that the weight is on the theory, not on the evidence. We shall return to the theoretical approaches of each in
the next chapter, but at present we can dismiss both works from consideration as historiometric analyses: the selection of material is eclectic and makes no attempt to be either exhaustive or representative; the theoretical structure imposed is clearly the rationale for the inclusion of the material.

2.08 The difficulties of a historiometric approach are already becoming clear. For a start, its most obvious limiting factor is the relative lack of data about historical figures of undisputed creativity. The texts available to the psychologist interested in creativity are sparse: scope for significantly new work utilising this strategy is more or less non-existent. Again, such texts as are available are inherently 'unscientific': most were not written as clinical reports, are not always to be relied upon as regards veracity or insight, and in any case there is no reason to believe that what happens to be extant is representative. These two criticisms can be combined into the general one that the material upon which the researcher founds has not been obtained by him, and therefore he cannot claim it to have been scientifically obtained or selected. The further selectivity employed by the researcher as to how to choose and weight the materials available introduces an additional element of uncertainty. The sole advantage of this strategy is that it gives maximum weight to the considerations that the subjects under study should be unquestionably creative: those selected are those individuals regarding the merits of whose works there has been a general consensus through time. Not that this method by itself ensures that the subjects chosen are creative by
any incorrigible criterion: it simply founds on consensus. The advantage of a historical verdict of creativity however are out-weighed, as far as scientific research is concerned, by the disadvantages of this approach on almost every other basis: it can in no way be said to be controlled, it is not scientific, but at best illustrative and anecdotal.

"EMINENT PRACTITIONERS" APPROACH

2.09 The other two approaches accept the necessity of using living subjects, and of systematically drawing data from studying them. But how to select these subjects? The second strategy by far the most ambitious approach to the subject, and seeks to reconcile the demands that its subjects be unquestionably creative and the demands of scientific investigations. The selection of subjects is based, as squarely as may be, on their socially assessed creative output; the examination of subjects is conducted as far as possible in a stringently scientific way. Consider for example the pains to which Anne Roe went to select her subjects:

"I decided first to limit the study of biologists to men whose work was basic research into normal life processes. This meant that I would not include those who were studying pathology... It was easy enough to eliminate variation due to sex by studying only men... It was not necessary to set a lower age limit, since the criterion that the subjects should have obtained eminence would automatically do that... I set a top age limit of 61... In order to keep the cultural variables as limited as possible, only men born in the United States were to be included. It seemed desirable to omit men who had attained eminence in research earlier but were no longer actively engaged in research... The next step was to compile a list of the active research workers in the field as defined. This was simplified by beginning with those who had
already received some recognition of their work. Membership in the National Academy of Sciences is dependant upon election by peers, and indicates at the very least a high reputation for scientific work among men in a position to judge it. The same is true of the American Philosophical Society. It was a relatively easy matter to compile a list of men in these two societies whose work lay in the field of normal life processes, and then to eliminate those who were foreign born or over the age limit, or who were no longer doing research ... To this list were added the names of other men (chiefly younger) who had been singled out by invitations to important conferences, but who had not been elected to either of these societies ... In order to select the most eminent men from the list thus obtained and to add others who might have been omitted, I needed the help of men, themselves in the field, who would be in a position to make the necessary evaluations which I could not do ... The judges rated each man on the list whose work they knew on the 3-point scale. They also added the names of others who they felt should be included. This sort of rating is not an easy job. For example, if one is rating a man on the excellence of his contribution to science, how does one balance a man who has made one or a very few major contributions against a man who has produced many not quite so major ones?

When each judge's list had been returned, I ranked the men on the basis of the combined ratings. Here, too, there were some tricky problems. Not every man received the same number of ratings, since a rater whose field was biochemistry naturally did not know much about the work of men in vertebrate zoology for example. It seemed wise to exclude men whose work was known to only one rater (since it probably did not have as broad value as the work of others) and men who received the lowest rating from any rater, although there is one exception to the latter principle. (It seemed clear from his comments that one judge was personally biased in respect of a man who was given top rating by several others.) It was necessary, also, to see that the final list included men from all branches, but as it happened no further adjustment was necessary for this.

After all of these ratings and rankings I had a list of 23 men."

(Roe, 1952, pp.21-26)
By the time a few of the potential subjects had declined to take part, she had 20 biologist subjects. By analogous means, she selected 22 physical scientists, and 22 psychologists and anthropologists. Given this exhaustive selection process, what could she confidently claim about her subjects?

2.10

(a) She could be relatively confident that each group was homogeneous internally and with each other as regards such gross characteristics as age, sex, cultural background and current research status. This meant that these factors would not be unduly distracting in the analysis of the data.

(b) She could be fairly sure that her subjects covered the range of the sub-disciplines within the sciences under study; the representativeness of her groupings, however, should not be overstated, in view of the smallness of the numbers involved.

(c) She could have some confidence that the subjects selected for study resembled the majority of scientists in their fields, because of the subsidiary study undertaken, in which the maximum number of university faculty members took, in batches, the Rorschach test as a group test. The results indicated that differences between disciplines found between the most eminent mirror very closely the differences found in the disciplinary populations as a whole. This conclusion must be regarded as tentative, as it depends on the group Rorschach test and the individual administration of it as being comparable, and as only the one test was cross-validated.
(d) She could conclude that the subjects selected were regarded by their peers as eminent in their field: but she could not elide this into the conclusion that the subjects selected were the most creative in their field. By leaning so heavily firstly upon objective indicators of election to the associations appropriate to the discipline, and secondly upon subjective rating by "experts", what is obtained is a list of those enjoying contemporary success: and it may be argued that those whose work is most congenial to their peers need not be, and indeed might often not be, the most creative. Throughout history there is a typical kind of creative individual who has for much of his life been unpopular, perhaps because his work is too disturbing to the majority of his peers: the sort of creator that in retrospect we say was ahead of his time. This kind of person would not be picked up by this kind of methodology, and to that extent, at the very least the selection process cannot be said to be exhaustive. Eminent judges may play favourites in a more personal sense as well: Roe explicitly refers above to one example of this that she was able to pick up, but it is reasonable to assume that more moderate and more subtle manifestations would be less identifiable.

2.11 I have concentrated on Anne Roe's selection methodology, but examination of the strategies adopted by Frank Barron, for example, or by David McKinnon would turn up much the same pattern, and much the same conclusions could be drawn. Barron, even more than Roe, sets out to check that his subjects and those who chose not to participate were not significantly different: the ratings
(independently, by senior experts in the field) of those who accepted and those who declined were checked and found to be virtually identical; and to check this further an independent panel of experts were asked to rerank all those invited, and again differences between accepters and decliners were found to be non-significant. None of this however touches the central problem of to what extent the most eminent are the most creative, as Barron himself acknowledges.

"PAPER TEST" APPROACH

2.12 Nonetheless, it seems reasonable to assume that the most eminent full time practitioners chosen by this sort of method are creative, even if the question of whether they are typical of all creative people has to be set aside for the moment. There is therefore at least some reason to suppose that they are the right sort of subjects. No such confidence can be attached to the selection of subjects via the third (paper test) method. This approach is far and away the most popular with researchers, partly because of the dominant considerations identified earlier in this chapter, but presumably partly as well because of practical considerations: the "eminent practitioners" approach is inevitably costly in terms of time and therefore of money, and there are a limit to the number of eminent practitioners who can be studied. Setting aside practical considerations, however, this strategy, being free from reliance upon rating and raters, looks objective. It starts from the assumption that certain tests can validly discriminate between more and less creative individuals. If this is so such tests can therefore be
administered to any group or population, and on the basis of scores obtained a group of highly creative subjects can be isolated. The validity of any conclusions based upon responses of this group however turns upon the prior validity of the test of "creativity" previously administered.

Upon examination, claims that such tests can serve as measures of creativity are found to rely on a highly suspect circular justification. (That is, when they are explicitly validated at all.) Guilford, having described his theoretical assumptions about what factors are relevant to creative activity, and how the various tests for these factors were constructed, and how the data obtained have been manipulated to reveal interconnections, simply has this to say about the crucial question of validation:

"The question naturally arises, how do we know that the abilities measured by the tests are related to creative performance in everyday life. There is not time to present all the evidence, but there is increasing information to the effect that other, independent assessment of some of the same abilities and some performances in practice are correlated significantly and even substantially with scores from some of the tests. Some of the qualities assessed have been the creative performances of students of arts and of sciences, of military officers, and of engineers."

(Parnes and Harding, 1962, pp.159-60)

On the whole this seems a rather faint confirmation regarding the crucial question that can be asked about this methodological approach; and it is so loaded with qualifying phrases and so lacking in specific reference that nothing can be concluded from it. In fact, as far as can be ascertained, very little indeed in the way of validation
has been done for the kind of tests that are used as "tests of creativity". Barron (1955) and Drevdahl (1956), both working with populations not selected for creativity, demonstrated positive correlations between scores from tests of originality with ratings of degree of originality by observers. Sprecher (1957) also found positive correlations between tests of originality with ratings of inventiveness and with performance on original technical problems amongst graduate engineers. There are a few other studies which give limited support to the claim that individual "tests of creativity" correlate to a certain extent with some other indicators of creativity, but in general the literature is very sparse and none of the studies turns up results strong enough to serve as convincing evidence of the test validity of the particular test used, let alone to justify the broad acceptance of such "tests of creativity" as validated. Even if we accept the modest level of correlation demonstrated, say by Barron, as indicating a real connection, it cannot be assumed that this means that the test of originality used is a valid selector of original thinkers. If most creative subjects selected by ratings were to score highly on tests of fluency, it could not thereby be concluded that most of those who score highly on tests of fluency are creative. The case for the validation of these tests of originality, fluency etc. as revelatory of creativity must be considered to be not proven. Consequently research results based upon this strategy should be viewed with the greatest suspicion, as being scientifically sound but conceptually inadequate.
It seems to me curious that there should be so little in the way of validation of these tests, which are used without hesitation by researchers throughout the world. Taylor's comments on the subject, in which he seems to imply simultaneously that a lot of work on validation is urgently necessary and that as the personal traits that have turned up in creativity research show a surprising degree of consistency, there is little to worry about, are somewhat incoherent. Face validity of some of the tests in question, as tests of verbal fluency etcetera, should not normally lead psychologists to accept them as general tests of general creativity without further question; and the limited degree of correlation between some of the "creativity tests" gives little ground for belief that a single talent is being tapped by the various tests. And yet by and large the tests have been accepted as valid, and used, with little reflectiveness, to discriminate between persons with more or less creative potential.

We shall return in a moment or so to the general question of validation in the context of psychological research in creativity: at present, however, let us complete the consideration of the implications of the "paper test" approach to creativity research. As I said earlier, this approach has both a practical and a "psychological" advantage to researchers. Its "psychological" value is that in structure it is closely analogous to the sort of approach very familiar in psychological research: it feels scientific, it feels as if it is objective, it feels as if it is under control, it feels as if it can be quantified and will yield
hard data. All of these characteristics typify mainstream psychological practice as, in the eyes of its practitioners, it ought to be conducted. To conduct psychological research in any other way is generally felt to be less satisfactory as it does not yield up the same degree of confidence in the results obtained. We shall explore in Chapter 4 the question of objectivity and research into creativity; in Chapter 5 the question of control; in Chapter 6 the problem of quantification; and finally in Chapter 7 the underlying considerations about psychology as a science. The "paper test" approach by psychology researchers into creativity is significant for our purposes because it exemplifies the attempt to be seen to be scientific in methodology irrespective of the demands of the subject matter.

CRUCIAL QUESTIONS ABOUT CREATIVITY

2.16 We have looked cursorily at the background against which the interest of psychologists in creativity developed: an external background of technological competition and push towards greater inventiveness, and an internal background of an established tradition of identifying intellectual ability by performance tests which could be objectively scored and compared. Both external and internal considerations shaped the way in which psychological research about creativity developed. What is de-emphasised is as interesting as what is stressed. Take, for example, the preoccupations of the three University of Utah conferences on "The Identification of Creative Scientific Talent": the title chosen for the conference is itself expressive of the practical rather than academic bias dominant in
creativity research already noted. Taylor and Barron, in their preface, review the achievements of the conferences, attended by highly distinguished casts from the academic world and from government and industry on an invitation basis. Overall, a total of 37 participants presented 52 reports on various subareas of creativity; and in addition nine subgroup reports were presented. We should have here if anywhere, therefore, comprehensive coverage of the crucial questions relating to the identification of scientific creative talent. Taylor and Barron, indeed, categorise the papers included in the collection under four broad headings, which (with subdivisions as necessary) correspond to the crucial questions.

I The criterion: under this heading it is possible to separate out questions about the criteria of creativity (i.e. the grounds on which we should be entitled to describe someone as creative) from questions (much more dominant in the actual papers included in this section) about predictors of creativity.

II Intellectual, personality and motivational characteristics: this general heading provides a context for speculation and research about what kind of person is creative.

III Environmental conditions and specific situational determinants: unlike the previous category, which concentrates on the internal dynamics of creativity, this one pulls together questions about the external dynamics of creativity.

IV Theoretical analyses of process: questions about the creative process have tended not to be of primary interest to psychologists, and theoretical consideration has been left for the most part to outsiders.

These categories are interesting for what they emphasise and de-emphasise in what they include; how they are ordered, and what they omit.
THE CRITERION QUESTION

2.17 It is clearly logically appropriate to start with questions about
the criteria on the basis of which the term "creative" is
ascribed to a person. We shall return in a more systematic way
in the next chapter to the possible criteria of the appropriate
use of the term "creative", and the implications of the family
of criteria which govern the use of the term: at present, it is
sufficient to recognise that there clearly is a "criterion problem"
about creativity. We have already implicitly recognised this in
the preceding discussion about the selection of subjects: there
is nothing clearcut about the ascription of creativity. One is
on safest ground when referring to individuals upon whom the
verdict of history is that they were creative, that is, when there
has been over a considerable period of time a general consensus in
favour of that opinion. In the history of the sciences and the
arts there are numerous examples of individuals hailed in their
lifetimes as creative geniuses, but nowadays with hindsight,
maturer judgement or transformed taste, considered only as
curiosities, if not altogether forgotten. On the other hand, one
can equally call to mind savants and artists treated in their
lifetimes as idiot or evil or worthless (or a combination thereof,
or all three together) whom we should now consider to be highly
creative. It is exceedingly difficult to avoid the conclusion
that in ascribing creativity to any person we cannot be certain,
we are relying on evidence rather than proof. The "criterion
problem" therefore in practice turns on evidence rather than proof:
but even on those terms there is much that is difficult for a
jobbing researcher if (to anticipate a little the argumentation of
the next chapter) we accept that someone who has produced a number of highly original ideas, which have had profound implications for the discipline in which he is working, and who has had a degree of critical acclaim within his discipline has at least an a priori claim to be considered creative (whatever view history may take), there is nonetheless the problem that it is all too likely that the ascription can be made only comparatively late in his working life, once his reputation has been consolidated. In Anne Roe's study, for example, although her youngest subjects were 31 (physicist), 35 (social scientist) and 38 (biologist) respectively, the general trend was such as to pull the average age up to 45, 51 and 48 respectively. Other researchers using the "eminent practitioners" approach similarly find themselves of necessity working with mature, established subjects — it is in the nature of the approach. This raises two difficulties, one theoretical, one practical. Firstly, how to avoid the possibility that subjects who have already completed a large part, if not almost all, of their creative work are not different from the young turks that the "eminent practitioners" approach cannot identify? It is not hard to hypothesise, for example, that the prestige and comparative affluence that go along with eminence and are a just reward for a lifetime of creative endeavour will make such subjects somewhat different from the unrecognised tyros in the same disciplines. I refer to this as a theoretical difficulty because, unless it can be faced and dealt with satisfactorily, it throws doubt upon the adequacy of the results obtained, and thus on the adequacy of the
theory propounded. Secondly, there is a practical difficulty in terms of the overriding objective already noted, to identify potentially useful creative people as early as possible. Clearly, psychological research will not be of much practical assistance in the fulfilment of this objective if it can do no more than provide information about those individuals who have already forced their way to the top of the tree.

THE INDICATOR QUESTION

2.18 The first problem has been often recognised in research, but there is little indication that much has come of this. We shall return to the subject in the next paragraph. As far as the practical problem is concerned, however, a good deal of work has been done in an attempt to establish strong predictive indicators which could, at a relatively early stage, mark out an individual as a potential high-flier, creatively speaking. The rigour with which this has been pursued is matched only by the aridity of the results. To be brief, there are no objective factors or measures which are available at an early stage of an individual's career which have any significant relationship to creativity in later life. The only two possible contenders are number of patents (in disciplines where this is appropriate) and number of publications, and even here the possible ranges involved (for example, between a man who, wishing to check his results scrupulously and/or from diffidence, publishes only one seminal work and another who knows the way up the ladder of material attainment rather better and who therefore publishes widely, often in collaboration, possibly
working his way systematically through variations, of indifferent
importance, on a mundane theory) makes these indicators of little
practical value as predictors of creativity. Nonetheless, the
pace in the search for reliable predictors does not seem to
slacken in the face of so much defeat, possibly because the prizes
involved are so large. The first section of Taylor and Barron's
book is a monument to this continuing search for the will o' the
wisp.

2.19 So here we see an example of what counts as a crucial question about
creativity in the eyes of psychologists: how can we isolate
factors which will predict, with a sufficiently high chance of
success, which individuals will turn out to be notably creative?
At the same time, we see a significant lack of interest in another
question: how can we clarify in our minds what is involved in
ascribing creativity to a person, and can we arrive at a more
systematic way of making such judgements? The latter question
has logical priority, but has nonetheless been nibbled at rather
than masticated by psychologists. It is revealing that, in the
section described as being on the criterion, only one (and perhaps
a half) out of seven papers in fact deal with criteria: the rest
are concerned rather with predictors. The substitution of one
question for another has two obvious implications: firstly the
question which is perceived as the more important is the one which
has the most practical pay-off; secondly, the question which is
perceived as less important is in fact conceptually foundational to
psychological research into creativity, and, in the absence of
attempted answers, researchers must simply assume that they know how to identify creative people, and take it from there.

THE VALIDITY QUESTION

2.20 This brings us back to the question of validity. This is a question which does not figure in the extended corpus of Taylor and Barron's compilation; although there are references in various of the papers included to the validation of measures and tests of "creativity", there is no systematic consideration of the question, and Taylor and Barron's introduction, which describes how the successive conferences went about their work, makes no mention of the establishment of a subgroup to look into this. Again, what we find is that assumptions about validity go unchallenged (and sometimes unrecognised); they are common property, but no-one's responsibility to check out. I have already suggested that this can be connected with the pressure to come up with a practical test for creativity: attention has tended to be focussed on the desired end product rather than on the checking of its validity. But, given that psychology claims to use scientific methodology, this is very far from being an explanation of what appears to be a collective uninterest in whether the right thing is being measured. For at least a partial explanation, it seems to me that we must look to the following factors:

(a) Although it is treated rather disparagingly in most standard texts about methodology in psychology, face validity is in fact a very powerful influence when it comes to the choice of measures: researchers often have comparatively little time to prepare their programmes, and will tend to go for a test (whether constructed by them or picked off the shelf) that feels right. Provided the results obtained contain no obvious nonsenses, any subsequent validation undertaken may be cursory;
(b) Another practical consideration which may contribute to an explanation of why current research in this field pays so little heed to validation is that researchers (as has been said, busy people) tend to go for standard measures or variants of them. This should not be seen necessarily as conformity so much as conditioned by shortage of time, keenness to be seen to be working in the mainstream of the subject, and the recognition that scientific researches must be co-operative in character. In utilising variants of the more common tests of "creativity", therefore, the researcher is behaving quite reasonably, and may well assume that the tests selected have already been validated, since they are so prevalent;

(c) A further contributory factor, which adds to the face validity of the measures, is that the basic kind of test of originality as produced by Guilford or Torrance is sufficiently akin to the tests of intelligence that psychologists are familiar with for them to feel comfortable. What the tests share is a quantitative approach (how many responses can be collected in the time allowed), an emphasis upon a very "test-like" situation, and data which could be manipulated in the customary ways. We can see therefore a link to the dominant tradition in psychology mentioned earlier: the lack of pressure for validation has at least in part derived from the assumed appropriateness deriving from the fact that the measures used do not jar with this dominant tradition.

Where does all of this take us? To the provisional conclusions that the questions seen as crucial by psychologists studying creativity are not always those that a more dispassionate analysis might expect; and that consequently the work done by psychologists in this field may well be, at the very least, shaped in its orientation and perhaps influenced in its methodology by the dominant ethics of creativity research.
QUESTIONS ABOUT INTERNAL CHARACTERISTICS

2.21 This is the second main category into which Taylor and Barron arrange the Utah Conference papers: it is not, I think, chance that the quality (in terms of originality and insight as well as calibre of the research reported) of the papers in this section is appreciably higher than in other parts of the book. Rather, I would suggest that this reflects the primacy within psychology as a whole, and within creativity research in particular, of the study of intellect, personality and motivation: in this field, we are with the real professionals. In general, questions about what sort of person is creative are considered to be of great importance, and have several sorts of implication: the answers resulting from the studies shed light on questions about process, questions about how creativity could be stimulated, and of course on how creative persons can be identified. And yet of course, in order to have any assurance about one's answers, one needs to be satisfied that the creative persons whose intellectual/personality/motivational characteristics are under study are indeed representative and bona fide creative persons, that is, the adequacy of the selection process used determines the extent to which the research may be valuable. In spite of a certain unevenness in this respect, researchers are perhaps nearer to turning up a coherent set of answers to questions about the internal characteristics related to creativity than to any of the other question-areas: there emerges from the literature a picture of the creative individual as someone who is intelligent, but in a particular kind of way - a questing, darting mind rather than necessarily a powerful one; who has a high
need to succeed on his own terms and to work, sometimes obsessionally hard. More important than level of intelligence (although a minimum of intelligence is required, beyond this point there appears to be no relationship between creativity and intelligence) are personality features: a certain consensus has emerged, and when differences occur these are traceable back to the bypassing effect of different selection criteria. Typically, the creative individual has been found to be independent-minded, highly curious; humorous; able to tolerate chaos; open to new ideas and experiences: psychologically healthy and yet more neurotic (the latter characteristic held in check by a strong sense of self and well-developed control mechanisms); with a well-thought out personal philosophy. Both Roe and McClelland report (in researching physicists) a significant relationship between creativity and avoidance of personal relationship, but other researchers working with groups selected on different criteria report that creative people are socially sensitive. This is a case in point where differing criteria result in differing methodological strategy and thus directly in apparently incompatible outcomes. Roe's subjects are mature scientists, selected by the ratings method, i.e. the operational criteria of creativity is taken to be adjudged eminence in the eyes of one's peers; Hudson's subjects are school boys selected by the paper tests method, i.e. the operational criterion is taken to be performance in these tests. Thus stated, there seems to be no reason to assume that the two groups should exhibit throughout the same characteristics: but the change from decision about criterion to methodology to outcome has to be spelled out before
this is appreciated. The atypicality of physicists (separately noted by Roe) may also be a factor. A similar discrepancy can be similarly explained: Roe and Storr suggest that the creative person is likely to have had to overcome misfortunes in youth, whereas Wallach and Kogan suggest that a home attitude of warm openness and undemanding acceptance is most likely to produce a highly creative child.

