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Psychiatry: Science, Meaning and Purpose

By PAUL E. BEBBINGTON

The Gods did not reveal, from the beginning, all things to us; but in the course of time, through seeking we may learn, and know things better.

XENOPHANES

INTRODUCTION

In this paper it is proposed to examine the nature of what the psychiatrist does in the clinical situation and how this relates to recently disseminated ideas on the appropriate activity of the scientist. This will involve consideration of the status of so-called causal explanation and psychological understanding and the relation between them. The type of description necessary to the nature of man as a purposive being will be examined. Psychoanalytic theory has been attacked for its non-scientific nature and has been defended as a psychology of meaning and as a mode of description appropriate to a teleological system. There is, therefore, both a question of the matter to which the tenets of psychoanalysis have been applied and a question of the nature of the answers it gives. An evaluation of this debate will serve to illuminate the general nature of the activity of psychiatrists, both in an empirical and in a normative sense.

Karl Popper and the Demarcation of Science

The work of Karl Popper has had an increasing influence on the thinking of psychiatrists (e.g. Lewis, 1958; Slater, 1975; Birley, 1975), as it has influenced workers in other fields (Magee, 1973). This is appropriate, as Popper's ideas on the nature of scientific activity received an admitted impetus from his work with Alfred Adler in Vienna and the difficulties he saw in regarding the hypothesizing activities of the Adlerians and Freudians in the same light as those, for instance, of Einstein (Popper, 1963). The important epistemological problem for Popper is that of the growth of knowledge, and, as he says (Popper, 1959, p 18), 'a little reflection will show that most problems connected with the growth of our knowledge must necessarily transcend any study which is confined to commonsense knowledge as opposed to scientific knowledge'. He is concerned, then, to distinguish commonsense knowledge and scientific knowledge, the problem of 'darmarcation', or 'Kant's problem' as Popper calls it. In doing this, Popper was applying himself to a question of concern to the Vienna Circle in general, but his approach was radically different from the logical positivism of the other members. As he says, positivists usually interpret the problem of demarcation in a naturalistic way: 'they believe they have to discover a difference, existing in the nature of things, between empirical science on one hand and metaphysics on the other.' To do this, the philosophers of the Vienna Circle attempted to find a way in which all non-scientific statements could be shown to be meaningless. Popper avoids this difficulty: he does not deny that knowledge exists which is non-scientific but nonetheless meaningful. 'I wish to distinguish sharply between "objective science" on the one hand and "our knowledge" on the other' (Popper, 1959, p 98). He regards his demarcation as being between science and non-science and not between science and meaninglessness. This demarcation is not an empirical statement but a *conventional* one, involving a value judgement: 'I define science thus.'

Popper's demarcation arises from his consideration of the problem of the inductive gap: how many white swans must we see before we conclude 'all swans are white'; how much confirmation confirms a scientific theory. Popper regards this not as a logical conundrum but as a psychological one relating to the process of conviction. We can, however, test hypotheses in a logical way if we attempt not to confirm but to *falsify* them. If we find *one* black swan we can

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refute the statement 'all swans are white' using the propositions of *deductive* logic. Hence, Popper adopts refutability as the criterion of a scientific hypothesis and the objective of science is to produce low probability statements which are subjected to all conceivable tests of refutation. 'Not for nothing do we call the laws of nature "laws". The more they prohibit the more they say' (Popper, 1959).

Popper's statements, as he says, are essentially normative. Kuhn (1971) has criticized Popper on the basis that we do in fact hold on to theories which we know fit their data imperfectly; in other words, refutation is not apparently grounds for rejection. If this is so, says Kuhn, Popper is obliged to say what *degree* of misfit is a criterion for rejection. He feels this places as much difficulty in the way of Popperian demarcation as arises for the various probabilistic verification proposals for overcoming the problem of induction. However, the writer would feel that this criticism raises a problem of the psychology of theory acceptability which would not over-concern Popper. He would still emphasize that scientific activity should seek refutation, even if we do not, for reasons of our own, follow up our findings in a perfectly logical way.

Popper himself has pointed out that psychoanalytic theory is couched in terms which are irrefutable. 'It was precisely this fact—that they always fitted, that they were always confirmed —which in the eyes of their admirers constituted the strongest argument in favour of these theories. It began to dawn on me that this apparent strength was in fact their weakness' (Popper, 1963).

This claim has been taken up by others (e.g. Medawar, 1969; Cioffi, 1970; Slater, 1975) and has been the point of departure of other authors in defence of psychoanalytic theory (Farrell, 1970a, 1970b; Rycroft, 1968). Popper (1963) himself is of the opinion that psychoanalytic theories 'describe some facts, but in the manner of myths. They contain most interesting psychological suggestions but not in testable form'. At the same time he feels such pre-scientific myths may be developed and become testable. In their present form they cannot claim to be *backed* by empirical evidence in the scientific sense—although they 'may easily be, in some genetic sense, the result of observation'. Hence, Popper most certainly does not deny some empirical content to psychoanalytic theory. Farrell (1970a), however, disagrees with the term 'myth'. 'If psychoanalytic theory were a myth, it would presumably be a closed story. But this