2.22 All of these makes for a fairly coherent and persuasive picture of the creative person. However, it should not be assumed that the characteristics highlighted make up a sufficient as well as a necessary specification for creativity. This is the drawback to the attempt to derive directly from the picture conclusions about the process of creativity, the fostering of creativity or indicators of creativity.

QUESTIONS ABOUT EXTERNAL CHARACTERISTICS

2.23 It is of course always artificial in psychological research to separate out the effects of the environment from internal factors related to the characteristic under study: the inner and outer lines not only interact throughout, but are incoherent without the other. Nevertheless, it is a convenient device to consider individually the internal characteristics and the external events shaping a piece of behaviour: and there is nothing unusual in the way in which research into the situational and environmental aspects of creativity has often been pursued separately. What is more challengeable, however, is the emphasis within this aspect of research
on the relationship of creativity with the institution. This is clearly one area of academic interest; and the practical considerations which are a powerful influencing factor (in terms of ambiance and sometimes of funding) probably have a good deal to do with the noticeable concentration on how organisation man can be creative man, and vice versa. These are two separate questions: on the one hand, there is considerable advantage in finding out what kind of organisational atmosphere is most conducive to creative people, to enable them to give of their best within a structure, whether academic, industrial or whatever; a more cynical variant of this, perhaps, is the attempt to find out what kind of organisational atmosphere is least oppressive to the creative spirit, given the marked anti-authority cast of mind frequently revealed in the typical personality profile of someone creative. However, given that it is extremely difficult to effect more than marginal changes in the character of an organisation, there is a clear limit to the practical consequences of such study. Patterns of work and the workplace itself can be slightly modified to make working within an organisation less obvious and more acceptable to the creative individual, but there is no scope for real change. The other question, of how to make the rank and file more creative, however, is much more promising, from the point of view of practical consequences, and a good deal of work has been done to develop both theories of educational style and specific training programmes. Now, this approach exemplifies what psychology can do best: a distillation of current academic studies into practical programmes of relevance in the world at large. However,
those programmes which have emerged to date must, regretfully, be
treated with a good deal of caution, because the research findings
upon which they are built are themselves constructed on rather
poorly validated pre-suppositions. Nevertheless, the practical
application of research findings to the construction of educational
programmes is a crucial area for psychological research; but by
its very nature, this application can only be as valid as the
weakest link in the chain leading to it.

THEORIES OF PROCESS

2.24 Of the three ways of approaching creativity (discussed more fully
in the next chapter) --- product, person and process --- psychologists,
who are after all chiefly interested in individual behaviour, have
concentrated overwhelmingly on the person. Theoretical consider-
ation of the creative process has nearly always been left to
outsiders, often "amateurs" in the field of creativity research, or
occasionally eminent creativity researchers have indulged themselves
with a spot of introspection on the side. The general pattern has
been of theorising without benefit of much in the way of empirical
underpinning. Because of this, much of what is written is
interesting, stimulating but marginal. In the final chapter, I
shall look in more detail at the interaction of theory and empirical
research, but (to anticipate a little) the reluctance of psychologists
to engage themselves in the critical testing of models and their
preference for the testing of single hypothesis shows itself clearly
in the relative lack of interest in process of creation.
There is no consistent line in the answers proposed to the question of creative process, as there is, by and large, to the question of personality characteristics associated with creativity. The kind of model suggested tends to reflect the background and interests of the author: thus, for example, Storr adopts a model located firmly within the psychodynamic field in which he is primarily interested; Fromm and Rogers propound theoretical constructs reflecting their rather different view of self development. Nonetheless, though the models vary, a faint similarity of pattern can be detected. Such tentative answers as there are available tend to confirm the centrality to creativity of strength of sense of self on the part of the creative individual and of an appreciative and critical environment around him. It is interesting that such conclusions emerge not only from writers like Koestler and Storr who are extremely sympathetic to person-centred psychology but equally from the writings of, for example, Torrance, who starts off from a far more orthodox approach to psychological research. While he is firmly within the "paper test" tradition, Torrance's model recognises the crucial importance of subjective elements of self regard and social evaluation as the key factors in eliciting high level divergent performance, even though he eschews dependence on such factors as criteria for selecting his subjects. From the non-psychologists, such as Ghiselin, there is also stress on the active role of the person as being someone who is creative, rather than someone the process happens to. Finally, throughout the various models suggested, the concept of the accepted corpus of knowledge in any subject area being simultaneously the source of and re-organised by a work of creation is recurrent, at least in the writings
of non- or "amateur" psychologists. I suspect that the popularity of the "paper test" approach to creativity, which, by reliance on tests of originality etcetera, manages to avoid the need for a product to be identified as creative other than test performance, has led to a dearth of interest in the creative process: where there is no identified creative process to be investigated, questions of how and why do not easily arise, and consequently there is a "spare-time" atmosphere present in some at least of the writings available on process. And yet, the question of how creativity happens is far more of a natural partner and preliminary to the question of how creativity can be fostered than questions of situation and environment.

CONCLUSIONS

2.26 The questions that psychologists tend to concentrate on in creativity research turn out often to be those which either chime in well with questions with which they are familiar in other branches of psychology, or can be answered by means of methodologies with which psychologists are at ease, or appear to have direct relevance to the practical considerations which led to interest in creativity research in the first place. These are all good reasons for choosing questions to answer: however, the effect overall has been to limit psychological research into creativity to research structures which may not be wholly appropriate, or at least solely appropriate, to the enterprise in hand. There has been a disappointing emphasis on techniques and theoretical models taken over from intelligence and personality research; these latter fields clearly have something in common with
creativity research, but it is regrettable that, in spite of Guilford's proclamation of a field of study unlike intelligence, there has been relatively little in the way of new methodological approaches to creativity, but rather judicious or indiscriminate borrowings and remodelling from existent fields. What has been signally lacking, in fact, have been radical theory-led studies of creativity: a procedure from analysis of what might be involved to criteria for success to methodology development to review of findings. Too often the theoretical component seems to be, whether deliberately or as it turns out, the final rather than the first step in the process. And yet, without preliminary analysis of theoretical considerations, the methodology selected may be less than optimum or may even result in unsatisfactory data, thus vitiating to a greater or lesser extent the merit of the total research.

2.27 In an attempt both to evaluate more closely the psychological research into creativity and to suggest new directions for such research, the next chapter will look at what is involved in being creative: it is hoped that, upon this foundation, implications for the most appropriate methodology to be adopted for research into creativity can be drawn out.
CHAPTER 3

AN ANALYTICAL APPROACH TO CREATIVITY
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INTRODUCTION

3.01 Creativity names a quality which a person A may describe another person B as having, on the basis of some performance by B. In this chapter, I shall consider in some detail the criteria upon which the ascription takes place, by analysing the contexts in which the term is used and makes sense and is not used and does not make sense; drawing at all stages upon empirical studies of creativity and where relevant indicating discrepancies. In summary, I shall draw attention in turn to the need for some definite performance which acts as evidence for the ascription; for that performance to be an original effort on the part of the creator; for the performance to be by A in the sense that it is an intentional action and not a random or a rote occurrence; and for the presence of at least another person B to provide social context for the ascription.

I shall conclude by proposing that these criteria are indispensable to the adequate psychological investigation of creativity. Where these criteria have not been sufficiently taken into account, the value of research findings is vitiated. This will in turn lead us into prescriptions as to the proper conduct of empirical research into creativity; and will also point the way forward to a more general consideration of psychological methodology.
There are two kinds of questions that we can ask about creativity or any other human activity: questions within the domain of facts, and questions within the domain of meanings. Investigations of activities in which persons engage require both observation and manipulation of the activity, and elucidation of the criteria to which the activity must conform. Before empirical research in the human sciences can get started, there is a need for prior consideration of what the concepts involved mean; and in particular (because the empirical researcher is not interested in analysis for analysis' sake) of what criteria bound the relevant concept or concepts. The recasting of the web of meaning in which concepts are held to criteria for identifying the related activity is the work of the philosopher; the use of these criteria as operational markers for empirical researchers is the work of the social scientist. Empirical research undertaken with inadequate regard for the exegesis of relevant criteria is likely to be vitiated by this lack of regard. Yet, why should the preliminary identification of criteria be necessary when a scientist is dealing with ordinary terminology? It may be objected that, whereas an anthropologist who has an insufficient understanding of a culture may need to work out the criteria to be employed in delimitation before he can draw empirical conclusions about a native activity, it is unnecessary to go in for esoteric conceptual analysis of something like creativity, which everyone understands. On this view, the psychologist who sets up an experimental situation designed for finding out more about creative people knows without ratiocination what he means by creativity. The difficulty about this is that it is clear, both
from the theoretical writings of psychologists and from reports of experiments that there are considerable differences between what A means by creativity, and what B and C mean by it. For the most part, such differences must be inferred: they are rarely stated clearly (with some notable exceptions) because psychologists tend to assume that their intuitive conception of an attribute is shared by everyone else, and are not aware of these differences themselves. This is a dangerous situation because research outcomes based on unsignposted differences in conception may well be taken as confirming each other, or disconfirming each other, when, more accurately, they bypass each other. In practice, psychologists do not share a common intuition as to what creativity consists of: nor is it apparent within the literature that the practical effects of the resultant discrepancies are generally appreciated.

PRODUCT CRITERION

3.03 A researcher who is interested in creativity may be primarily interested in the product, the process or the person: that is, in the material which is created, whether this is a physical object such as a painting, quasi-conceptual material such as a piece of music or a poem, or wholly conceptual material such as a theory; in the historical sequences which led up to the production of the created item; or in the characteristics, whether biographical, intellectual or to do with personality of the person who created the product. All of these are subjects for study. It is most
natural to relate the word "creative" most directly to a person: to refer to either the process or to the product as "creative" is something of a strain upon the language. Nonetheless, upon consideration we see that the important thing about ascribing creativity to a person is that there is a product. The base upon which the concept of creativity rests is the created product: the process and the person can be identified only in terms of the product. Research into either the kind of person who is creative, or how the creative person brings a product into existence are or should be dependent upon the prior identification of subject matter in terms of what has been produced.

3.04 What is created must in some way be distinguishable from the creator. This is important because it means that the product can be appreciated and evaluated by someone other than the creator. I cannot be said to have created the definitive novel of the twentieth century if I have simply worked it out in my head: it would be over credulous of you to hail me as a great creative genius simply on my say-so, without more tangible evidence which you could yourself evaluate. If on the other hand I have written the opus out completely then it potentially has a life of its own separate from me, and is potentially accessible to someone else. If I have created something then from my point of view I may make some claim about my creativity in this case; but no-one else is justified in endorsing the claim without some existent of some kind to which the other is able to make reference. Creative ideas can be identified only in retrospect, once they have resulted in created entities.
Without the possibility of independent existence there can be no actual ascription of creativity.

3.05 This brings us to the question of the ascription of creativity to a person on the basis of what can only tenuously, if at all, be called a product. It has been suggested by certain psychologists, whose work in creativity has developed out of an interest in psychotherapy, that the development of a personally satisfying mode of life, or of a life rich in meaningful relationships with other people, can be as creative as the production of objects.

There has been at the same time a general burgeoning of the use of the term "creative" particularly in the United States as a general hooray-word to connote "emotionally satisfying" or "personally fulfilling". In these contexts, the concept of creativity does not turn upon a product as evidence of the suitability of the ascription but rather upon a wholly subjective introspected feeling of satisfaction. It seems to me however that such use arises out of a less than clearly thought-out pressure into service of vocabulary to express approval of a non-specific kind. Such usages are at best to be regarded as secondary to and parasitic upon the narrower employment of the term. Unless the ascription of creativity can be tied to the production of something objective, the further criteria limiting the use of the word have nothing to bite upon.

The difficulty with discarding the criterion of the existence of some objective product as is implied in the ascription of creativity as a general endorsement term of subjective satisfaction is that none
of the further criteria relating to the evaluation of that objective product can then come into operation: the only criterion that can function is the one of introspective satisfaction. The case of the ascription of creativity upon the basis of lifestyle is more borderline: there would presumably be something in the way of evidence to allow others to conclude that A was prima facie leading a rewarding life - a visibly varied range of activities and associates, perhaps proclamations by others of A's successful personal relations with them. All of these could serve as indicators for the public ascription of creativity to A on the basis of how he lives rather than upon what he has produced. Nevertheless, such indicators have a secondary status in these circumstances, as the primary criterion is A's personal satisfaction. Indicators of varied lifestyle may easily co-exist with extreme dissatisfaction, and in that case one could not say that A was fulfilled by his living experiences. The crucial characteristics in these circumstances therefore is also the sense of personal satisfaction, which takes us back to the objection made above.

3.06 The best developed argument in favour of "personal satisfaction" being a criterion of creativity is by Carl Rogers. His argument is essentially a two-pronged one, founding partly upon the inadequacy of historical assessment of value as a criterion of creativity, and thus the inadequacy of any criterion which turns upon the social assessment of the value of the product; and partly upon his conviction that creativity arises out of the tendency of man to self-actualisation. He therefore assumes that the criteria for
ascription must reflect this aetiology. On his view the creative act is correctly characterised as such when it brings out the essence of the real "I": this criterion is finally open to subjective evaluation by the individual himself; although Rogers does set up certain indicators by which an observer might judge whether an individual is "complete", at best these can be used only to identify potentially constructive creativity, and not in any way as crucial criteria. Such indicators can be present when satisfaction is not (e.g. Kafka or Swift) and can be absent when satisfaction is present (e.g. Caligula). In neither case should we naturally wish to agree to the ascription or lack of it simply and purely upon the basis of the individual's feelings. Rogers arrives at this position because he rejects the position that a socially derived criterion can be efficacious. Out of this rejection arises the proposal that a primary criterion of creativity should be personal satisfaction, and that this can be applied only by the individual himself on the basis of introspection. The lack of necessity for an objective product can be obtained only at the cost of saying that only the individual can evaluate his own creativity: the claim to be engaging in creative activity then becomes unchallengeable, which in the ordinary way it would not be considered so. At best, the ascription of creativity on the basis of other than a product is parasitic on the central condition of such ascription, and functions by analogy with such central ascription.
Most obviously, when we ascribe creativity to a person we recognise that he has produced something original. This is however far from sufficient in itself. A person who developed a product of some kind independently of any similar formulation by someone else might well be said to be creative, although what was produced was not in an absolute sense original. This is, somewhat surprisingly, a relatively common experience in scientific discovery: artistic examples are rather fewer, but by no means non-existent. A number of tests of creativity turn out on closer inspection to be tests of originality: and sometimes originality is itself defined in an apparently arbitrary fashion (e.g. as a one in a hundred occurrence). This clearly depends on a person-centred definition of originality - that, as far as its instigation was concerned, it was an original response. But in some cases, the "original" response may be extremely hackneyed (for example, there is nothing particularly striking about the suggestion that a brick could serve as a paperweight, even though this may represent a conceptual breakthrough for the suggestor). What is clear, however, is that at the opposite end of the spectrum, a conclusion which follows inexorably from previous information does not entitle the ascription of creativity. The conclusion must contain something novel, something unpredictable for that. But what matters, it seems to me, is the aetiology of the novel production, not its absolute or relative originality. To satisfy the originality criterion, a product must not simply be not previously produced, but produced in a way that makes it not a matter of vote. There must be, so to speak, something of the
miraculous in the product. This thought, expressed in different language, is common to many who reflect about creativity: Bruner, for example, refers to the creative enterprise as an act which produces effective surprise; Ghiselin speaks of every creative act overpassing the establishment order in some way; Koestler describes the moment of creation or discovery as the "Eureka process". It is perhaps simpler to refer to this feature of creativity as transcendence: the act of creation transcends the present circumstances.

3.08 The distinction can be illustrated by reference to that familiar process, the solving of a crossword puzzle, an example which I have consistently found useful in puzzling about creativity. There is nothing open-ended about a crossword: for each clue there is one and one only correct solution, and one error is likely to throw the would-be solver off the track for several other clues. And yet for the most part for the reader puzzling over the clues the reaching of each predetermined answer is a creative process, as he attempts to build together part-solutions to the parts of the clue, taking into account of course the length of the word(s) and whatever letters he is already sure or half-sure of. The moment of seeing the answer; of welding together the disparate elements into the only possible answer brings, to me at least, a sensation of astonishment and glee, a true "Eureka" experience. This is very different from the sensation I got when, usually toward the end of the crossword when I am faced with three or four clues which resist solution, I laboriously consider, in a more or less
mechanical way, all the possible combinations of letters which might fill the gaps. Of course, because I do not have all the time in the world and because I have learned a trick or two in twenty years of doing crosswords, I employ certain heuristic devices: I calculate where the vowels will be and try to establish them first, I look for hints about the word-structure in the clue (is it plural, or an adjective?) etcetera: but it is still a mechanical exercise very different from my first efforts at the puzzle, and there is no sense of euphoria attached to arriving at the solution by this "counting-out" method. In Chapter 5, I shall return to this question; at present, it will suffice to note that, when we talk about a person as creative, we normally presume that the product is not simply a result of a systematic sorting process, but rather of some kind of leap into the unknown. Koestler has written at some length about the possible bisociative process culminating in the "Eureka process" and I shall be examining the conceptual implications of his theory in Chapter 5. At this point, however, it is interesting to note that he feels it necessary for his theory to cope with this element of transcendence, with its accompanying sensation of surprise, as Bruner astutely notes. This quality of the creative moment is stressed by many of the creative individuals whose self analyses are anthologised by Ghiselin (although it might cynically be suggested that Ghiselin's selection tends to reinforce his own theoretical bias): whatever the discipline, the concept of transcendence seems common.
Originality by itself, however, is far from being sufficient to
categorise creativity. The products of a schizophrenic may be
original but on the whole are unlikely to be described as creative.
Originality needs to be linked to the context in which it is
produced: besides the act of transcendence, it must connect with
what is transcended. Again, this thought is widely recognised
in the literature: such a criterion of relevance to context has
variously been formulated as adaptiveness to reality; selective
ordering of universe and accommodation of universe to man's world;
radical restructuring of man's cognitive system; the emergence of
a new and fitting organisation of the elements of the problem. Some
such formulation is common throughout writings on creativity, and
it is clear that most writers on the subject see the need to qualify
an "originality" criterion with an "appropriateness" criterion.
To see this need, however, is easier than to formulate it clearly,
and the various attempts at formulation tend to have little in common
apart from the conviction that something which takes into account
the appropriateness of the created product is necessary to augment
the requirement of originality. It seems to me that the difficulty
arises out of the attempt to conflate (or, at any rate, not to
distinguish between) a number of different ideas into one criterion.
On the one hand, the putative criterion faces in the direction of
the creator, and attempts to ensure that the original occurrence was
not accidental but intended. On the other hand, the putative
criterion faces in the direction of the universe to which the criterion
is a response, and attempts to ensure that the original occurrence was
not arbitrary, but **suitable**. Because an appropriateness criterion conflates these two aspects, there is a tendency for formulations which do not make this explicit to be either incoherent or inadequate.

3.10 Let us begin with the implication of intentionality. There are a number of ways in which this criterion might be operationally investigated: for example, there might be a replicability requirement, where the creator must show that the product was no shot in the dark by producing similarly original results in other instances, or of a probe into the creator's ability to explain the processes which led up to the creation, and the implications of that creation. And yet this at first blush appears to fly in the face of that characteristic already noted of creativity, that it should come as a surprise, to its creator as well as to everyone else, that the "Eureka experience" should be a moment of sudden insight rather than a logical corollary. There is, however, within the concept of "insight" the germ of the solution. Where an event happens by accident rather than by (at some level) intention, a whole family of concepts do not come into play, and we should not use the term creativity in connection with such happenstance. If when I am tinkering idly at the piano someone informs me that I have just played the opening bars of the Trumpet Voluntary, this would not be considered to be a case of my being creative. There is no intention on my part to produce a specific product: there is missing any aim at, or insight into, this product. At the true "Eureka" moment, there cannot but be awareness of what one is producing: the creator cannot but take credit and be responsible for his creation. In
general the concept for responsibility for action is not difficult to handle in ordinary language. Because of its potential importance in the assessment of crime and punishment, its criterial implications have been well worked over in moral and legal contexts. The Freudian theory of parapraxes has opened up fresh questions regarding the difference between action and accident, but we do not normally censure or praise someone for an accident: we tend to evaluate it purely in respect of its consequences, without regard to its perpetrator. In so far as we are in the habit of censuring or praising people in respect of products they have created, it is clear that we do not treat these products as accidents, but as the outcome of intentional actions.

3.11 A possible counter example might be the accidental pun, where the mind slip producing the joke is unintended. Although we are amused and appreciative of or irritated by the felicitous pun, we would, I think, reserve our approbation or disapprobation of the punster as someone who might be creative for someone who made a habit of punning and obviously went to some lengths to produce "bons mots" on appropriate occasions. It makes sense to evaluate Oscar Wilde as witty, as the polished aphorisms that fell from his lips in conversation were clearly deliberately concocted, if not previously rehearsed. It would however be odd to describe the unfortunate Reverend Spooner as witty, though many of his (apocryphal) inadvertent transposition are extremely apt and therefore funny. A possible counter example thus upon examination can be shown further to bear out the case for creativity as intentional.
But what of the familiar claim by creative individuals that part or all of their creation just "came" to them? We have, for example, the testimony of Poincaré that crucial material for a mathematical proof was revealed to him in a dream; of Mozart that often his compositions were conceived quite effortlessly in final form in the midst of his normal activity; of Coleridge that quantities of verse appeared to him in an opium daze. In the first place, although there is no apparent sense of the creator labouring to produce what comes to mind, in all cases the production is very much short of automatic: in each case the mind is prepared and ready to produce, and able to recognise the significance of the production. Secondly, such spontaneous production tends to be fragmentary: the creator has to bind up the process into a coherent whole, or to check the given out exhaustively for flaws. On closer inspection, therefore, while accepting that on occasion relaxation of the hold of the conscious mind on a problem or specification allows the less than conscious mind to produce an answer, there are grounds for thinking that there is in such circumstances no diminution of the responsibility of the creator for his product. From case to case the relative proportions may vary, but inspiration is not available without some degree of perspiration, and the moment of finding, of "Eureka" must be set in a context of search and verification before it has meaning.

The context of an act of creation is important not only in respect of assessing the intentionality of and therefore responsibility for the creation, but also in the evaluation of its suitability. Without
reference to its context, an original product is incomprehensible, and therefore unrecognisable as meritorious. Koestler persuasively sets out the background to a diverse collection of examples of scientific creativity, examples which a priori cry out to be identified as true acts of creation and shows how each arose out of as well as transcended its context. A parallel process can be seen at work in relation to artistic masterpieces: most usually, lack of comprehension of a putative work of art (such as the Tate Gallery's stack of bricks, or one of Radio Three's more eclectic offerings of electronic music) stems from a failure of the observer or hearer to relate the work successfully to a context; and this failure often results, interestingly enough, in accusations of fraudulence on the part of the artist. It is the perceived context which makes the original product comprehensible: against a background, the creation becomes explicable, to the extent that others can see where it came from, although this is very far from saying that viewed against its background, a work of creation loses its mystery, its magic: rather, one can see a bit more clearly what the magic is like.

Now this necessity of reference to a frame is conspicuously absent in many of the most familiar tests of "originality", often used as analogues of, or sometimes in mistake for, tests of creativity. The instructions for scoring "original" responses to the "Uses for Things" test employed by Getzels and Jackson, for example, note that responses were tallied and classified as common or uncommon: the subject has been given two scores, (a) the number of different
uses suggested for the object and (b) the number of uncommon uses suggested. Getzels and Jackson's test is based upon, and very similar to, Guilford's "Unusual Uses" and "Brick Uses" tests, which are similarly though not identically scored for unusualness. Common to all these tests is the emphasis upon high performance equalling the generation of as many as possible and as uncommon as possible responses. The philosophy behind this is that such tests assess fluency or flexibility, on the assumption that the ability to suggest unhesitatingly numerous unlikely uses for an ordinary object is an indicator of an ability to come up with original ideas with facility. This is plausible enough as far as it goes, but it is important to be clear about two points: (a) there is a distinction to be drawn between "fluency" (i.e. number of responses) and "originality" (i.e. number of unusual responses), and though both Guilford and Getzels and Jackson keep the two elements separate, others who have borrowed these tests are sometimes not so scrupulous; and (b) because these tests make no attempt to assess relevance of response, the important element of relation to context is altogether missing. Indeed, given that unusual responses are scored as separately and on the basis of rarely occurring responses, little credit is given to appropriate as opposed to far-fetched responses. Similarly, Wallach and Kogan score purely for number of responses and for unique responses. The five "creativity instruments" (verbal and visual) that they employ, although they indicate that responses were screened by two independent judges so as to eliminate any inappropriate responses (i.e. responses that could not conceivably fit the stimulus requirements). Again, Torrance in his
Test of Imagination (Form D) (in fact a battery of tests) scores for fluency, flexibility and originality in four of the tests, and for fluency and adequacy of response to the fifth, Ask and Guess Test. He states that "responses reflecting a lack of adaptation to some degree of reality are not counted" (page 269), but otherwise the degree of appropriateness is not taken into account. To use such tests as analogues for creativity measures is therefore totally unsatisfactory.

3.15 None of these tests are wrong or inadequate as they stand; but they do not test "creativity" adequately, nor even "originality", and when they are used for such purposes, this is inappropriate, and potentially a cause of misleading raw data. That this is so is seen clearly in Getzels and Jackson's revelation of how low the correlation is between the "Uses" test and the other tests employed (Hidden Shapes, Fables and Makeup Problems) which are scored for appropriateness. The "Uses" test does correlate highly with the other instrument, the Word Association test, which seeks to measure fluency: the clear implication (though Getzels and Jackson do not make the point) is that fluency and appropriateness are separate, and to a degree independently varying, elements. There is therefore every reason to doubt the validity of data obtained by a "fluency" measure as being necessarily indicative of "creativity". In fact, when one stops to think about it, one would intuitively distinguish between the ability to produce novel answers off the top of one's head and the ability to be innovative within a context. There is a good deal of difference between thinking of lots of uses for bricks and thinking of the best uses for bricks.
Typically, it takes an outsider to point this out. Thomas Kuhn, whose work on scientific revolution has been much quoted and admired if less understood by psychologists working in the creativity field, clearly took a certain delight in paradoxically hymning the praises of convergence rather than divergence as the important characteristic of creativity. Shorn of its delight in studied antithesis, Kuhn's argument is that the concept of breakthrough, or breakout of paradigm, is senseless except by reference to the paradigm or contextual setting, that the true innovator must be so aware of the context in which he is operating that he can then (and only then) transcend it. The implication to be drawn from this is that the tendency to identify divergence with creativity, and further, to identify divergence with a very basic kind of ideational fluency is at best too narrow and at worst plain wrong.

THE INTEGRATING "I"

The need to introduce ideas such as "meaningfulness", "intention" and "responsibility" as inextricably bound up with the concept of creativity leads to the conclusion that it is insufficient to talk about creativity without some reference to the creator. At the beginning of this chapter, the logical priority of the object created was established: we have now reached the stage where the creator as subject can be seen as also a vital element. To put it simply, there is a need to postulate a central and dynamic organising consciousness in the ascription of creativity. As soon as we start talking about "intention" we need to broaden our vision from the merely objective: we move into a subjective frame of reference,
away from the determined "push" of objective events to a conceptualisation which also includes the "pull" of intentionality. Again, we found it impossible to avoid the consideration of responsibility, as it related to the ascription of merit: responsibility, however, is meaningless in relation to a process except insofar as we are assuming a central organising consciousness controlling the process. A process *per se* does not attract the concept of responsibility: a stone rolling downhill is not responsible for crushing blades of grass in any sense that matters.

The linked concepts of praiseworthiness and blameworthiness do not operate unless in connection with an agent who does something. In any investigation of creativity, therefore, it looks as though the experimental design should take into account this idea of agency.

3.18 Empirical support for this conclusion comes from two separate kinds of source. On the one hand, there has been considerable investigation of the relationship between creativity and psychiatric disorder. It is a commonly held notion that creative genius is often closely allied to madness; and this would seem to fly against the argument that creativity requires a strong ego. From both historiometric and "eminent practitioners" studies, however, a surprisingly coherent picture emerges of creative persons being, if not entirely thoroughly normal, at least very much in control: they may allow themselves freedoms which set them apart from the common run of humanity, but they are far from being mentally unbalanced. The most interesting study on this subject is by Anthony Storr, whose approach is firmly psychoanalytic. He examines a number of undoubtedly creative
persons, who at the same time revealed distinctly aberrant psychological traits. While carefully differentiating among a number of different psychological types amongst creators, he shows persuasively how in each case the act of creation was useful, sometimes even vital, for them. A potentially schizoid character like Kafka, for example is enabled through his creativity to gratify his need for omnipotence and control, imposing his own will on his fictional world, and exploring and simultaneously mastering his anxieties: for him, creativity is a defence against threat of disintegration. For others, such as Newton or Descartes, being creative enables them to detach themselves from the world of experience. Conversely, there are some individuals, such as Frederick Rolfe (the self-styled Baron Corvo) and the late Ian Fleming, to whom creativity appears to have been sheer wishful-filment; others again, potential obsessionals, find in the creative experience satisfaction of their compulsive need for order. What becomes clear from Storr's careful analysis is that there is no one dominant psychological tendency which is likely to be associated with creativity: rather, Storr's conclusion is that creative activity tends to have real practical value in enabling various people with disparate, potentially damaging psychological tendencies to cope better with living. He connects this thesis with the observation that any fragmentation of personality tends to result in cessation of creative activity: the mentally ill, being unable to integrate their visions, are unable successfully to express themselves in a creative way. His conclusion is that creativity is one mode adopted by gifted people of coming to terms with the internal conflicts
and dissociation all human beings suffer from to some degree. Creativity is therefore functional. It requires, and promotes, "a strong ego, that is an actively executant aspect of personality which most schizophrenics conspicuously lack". Now this sits rather well with the findings of Roe, Barron and MacKinnon, that creative persons have highly developed control mechanisms.

3.19 It also fits well with evidence from a rather different source. Margaret Mead, considering what light social anthropology can throw upon the relationship between creativity and a strong sense of self, has argued persuasively that changing social systems affect the self-image of the individuals making up a given society, and this in turn has an effect upon creative performance: in a fascinating longitudinal comparative crosscultural study, she demonstrates that the potential within various cultures for creative performance by members both reflects and contributes to the self esteem of those members and to the stability or otherwise of the social system. In different ways, the societies — the Samoans, the Manus, the Arapesh and the Balinese — are or are not creative in ways that relate directly to the degree to which the culture as a whole is mentally healthy.

3.20 The implication to be drawn from all this is that creativity is inextricably bound up with, and demonstrative of and perhaps conducive to, a sense of self. What the disparate sources cited underline is the degree to which creativity involves the self — as dynamic and as integrator. The loss of sense of self means the loss of creativity,
which requires the integrated "I" to integrate experience into a creative leap forward. Varied and disparate elements are organised into a coherent unity in a positive way. Coherence is not however a matter of simplicity of structure: there is a wealth of research data to the effect that creative persons typically have a wide range of interests and a high level of curiosity in exploring new ideas. Their personalities are not only coherent, but also complex. For example, there is the evidence that on the Barron-Walsh Art Scale, creative individuals were far fonder of complex and asymmetric patterns than were controls. This was true not only of artists, but also of all other creative groups. In the light of the other evidence that we have about creative persons, a plausible explanation seems to be that patterns that are incomplete in various ways have more challenge for the creative individual who is after all highly interested in the world around him: what asymmetric patterns require is the perceiving consciousness to complete them, that is they require active rather than passive viewing. The individual must join in, and, in joining in, must integrate his consciousness with the pattern.

In this context it is interesting to consider the view of various eminent philosophers, historians and psychologists of art, that art is the primary vehicle for the development of human consciousness. Approaching what is clearly the same question from a different angle, they pose the questions of how art ever developed, since it is apparently non-productive; and further, since it did develop, what can be its function. In each case, the answer propounded is that
the function of art has been to develop and aid the individual's and the culture's grasp of reality: the act of abstracting and formulating, of placing a certain distance between oneself and one's immediate experience enables that experience to be explored and complemented. By the act of creation the world is made more meaningful because it is integrated, brought together by the "I". By the act of creation, the world is also brought under control.

3.21 What all of this brings us to realise is how much the concept of creativity relies upon an infrastructure of assumptions about the centrality of an organising dynamic, which I have referred to as the integrating "I". All of this arose out of a consideration of how a criterion of originality has to be supplemented. We have moved far away from anything that could be a straightforwardly objective criterion. This is not to say that there is no possibility of scientific study. Nonetheless, this clarification of the sort of ideas that are linked to creativity is of direct relevance in deciding how one should go about scientifically studying creativity, and, more importantly, in advising what sorts of approach are likely to be inadequate and therefore uninformative. Clearly, any experimental design which leaves no room for the "I", which insists that the subject be purely passive, which takes only objective factors into account, is going to tackle the subject area less than squarely.
THE RESPONDING YOU

3.22 Investigation of the relationship between creativity and meaningfulness led us to an infrastructure of assumptions about the integrating "I". Consideration of how to apply a criterion of suitability to context (again as a necessary supplement to a criterion of originality) brings us to the problem of value. Here more than ever, it is difficult to see how this can be established as an objective criterion: the evaluation of creativity throughout history has always been a chancy business, and it is not difficult to adduce, from where we stand today, examples of what seem to our eyes false positives (i.e. persons recognised as creative who were not) or false negatives (i.e. persons not recognised as creative who were). The evaluation of a piece of work as a creation, and therefore of its producer as creative, has inbuilt social implications, and there seems no final objectivity to be gained.

CAN WE DO WITHOUT A VALUE CRITERION?

3.23 Rogers, for example, makes a forceful case against this inherent subjectivity: the value that society puts upon a creation, he argues, fluctuates too much to be a criterion of its creativity. Moreover, certain true creations may never be noticed at all, let alone evaluated. The artist may starve in his garret, after all, surrounded only by the paintings with which he cannot bear to part. Rogers therefore rejects value as a criterion, and retreats to a criterion of personal satisfaction which as we have seen entraps him into incorrigibility of ascription of creativity on the basis of
self-evaluation. However, incorrigibility of ascription of creativity does not seem to square with how we would normally use the term creativity: let us therefore consider whether perpetual corrigibility, as implied by a criterion of social evaluation, is as reprehensible as Rogers clearly thinks it is. What this would mean is that we can never be completely confident in ascribing creativity to a person on the basis of a piece of work: we may always be wrong about whether a person is creative. The first thing to note is that in most cases although the ascription of creativity by social consensus is perpetually corrigible, it is nonetheless pretty stable. Although the consensus opinion about, for example, Vincent van Gogh may have changed, although notably creative work in the present day is undoubtedly overlooked and reviled, there is still a high degree of agreement between persons about the ascription of creativity in individual cases. We seem to get it together more often than we are hopelessly adrift. And this is because lack of objectivity of assessment does not mean arbitrariness of assessment. Given that taste is individual and varies greatly from one person to another, there is nevertheless a universality of agreement as to what are and are not, or could or could not be, works of art, and this consensus is largely independent of individual taste and fancy. Evaluation of the creativity of a person is not simply emotive reaction as a result of one's consulting one's guts, but draws upon a socially evolved and evolving universe of discourse. Such a universality of discourse about aesthetic judgements is based upon my and our recognition that you are a person like us, that "I" can put myself in place of "you", and
my comprehension of what you do draws upon my awareness of what I have done and can do. Such shared comprehension provides the social web in which social evaluation of performance takes place. The rationality, that is to say the structures underpinning the coherent use, of interpersonal terms is based upon this shared comprehension. This is why an explanation of someone's action can be complete whilst stopping a long way short of exhaustive statement of antecedent conditions and indeed a long way short of reductionism. Such considerations dispel the conclusion that subjective judgements of value must be arbitrary. On the basis of interpersonal comprehension, order but not objectivity can be introduced into the analysis. In terms of the ascription of creativity, the responding "you" is able to evaluate the integrating "I's" accomplishment, because the "you" is able to relate to the "I" as a person like himself.

3.24 To summarise, we have teased out a number of criteria which seem to govern our use of the concept of creativity. We have established that there needs to be a product, which is original but not arbitrary; that we have to think in terms of the creator as an integrating "I" and of his creativity as being both expressive of and as contributory to his ability to integrate experience; and that evaluation of creativity is not conclusive, but neither is it unstructured. Armed with these working criteria, we can see that creative action is by no means critically different from normal action, which shares many of the attributes discussed. Rather, activity which results in a creative product is typical human behaviour plus — typical in
that it is free and intentional and appropriate, but with that additional element of significant and valuable transcendence. Poincaré arriving at a mathematical proof is doing something special, but not exotic, something marvellous but not incomprehensible. This leads to the consideration that the investigation of creativity cannot be totally unlike the investigation of other free and intentional human behaviour; that whatever conclusions we draw about the practice of psychology in respect of creativity may have relevance to the general practice of psychology.
CHAPTER 4

EMPIRICAL STRATEGIES:
THE OBJECTIVIST
DIFFICULTY
CHAPTER 4

EMPIRICAL STRATEGIES:
THE OBJECTIVIST DIFFICULTY

INTRODUCTION TO THE PROBLEM

4.01 The philosopher's task is a peaceful, deskbound one. To sit and tease at concepts may be intellectually demanding, but it is mercifully largely free of mundane constraints. On the other hand the empirical psychologist is constantly faced with the necessity of shaping the actual in order to achieve his ends. Empirical research is inherently a succession of compromises, in an attempt to recreate and manipulate and extrapolate from the real world as closely as possible fitted to the ideal. Abstract beliefs or suppositions about the way things happen must be translated first into narrow hypotheses which can be tested, and then into test situations. The necessity of interaction with the real world in order to derive ideal conclusions affects the empirical scientist's approach to theory: his prime concern must be, not "how adequate is it as a theory?", but "can it be tested?".

4.02 Psychologists studying creativity are faced with the need to develop effective empirical strategies to test theoretical approaches. In the second chapter, we looked briefly at some of the more notable strategies adopted by psychologists, and considered the problems
inherent in such approaches. In an attempt to find a firmer footing for psychological research into creativity, we then went on in Chapter three to consider, on a theoretical basis, what was involved in the ascription of creativity, in the hopes that this might serve as a foundation for the development of adequate empirical strategies for the investigation of creativity. In doing so, we were led to the conclusion that the ascription of creativity, that is the identification of an object or process as being a work of creation, or of a person as being creative, or of an activity as being an act of creation, could never be certain. We have found that it is inherent in the concept of creativity that when we describe persons as creative on the basis of their products, we are not referring to some fixed and observable attribute which they possess, but rather are making a judgement, which can only ever be provisional, on the basis of evidence available to us. This has necessary implications for the ways in which we can go about studying creativity in a scientific way. To say "X is creative" is to make a judgement on the basis of evidence which can range from the purely personal to the purely social. If I take as evidence for making such a statement only my personal "gut feelings" about X's product, then I am more likely to come off worse if my assertion is challenged, than if my evidence for my assertion is a long established and widely held consensus about the value of X's works. The sorts of reasons that I can adduce for the plausibility of my assertion will typically relate to the criteria already teased out: that what X has produced is original, is valuable; that I (and preferably others as well) judge that it is both
distinctly novel and can be integrated into and integrates its context. If I cannot articulate some such reasons as these, then my assertion will be improper. But whether my ability to back up my assertion is limited or extensive, I am always expressing a judgement and not stating a fact. To enquire in a systematic way into creativity, therefore, is not to analyse facts but to explore judgements.

4.03 The scientific study of creativity might therefore seem to be a paradox: the attempt to investigate objectively a domain saturated with subjectivity. I think that this is by no means a real paradox, and that it can be shown that genuine systematic scientific investigation is perfectly compatible with the recognition of one's field of study as being subjectively-saturated. However, it is certainly true that there is, at the least, a tension between a narrow view of objective methodology and subject matter which is not, or not simply, objective. One way of relieving this tension is to disregard, for practical purposes, the subjective elements: this covert (at one level or another) redefinition of creativity in terms more amenable to quantification and objective certainty has sometimes occurred in psychological studies of creativity. In Chapter two, some objections were raised to this approach: adequacy of explanation suffers in the attempt to improve objectivity in this way. The alternative is to refuse to accept the false equation of subjectivity and arbitrariness: the erroneous conflation of two dichotomies of subjectivity/objectivity and unreasonableness/reasonableness. The conflation can occur because of the high (not to say overstated) regard in which scientists hold objectivity:
its antithesis therefore begins to be seen as a bad thing, something to be avoided. As has been pointed out earlier, however, people in their normal lives get by quite well without needing the comfort of objective validation, managing to understand and be understood; yet chaos fails to enter in. Meaningfulness then does not have to mean strict objectivity. Scientific research of course is not the same thing as ordinary intercourse; it has to be organised and accessible in a way that is unnecessary in ordinary life. Nonetheless, this does not imply that there must be total discontinuity between the two: rather it behoves us to consider rather more carefully what is required of scientific study.

4.04 The aim of any scientific enterprise is to elicit evidence which will tend to confirm or disconfirm a hypothesis. In terms of empirical research; this is done by eliciting (whether by engineering it or simply observing it) an outcome, having first identified and controlled all or as many as possible of the conditions antecedent to the outcome. By so doing, the researcher tries as far as possible to "contain" the effects which he is investigating, both in order to make his research as adequate (because as complete) as possible, and in order to make his research as replicable as possible. He could fail to maximise the chances of himself or another researcher replicating his study if he left out of the picture, whether because he did not think of them or because he could not see how to control them, conditions affecting the effects studied. Thus, for example, a researcher studying taste discrimination is unlikely to produce either replicable or useful results if he fails to allow for some of his test-subjects having bad colds. Adequacy of structure of
research matters because the research fails to reflect sufficiently what is being studied, its results are vitiated; and I shall come back to this point later. Replicability also matters, because an unreplicated (and even more so an unreplicable) experiment is very poor proof indeed of anything. In order to make your experiment replicable, you must be as explicit as possible about what you have done, and what you know about the conditions in which the events occurred. To the extent that you are silent or misinformed or inaccurate about the conditions of the experiment another will be unable to replicate your results.

4.05 In the interests of replicability, scientists attempt to be as precise as possible about their experiments. This has inevitably led to a preference (where a choice is possible, that is) for the closed, clinical laboratory setting, where (in theory at least) all the variables can be controlled, rather than a more naturalistic setting. As far as psychology is concerned, the urge to go about research in an objective way does not simply stem from the wish to be precise and therefore replicable, but is in part also a defensive desire to look as scientific as possible, and in particular to model psychology upon the paradigmatic sciences such as physics, where adherence to proper procedure and tight control are prominent. Thus there are for psychologists ideological as well as practical reasons for seeking the falsifiable hypothesis, the circumscribed strategy, the controllable scenario, the measurable outcome. The difficulty is that while objectivity in itself is vital if the enterprise is to be scientific, objectivity by itself is inadequate for most areas of psychological research. In the case of studies of creativity, we
have already seen how adequate research must take into account the subjectivity-saturation of the governing concepts. It is only if objectivity by itself is held to be what matters that the paradox mentioned above between subjective material and objective methodology becomes real; and can be resolved either by implicitly denying the subjective elements of the material or by rejecting the need for objective methodology. This Scylla and Charybis (both antithetical to productive research, since one fails to satisfy the need for the scientific research to be adequate, and the other the need for the research to be investigative rather than anecdotal) can be avoided by accepting that objectivity has a place but is only a part of the enterprise.

4.06 Effective empirical strategies for investigating creativity must allow for the inevitability of subjectivity, while surrendering as little as possible the rigour necessary for a scientific endeavour. Specifically, allowance must be made:

(i) at the stage of isolating the subject material, for the uncertainty involved in adjudging subjects as being appropriately selected: that is, the researcher must maximise his chances of having selected appropriate subjects by having regard to the criteria which have been delineated, but must also recognise that this selection cannot be certain;

(ii) at the stage of test construction and administration, for the centrality to creativity of the integrating "I": that is, the researcher must employ measures which leave space for the personal dynamic of the subject, which enable the subject to respond as a person, not simply as an object;

(iii) at the stage of data evaluation, for the shared experience of, and dynamic interaction within, the test situation as an influence upon the results
achieved: that is, the researcher must recognise the necessary contamination of his data.

So how is that balance between the subjective features of the subject-area and the desire of objectivity in research to be struck in practice? For the rest of this chapter, we shall look at three different solutions to this dilemma, and in evaluating the effects of the empirical strategies adopted in each case, we shall hopefully come closer to understanding what demands are made upon psychological methodology by the psychological concept of creativity.

THE EMPIRICAL STRATEGIES OF ANNE ROE

Anne Roe approached the study of creativity as a trained clinical psychologist, and this background colours both the kind of results she looked for and the empirical strategies adopted. Having selected her subjects, she asked them to provide information which can be divided into three sections:

(i) biographical material, about background, early preferences, choice of vocation and interests;
(ii) information about character traits;
(iii) information about intellectual abilities.

She was then able to test out possible correlations amongst these three sets of data. By including all three areas as being of possible relevance to creativity, she adopted from the start a very broad and flexible approach to the subject: she did not make any a priori decisions about whether personality or intellectual characteristics would turn out to be the more important. A similar broadmindedness is shown in her selection of measures to collect the required data.
The biographical material was gathered primarily in the context of open-ended, loosely structured interviews conducted in private in as friendly an atmosphere as could be engendered. The material thus yielded was not scientifically elicited, but rather gathered in a context which drew as much as possible upon the model of a quiet conversation between persons: the interviewer's aim was to put the subject at ease, so that the responses were as considered and as deliberate as possible. It was not her aim to catch the subject out or off guard. Rather, interviewer and interviewed participated in drawing together as adequate a picture as possible of the subject's background. The information Roe elicited from these interviews was partly "hard" (facts about family size etc.) and partly "soft" (material filtered through the subject's memory and awareness, such as parental attitudes to education and earliest preferences) and often a combination of the two. The "hardest" data could be extracted and tabulated, but the biographical material is most interesting as an anecdotal quarry. By itself, the biographical material cannot prove anything, but it does provide a context into which the more "scientific" data can be placed and viewed. The second set of data, about personality characteristics, could have been gained in a variety of ways: there are innumerable tests in this general field. Partly because time considerations meant that highly general rather than specific tests would be most valuable, and partly because she was already accustomed to them, Roe chose to use a Rorschach test and the Thematic Apperception Test. Both of these tests are most frequently used in a clinical setting, where the aim is usually diagnosis rather than measurement.
Consequently the atmosphere is not test-like, and there is as little pressure as possible upon response, which is open-ended. The responses given, to enigmatic inkblots of the Rorschach test and to the ambiguous story-pictures of the TAT, are recorded near-verbatim, and are as far as possible spontaneous, unprompted. The raw material is therefore extremely diffuse and incommensurable, and needs to be analysed, in the light of generally accepted, but far from objective, protocols, in order to be used as data for the study. The open-endedness of the test situation, its minimal structure, the diffuseness of the raw data and the interpretativeness of the results are not drawbacks when the tests are used for clinical purposes, amongst a battery of other tests, in order to arrive at suggestions as regards diagnosis on an individual basis. However, their use as measures of performance in the way that Roe uses them in her study of creativity is more open to question, on two opposing counts. Firstly, by using these projective tests to derive data about her subjects' hopes, fears and general casts of mind, within the overall context of a specific inquiry into creativity rather than of a general psychological profile, Roe may have influenced the attitude of her subjects to the test, and consequently have affected the sorts of response elicited. Secondly, the need for interpretative scoring of the responses may render the results insufficiently objectively derived from analysis. The first is a claim that these tests in this context are too objective, the second that they are not objective enough. On the first count, Roe certainly sought to minimise the "testlike" effects of the TAT and Rorschach for her subjects by administering them once a rapport had been built up between tester and subject by means of the biographical interviews. Nonetheless,
the difficulties of asking subjects, particularly eminent subjects who may feel that in some way their reputations are at stake, to perform in this way are considerable, and at least some of the resistance that Roe reports in some of the eminent scientists to the construction of fiction and the making of guesses may be ascribed to their fear of "failing" the test. Clearly this performance aspect does not loom so large in a clinical rather than a research context. It is difficult to see, however, how Roe could have excluded entirely any sense of "test" from the use of these measures, and certainly her administration of them compensated as far as was possible for their potential difficulties. On the first count, then, we have to conclude that any residual bias built into the results as a result of the test itself is kept to a minimum.
The second objection is more serious, as Roe herself recognises, as she spends several pages discussing the validity problems of using these measures in this context. Responses given to these tests have to be analysed by the experimenter into discrete categories before they can be statistically manipulated to allow conclusions to be derived: but the analysis, like the raw responses, is essentially a matter of selection and judgement, and while experimenters may agree on the broad outlines of an analysis, there is likely to be considerable difference in emphasis at the margins. In the clinical setting this may not matter so much, but where the results are required to be comparable and "hard" enough for research purposes, doubts must be raised about the objectiveness of the analysis. Again, Roe minimises difficulties of this kind by using the material derived from the projective tests mainly for anecdotal rather than analytical purposes. She further checks out the
objectivity of the scores obtained by the various groups of eminent scientists by a subsidiary study, administering the same research images as a group test on 350 workers in the same field. This acted as an independent check on the results obtained from the individual testings, even though the Rorschach as a group test cannot be identical as regards administration or scoring with an individual test, thus rendering it necessary to be careful about extrapolating from one set of results to another. What emerges from the subsidiary study simply bears out the distinctions between specialisation already suggested by the individual tests: the two taken together then powerfully suggest that differences between men in different fields will be shown on the Rorschach, whether the men are selected from the best in the field or from those who are competent in the field. Finally, Roe administered a three part test of intelligence, specially constructed to be difficult enough for the subjects. The verbal part of the test was of vocabulary; the spatial part of the test required subjects to draw conclusions about geometrical figures; the mathematical test tested mathematical reasoning. The rationale behind the tripartite test was that Roe was particularly interested in style of thinking, and thus was more concerned to discover in what ways her subjects were intelligent, rather than how intelligent they were. In fact, there turned out to be problems with each part of the test: the verbal test was clearly too easy, and failed to differentiate well enough amongst the subjects; the spatial test turned out to converge significantly and negatively with age, and there was in any event some doubt as to exactly what was being measured in this test; and the mathematical test was probably a little too difficult for the "soft" scientists,
and far too easy for the physicists, very few of whom were even asked to do it. Roe cross-checked the test as a whole by arranging for a group of PhD students to take it in the context of a battery of other tests, thus enabling the scores on the VSM test to be converted into scores on other, well standardised, tests. The results tended to show that there are slight differences amongst the groups as a whole as regards verbal ability (though this result is swamped by the effects of the individual scores); that physicists are better at manipulating spatially than the others (though the age factor may affect this); and that higher mathematical ability is shown by those working in fields that require such expertise. Some rather more interesting results emerge from considering intra-group differences (for example, theoretical and experimental physicists tend to differ in their overall verbal or spatial bias) but in general terms this test produced little of real substance.

4.08 What then can we say in overall terms about Roe's empirical strategies, and in particular about her resolution of the difficulty of an approach which is at once valid and objective? I have stressed that she took from the start a holistic view of her subject-matter, and this was reflected both in the measures chosen (which are all, with the exception of the somewhat unsatisfactory VSM test, "soft") and in the use to which she put the material thus derived. In interviewing and in testing her subjects, she is constantly at pains to treat them as persons, to explore with them and to let them explore the stimuli, to play down the "test" aspects and play up the
"social exchange" aspects of the situation. In doing so, she derives a great deal of material which can at best be described as idiographic, and at worst anecdotal. She is primarily interested in her subjects as whole persons, and the various situations simply allow her to explore with them various approaches to the whole person. In doing so, she leans heavily toward the "validity" side of the argument, and consequently is rather more open to criticism as regards the "hardness" of the data she derives. She is very sensitive to the difficulties of treating data thus derived to the full rigour of complex statistical analysis, and never pushes her analyses beyond the simplest operations. The effect of this is to limit the usefulness of her research as far as hypothesis-development and confirmation/disconfirmation is concerned. Her researches are often quoted, and rightly much admired, but primarily as descriptive and inspirational rather than as blockbusters, scientifically speaking. Her major achievement is her self restraint in the treatment of her data; and the elegance of the cross-checks which she is able to make (through the subsidiary study of the Rorschach, for example) indicate a possible direction which she could have followed to overcome the inherent limitations built into her results because of this restraint.

THE EMPIRICAL STRATEGIES OF FRANK BARRON

Frank Barron is one of the most influential figures in creativity research in America. As well as his own research and that done by his associates at the Institute of Personality Assessment and Research at the University of California, he has been one of the forces behind the series of conferences at Utah on creativity, at which disparate
work in the field of creativity research has been integrated and developed. The work that Barron has been directly or indirectly responsible for therefore is extensive, and in this section I shall be commenting on only one study, of 100 captains in the United States Air Force, not specially selected for originality. The general empirical strategy as adopted by Barron in his researches was modelled on the "country house" assessment programme used by the Office of Strategic Services during the Second World War, similar in many ways to officer and civil service selection procedures employed until comparatively recently in this country. The subjects and their testers are housed together for a period of several days, effectively isolated from other intercourse, and are thrown together in an intensive programme of formal tests of different kinds and informal social interaction. The tests used are very varied: pencil and paper tests of different kinds, group exercises, interviews and laboratory experiments. Clearly this kind of atmosphere will be very different from that engendered in the course of Roe's study: because the candidates are very aware of each other, for example, there will be a much more overt sense of direct competition, even when subjects are being tested on an individual basis; there will also be an intensity, an awareness of need to participate fully, which Roe carefully avoided in her situation. Compared with Roe's, this is a very structured experimental design, much more of a production line for processing subjects than Roe's individualistic approach. This judgement is reinforced when we come to consider the actual tests employed in the first stage of the study (to separate out those who were regularly
original and those who were regularly unoriginal);

(a) Three tests taken from the creativity battery developed by Guilford and his associates, which are all intended to measure originality — Unusual Uses (asking for alternative uses for common objects such as a brick); Consequences B (asking for alternative consequences of a given change) and Plot Titles B (asking for alternative titles for story plots). All of these tests are timed, require written response and have minimal tester/subject interaction and are "testlike" in construction and administration. The responses are scored (with some weightings for uniqueness) on volume.

(b) The Rorschach and the TAT were used, though scoring was limited to original responses, as judged by two scorers, working independently.

(c) An Anagram test was used: subjects were asked to give as many anagrams as possible, taken from the word "generation". Scoring was on a volume basis, with only relatively (3 in 100) uncommon words being scored.

(d) Two tests developed by Barron himself were used, the Barron Word Rearrangement Test, in which the subject is told to make up a story using as many as possible of the 50 given words and, his story rated for originality on a 9-point scale; and the Barron Movement-threshold Inkblots, a kind of quick-fire Rorschach whereby the subject is allowed only one response to each shape, with responses weighted according to frequency of occurrence and thus scored.

4.10 What Barron is attempting is to use a variety of measures of originality to sort out from the pack those who regularly give original responses and those who regularly give unoriginal responses, and to test a number of hypotheses about originality by comparing the scores of the two groups on a number of other tests. Some criticisms of this approach to identifying subjects in this way have been expressed in Chapter 2. At this point it will suffice to say that the objectivity
of the measures used is vitiated by doubts about the extreme narrowness of what is being measured. Although Barron claims that the various tests measure originality via diverse media (verbal fluency, epigrammatic skill, perceptual reorganisation, visualisation etc.) the kind of test situation and the kind of response required vary very little from one test to another. In each case, the subjects are presented with a pencil and paper test asking for as many responses to a stimulus as possible within a time limit. In the circumstances, the low intercorrelations between test scores is surprising, in spite of Barron's argument that the low levels of correlation are to be expected, given that it is unrealistic to expect even regularly original persons to be outstandingly original in more than one or two ways. If, however, we do accept Barron's argument, this must throw doubt upon the extent he can generalise from such correlations (whether at this stage or later) as he does turn up. This is far more of a problem for Barron than it is for Roe: the measures that he uses are easily replicable, and are controlled, structured and limited in a way that Roe's are not, but while their edges, as it were, are clean-cut it is difficult to go beyond them. The measures that he uses are much more satisfactory than Roe's in terms of their objectivity: they are open to challenge sometimes, and to doubt always, in terms of their validity as indicators of anything other than ability to do this particular test.

4.11 The selection of two groups (15 individuals in each) of regularly original and regularly unoriginal persons respectively, is however
simply a first stage. Having established the two groups, he is able to test out a number of hypotheses about original people, by selecting test material to bring out differences of response in various areas, and then comparing the mean and standard deviation of the two groups. The hypotheses, derived from suggestions implicit in earlier work, are:

(a) that original persons prefer complexity and some degree of apparent imbalance in phenomena (tested by the Barron-Walsh Art Scale);

(b) that original persons are more complex psychodynamically and have greater personal scope (rated by psychiatric interviewers);

(c) that original persons are more independent in their judgments (Independence of Judgment Scale);

(d) that original persons are more self-assertive and dominant (ratings plus two scales from the California Psychological Inventory plus a Phallicism Scale);

(e) that original persons reflect suppression as a mechanism for the control of impulse (four different scales plus ratings).

At first sight, this is an attractive experimental design. Complex hypotheses can be tested out simply by examining the difference between group scores in tests which are maximally objective in their administration and scoring. (The same research strategy is employed by Getzels and Jackson, and by Wallach and Kogan.) There are however a number of difficulties which we shall look at in a moment.

4.12 Barron had a further objective in this study. He was also interested to study relationships between scores on the measures of originality already discussed and scores indicative of certain other aspects of personal and intellectual functioning, with a view to determining the
main correlates of originality. Responses to the wide variety of tests used, and expert ratings in the course of the intensive assessment period were analysed statistically to reveal significant differences in personality variables and in intellectual style between the more and less original. On the basis of these detected differences, Barron is able to speculate generally on the nature of creativity. Or is he?

4.13 Both prongs of the study — the testing of specific hypotheses, and the broader approach to test correlations — share a common strategy. The assumptions are made that the objective tests chosen accurately tap the factors featuring in the hypotheses; and that correlations amongst scores on such tests accurately reflect real relationships amongst the factors postulated. Because the conclusions are drawn directly from manipulation of the scores, a very great deal turns on the adequacy of the raw data. In the case of Roe's study we saw that the derived data was used with very great caution to draw conclusions, because of doubts about its objectivity. With Barron's study, the reverse is true. The objectivity of the raw data is assured: however, questions must be raised about the extent to which the data can be used for extrapolations beyond the measures themselves, that is, about the validity of the research. These questions arise from doubts about the subjects, doubts about the test situation and doubts about the measures. To begin with, the subjects are not specially selected in any way, and although they are sorted on the basis of the various originality measures employed on to an originality spectrum, they are not, as Roe's subjects are, undoubtedly creative people. Military men are not often thought to be a particularly
creative group. Thus, as Barron himself points out, the correlations reported may not reflect anything concerning the way in which highly creative people differ from the norm. Secondly, as has already been noted, there was comparatively little variation from one test situation to another; though the measures were diverse, the kind of response required (with a few exceptions) was often similar. The overall test situation was highly structured, and the role of the subject limited. Whereas in Roe's research, much depended upon the rapport established between interviewer and subject, in Barron's the testers within the test situation interact only minimally with their subjects. Roe's subjects were treated individually: they did not know who the other subjects were, though they were aware that they were "one on a list". Barron's on the other hand can watch their associates performing as they perform; and are brought into social contact with them throughout the test period. In short, the atmosphere is non-naturalistic; extremely competitive; and the subjects do not participate in the research, but only perform limited and clearly defined ways. Thirdly, the measures employed are very narrowly utilised to produce scores which can be statistically manipulated to yield outcomes. These measures are augmented by and checked against observer ratings, but this does not do much to widen the range of data. Taken together, these three areas of doubt can be seen to have a common form: the effect of the high level of objectivity introduced into the study by the empirical strategy chosen is vitiated by unresolved doubts about the broader application of results to the general question of creativity outside the laboratory setting.
THE EMPIRICAL STRATEGIES OF LIAM HUDSON

4.14 Liam Hudson came to the study of creativity through his interest in the differences between arts and sciences specialists. This rather slight study was in some ways unsatisfactory to him, and in an attempt to fill in the gaps he became interested in the work done by Getzels and Jackson in comparing the results obtained from intelligence tests and tests of "creativity". In particular, he hit upon using the tests (originating from Guilford's research team) of convergence/divergence to re-open his studies of the arts/science divide. In a carefully developed series of experimentation and theoretical speculation, Hudson has opened out creativity research beyond the narrow circle of test correlation which many American researchers (of whom Barron has been cited as an example) have become caught.

4.15 What is striking about Hudson's work is its particular combination of breadth of theoretical speculation and elegant precision of hypothesis-testing. In almost total contrast to Barron, whose empirical strategy is to go for a blockbusting battery of tests and then to use the data thus obtained to test out a multiplicity of hypotheses, Hudson concentrates on one hypothesis at a time, cross-checking his results in exhaustive attempts to weed out alternative interpretations of his data. His work is characteristically strategically complex but very simple at the stage of experimental execution. For example, his early research having established clear differences in response in the various tests used between convergers and divergers, he did not stop at that point, but instead experimented with changing the introduction to the test, substituting for
the open format precise instructions and a worked example of a "good" response. Under these conditions, many more of the subjects met the standard implicit in the model provided. Hudson considered that this might show that the converger was not incapable of thinking divergently but had to be told unambiguously that this was what was required. He showed that the fluency of most boys could be made to vary over a wide range. This discovery of the importance of context in eliciting response led him to a further series of experiments, in which the subjects were invited to role play, to respond as stereotypical scientist and artist. While this threw up the expected results - that fluency increased with the freedom from personal responsibility brought about by role playing - Hudson was somewhat surprised by the wholesale enthusiasm (not to say violence) with which some boys adopted the artist's role. This led Hudson on to consider that the most rigid converger may nonetheless have access to buried personae normally suppressed; and leads him into phenomenological speculation which in turn leads on to further research strategies.

4.16 Hudson uses for the most part a very narrow range of tests. His major tool is a version of Guilford's "Uses of Objects" test, as previously described as used by Barron. The objects used are occasionally varied, and, as mentioned above, the instructions are also sometimes modified. This is scored on the basis of total responses, but special note is taken of humorous, violent, sexual and generalised responses. He counter-points this test with another Guilford test, "Meanings of Words", which invites subjects to list
multiple meanings of a number of words. This test is of course not truly open-ended and arguably tests flexibility rather than divergence. An intelligence test is also used, biographical data obtained; and two other measures are also used as sources of additional material. These are a drawing test (with a title inviting punning responses) and "Controversial Statements" which invite thoughtful discussions of interesting topics. Scoring of the latter two is largely judgemental. Hudson, mindful of the low correlations between scores on the various tests, rarely pools data from the various measures: rather, like Roe he often uses the material as illuminative. He is confirmed in the necessity for this by the discovery that, for each individual, it is the bias between convergence and divergence that matters; not the absolute score: this further argues against additivity of scores being wise. In his later work he uses a specially devised test of yielding, which did not (as the classical tests of Asch and Milgram do) involve any element of deceit, and a variety of tests which derive more or less directly from Osgood's "Semantic Differential". All of these are straightforward to score.

4.17 Like every other researcher, Hudson has to accept the practical limitations of the test situation that he can construct for himself. His subjects are usually schoolchildren, boys from public or grammar schools, and at the top end of the academic spectrum. The tests were administered in a school setting in school hours, in groups: as far as possible the test conditions were held constant, with set instructions (which invited rather than demanded responses
to the stimuli) and with a time limit. This uniformity of setting enables Hudson to be reasonably happy about the objectivity of the test situation: however, he himself notes that this very objectivity raises doubts about the validity of the responses. As his research develops, he becomes more convinced that the differences in response on the divergence/convergence scale relate to different responses to context, to different judgements about what kind of behaviour is appropriate, and this has clear implications - that an implicitly authoritarian situation may well have differential effects on the subjects. The surprising variations in response to the role-play experiments tend to bear this out. Hudson attempts to cut across the "test like" atmosphere of the experimental process (which is of course necessary in order to maximise the objectivity of his results) by making the tests themselves fun: his modifications to the Guilford material is always in the direction of making the tasks set more attractive. Again, unlike Barron, he does not demand a great deal in terms of the number of tests any subject has to do: that is little chance that the subject will become bored (particularly crucial with young people).

4.18 In terms of overall empirical strategy, Hudson occupies the middle ground between Roe and Barron. He employs tests of originality, as does Barron, to investigate relationships between originality and other variables; but he places far less reliance than does Barron on the scores thus derived. Rather, he is at pains to emphasise that it is the comparative bias, and not the absolute incidence, of scores that matters in any given case. Because his material is
expressible statistically, however, he can do far more with his material than can Roe, whose data relies far more on interpretation and judgement, and who is often reduced to anecdote rather than analysis. His material is thus satisfactorily objective as regards the way in which it was obtained and the way it is scored and analysed, but at the same time he consistently bears in mind (as Barron sometimes does not) the potential limitations and disadvantages of such objectivity. He is far more sensitive to the effects on data of the test situation, and indeed turns this limitation into a research variable. His main difference from Barron, however, is in the way in which he relates theoretical speculation and experiment in an elegant and economical development. Where Barron fires out tests on all fronts simultaneously, and where Roe limits testing to informal, unstructured measures so as not to take too narrow a view of her subjects, Hudson is able to construct simple objectively scorable measures to explore subtle and complex questions about creativity. He is able to do this because of his parallel recognitions that tests used must be objective and that objective tests are severely limited. His research strategies are complex: his measures are simple.

CONCLUSIONS

4.19 This brings us back to the means of resolving the apparent dilemma articulated near the beginning of this chapter, between the need for objective investigation and a subjectivity-saturated domain. Systematic scientific investigation requires controlled experimentation and straightforward, replicable scoring: within the test, objectivity
is crucial. However, the overall empirical strategy of which the test is the experimental portion must be adequate to the subject matter to be investigated, and if appropriate this overall strategy must take into account the subjective elements of the subject matter, and the potential limitations of the objective possibilities of the test. By and large, it is usually possible to experiment objectively, even in areas which are subjectivity-saturated, without doing violence to the concepts involved, provided that the limitations of the test are recognised and the experimental design constructed for maximum effect. Without objectivity at the experimental level, psychological research is weakened and fails to drive forward: Roe's interesting, but underpowered, research shows the effects of insufficiently objective measures resulting in soft conclusions. Objectivity with insufficient regard to the need to take cognisance of the broader aspects of the subject matter leads to inadequacy of research in terms of validity. The sort of research strategy employed by Hudson shows how in practice "hard" experimentation can occur in the context of "soft" theorising, without vitiating the demands of either objectivity or validity. The blend of imaginative and provisional frame and rigorous testing is crucial.
CHAPTER 5

EMPIRICAL STRATEGIES:

THE DETERMINIST

DIFFICULTY
CHAPTER 5

EMPIRICAL STRATEGIES:
THE DETERMINIST DIFFICULTY

THE PARADOX OF CREATIVITY

5.01 Because the ascription of creativity involves the assessment of the creator as a person, a doer, someone who makes something happen, rather than simply an element in the process, the practical investigation of creativity is in difficulty, not only as regards objectivity, but also in respect of determinism. The difficulty may be formulated as a paradox, connecting together the nonarbitrary nature of a creation and its nonpredictability. Unless this paradox is felt to bite, an adequate response cannot be made to the question of creativity. An explanation which ignores one side of the autonomy may be condemned on this account. In order to be creative, a product must be capable of being set within a context, and this context makes the product comprehensible. Elucidation of context enables a better comprehension and appreciation of what is created. An art critic who draws attention to the way in which colour tones shift from left to right in a painting, voluminous sleeve notes for an LP of a horn concerto discussing the recurrent shaping phrases of the work, a preface to a nineteenth century work suggesting that the language of the work is consistently of fog and confusion: all of these make me more sensitive to the structure of the creation, by focusing my attention on aspects that I am unlikely to discover
on first acquaintance for myself. Alternatively my appreciation may be enhanced by some exposition of the historical context of the work in question, of the events in the creator's personal life which led up to this creation, as well as of the broader setting of taste and background in which he was enmeshed. To put it in a nutshell, it must be possible to explain to some extent a work of art, and it can be appreciated only to the extent to which it can be comprehended. Person A has potentially a truer appreciation of "Der Rosenkavalier" than person B, if A can relate the structures of plot and musical phrasing to "The Marriage of Figaro" whereas B simply likes the um-pah-pah tunes in Act III. Not that it is simply a matter of knowing the background in the cognitive mode: appreciation is also simultaneously a matter of affective response, but pure intuitive response without information is rare and prone to error, because the spectator in this case provides by himself the whole context in which to place and so to comprehend the creation. Where no explanation is either possible or available, then the act cannot be seen as creatively produced. For example, an undoubtedly creative work such as a raga cannot be appreciated to the extent to which its cultural and structural context cannot be grasped at. If I have no background in Indian culture, or at least in non-chromatic music, then I cannot tell a good raga from an indifferent one in any way that counts. It is only where explanation begins to be possible that a product can be appreciated as creatively produced. That is, it is only when the process of creative activity can be seen to be in some way rulegoverned that the terminology of creativity comes into play.
Now for the other side of the paradox. The act of creation brings into existence something which does not simply follow from what is already in existence, but which is an unpredictable leap forward in the darkness. The creative process is comprehensible but not predictable, rule-governed but not rule-bound. A great work can be explained, but not explained away. Awareness of context helps to explain, but cannot explain away. All the relevant facts available cannot determine the work — the interaction of factors do not constitute the act of creation. The missing ingredient is the creator. Poincaré's well-known description of the moment of illumination in scientific discovery bears out the essential spontaneity of the creative moment, sneaking up and catching him from behind. It also bears out the necessity of preparation and systematic effort. Without preparation the creator could not recognise his own creation, the relevance of the new to the old, the value of choosing one course rather than another. But preparation is not by itself sufficient for creativity.

There is a space in between the sphere of potentially predictable behaviour and the sphere of incomprehensible behaviour. On one side, determinate explanation is always in principle at least available. On the other, no kind of explanation can begin to be made. But in the middle, explanation but not prediction is possible. A piece of behaviour can be transferred from non-explainability to explainability without difficulty — if I switch on the radio to a cacophony I reorient myself (with the help of the Radio Times) when I recognise it as a work by Schoenberg i.e. I transfer it from
the category of chaos to a category of order according to contextual interpretation. It is a matter of orientation between chaotic things and orderly things and the sort of understanding that they therefore require. Transfer between the two distinguishable categories of order - between the realms of determinate explanation and of indeterminate explanation - is not the same, because the appropriateness of the type of explanation depends on the sort of behaviour requiring to be explained.

5.04 A determinist explanation gives the unique solution to a state-equation. A determinist explanation shows that in the circumstances nothing else could have happened. Deterministic explanations relate to closed systems: the answers to questions like "Why did A happen?" "How does B affect C?" "What will happen after C?" within the closed system can be exhaustive, and prediction is possible because the system is closed. Prediction is possible only where all the relevant factors are known. What makes this kind of statement possible is that within the closed system there are highly bounded variables, linear in space and time. Without spatial conjunction, and temporal separateness, a determinist explanation cannot be given. Moreover the items must be clearly separable with clear and finite possible relationships: they must be of the same order, with the same sorts of properties.

SOLUTIONS

5.05 It is difficult to see how this sort of explanation can be given in relation to creativity. If we could produce a complete "explanation"
of a creation, that is, exhaustively itemise the relevant conditions which led to its production, then the paradox of creativity would be dissolved: in principle at least, creativity would become predictable. In order to see whether this sort of approach is viable, we shall consider a number of theories which can be viewed as explicitly or implicitly offering explanations or at least explanatory schemata about creativity. In each case we shall have to consider its adequacy in range and in depth as an explanation of creativity. The difficulties we encounter will consistently bring us back to this paradox of creativity, from one side or the other. We shall eventually come to a provisional view of how a practical approach to the scientific investigation of creativity might best be developed.

B.F. SKINNER

5.06 The starting point for our survey is the high priest of determinism in psychology, B.F. Skinner. He takes the view that potentially psychology should be able to give cause-and-effect answers to questions about human behaviour.

"As a science of behaviour adopts the strategy of physics and biology, the autonomous agent to which behaviour has traditionally been attributed is replaced by the environment - the environment in which the individual is shaped and maintained ... In shifting control from autonomous man to the observable environment we do not leave an empty organism. A great deal goes on inside the skin, and psychology will eventually tell us more about it. It will explain why behaviour is indeed related to the antecedent events of which it can be shown to be a function."

(Skinner, 1973, p.180)
In other words, the problem with explaining human behaviour is not that it is qualitatively different from the behaviour of a billiard ball, but that it is more difficult to identify and measure the antecedent events producing it. The problem is therefore a practical one of obtaining sufficient data. It is important to distinguish between conceptually based objections to this view and general antipathy: a theory may be unpalatable and yet cogent. Much of the criticism to which Skinner has been subjected has been an ill advised indiscriminate mix of moral revulsion and academic analysis: only the latter has any force, though the former undoubtedly helps the critics feel better about the whole thing. Very simply, the analytical objection to Skinner's theory of behaviour can be put as follows: the strategies adopted in a science must be fitted to its material, and the material to which a deterministic explanation is appropriate is not the same as the material of psychology, especially as it relates to creativity. The essential difficulty for Skinner is that actions such as creating a symphony do not happen within a closed system, unless one is to view the whole universe as a closed system, like a Skinner box. If we could exhaustively itemise all relevant factors leading to the creation, we should have to include both external and internal variables, since the agent's beliefs, desires and emotions are clearly relevant to his actions. To get beyond the statement that certain conditions are relevant to the act of creation to a statement that these conditions cause the act of creation requires the possibility that the separate conditions can be quantified as to their effect and some connecting formulation drawn up. In order to
explain it, account would have to be taken of the sum total of my experiences, plus my genetic endowment: but at this point any determinist account of any one action would be infinite in its reference. In spite of Skinner's optimism that researches into causality at the very simplest levels of behaviour will form the foundation of a scientific psychology of determinate cause and effects, there is no sign that psychology is developing along these lines. Rather, contemporary researches are becoming "softer", taking more interest than previously in the more philosophical, less technological aspects of the field, and eschewing the stimulus-response approach to psychological theory. The forecast development from the most basic to more complex behaviour has failed to take place.

5.07 An often discussed difficulty of a determinist theory of human action is that it dissolves the notion of responsibility. There has been a general tendency over the past hundred years for the commonsensical notion of individual responsibility to be eroded, or modified, by the currency of general determinist theories (Darwin's, Marx's, Freud's) that a man is shaped by his past and present environment. Even High Court judges are susceptible to this notion. The recognition that environmental factors affect persons' actions, however, does not by itself dissolve the central responsibility for one's actions, though of course it does offer some explanation of how the central responsibility may be lessened by circumstances. What does dissolve responsibility is the claim "it could not have been otherwise" which is implicit in the tendering of a determinist
exegis of an action. If the chain of explanation has no space for choice, then the concept of responsibility has nothing to latch on to. Now the fact that this view of human action is to most people repulsive is not in itself an argument that it is a false view: and it would be wrong to reject Skinner's vision of mechanistic explanation of human action simply on this account. It is however perfectly legitimate to point out that evidence to date does not bear out his case except as regards the most basic of actions; and that in the case of the most complex kinds of action such as creativity it is difficult to see how a mechanistic explanation could ever be sufficient. The difficulty for Skinner rests in his inability to purge his science of behaviour of person-laden concepts in favour of a special "scientific", theoretically-neutral language of objectivism which would eliminate subjectivism. The dynamic of behaviourist theory is in the ability of the organism to want and be satisfied: this is the basis of reinforcement, the basic shaping principle of behaviour. Yet reinforcement itself, either at the most basic or at the more complex levels, cannot be satisfactorily explained by behaviourism. This is partly disguised by the employment of terms which are distinguishable from their common language equivalents, but this by itself is insufficient: "drive", "reinforcement" etc. are non-informative with regard to the theory in which they are embedded, (i.e. they do not have adequate explanatory power in depth). A shifting, unhappy argument about biological evolutionary factors, which is all that Skinner can produce to explain why reinforcers work does not help his case: he is not entitled by the terms of his own theory to make the sort of
general assumptions he does about the teleology of the human race. Even more markedly, terms like "drive" and "reinforcement", even accepting this dubious evolutionary exegis, are unable to cope at all with key psychological concepts relating to complex behaviour such as creativity (i.e. they do not have adequate explanatory power in range). There seems, by the terms of Skinner's theory, to be no reason why people should indulge in creative behaviour. The evolutionary argument is a broken reed for this purpose. If Skinner's theoretical approach is to provide a coherent solution to the question of "how", therefore, it fails thereby to tackle the question of "why".

5.08 Skinner's determinist model of a piece of verbal behaviour (adapted from his general theory of behaviour) as a response to a stimulus and being in principle totally predictable and controllable was comprehensively demolished by Chomsky in his 1959 review of Skinner's *Verbal Behaviour*. In short, Chomsky showed that in practice in real life the seemingly precise terms of "stimulus" and "response" can be used only circularly, vacuously, and retrodictively: the stimulus can be identified only when we hear the response. Innumerable responses are feasible to a real-life stimulus situation (such as a painting in an exhibition) and in each case the response can be determinately explained after the event by some aspect of that general stimulus situation. The explanatory model is thus dissolved into an infinity of possible Ss leading to an infinity of possible Rs. Such a model can remain determinate only at the expense of total vacuity. Whilst the specific theory destroyed by Chomsky
related to a verbal behaviour it is noteworthy that the criticism bites upon a Skinnerian account of behaviour in general, and that in particular it can be deployed against a simple mechanist theory of creativity.

NOAM CHOMSKY

5.09 Chomsky's is less obviously a case of mechanistic explanation. There is a certain impression amongst psychologists and philosophers that Chomskian theory can be adduced on the side of the humans (if not of the angels) against the machines. The misapprehension arises from the assumption that behaviourism in one form or other is the only extant version of mechanism, and that, since Chomsky is undoubtedly opposed to behaviourism, he is also opposed to mechanism. In fact it may well be argued that Chomsky's insistence upon the need to postulate that the complex principles underlying the structure of language are innate derives from his unshakeable conviction that mechanistic explanations are necessary. The fact that human language is creative, that young children can effortlessly produce an indefinitely large number of utterances which they have not previously encountered and which are germane to the context in which they are uttered, convince him that a behaviourist learning theory of associations built up through experience and through trial and error is unsatisfactory. This creativity however remains for Chomsky a problem, since it prima facie resists mechanistic explana-

nation. His solution to the problem is that capacity for language can be explained only as a manifestation of some internal constructed
mechanism. Throughout his writings Chomsky's arguments for the existence of some innate schematism which is genetically determined and triggered by a minimal quantity of experience are negative rather than positive. Given the problem, what other solution will do? If the acquisition and use of language cannot be adequately explained as determined by experience, then it must be augmented by an explanation of its being biologically determined. Chomsky is of the opinion that it may well in the future become possible to provide direct empirical verification of these cognitive structures:

"It is an interesting question whether the functioning and evolution of human mentality can be accommodated within the framework of physical explanation, as presently conceived, or whether there are new principles, yet unknown, that must be invoked, perhaps principles that emerge only at higher levels of organisation than can now be submitted to physical investigation. We can, however, be fairly sure that there will be a physical explanation for the phenomena in question, for an uninteresting terminological reason, namely that the concept of "physical explanation" will no doubt be extended to incorporate whatever is discovered in this domain, exactly as it was extended to accommodate gravitational and electromagnetic force, massless particles, and numerous other entities and processes that would have offended the common sense of earlier generations."

(Chomsky 1972, p.98)

This is comprehensible only if the concept of "physical explanation" is taken to be structural rather than material, that is, to refer to the determinateness of the explanation that eventually can be given, rather than to the elements which will figure in that explanation. And indeed the reference to "gravitational and electromagnetic force, massless particles" et seq bears out this reading, since what is shared by classical and modern physics is
structure of explanation, not constituents. And just as quantum
physics in requiring multi-determinate terminology has modified
the structure of explanations in physics, so it may be that
linguistic phenomena will similarly make inroads upon physical
explanations. What Chomsky is not prepared to accept, however,
is under-determination in explaining human behaviour.

5.10 How does Chomsky's theory of language relate to a theory of creativity?

Chomsky himself sees language-use as an example of creativity, and
at one level this is so: speech is often the generation of novel
and appropriate constructions. However when we describe someone as
creative, we generally intend to imply more than that he is able to
take part in conversation. We need not get too involved in this
at present, however: it is sufficient to note that what is at the
root of Chomsky's theory of language is the need to resolve, in
relation to language acquisition, what we have referred to earlier
as the paradox of creativity - that it appears to be rulegoverned
but not rulebound. His theory of language is an attempt to cope
with this paradox. His rejection of behaviourism is based on the
conviction that it fails to deal with the paradox, that reference
simply to environmental factors shaping verbal behaviour is inadequate
to explain that behaviour. Here he clearly means by "explanation"
some form of determinist exegesis. He argues that capacity for
language can only be explained as a manifestation of some internal
mechanism: he does not consider the alternative that a mechanistic
explanation may not be required at all. He shares with Skinner the
conviction that a mechanistic explanation of some kind is necessary:
what he does is to replace Skinner's external conditioning with an internal structure which equally determines behaviour. Chomsky, for example, compares the growth of language to the development of a bodily organ: he implies that the construction of language must be as rigorously delimited as physical cell development is constrained by the genetic code locked into the DNA structure. The effect of this shared belief in the necessity for a mechanist explanation of creative behaviour is to attract upon Chomsky's head the criticism previously directed at Skinner. Chomsky manages to provide an exhaustive determinist account of language only by virtue of postulating an inaccessible structure which is used to explain all language acquisition.

5.11 There has been a quantity of discussion as to what would constitute verification or falsification of Chomsky's theory. Chomsky sees no need to use experimental approaches or complex analysis techniques, as are normal in the behavioural sciences. He prefers simply to consider how a native speaker would interpret or reject a number of sentences formed according to varying transformational rules. He has said that he would accept that his theory would be rejected if an artificial language which violated some of the fundamental principles of language could be learned with ease and efficiency; but for practical reasons this is not subject to direct empirical verification (since it would not be practicable, even if it were to be considered ethical, to bring up a child from birth with no knowledge of any natural language, exposing him only to utterances in artificial language). Besides this would only disconfirm a specific formulation of the rules governing speech. Chomsky's theory
is therefore to a large extent unfalsifiable, and therefore inaccessible to scientific research.

5.12 There is however at least one situation which would tend to undermine the concept of innate linguistic competence: the successful teaching of an ape to use language creatively. This was forbidden by Chomsky in the following terms:

"It is a reasonable surmise, I think, that there is no structure similar to universal grammar in non-human organisms and that the capacity for free, appropriate and creative use of language as an expression of thought, with the means provided by the language faculty, is also a distinctive feature of the human species, having no significant analogue elsewhere."

(Chomsky, 1976, p.401)

A previous version of this statement in Language and Mind (1972) has a footnote referring a report by Ferster (1964) on the attempt to train chimpanzees to match number to objects. It is curious then that he makes no reference to the much-publicised case of the chimpanzee Washoe, who certainly prima facie appears to combine words spontaneously, appropriately and on occasion inventively. It used to be a fundamental tenet that men had language and animals did not: and yet the Gardners were able to rear a chimpanzee to converse in Ameslan, a sign language developed for the use of the deaf and dumb. The chimpanzee's achievements are impressive: after five years, she had a vocabulary of over 160 words, which she used singly and in combinations in a variety of conversational situations. From the beginning, she was spontaneous in her use of language, inventing signs as well as applying them in novel and unexpected ways. Other investigations of the same kind have thrown up similar results. Now
it is always open to Chomsky to revise his position to allow that chimpanzees, like humans, have access to universal grammar structures: the findings relating to apes do not disconfirm his theory, since, as has already been indicated, it is impervious to falsification. Nonetheless the ape studies do tend to undermine the persuasiveness of Chomsky's argument. In the circumstances, his omission to discuss these well-known ape studies which bear directly upon his thesis must be interpreted as defensive. The choice appears to be between assuming that chimpanzees also have innate cognitive structures relating to language use (which have hitherto been latent) or to reconsider alternative ways in which experience and consciousness interact to produce creative use of language.

ARTIFICIAL INTELLIGENCE

5.13 The most interesting research in psychology today is done not with rats but with computers. By utilising mechanical systems of data storage, retrieval and selection it is possible to attempt to represent various simple and not so simple human behaviours mechanically. The ability of psychologists to represent human behaviour mechanically would have vast implications for psychology and philosophy. The hypothesis from which artificial intelligence research begins is that whatever is orderly can be formulated in terms of rules. If the empirical findings of AI and their theoretical implications can be shown to disconfirm that hypothesis, then this in itself makes AI research vital and successful. The problem is that many though not all AI researchers do not realise that the above is a hypothesis under test but rather assume it to be a received truth on which the
validity of their efforts depends. AI researchers tend to find their failures uninteresting whereas it is their small triumphs which are unimportant. No-one would question that a human being can function like a computer so that the ability of a computer programme to reproduce one tiny human behaviour indicates nothing. But the failures of computer programmes are illuminating and important because of why they fail. In this section I want to look specifically at the model of "creative thinking" offered by Newell, Simon and Shaw. On the basis of work done on artificial intelligence, they propose that creative behaviour can be modelled. If the computer programmes they cite were adjudged adequate analogues of creative activity, in that they resulted in outcomes that we were willing to call creative, this would constitute a very powerful argument in favour of the thesis that a theory of creative thinking should consist of a complete operational specification for an organism that would, with appropriate initial conditions, think creatively; and would cast doubt on the view that creativity must be underdetermined.

5.14 Newell, Simon and Shaw begin by considering how to construct a working definition of creativity, which can be used to create computer programmes. Their working definition of creative thinking relates creativity very closely to problem solving, with the following additional conditions:

(i) the solution must have novelty;
(ii) the solution to the problem must have value;
(iii) the process of attaining the solution must modify or reject accepted ideas;

(iv) the process of attaining the solution must demonstrate high motivation and persistence;

(v) part of the task is to formulate the problem itself.

In other words, the criteria include those that we have already discussed of originality and value but exclude that of meaningfulness, substituting for it criteria of persistence and design of problem.

On the basis of these criteria, Newell, Shaw and Simon claim to have constructed programmes which simulate human problem solving i.e. the programmes solve the problems by using techniques and processes that resemble more or less closely those used by humans. Such programmes can discover proofs for mathematical theorems, compose music, design engineering structures or play chess. On this basis it is suggested that the programmes may be regarded as creative.

Each programme works on a very small scale within a highly precise and limited context. The goals are set by the programme i.e. ultimately by the programmer and all the required data and rules for processing the data are given. In both these respects (i.e. specificity of context and singularity of goal) the programme is unlike the human being. This should not be seen at this point as a criticism but rather as an observation which may have repercussions later. The narrow range of the typical AI programme is not a rebuttal: but it is important that we see in some way that what the programme can do can conceivably lead forward to the more complex activities in which we are interested.
There seem to me to be a number of difficulties in accepting Newell, Simon and Shaw's claim that they have provided the basis for a model of creative behaviour. The first lies in the working criteria of creativity selected. It seems to me rather odd that the assumption that creative thinking is a special kind of problem solving behaviour is passed without scrutiny, since, although certainly some kinds and parts of creative performance are undoubtedly a process of problem solving, much creative thought and action is, as it were, supererogatory i.e. appears to arise out of no particular need. One of the most puzzling aspects of creativity is in many cases its lack of necessity. Turner's "Fighting Temeraire", for example, or the Ballad of Kubla Khan, cannot be plausibly glossed as a solution to a problem, though the artist and poet respectively undoubtedly solved a number of problems as they set about their imposed tasks. Certainly once the creator knows what he wants to do he may be said to face the problem of getting the product "right". Turner, dissatisfied, may experiment with highlights until he gets exactly the effect he wants; Coleridge may cudgel his brains to retrieve an acceptable formulation of the last verse of his poem. But to describe the entire process of creative thinking as problem solving is to win the battle in advance for mechanist analogues, and thus for mechanist explanation, as we shall see.

The second difficulty is closely related. Even where the "problem" is defined, the person and the computer programme go about the business of finding a solution in very different ways. Newell et al consider an abstract model of problem solving behaviour in terms of
of a maze of which \( P \) is the set of all possible moves and \( S \) is the set of "good" (according to some set of criteria) moves. The generation of members of \( P \) which are possible solutions to the problem may be by trial and error search in arbitrary order, or by more sophisticated processes drawing upon heuristics which reduce the search time in particular circumstances. It is certainly true that this reflects some aspects of human problem solving, as Newell et al have verified by comparing the way the programme works with the actual reported processes of persons solving the same problems. But again this is very far from the generation of creative behaviour, which essentially consists of insightful response, not methodical progression. Newell et al are generalising from what may be an adequate analogue of some elements of all behaviour, creative or otherwise, which lean heavily upon the evaluation of alternative strategies, and counting out of alternatives. Such elements do figure in various kinds of problem solving e.g. consideration of the best chess move; solving of crossword puzzle anagrams etc. - but they are not typical of successful creativity.

Computer programmes operate essentially on the principle of exhaustive "counting out", modified as appropriate by heuristic devices which allow shortcuts to be taken. Humans, by contrast, operate on the basis of "counting out" or use heuristics only on specific occasions. A person engaged in creative activity may employ such strategies: a painter may consciously have to choose amongst shades of blue for a particular part of his painting, and even novelists when they get stuck have recourse to the Thesaurus to find the exact work they are looking for. And this is why we are brought back to the previous
point: the kind of search patterns exhibited by computer programmes are appropriate only when one knows exactly what is wanted, but not how to arrive at it; where all the possible solutions can be made explicit.

5.18 Now this is very far from the way in which creative persons work: (a) in producing something creative, the creator may have only the vaguest idea of what he wants to do; (b) he may have no idea at all how to go about his endeavour, but will recognise in a flash how to progress. The model of creative thinking preferred is grossly restricted: but more important it is restricted to those features of human problem solving which have little relevance to creativity. Whether in the arts or the sciences, creativity is not simply information-processing, resulting in the selection of a solution: rather, it is much more the production of a problem than its solution. The seeds of solution live in the act of defining the problem, of pulling together the relevant strands from which the solution will emerge. Newell, Shaw and Simon themselves recognise that they by no means have a complete analogue of human creative thinking: but the way in which they consistently restrict the field which they seek to explain entails that they can provide an analogue only for those parts of problem solving which are least paradigmatically creative: and that the aspects outside their restricted range can never be explained, even with extension and development of their models. The mechanical explanation of problem solving does not begin to cope with elements of creative behaviour such as its supererogatory nature and its insightful nature. These contribute to the non-arbitrary but non-predictable nature of creative activity and production.
5.19 We have now looked at three different approaches to a determinist theory of creativity. None of them have been considered to function adequately as explanations of creativity. We are brought back therefore to the paradox of creativity, and to consider whether the tension between the rulegoverned and non-rule-bound qualities of creative behaviour can be resolved other than by denying the non-rule-bound side of the paradox, that is, whether it is possible to develop a theory of creativity which allows for non-determination. The difficulty with all the determinist accounts considered is that in some way or another the model that they offer of creative behaviour is inadequate: either the model is vacuously large and nonexplanatory (Skinner); or unchallengeable but unlikely (Chomsky); or far too narrow and perhaps misleading as regards the kind of behaviour it offers as explanation (artificial intelligence programmes). The common feature of all these explanations is that they attempt to proffer a linear explanation of the creative process, in which each step is conditioned by the previous one, in other words, an explanation without gaps. We have to begin to consider why this sort of explanation does not seem to be adequate. The inadequacy seems to be (a) as regards the impetus to creativity - why does creativity happen, or what causes the itch to which the act of creation is the response?; and (b) as regards the essentially insightful element of the creative response. In both cases, there does seem to be a need for gaps in the explanation: or rather, where there are no gaps in the explanation, there appears to be something wrong with it as an explanation of creativity.
Earlier we identified the agent, the "integrating I", as an important factor in the ascription of creativity. The gaps could be categorised as spaces in which the "I" has room to function, spaces where there are choices to be made rather than paths to follow, spaces where the "I" is not pushed along to the next step but takes action of its own volition. Choice rests on being able to do otherwise. The concept of an action implies this kind of freedom - and this is in turn bound up with consciousness.

Without the "I" there is no action and no free behaviour. For example, in Arthur Koestler's novel *The Callgirls* a mechanically controlled psychopath is the prize exhibit of a grotesque Behaviourist at a convention: as events turn out her behaviour is not chosen but determined. A controlled psychopath who cannot act other than unaggressively cannot act at all: where there is no freedom to choose, there is no action, only performance; there is no responsibility, only events. Now we need the concept of responsibility when we are considering human actions: we need it to make sense of praise or censure. In jurisprudence, this mode of thought has been much developed, because of the urgent practical need to measure the relationship between the outcome of events and liability to punishment. A person is not liable (or not much liable) if events were outwith his control for external reasons (if the brakes of my car fail to operate, for example, I am less liable in respect of an accident than if I stepped on the accelerator instead of the brakes, or *a fortiori* if I deliberately accelerated); or if events were outwith his control by virtue of his mental circumstances. The McNaghten Rules are an attempt to recognise and cope with this aspect of responsibility. Where the
apparatus for making decisions is permanently or temporarily defective (i.e. the individual could not, not just did not, take decisions regarding his action) responsibility is held to be diminished. In all other cases it is held that I could have done otherwise, that the responsibility is mine. In the case of moral action, the kind of autonomy we have been considering also arises, therefore: although actions which are morally judgeable are rational (i.e. dependent on rules) they must nonetheless be free at the level of decisions, of consideration of rules.

5.21 Perhaps now we can see more clearly what is involved in this paradox of creativity. Again we must allow room for responsibility, though here we are looking for the opportunity to applaud critically a work of creation, not to censure it morally. The element of judgment is however vital in both cases, and just as the action of a person who can be adjudged not morally responsible for his action is held to be one for which it is, to some degree, inappropriate to hold him liable, so an act of creativity to which we cannot attach personal responsibility is not one which we can evaluate. The process therefore does not have a meaning separate from the person. If the process is a necessary one, that is, determined all along the line, then there is no room, and no need for the person, and therefore no possibility of credit or censure. The gap is necessary in order to bring into play the vocabulary of judgment: an explanatory model with such gaps is therefore not weak, but necessary. Such a model alone can be adequate.
The best known explanatory schema of this kind (indeed perhaps the best known explanatory schema of creativity) is that of Arthur Koestler. Koestler resolves the paradox of creativity by hypothesising that creativity occurs when conscious controls are temporarily relinquished, liberating the mind from certain constraints which are necessary to maintain the disciplined routines of thought but which may block the creative leap. As these constraints are removed, other more primitive types of thinking come to the fore. (There are certain research findings that may go some way to providing empirical confirmation of this suggestion, e.g. findings by Roe, Mackinnon, Hudson that creative people are significantly more open to the promptings of their subconscious.) At the critical moment two different planes of thought (or matrices as Koestler calls them) are simultaneously active in the creator's mind and are perceived together (biseconds).

"We learn by assimilating experiences and grouping them into ordered schemata, into stable patterns of unity in variety. They enable us to cope with events and situations by applying the rules of the game appropriate to them. The matrices which pattern our perceptions, thoughts and activities are condensations of learning into habits ... There are two ways of escaping our more or less automatised routines of thinking and behaviour. The first, of course, is the plunge into dreaming or dreamlike states, when the codes of rational thinking are suspended. The other way is also an escape from boredom, stagnation, intellectual predicaments, and emotional frustration - but an escape in the opposite direction; it is signalled by the spontaneous flash of insight which shows a familiar situation or event in a new light, and elicits a new response to it. The bisociative act connects previously unconnected matrices of experience; it makes us understand what it is to be awake, to be living on several planes at once..."

(Koestler, 1969, pp.44-5)
The effect of bisociation is rooted in both matrices (and thus comprehensible in their terms) and beyond each, taken individually. Any move within the matrix can be explained exhaustively, by reference to the code which governs the matrix. At the moment of flight out of the matrix however the code of that matrix is insufficient of itself to account for the act. Epistemologically a creation results from the fusion of separate planes of thought; operationally the impetus to insight comes from a relaxation of the constraints of the straightforwardly rational conscious mind and the consequent welling up from the unconscious of apparently irrelevant ideas. Taken together, a morphogenesis of creativity is provided which can cope with the central paradox of creativity. The fusion cannot take place without "ripeness" i.e. without the potential creator's being prepared with the skills which will enable him to capitalise upon the moment of insight. But ripeness is a necessary but not sufficient condition of creativity: without that moment of relaxation out of which is born the realisation of possible connection, preparedness can never result in creative outcome.

5.23 Koestler's theory of creativity is linked to a general antireductionist concern which can be traced throughout his writings. Both in his fiction and his theoretical writings, he has consistently emphasised his conviction that the objective universe of discourse, of cause and effect, is not the only mode, that human action cannot be reduced into objective terms. In order to maintain the separate ontological status of the personal, he argues for the irreducibility of explanation at one level to a lower level, and especially for the
irreducibility of explanation of biological events into simple cause and effect terms. He stresses the fallacy of such reductionism, whether he is arguing against behaviourism or for the indispensability of the "grammatical fiction" of "I". There are two aspects in which Koestler's model of creativity is antireductionist: in the first place, in his insistence (in some ways an echo of Freud's) upon the ability of the unconscious to throw up fertile analogies, if given the opportunity; and in the second place, in his delineation of "the moment of truth" (which may nonetheless be mistaken truth, since verification of the inspiration can occur only after the event, and by exhaustive checking) as being spontaneous, a leap out of the prison. Both elements in the creative process are irreducible into causal terms: both depend on the existence of the "grammatical fiction" as a nonobjective participant.

5.24 Koestler clearly sees the act of creation as being specifically rooted in the "grammatical fiction", the non-reducible element in human beings. In a sense, the act of creation is a celebration of that element:

"the creative act, by connecting previously unrelated dimensions of experience, enables him to attain to a higher level of mental evolution. It is an act of liberation - the defeat of habit by originality."

(Koestler, 1969, p.96)

Beyond this land of formulation, however, Koestler is unwilling (because of his antireductionist stance) to go further into the provenance of the creative act. At one point, he likens the sudden emergence of a new insight to:
"an immersed chain, of which only the beginning and the end are visible above the surface of consciousness. The diver vanishes at one end of the chain and comes up at the other end, guided by invisible links."

(Koestler, 1969, p.211)

It is not contingent for the purposes of Koestler's schema, but necessary, that the chain be invisible: if it would be raised up into sight, the under-determination of the creative moment of insight would evaporate. The image of the chain is in many ways an ill-conceived one, since it opens the way back to a purely causal account of creativity, rather than the integrative account which Koestler is otherwise at pains to elaborate: his use of the image is perhaps most charitably not made much of; but it is an interesting slip on his part, an indication of a residual longing for the completeness of a causal explanation.

5.25 It is interesting that very similar feelings of antipathy to the dogmatism of totalitarianism brought Koestler to an antireductionist stance in defence of the individual; and brought Karl Popper instead to an emphasis on objectivism, arguing that conjectures about the world must be in a form which makes them refutable. Accordingly, Popper is unsympathetic to an antireductionist stance as regards the explanation of creativity; though he makes no difficulty about "consciousness" (he regards behaviourist attempts to dissolve references to internal states as ridiculous), his approach to creativity is robust:

"What characterises creative thinking, apart from the intensity of the interest in the problem, seems to me often the ability to break through the limits of the range - or to vary the range - from which a less
creative thinker selects his trials. This ability, which clearly is a critical ability, may be described as critical imagination. It is often the result of culture clash, that is, a clash between ideas, or frameworks of ideas. Such a clash may help us to break through the ordinary bounds of our imagination ... what is essential to 'creative' or 'inventive' thinking is a combination of intense interest in some problem (and thus a readiness to try again and again) with highly critical thinking; with a readiness to attack even those presuppositions which for less critical thought determine the limits of the range from which trials (conjectures) are selected; with an imaginative freedom that allows us to see so far unsuspected sources of error: possible prejudices in need of critical examination."

(Popper, 1976, p.47)

Though this exposition is in many ways similar in content to Koestler's approach, it is crucially different as regards the dynamic of creativity. Popper eschews any kind of miracle at the moment of insight, whereas for Koestler the leap is its most characteristic feature. Rather, Popper views creativity as exemplifying a logic of discovery: while accepting that ingenuity and imagination play their part, he emphasises the logical processes of change through error elimination, and considers that creativity can be fitted into the pattern of dogmatic theory formation, followed by a phase of rigorous criticism in an attempt at error elimination, leading in turn to revised theory formation et seq.

5.26 An immediate difficulty with this is that it appears to explain too little: to say that highly creative people are simply more persistent than other thinkers seems at odds with much of what we know about the way in which creative discoveries come about. The concept of bisociation, though in many ways vague and untestable, seems a more satisfactory characterisation of the creative process. The same
kind of dissatisfaction with Popper's delineation of the logic of
discovery as a complete answer to the development of scientific
thought is behind Kuhn's *The Structure of Scientific Revolutions*,
where it is argued that revolutionary breakthroughs are not simply
logical developments but supra-logical and discontinuous restructuring
of the accepted universe of thought. Kuhn's paradigms and Koestler's
matrices have much in common: both stress the revolutionary rather
than evolutionary nature of creative breakthrough. Popper is of
course much interested in Darwinian (or neo-darwinian, since he has
himself developed a modification of the original theoretical line)
theory as a metaphysical research programme, that is, not itself
a testable scientific theory but a possible framework for testable
scientific theories. Implicitly, his theoretical approach to
 creativity is neo-darwinian: the selection by the environment of
randomly generated attempts at solutions.

5.27 This is explicitly developed in a paper by Briskman,
"Creative Product and Creative Process in Science and Art", in which
an attempt is made to deploy a darwinian dynamic as explanation of
the creative process. The essential difficulty for an explanation
of the creative process, says Briskman, is to make rational sense of
the possibility of creativity, without dissolving its miraculousness,
by arriving at an explanatory form which would explain its necessity.
This is presumably an alternative formulation of the point made at
the beginning of this chapter, that the creative product is rule-
governed but not rulebound. Briskman rules out (somewhat rapidly)
the possibility of any mechanist account being able to deal with the
emergence of transcendent products, and similarly rejects randomness
as a nonexplanation. Instead, he offers a darwinian combination of
blind variation and selective retention. Such a theory of plastic control, he suggests, does not explain too much—by guaranteeing success—thereby removing the element of the miraculous, but it does introduce an element of conscious control to supplement the blindness of random generation. Such an explanatory schema certainly avoids explaining away creativity by making it necessary given the antecedent conditions; but it is difficult to see how this theory greatly illuminates the possibility of creativity. The emergence of a transcendent product is not ruled out by this darwinian account, as it would be by a mechanist account: but we are not taken much further forward in terms of explanation of the emergence of the transcendent product. A schema of random generation and selective retention simply pushes back the unexplained element a stage: whereas Koestler's account leaves the whole process of bisociation unexplained, Briskman's leaves unexplained the process of selective retention. Whereas Briskman's theory can explain the generation of the transcendent product, it does not illuminate the crucial recognition of one product as being worthy of retention amidst the others to be discarded. And it is difficult to see how this could be otherwise: if selection criteria could be given in advance, the product could not be transcendent, could not be miraculous.

5.28 It seems then, that the alternative to a mechanist account of creativity (which, in the various versions considered, fails to do justice to the process of creativity) is some account which, while doing justice to the non-rulebound nature of creativity, must be essentially suggestive rather than exhaustively delineated.
CHAPTER 6

EMPirical strategies:

The statistical difficulty
CHAPTER 6

EMPIRICAL STRATEGIES:
THE STATISTICAL DIFFICULTY

THE ROLE OF STATISTICS

6.01 The aim of the use of statistics in psychological research is to assess in a systematic way the results derived from experimentation, and thereby to draw conclusions about the subject material. The importance of statistics in psychology springs from the desire to be properly scientific; to organise a mass of disparate information about persons into coherent form. Lord Kelvin's dictum "If you cannot measure, your knowledge is meagre and unsatisfactory" makes it clear that the ability to quantify is the passport to scientific status. R.A. Fisher said of statistics that it is "principally by the aid of such methods that (social) studies may be raised to the rank of success". Thus the desire to conduct the business of psychological research in such a way as to allow statistical manipulation to be used is a powerful determinant of the shape of psychological studies. Just as the need to introduce order into the investigation of psychological phenomena prompted emphasis on objectivity in experimentation, so that the results produced can be categorised, and replications can be done with confidence, so the need to introduce order at the next stage prompted emphasis on statistics to simplify the mass of data obtained by experimentation; and to facilitate the drawing of conclusions from the data.
In part, the use of statistical devices enable the researcher to summarise his data into a few figures. Also, the researcher is usually looking to find relationships or differences in the data: for this purpose he will need to apply statistical techniques to find out (a) whether such a relationship or difference is revealed, and if so how strong it is; and (b) how likely it is that this is not simply a chance finding. The most usual statistics in psychology are therefore (a) correlation coefficients and (b) significance levels.

6.02 Statistics is an exact science, based not on experience but on mathematical theory. The application of statistical theory to data cannot make that data valid or invalid, cannot add or take anything away. Statistics is neutral with regard to data. Its value is that, through sifting and focusing, the data can be clarified, arranged into a more meaningful (but not more accurate) pattern. Thus Lord Kelvin's dictum should not be stood on its head to read "If you measure, your knowledge will be meaningful". The mania for data collection without regard to theory building arises from the conviction that, because measurement is the hallmark of scientific research, measurement is all that is required for scientific research. But the validity of measurements (and a fortiori the manipulation of measurements) is in proportion to its adequacy in measuring the material under study.

6.03 The role of statistics is a secondary one: it begins and ends at the level of number, and never connects directly with experience. The crucial part of research is experimentation: however, statistical
analysis can perform a useful function by telling the researcher
more about the results obtained by experimentation, especially about
the extent to which the results obtained bear out the theory being
tested.

THE THREE DOMAINS

6.04 When a researcher uses statistics, he bridges three domains:

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<tr>
<th>A</th>
<th>B</th>
<th>C</th>
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<td>domain of</td>
<td>domain of</td>
<td>domain of</td>
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<tr>
<td>experience</td>
<td>measurement</td>
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<td>(the &quot;real</td>
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<td>manipulation</td>
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<td>world&quot;)</td>
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by experimenting in domain A, transcribing the results of his work
into scores in domain B and then translating these into pure numbers
to be manipulated in domain C. Statistics begin and end at the
level of pure number, and never connect directly with experience:
yet statistics are useful to the researcher only as a tool to be
employed in his investigation of the real world. What is established
by mathematical manipulation is a numerical outcome, not an empirical
one. Mathematics cannot make the answer any righter than the raw
data it has to work upon: but it can help the researcher to compare
the hypothesised relationships between variables and the patterning
of observed value distributions of measurements made. As a result,
he is able to make decisions within domain C about the null hypothesis,
and consequently provisionally to confirm or modify or disconfirm the
theory posited about domain A.

6.05 In order for the outcome to be valid, the transposition between
domains must be properly made. Events in domain A must be adequately
reflected in measurements in domain B, which must be adequately treated
as numbers in domain C. Stevens defines measurement as "the assignment of numerals to objects or events according to rules". Objects and events are not in themselves directly measurable; decisions about boundaries and frequencies must be made. The properties of a numerical series in domain B, which determine how the numerical series may be handled in domain C, are dependant upon the empirical operations possible in domain A. If there is isomorphism between the properties of the numerical series and the kind of empirical operations which we perform with the material under study, the series can be used as a model to represent aspects of the empirical world.

<table>
<thead>
<tr>
<th>OPERATION POSSIBLE AT A</th>
<th>SCALE AT B</th>
<th>OPERATION POSSIBLE AT C</th>
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<tbody>
<tr>
<td>classification as the same sort of thing ( x' = x )</td>
<td>nominal</td>
<td>number of cases</td>
</tr>
<tr>
<td>ranking ( x' = x - n )</td>
<td>ordinal</td>
<td>mode</td>
</tr>
<tr>
<td>ranking with equal intervals ( x' = ax + b )</td>
<td>interval</td>
<td>contingency correlation</td>
</tr>
<tr>
<td>ranking from zero with equal ratios ( x' = ax )</td>
<td>ratio</td>
<td>median</td>
</tr>
<tr>
<td></td>
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<td>percentile</td>
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<td>rank order correlation</td>
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<td></td>
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<td>product moment correlation</td>
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<tr>
<td></td>
<td></td>
<td>coefficient of variation</td>
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<tr>
<td></td>
<td></td>
<td>variation</td>
</tr>
</tbody>
</table>

In psychology, accurate knowledge of equality of intervals and establishment of zero is largely lacking. Much of the data yielded by psychological tests is at the ordinal level: results are ordered, but the linearity and the fixity of the intervals are open to question.
In order to test out a theory about events in the real world, researchers employ measures to a sample population. Both the sample and the measure are artificial devices, used to represent in manageable form the real world. Insofar as these devices are satisfactorily isomorphic with the real world, the data obtained and the results which are drawn from that data will also be satisfactory.

The sample from which the raw data is obtained must be sufficiently large and sufficiently representative for the statistical manipulation which is to follow. The answer to the question of how sufficient is sufficient will depend very much on the practicalities of the situation, on what sort of manipulation is intended, and the state of the research field. Generally speaking, the larger the sample the better; and the smaller the sample that one has to make do with, the more care is needed to ensure that it is representative. Sampling is based on choice and selectiveness: it is obviously impossible, for practical reasons to test the entire population, and wildly risky to test only one individual, who may not be at all typical in his responses. It is therefore important to choose a sample in which as far as is known all relevant aspects of the population at large are represented in the appropriate quantities. This can never be done perfectly, since that would imply a first hand, first class knowledge of the population: if the researcher had this he would not need to undertake research. The larger the sample taken from a population, the greater the probability that the data obtained from the sample will be within a given margin of the parameter in the
population at large: the larger the sample, the less will be the variability in the data, because the unusual incidences assume less importance within the total incidences. In most cases, however, large samples are practically or economically difficult to obtain: and in such cases, a tradeoff will have to be accepted between sample size and accuracy of sample statistics as estimators of the population parameters. If you have doubts as to whether your sample can be in all relevant respects similar to the total target population, and you cannot enlarge your sample, you are thrown upon the techniques of randomisation developed by Fisher in the field of plant breeding. Fisher's work has proved attractive to social scientists because he offered a way of scientifically handling small samples and of bypassing long term replication and control groups. However the extension of statistical techniques developed in the field of plant breeding to psychological research should be treated with caution. The lynchpin of Fisher's experimental procedure is the elimination of intrusive other variables by distributing all inequalities randomly: this should result in a standardised sample. If an experimenter can be sure by using such techniques that his one and only sample is standard (or failing that, that the degree and manner of its atypicality is known through analysis of covariance) then he need not expose himself to the interpretative risks inherent in either temporal replication or control matching. But this is only possible if the experimenter can be confident that his efforts to randomise the situation are adequate to his purpose, that he has taken all possible relevant factors into account. Especially where the total population is all human beings, this is extremely difficult.
Theories about variables in the empirical world have to be investigated by the selection of indicators, their measurement and the assessment of the resultant data. The construction of a test to measure a psychological variable is therefore crucial. We have already noted the difficulties which can be raised about the validity of some "creativity" measures used for example by Guilford and others. As far as statistics are concerned, if there is reason to doubt the isomorphism of data and events because of doubts about the validity of the measure chosen, no matter how well performed the statistical manipulation, it cannot make the results any more valid. The validity of the test is a determinant of the validity of the scores for processing: and, as we have seen, comparatively little work has gone into the validation, as opposed to the construction of tests of creativity. The test must be unidimensional, or the scores will measure more than one thing indiscriminately; it must be broad enough, or the score range will be distorted; it must be precise enough, or the score range will be distorted in a different way. A test which is well developed to measure a particular variable can yield data which can be assessed with some confidence.

It may be easier to see what is meant by the importance of isomorphism between domains A and B by looking at a specific case. Getzels and Jackson wished to investigate the thesis that creative giftedness was not identical with high intelligence. They began by testing
a sample for intelligence and creativity. As a measure of
the first they used either the Stanford-Binet test, the Henmon-
Nelson test or the Wechsler Intelligence Scale for Children.
As a measure of the second they used a number of tests based on
the battery produced by Guilford's team, which tested ability to
deal inventively with verbal and numerical symbol systems and with
object-space relations. The sample to which these tests were
given was drawn from children attending a Chicago private school:
the children came from white, white collar homes, and were all
bright (the mean IQ was 132). In all, the final sample consisted
of 245 boys and 204 girls. The sample is larger than most in
psychological experimentation: but, given that Getzels and Jackson
are attempting to test a thesis about the general population, it
is hopelessly unsatisfactory in terms of representativeness. It
is very homogeneous, in social background and intelligence. The
advantage of the homogeneity is that variation in scores can be
assumed with some confidence not to be the result of these extraneous
variables. The drawback which is critical in this case, is that,
once you have in effect eliminated a factor (such as intelligence)
by choosing a homogeneous sample, you are not entitled to attempting
to discriminate within that sample as regards that factor: the
distinctions between high IQ and low IQ subjects brought out by the
intelligence tests used are simply swamped by the overall similarities
amongst all the subjects on this variable. A further disadvantage
is not technical but theoretical: Getzels and Jackson are not
entitled to postulate that results derived from such a homogeneous
sample are generalisable to the population at large. From the start,
therefore, the practical advantages they derive from using the
sample chosen limit the possibilities of their research. Further
difficulties arise over the next stage of their research. From
the responses to the two sets of tests, two groups are sorted out:
(a) subjects in the top 20% as regards creativity but in the bottom
20% as regards intelligence; (b) subjects in the top 20% as regards
intelligence but in the bottom 20% as regards creativity. In spite
of starting with a total sample of 449, Getzels and Jackson manage
to select only 26 and 28 subjects respectively for the groups.
Any group with under 30 subjects makes statistical manipulation less
reliable: the groups chosen are therefore on the borderline of
what is scientifically acceptable. Worse than their smallness,
however, the groups are, in spite of all, suspiciously like one
another, because of the homogeneity of the subpopulation from which
they have been sorted out. Because the rest of the research
programme consists of comparing the performance of the two groups on
a variety of factors - scholastic achievement, motivation, career
aspirations and parental variables etc. - the suspicion that the
two groups are not all that dissimilar on the test variables is
crucial. From the point of view of the sampling practice alone,
there are good reasons for being extremely circumspect about this
piece of research.

6.10 There are also difficulties about the measures used, as has been
noted earlier. The "creativity" measures used are at best measures
of originality of a certain kind, or certain kinds (since the
correlation between the five measures is low). It may be therefore
that the isomorphism between the test performances and events in
the natural world is limited. Now neither this nor the doubts
expressed earlier about the sampling matters very much if, as is
the case here, the research is of a highly tentative nature: in
order to arrive at a first-approximation approach to the question
of creativity, researchers had to work with whatever tools were
available. Whatever the technical shortcomings of Getzels and
Jackson's approach, it was in many ways a valuable and innovative
piece of work, in that it opened the way for further studies.

A SECOND EXAMPLE

6.11 Researchers into creativity often choose samples from school
children or students, as has been explained earlier, because they
are the easiest to get hold of. We have already considered the
built-in difficulties of a school situation as an influence on the
results obtained. A relatively homogeneous sample, obtained from
using a narrow band of schoolchildren, has drawbacks, as we have
seen: but whether these drawbacks matter a little or a lot depends
on how the sample is used. Compare the following with Getzels and
Jackson's sample:

"No attempt was made to ensure that the schoolboys
and girls taking my tests were a representative
cross-section of English boyhood or girlhood. In
several respects, in fact, they were atypical.
All were above the national average in intelligence
and academic ability, and the majority stood a good
chance of reaching university should they wish to
do so. Most fell within the top five per cent
of their age-group, academically speaking; all,
or almost all, within the top 15 per cent ... The
sample was biased, too, both geographically
and socially. With the exception of the students
mentioned in Chapter 8, all were at schools in the South of England; and the large majority came from lower-middle, middle, or upper-middle-class homes. Relatively few were from the working class; and only one or two were more than vaguely aristocratic.

Except in the most exploratory of the studies, however, I did my best to establish a balance between the public and private sectors of the educational system. Unless stated explicitly in a footnote, each result was based on at least two (and sometimes four or five) independently collected samples...

(Hudson, 1968, Appendix 4)

Where Hudson differs from Getzels and Jackson is in his awareness of the implications on his research findings as a whole of his sampling. He recognises that the limited isomorphism between his individual samples and the population as a whole must inhibit his confidence in the extent to which the scores obtained are appropriate to the whole population.

6.12 There are then two main respects in which researchers can maximise their chances of their scores in domain B being isomorphic to the situation in domain A:

(i) the sample from whom the scores are elicited should be as representative as possible of the population;

(ii) the measure by which the scores are elicited should tap as validly as possible the variable under study.

RELATION BETWEEN DOMAINS B AND C

6.13 Once scores have been obtained, the researcher must analyse the raw data into something more meaningful which will enable him to come to a conclusion about the theory he started off with. As described above, a great many of the scores yielded by psychological research
are, explicitly or implicitly, ordinal scales. The question then arises as to how such scores should be handled. Numbers separated from context have the appearance of ratio scales: their limitations are not apparent. Now, the most powerful statistical techniques for analysing data are parametric, that is, they are predicated on sampling from a normally distributed population and assume scores are on at least an interval scale. One point of view is that the results of measurement operations are sets of numbers, and statistical techniques are methods for drawing inferences about sets of numbers: once numbers are available, the statistician is free to proceed with his method without bothering about any outside considerations, so that no properties of the scale of measures need have any relevance for statistical operations. Against this, it is argued that where the test assumptions are in fact violated, distortion sets in if the test is used. Where data yields only rank ordering, transformations change the shape of the distribution so that it loses structure and meaning and the result fails to mirror the empirical domain.

"When the assumptions constituting the statistical model for a test are in fact not met, or when the measurement is not of the required strength, then it is difficult if not impossible to say what is really the power of the test... Although some empirical evidence has been gathered to show that slight deviations in meeting the assumptions underlying parametric tests may not have radical effects on the obtained probability figure there is as yet no general agreement as to what constitutes a 'slight' deviation."

(Siegel, 1956, p.20)

In view of these doubts, Siegel argues for the more widespread use of non-parametric tests, which make no assumptions as regards the
population distribution, and can be used with scores which are not numerically exact. Additionally, they can be used with confidence with smaller samples, and are in many ways simpler than the higher powered parametric tests. They are, however, undoubtedly less powerful in the technical sense, tending to lead the researcher into type II errors. Because they take data to be ranks rather than full blown numbers, the nonparametric tests discard some of the information implicit in the data as not really known but simply assumed. In this, they are probably more realistic than their parametric counterparts, but on the other hand they are weaker when it comes to arriving at conclusions: they enforce tentativeness on the researcher. A researcher may choose a parametric test in spite of having only ranked scores, on the grounds that research has shown that both the t-test and the f-test are relatively invulnerable to violation of their underlying assumptions, providing that the samples are large enough, and are similarly distorted. The question remains: how invulnerable is relatively invulnerable, how minor is a minor distortion? It can be answered only in the light of individual circumstances. Theoretically, when material is not up to the standards required for parametric tests, it is preferable to use a non parametric test, because it is more appropriate, even if no great harm may come of using the stronger test. The unconsidered use of theoretically unsuitable tests is dangerous, in that it leads the researcher into misregarding his data. Again, though the power of a parametric test is appealing, there are advantages in a weaker test which more readily preserves one's data, in that it can reinforce the researcher in a tentative attitude towards his data, and a provisional stance towards the theory
under test. This is particularly appropriate, as has already been suggested, in psychological research.

CORRELATION

6.14 In the majority of research projects, what the researcher will be concerned to derive from his data is an indication of a relationship between two indicators, as confirmation of a postulated relationship between two variables in the natural world. Correlation coefficients are calculated mathematically from the available data: but a numerical process can provide only a numerical answer. The extent to which the numerical answer can be translated back to reality depends on the isomorphism at the various stages. Correlation coefficients can be calculated in a variety of ways, depending on the status of the available data: they are expressed as a value in the range between -1 (perfect negative correlation) and +1 (perfect positive correlation). The closer a coefficient value is to zero, the less is the correlation between the two sets of data. Where it can be assumed that there is a linear relationship between the two variables, and that the data is distributed normally, strong statistical techniques can be used. Where however such assumptions cannot be made, less powerful techniques, such as Spearman's $p$ (for rank correlation), contingency coefficient (for variables for which only categorical information is available) and the chi-square test (which, although developed primarily to test divergence of observed from expected frequencies, can also be used in contingency tables to determine the extent of correlation as chance departure from zero) are needed.
SIGNIFICANCE

6.15 The other main use that psychological researchers have for statistics is to gain some idea of how much confidence they can have in the results they have found, i.e. how likely the results are to have arisen by chance. Again the kind of test for significance that can be employed depends on the status of the data (though, as we have noted earlier, techniques assuming normally distributed data will have little distorting effect on data which is not normally distributed, provided that it is not too aberrant). At this point the researcher is seeking to disprove the null (or converse) hypothesis to the one being tested: indeed, classically every experiment should be seen as a chance to disconfirm the null hypothesis, or, in Fisher's words, "every experiment exists only in order to give the facts a chance of disproving the null hypothesis". However, confirmation or disconfirmation of the null hypothesis is not the basis for conclusive decisions about the outcome yielded by the data, but rather affects the strength of one's belief in the hypothesis under test. The null hypothesis is not a touchstone of truth, but a tool, to aid perspective and to be provisionally accepted only. The experimental outcome is assessed against the theoretical outcome in an attempt to gauge its likelihood: but likelihoods are not certainties. For any experiment, the null hypothesis states that the observed results could have arisen simply on a chance basis: tests specify the probability (lying between 0 and 1) of a given event happening by chance, and thus (depending on the level of significance chosen by the researcher) give the opportunity to rule out the null hypothesis, opening up the possibility that the original hypothesis
may be true. The researcher specifies in advance the level at which he will accept that the given result did not happen by chance: the selected test will yield a figure measuring the actual result's deviation from a theoretical result, which can (by reference to conversion tables) be expressed as a level or probability of this result occurring out of all the possible results which could have occurred. Wherever possible and appropriate, a two-tailed test of significance, which does not make any assumption about which end of the theoretical distribution of possible outcomes the region of improbability which would sanction rejection of the null hypothesis would lie, is to be preferred, as more conservative. The most commonly used tests of significance are the Z scales, the E scale (which can be used for small samples) and the f test (all of these assume parametric data); and there are a number of nonparametric tests, of which the chi-square is probably the most used by psychologists.

6.16 Back to examples. Getzels and Jackson arrive at scores for creativity for their students by summing the individual scores achieved on each of the five measures used: the total scores therefore reflects a mixture of incidences (both correct answers - to the Hidden Shapes test - and acceptable responses - to the Word Association and uses for things tests -) and ratings (on the Fables and Makeup Problems tests). Two subjects could therefore have identical summated scores but very different profiles over the five tests. This might not matter, but the relatively low intercorrelations among the five creativity measures is worrying in this
respect. The summated scores cannot, given their origins, be assumed to be normally distributed. The "creativity" score pattern is then compared directly with the scores on the IQ tests: while the Stanford-Binet test has been carefully standardised on the population as a whole, the fact that all the subjects in the sample score at the top end of the scale may mean that the distribution within the sample may not be particularly normal. The resultant two groups (High IQ and High Creative) derived are then compared on a number of other factors.

(a) School achievement: significant differences between each group and the total population (of 449) were found, employing the t-test of significance: given the nature of the measures of school achievement, this is an acceptable use of a parametric test;

(b) teacher-preference: significant difference was found between the High IQ group and the total population: again the t-test is used;

(c) need for achievement: no differences were found;

(d) personal qualities: a measure calling for ranking of descriptions was used, and subjects were asked to rank the descriptions in the order that they would like to be like them, that they believed teachers would like them, and that they believed that people with these various qualities would succeed in adult life. This time, Getzels and Jackson use a nonparametric technique, rank-order correlation, to examine difference between groups.

(e) fantasies: the frequency of certain characteristics (e.g. humour, violence, unexpectedness) in responses to the n Achievement test was compared, using the chi-square test, and significant differences found.

(f) career aspirations: t-test use reveals differences, in the number of occupations mentioned by each group; chi-square test use reveals differences in the number of unusual occupations mentioned.
There is nothing particularly wrong with any of these, particularly as the conclusions are treated very speculatively. A number of objections would strictly be raised about the use on occasion of the t-test but the significances found are clear enough for there not to be the possibility that the data was widely biased by the use of too-strong tests. Nonetheless there is an underlying carelessness as regards data's origin in the way that it is on occasion treated by Getzels and Jackson, a sense that numbers are numbers, no matter how derived. But as we have noted earlier, Getzels and Jackson are blazing a trail, and delicacy is not to be expected.

6.17 As a matter of interest, let us again draw some comparisons. In general, Hudson is much more sensitive to the relationship between the origins of data and the way in which it should be manipulated. He specifically notes the effects on significance level of small samples; and is inclined to be tenderminded as regards the use of parametric techniques on non-parametric data. He therefore consistently uses nonparametric tests to arrive at correlations: usually the simple matching coefficient, or (particularly in his earlier research) an estimate of the strength of association in terms of proportions of sorts to missorts. He uses the chi-square test as a rule to test for significance. In this he is somewhat unusual: most psychologists in the field of creativity research take a more robust attitude to their data, and parametric tests are nearly always used. Barron, for example, regularly uses f or t tests to calculate significance, though his data is usually nonparametric.
CONCLUSIONS

6.18 What does all the foregoing suggest for the maximisation of a researcher's chances? He should

(a) base his study on as large and as representative a sample as possible;

(b) employ measures which are appropriate to the variable under study;

(c) have regard to the kind of data which has been yielded in deciding upon what kind of statistical tools to employ.

All of this is commonsense: and in broad outline nearly all of the research into creativity which has been reviewed conforms. However there is a need to appreciate that failure to have regard to these principles vitiates the value of findings, because the potential isomorphism with the natural world is limited.

6.19 A more general conclusion emerges, namely that an open approach, taking as broad a view as possible of what might be taken from the evidence and keeping at all times an eye on theoretical implications, is much preferable to a narrow search for evidence to prove or disprove one's case. There are no discontinuities in the way statistical techniques work: no conclusive calculations, but only a greater or lesser degree of likelihood. What emerges from statistical manipulation then always contributes to a provisional view about events in the natural world.
CHAPTER 7

CREATIVITY AS A PSYCHOLOGICAL CONCEPT AND ITS IMPLICATIONS FOR PSYCHOLOGICAL METHODOLOGY
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CREATIVITY AS A PSYCHOLOGICAL CONCEPT

7.01 For reasons that have become apparent, the study of creativity is not straightforwardly accomplished within the framework of mainstream psychological research strategies. It could be concluded that there is therefore something wrong with the way the concept of creativity has been developed in this study: but it is more plausible to conclude that the methodology must be modified to fit the subject matter, rather than the reverse. In order to arrive at a systematic assessment of those respects in which conventional psychological research methodology is inappropriate to the study of creativity, and thereby at some framework for the development of more suitable modes of methodology, however, it has been necessary to look in some detail at the underlying principles of psychological research programmes, as they have in practice been deployed in the study of creativity.

7.02 The first difficulty lies in the subjectivity which is apparently inevitable in the ascription of creativity. This cuts across the prime requirement that psychological research be objective - in design, operation and analysis. The preferred shape of research is:
isolation of subject material, test and measurement. But with regard to creativity, the subject material is difficult to delineate, and impossible to pin down: it defies objectification. The decision as to whether an individual displays creative characteristics or not is a judgmental one, and cannot be converted to a factual one. A product may be the result of creative process, but the ascription of creativity to the producer is never certain or final. The effect of this is to enmesh the scientific study of creativity in uncertainty, outside the world of facts. The study of creativity is therefore bound to stray outside the safe closed circle of objective test and measurement. A further impediment to the straightforward utilisation of conventional methodology is the impossibility of dispensing with the concept of agency in the investigation of creativity: what I have called earlier the integrating "I" is unavoidably part of the picture. Objective explanations which are limited to exegesis of correlations between pieces of behaviour are insufficient: what is needed is an explanation which draws into account the subject as an active constructor of happenings. Such explanations are outside the range of a straightforwardly objective procedural approach to the subject. The emphasis within psychological research on objectivity stems from the desire to be properly scientific, to be analogous to older, more respectable sciences: the aim is to purify and to standardise subject material until it becomes tractable, sufficiently objective to serve as data. The difficulty is that the chosen medium tends to select the message: the need to control environment and activity leads to distortions and limitations in the
material which psychology is prepared to tackle. If objective methodology is not transcended, the study of creativity cannot be undertaken.

7.03 The necessity of including agency in any explanation of creativity also affects the kind of thesis one can develop to explain creativity, and thereby the kind of hypotheses that researchers ought to be examining. There is a need to retain under-determination as a feature of an explanation of creativity, and this rules out the kind of causal explanatory mode preferred, because most typical of "hard" science. Such a closed, cause-and-effect type of explanation of creativity would dissolve the sense of surprisingness, of non-predictability which appears to be a necessary factor in creativity. The researcher is thus left without the support of a determinist framework in which to formulate his thesis and research strategy: he is forced to take into account that his subject material will not yield results which will fit neatly into a pattern of explanation in which all ends are tied up. The irreducibility of the free agent in the study of creativity makes the kind of explanation necessary basically unlike explanations in other sciences, and unlike the sort of explanation that psychologists normally aim at.

7.04 Again, the type of data yielded in the course of investigation of creativity tends to resist treatment by parametric statistics, which are normally considered to be superior in psychological research, and which are typical of the "hard" sciences; and the difficulties in identifying subjects arising from the subjectivity-saturation of the concept of creativity make the controlled selection of a sample upon
which to work problematic. The sum effect of these difficulties is to diminish the researcher's ability to be confident in his results as conclusive and unchallengeable: rather, the researcher is faced with having to proceed very tentatively and conservatively and to limit his ambition to the potentiality of his data. This can be very discouraging to the researcher, who is primarily interested in establishing and proving, not in continuing to deal in possibilities and suggestions. However, it simply is not possible to handle data about creativity with the same sureness and firmness that is appropriate in other sciences.

7.05 All of the above limitations on methodology to be used in research into creativity can too easily be interpreted as weaknesses, as (somehow) failures in the material to measure up to the requirements of proper scientific investigation. But methodology must serve subject material, not the other way around. Psychology is, or ought to be, the study of the person at his fullest, not simply of his most basic psycho-physiological functionings. A methodology taken over from the "hard" sciences can cope well enough with investigation of those parts of a person's function which are most undemanding of him: because of this, psychologists have tended to concentrate on such functions with the result that there is a concentration on the uninteresting rather than the interesting features of human behaviour. There is a turning aside from the more puzzling (and more crucial) aspects of human performance. But it is these realms of distinctively human functioning - cognitive psychology, developmental psychology, psychology of social, moral and creative
behaviour - where the integrating "I" is most predominant, where the role of conscious decision and choice, and agency, is unavoidable. In these fields, scientific investigation must be person-centred. Here conventional methodological techniques, underpinned by the key concepts of objectivity and determinism as being essential to systematic and controlled scientific investigation, have less prominent roles to play. Consequently, such fields are often either avoided or explored inappropriately. Concern with the uninteresting (which can be studied in the "proper" way) exerts a malign influence over those investigating these fields: study of creativity, for example, degenerates into qualification of verbal diarrhoea, because it is presumed that it is preferable to reduce research to objective situations and objective measures. The outcome is banality not illumination. The way forward in the investigation of person-centred concepts is quite otherwise. It must start from the recognition that the subjects of study are not causal objects but conscious agents capable of choice and decision.

7.06 Creativity as a psychological concept undermines much of psychological methodology. It includes the subjective as well as the objective mode, and consequently necessitates interpretation as well as fact. Observation without insight will not yield information. The terms of any explanation given must refer to action, not simply to behaviour: the objective stance, which eschews the exploration of the meanings underlying human behaviour, cannot arrive at an adequate understanding of action, which is not only an observable event in time, but also a performance by an agent. For the ascription of action it is vital
that the active rather than the passive mood of the verb is primarily appropriate. The concept of a person, which is not reducible to a physiological or mechanistic concept, is the most basic datum which underlies commonsensical use of language about events which are actions, and which should underly a psychology of persons. In the language of physics, the verb "to act" carries only the connotations of cause and effect: but as regards persons the verb "to act" carries implications of self-direction and self-monitoring. Such implications go beyond the language of cause and event, of objective happening: but they do not thereby become incomprehensible. An agent-centred language of discourse is comprehensible to us, perhaps even more so than an object-centred one. Very young children, for example, will tend to ascribe agency to objects ("the table hit me"), presumably because agent-centred language has a priority to their minds - from first consciousness, they know about "I", whereas the independence of objects is grasped later. The agent is able to give reasons for his action, and I qua observer can often explain the action, both via analogy with my own experiences and via my understanding of the agent's previous history. In general there is no great difficulty in attaching meaning to the actions of another, since we share common social structure of meanings. Within the given context, we usually understand one another well enough, because we are both persons within that context. The thing-orientation of the "hard" sciences has led psychologists to take a static rather than dynamic view of persons. The empirical strategies conventionally adopted treat subjects as objects: and this inevitably leads to distortion of results.
Thing-orientation is not wrong, but it is insufficient; and it is most clearly insufficient in fields of study where the central agency of personhood is most involved: in the study of creativity, for example.

**IMPLICATIONS FOR METHODOLOGY**

7.07 In the study of creativity, objectivity is purchased at the cost of value, that is to say, objectivity is gained only by discarding the judgmental aspects which are central to the subject in hand. This must not be seen as a flaw in the subject-material, but rather as a mis-match of subject matter and methodology. An appropriate methodology must have certain features which are not commonly held to be virtues in the social sciences: (a) it must be provisional and tentative, rather than attempting to establish truth; (b) it must be tolerant of a proliferation of theoretical approaches; (c) it must work with and not on its subjects. Provisionality is of course an element in all scientific investigation: a theory can be regarded as established only until a theory which explains the phenomena more comprehensively comes along. The role of provisionality in psychology is rather more than this. It arises from the need for the researcher into creativity to recognise the necessary uncertainty of status of his basic data, which derives from the subjectivity-saturation of the concept of creativity. In these circumstances, a methodology must be developed which can cope with the interpretative nature of the data. As a corollary to the need for tentativeness, there is a requirement for tolerance of alternative approaches: a proliferation of partial approaches will serve the
development of understanding better than a narrow emphasis on the scrutiny of one line of investigation. Most of all, however, what is required is the recognition that the objectification of the data of psychology is impossible and erroneous. The attempt to disassociate psychology from anthropocentric assumptions is misconceived, because it avoids the central factor that the subject of an action is conscious, and is in many ways best placed to observe it. He can both monitor his own activity and interpret his actions in terms of reasons, based on his experience of himself and others. The prototypical action is intentional and comprehensive to oneself - even though such action may degenerate into semi-automatic habit, this is still parasitical upon the originally deliberate action. Similarly, actions may be self deceiving, but this again: depends upon the assumption that mostly one knows what one is doing and why. By enlisting the subject as a partner in exploration, and working with and through his consciousness may be the best way to get at an understanding of his actions. In an open context, which includes empirical cross-referencing and where explanations can be constantly checked and revised, self report can function as a crucial tool in investigation. In summary, the following are necessary elements of an adequate psychological methodology:

(a) it requires an integrative approach, setting as high a value on validity as reliability;

(b) it requires tolerance of alternative approaches;

(c) it requires the researcher to be imaginative as well as painstaking;
(d) it rejects the person as object as the ground of psychological research but rather emphasises the person's perceptions and reactions as vital.

RELATION TO A THEORY OF KNOWLEDGE AND DISCOVERY

7.08 It is now necessary to consider how such a view of psychology can be related to a general theory of knowledge and discovery. To begin with, we need to consider the role and effect of explanation both in scientific and in everyday experience. Explanations are ultimately subjective: an explanation links together discrete elements into a coherent pattern - coherent to the subject, that is. While there is undoubtedly much room for variation amongst persons as to what they find coherent, depending on their previous knowledge and experience as well as personality traits, there is nonetheless a vast area of common ground. In everyday living, you and I will usually judge an explanation similarly incoherent or coherent, although our boundaries may well vary a little. That is because, though we are different kinds of people, we are both people, with a great deal in common. The more we have in common, the more we are likely to coincide as regards what we find incoherent. Equally, the more our backgrounds diverge, the more likely it is that we should disagree as to whether an explanation makes sense or not. Now if explanations are straightforwardly logical chains of facts, then it is only possible to account for such divergences as occurring through ignorance or perversity. In some cases, where all the potentially relevant facts are available, and where the connections can be systematically tested, then unanimity is attainable, and any
residual divergence can indeed be attributed to such defects. A controlled experiment in physics, for example, is likely to result in a high degree of unanimity amongst observers as to what explanation can be given of the events. But the chances of getting two politicians of different parties (or even of the same party) to agree on an explanation of the country's economic situation are virtually nil: and this cannot entirely be attributed to the argument that politicians would be out of a job if they did agree on such explanations.

THE PRIMACY OF PATTERN

People are constantly arriving at, and acting upon, explanations about the world around them. The explanations which they assume to fit their environment make it possible for them to understand their world and act within it. Where we are confused about our environment, we are uncertain what to do; we are strongly impelled from within to try to make sense of our environment, to reduce our uncertainty and make action possible and meaningful. We make sense of our environment, we make it coherent, by arranging it into a pattern. We literally cannot proceed without patterning: an autistic child is helpless because apparently he cannot impose a suitable pattern on his environment; less drastically, a dyslexic child cannot impose a suitable pattern on written letters and therefore cannot get to grips with the written material as a whole. We constantly pattern and repattern our past and present experience so that we can go on functioning within the world. At even the simplest levels, there is room for difference. To a child from a
rich background and a child from a poor background, a coin looks
different; to a hungry man and a satiated man, a meal smells
different: these findings fail to surprise. At the level of
emotion and belief, differences are more the rule than the exception.
I say Joan is selfish; you say she is at last learning to stand up
for herself. We all select different evidence to incorporate in
the patterns of explanation we weave: part of the evidence available
to us is our differing background. Each of us is a different person
and selects differently in the light of necessarily unique different
circumstances. Some patterns are more widely held than others.
At the commonsense level, we have fairly fixed ideas about the
patterns imposed on the everyday world around us: if I persist in
denying that the chair in front of me is solid, I am likely to be
considered as not quite all there (or a metaphysician showing off
inappropriately). Society tends to regard those individuals who do
not share in the accepted patterns regarding the physical environment
as in need of psychiatric treatment. Ingrained shared patterns
enable reflexive action and interaction. It is because such patterns
are so ingrained that a whole class of art (trompe l'oeil etcetera)
has such a shocking effect. Those patterns are nonetheless capable
of revision on occasion (luckily for science fiction writers) but
largely inhabit the realm of held truths. The society in which we
find ourselves does not attempt to have me locked up simply because
I impose uncommon patterns on social events because of my religious
or political beliefs, though I may find it hard to get other people
to look at things the way I do. Other people will however find it
very hard to reason me out of my way of patterning social events:
my perspective, shaped by my experience, is hard to modify, even in the light of what opponents consider to be compelling evidence.

FACTS AND FRAMES

7.10 The activity of science is a matter of testing explanations. The scientist, in an attempt to make sense of an occurrence, posits an explanation and then considers whether it fits with reality. Mere observation can never yield an explanation and therefore understanding: without a theory to frame them, facts make no sense. A theory is a construct, designed to structure, explain and predict phenomena. A theory is not factual, but is a frame for facts; and in considering the adequacy of a theory, one is considering the fitness of the frame. The relationship of the facts and the frame, which is vital to the facts making sense is at the heart of all scientific theorising. The function of the frame in scientific research is to organise incomprehensible facts so that they are meaningful to the researcher and others. The aim is a pattern into which the observable facts will fit. Science, like art, aims to make coherent and satisfying patterns out of experience. Both the scientific and the aesthetic roads to truth proceed by an imaginative re-ordering of experience, in which the description (at the level of theory) is not simply co-extensive with the expression (at the level of fact). Facts are said to be true if they relate to observable happenings in experience; but what is the criterion of a true theory? Plainly a theory cannot be justified simply by reference to the facts which it transcends. Even the most empirically minded scientist must employ a theory to contain his
scientific data which is not itself empirically verifiable. The justification of a theory cannot be empirical, though its falsification might be. In Experiment E, Hypothesis_1 (H_1) predicts Observation_1 (O_1) and Hypothesis_2 predicts Observation_2 (O_2). Thus the failure to find O_2 confirms H_1 but O_1 cannot render H_1 immune to refutation by a potential Observation_n predicted by a possible Hypothesis_n. Thus there can be crucially falsifying experiments, but not crucially verifying ones. But it is always possible to avoid crucial falsification, by the importation of subsidiary conditions. In any attempt to bring a scientific theory to observational test, many subsidiary empirical assumptions have to be made, so that seemingly negative observations can in practice always be regarded as a function of the falsification of the subsidiary assumptions rather than of the theory, provided the researcher is determined enough to retain his preferred hypothesis. The irremediable inconclusiveness of the verification of the assumptions auxiliary to the hypothesis imposes a corresponding limitation on the falsification of the hypothesis. It can be argued, therefore, that choice between conflicting theories is not in terms of falsification but of plausibility; that no theory ever crumbles merely under the weight of refutation, but that what happens is that it loses its original simplicity and is subjectively recognised as in need of amendment.

SAVING THE APPEARANCES

7.11 There is a vast difference between holding a theory with great conviction and knowing it to be true, between the view that a theory is a convenient fiction and that it is a potential fact. The position that scientists have taken on this has reflected their
attitude to the status of objects, and events in the world, and
thereby of data derived from them. Plato's view, that
science cannot investigate the actual structure of the universe,
but is limited to the phenomena which are presented by that
structure to normal human consciousness, is a natural corollary to
his general theory of knowledge, which stresses the primary role of
the philosopher rather than the researcher. In the Timaeus
however he considers at some length the practice of science.
Science is possible where observational knowledge (of phenomena)
and extrasensory knowledge (of actual structure) are forced into
interaction, where hypothetical patterns have to be devised which
account for the actual phenomena without infringing the fundamental
principles. This process of "saving the appearances" (i.e.
reconciling them with higher knowledge and thus imbuing them with
a provisional kind of reality) is via the utilisation of hypothesis,
to tell a plausible story which superimposes relations on phenomena.
The hypothesis is a device only, and can never come to mirror things
as they are (i.e. have a connection with knowledge). This is
because phenomena, although treated by science as if they were
independent of man's sensory and psychological participation, are
not independent and therefore not knowable. The essential science,
where the need to reconcile observed regularities and knowledge was
manifest, was astronomy. Throughout the Middle Ages, scientists
were able to posit different hypothesis about the movements of
heavenly bodies in continuing attempts to save the appearances, in
the clear understanding that such hypotheses were fictions only.
The heresy of Galileo was not the construction of a hypothesis, but
the insistence that the heliocentric hypothesis not only saved the appearances but was true. The true Copernican turning point was the objectification of phenomena, enabling theory to represent speculation regarding the real relations of phenomena with each other. At this point, scientific research as we could recognise it today became an acceptable mode of investigation - because phenomena could for the first time be treated as objects to be studied. Thus the development of methods of quantification and analysis and all the trappings we know as "scientific" today - all these arose out of a redefinition of the role of theory as something potentially true, because of a redefinition of physical phenomena as having objective reality.

Scientific explanation and psychological explanation

It is for consideration how appropriate this kind of approach is to psychological phenomena. The point has already been made that psychological data, information about persons and their behaviour, involves an element of interpretation. How large the element is depends on the particular circumstances of the case. In everyday life, you and I can differ without absurdity about the painting in the exhibition, why Justin married Joan, what that dark blob is on the horizon. In many cases, where the totality of relevant background information is in principle potentially available, there is eventually no possibility of you and I being able to differ without absurdity; but where interpretation is crucial then differing explanations are tolerated. In fact their proliferation may well be encouraged, to make sure no plausible alternatives have been overlooked. Scientific explanations vary also from case to case in the extent to which they
are unchallengeable without absurdity. A scientist who is investigating the effect of an acid on a petroleum-based fabric is working in an area where the scope for conflicting explanations is narrow: rational people do not disagree about the events observed or their relation to the putative chains of cause and event hypothesised. Science investigating the physical world immediately available to us leaves little room for interpretation. Yet at the frontiers of modern physics, scientists are telling each other plausible stories, attempting to cope with observed phenomena as well as they can, but unable to do so in the traditional ways. Determinist (cause and effect) thinking embodied in Newtonian physics was appropriate to the explanation of matter in motion, which, since it ranged from planets to corpuscular matter, was for a long time seen as the whole of physics. For two hundred and fifty years physics was able to proceed within the determinate rules of Newtonian mechanics, gradually being faced with more and more anomalies, some of which (e.g. Uranus' irregular orbit) could be explained by adjustment of the existing explanation pattern, some of which (e.g. Neptune's irregular orbit) could not. Gradually it became apparent that to make sense out of the problems which arose it was necessary to produce a new kind of explanation which was not purely determinate. Such an explanation required the abandonment of determinacy at a certain level of explanation and the substitution of probability laws within a limited range of specified alternatives. What necessitated this explanation was an extension of the subject matter of physics to subatomic particles. What is important about such particles is not that they are difficult to observe (this is purely a technical
problem) but that they behave differently from the sort of objects that a mechanistic explanation was constructed to cover: sense is not made of them until the proffered explanation is able to deal adequately with their attributes. The explanation must fit the explicandum. In the case of psychological explanation, the attributes of the explicandum not only justify but require a less objective approach to explanation. Naive mechanistic determinism assumes that every state-equation has a unique solution, and that exhaustive knowledge of the conditions determining X can operate both forward and backward to predict X and backward to explain X. But in the realms of quantum physics, for example, this level of explanation becomes meaningless because subatomic particles/waves are not the kind of thing amenable to determinate explanation—the same "wavicle" is in several different places at once, or looked at another way, nowhere. The psychologist should not feel his scientific credibility lacking because he cannot make determinate predictions about particulars. It is commonly held that the social sciences are at a pre-Newtonian stage of science. Kuhn has hard words to say about a set of "sciences" which cannot even boast dominant paradigms; and social scientists themselves berate the dissidents within the ranks for letting the side down, preventing their sciences from getting into the first division. It would be truer to say that the social sciences are in disorder because they have consistently attempted to subsume themselves under a type of Newtonian rules for which their subject matter is not suitable. Rather, instead of moving to the defensive, the psychologist should look to his own discipline and positively try to match its methodology
to its matter. Two people in normal circumstances must agree about what colour the litmus paper has turned; whereas it is always in principle possible for them to disagree about a wave of the hand. To make sense of human behaviour cannot be a matter of establishing rules directly testable against experience but rather of discovering reasons, likely interpretations. Human consciousness is special and central in explanations about persons. To the extent that human consciousness enters into an explanation, that explanation must lose its purely determinate nature.

7.13 Conflicting patterns of explanation may indicate a science where interpretation is inescapable rather than a science which in its immaturity has not got enough background data. The "softness" of the data with which psychologists have to deal is not a result of poor techniques but an integral part of their science, and their methodology has to cope with this. As a corollary, their explanatory mode also has to cope with this "softness"; the recognition that competing patterns of explanations are in psychology not an intermediate stage to be superseded by a single "true" pattern of explanation, but the mainstay of scientific enquiry into psychological data. To regard this as a weakness of the social sciences is to miss the point: the social sciences are like this, and are neither alignable with the hard sciences nor to be dismissed out of hand as inferior. The difficulty is that it is generally held that scientific explanations have the objective not simply of "saving the appearances", but of being true. The dominant current philosophy is that of Karl Popper, whose crucial insight was that
though no theory can be proven true, it can be shown to survive the attempt to disprove it. This enables him at once to recognise that theories are always provisional (because they cannot be proved true) and to insist that they can and ought to be rigorously tested in a systematic way. The view thus is that, like boxers, the best theory is that which survives the most onslaughts. It is important to note that Popper's theoretical approach is grounded in the realism which was his earliest approach to fundamental problems. It is only because he steadfastly believes in a bedrock of irreducible fact that he is able to place such reliance on the hypothetico-deductive method - he believes that there is an available target to aim at, though it is never possible to know conclusively that a bull's eye has been scored. His is a commitment to truth via fact: he sees truth function turning not on empirical meaning (the logical positivist blind alley) but on empirical methodology. Objectivity is retained by a procedure of bold conjecture and rigorous attempts at refutation i.e. by a logic of discovery. According to Popper the logic of discovery must always be provisional but it is a logic and not a psychology, i.e. objective not subjective; and it must therefore rest on the possibility of objective evidence. The inaccessibility of facts where persons are concerned, the necessarily phenomenal account that must be given of human action, renders theories about human beings outside the methodological pale, therefore. Popper recognises that "the social sciences are rampant with verbalism in many forms": his manner of coping with this is the concept of World 3, to which he considers that the social sciences rightly belong. World 1 is
the world of physical objects; World 2 is the world of subjective experiences, which stand only in psychological relations to each other, and which cannot be contradicted. The content of thought processes, however, can contradict each other: there is therefore a World 3, the world of theories, of ideas, of problems — the world of statements in relation to each other. World 3 is essentially the product of the human mind, says Popper:

"It is we who create World 3 objects. That these objects have their own inherent or autonomous laws which create unintended and unforeseeable consequences is only an instance (though a very interesting one) of a more general rule, the rule that all our actions have such consequences.

... We may regard the world of problems, theories, and critical arguments as a special case, as a World 3 in the narrow sense, or the logical or intellectual province of World 3; and we may include in World 3 in a more general sense all the products of the human mind such as tools, institutions, and works of art."

(Popper, 1976, p.187)

The social sciences, then, operate upon the content of World 3 in the same systematic way that the physical sciences operate upon the content of World 1? Not so as far as Popper is concerned: theories, whether of the physical or social sciences, exist within World 3; and for Popper, only investigation of the truth of statements or theories is productive. The investigation of meaning is to him a waste of time, a false trail distracting the investigator from the logic of discovery. Insofar as the social sciences do not conform to the requirements of conjecture and refutation, Popper consigns them purely to the company of myths and stories. For Popper, the difficulty is that a psychology rather than a logic of discovery, by failing to function entirely by means of an objectively checked methodology,
cannot aim at objective truth. In his eyes, a psychology of discovery is little different from a work of fiction. However the notion elucidated earlier in this chapter of the explanation as pattern rather than the explanation as representation of truth is helpful in considering why this is not so. In the social sciences, the kinds of pattern which are typically invoked to explain events and phenomena involve "rampant verbalism", meaning and value as well as physical fact. Such patterns therefore need to accommodate both elements: and the scientist requires a methodological approach which will enable him to evaluate such patterns, and to develop new, better patterns. This kind of enterprise is not simply fictionalising: but neither is it purely the business of conjecture and refutation by empirical testing envisaged by Popper. What is required is some means of theory development and theory testing which turns upon value as well as fact.

THE ROLE OF MODELS

7.14 Human behaviour cannot be simulated by a unique determinate programme but it can be represented by a series of necessarily partial (in both senses) models. The construction of a psychological model gives psychology a generative role, while the assessment of the model ensures its link with fact. What is meant here by a model is a construction whether mathematical or physical or verbal which represents the interaction of forces and events related to the item under study. A model is a practical tool in scientific research and not an end product: there is no burden that a model should embody "the right" explanation but rather models should articulate
possible explanations. The function of the model is to recast imaginatively experience to make it understandable. A model is not something that can be proved or disproved: unlike a hypothesis its aim is to promote not truth but understanding. It sidesteps the problem of determinacy by eschewing the claim that all factors are included and that the force of each connection is quantifiable. A model is if you like a causal explanation with the crucial quantified bits missing: but a model does not simply have the "advantages" of vagueness and untestability - but also the honesty to accept that the alternative to such vagueness is invalidity. 

Empirical evidence affects a model but not as it would a hypothesis. Evidence affects a model by persuasion rather than by force. But the possibility of proliferating models can assist in the scrutiny of what the evidence really consists of in a way that a single hypothesis or a pair of converse hypotheses cannot. The insistence on models rather than causal structure gives greater prominence to imagination along with empirical activity; and makes possible the incorporation of interpretative as well as factual elements. A model is clearly less than a hypothesis: the model's function is to convince, not to prove. The point about a hypothesis is that it should be confirmed or disconfirmed, supported or undermined or rejected. No such use need be made of a model. The construction of a hypothesis serves to aid testing; testing serves to aid the construction of a model. The aim of explanation by hypothesis is causal exegesis; of explanation by model is understanding.

Extreme empiricism recognises no role for hypotheses at all, but rather assumes that knowledge springs ready armed from the head of
experience. Such severe inductivism however is ultimately self-defeating: without a hypothesis to start from, no amount of aggregated data can be moulded into a pattern, i.e. a conclusion about how those data hang together. More normally, the process is held to consist of the formation of a hypothesis, its theoretical elaboration, its testing against reality, the measurement of the goodness of fit between the observed and the predicted outcome and the consequent confirmation or disconfirmation of the original hypothesis. Where this kind of process is available, it is clearly to be preferred: but where causal determinate explanatory theories (with no holes and no dotted lines) are not appropriate, then models are all that one can have. All models are only partially isomorphic with what they model, and there is always a danger of overstretching the isomorphism of the relationship. Beyond those facets according to which the model has been constructed no further analogies can be drawn from the model. However, this can be seen in a different light: the scientist who constructs the model is bound to recognise that the model is inaccurate in some respects, whereas the scientist who constructs a hypothesis is bound for the time being to take that hypothesis as being completely accurate. Thus it can be argued that the use of models as a normal method of psychological procedure would have the effect of enforcing tolerance: the limitations of the model as tool for research and thought make such tolerance a necessity. Instead of science being a gladiatorial combat to set which hypothesis survives the slogging match a model-based approach encourages integration and an emphasis on the positive rather than the negative aspects of the theory.
There are theories which lack factual basis and yet have profound influence, because of their power to persuade. Freudian theory for example tells an imaginative story which has retained its plausibility over time, but, because its interpretation is couched in terms of unobservables, is unfalsifiable, in that it is impossible to posit a situation which cannot be interpreted in its terms. Thus the psychoanalytic theory is a myth, and it is the function of myth to be satisfying to those who have felt the need to postulate a hidden aetiology for given events. The power of the psychoanalytic theory is charismatic, not rational: its acceptance is purely a matter of emotion. However, a model, unlike a myth, is capable of being undermined. The explanation of inaccessible phenomena (like human actions) is necessarily an interpretation, and therefore a work of imagination; but models require imagination under rational control, and which can be checked out against and if necessary corrected by experimental studies at various points. Factual evidence can never compel the rejection of an explanatory model: but it can suggest its inadequacy. A model therefore neither disregards the universe of facts nor crucially depends on it. The assessment of a model does not fall purely within the objective domain. It requires neither exhaustive verification nor falsification since it is not the truth of a model that is at stake but its adequacy. The process may be summarised as follows: (a) theory (b) model (c) exploration of the model's range and depth of adequacy. A model will have adequate range to the extent that it is applicable to the widest possible number of relevant cases (e.g. a behaviourist model of moral behaviour might be said to have inadequate range in that it failed to take account of and
explain causes of anonymous altruism); a model will have adequate depth to the extent that it fully explains all the implications and features of the events requiring explanation. In other words a model with adequate range and depth will account for all the features of all the cases. Range and depth are clearly separable kinds of adequacy, but they are also complementary at the ideal level at least. An explanation may account extremely well for some cases, with a sensitivity and clarity of vision absent from a more generally applicable but less perspicuous explanation. A really good explanation though will meet both criteria. Adequacy is a global criterion which refers back to understanding not to truth; to meaning not to fact. But this clearly involves the amassing and consideration of empirical evidence in the same way that a hypothesis under test would. Margaret Mead's findings about the lack of adolescent crisis in Samoan society rendered the explanation of Western rebellious youth as physiologically based as inadequate, because it no longer could be seen as an explanation which had sufficient range to encompass all cases: Mead's findings necessitated a rethinking, a re-explanation of the importance of social values. But to see Mead's work as refuting a hypothesis of physiological trauma is narrow, since Mead herself held no such hypothesis and indeed conceived of her field trips as exploratory searches for understanding rather than experimental tests for proof i.e. meaning- rather than fact-oriented. Her aim is to understand, not to prove. Operating with a model or models whilst involving the empirical testing and stretching of the model does not however mean that the operation is purely factual. No factual evidence is
ever compelling. But if decisions are ultimately subjective this does not mean that they need to have no regard to objective experience: rather the weighing and comparison of objective experience can often only be done by subjective decisions because of the incommensurable implications.

CONCLUSION

7.17 The aim of psychology must be to explore human behaviour at its fullest and richest. It must be able to develop explanations which can cope with those traits which are quintessentially human: and in this regard creativity is a key concept. People who exhibit high creativity are rather special, and make significant contributions to society: but everyone exhibits creativity to some degree when they are allowed or allow themselves to act freely. The integrating "I" is essential to creativity, and inescapable in the development of an adequate explanation of creativity, and of adequate investigative procedures. Thus there are clear implications of creativity as a psychological concept for psychological methodology and explanatory modes. Psychology must be a synthetic, not an analytic, science; an imaginative, not simply a quantitative science; its aim to understand man through his behaviour; its objectives to give coherent and adequate explanations of human activity. It is a mistake to suppose that procedures deriving from this outlook must be unscientific: rather it is dogmatic insistence on narrowly set methods and explicanda which front the proper practice of science.
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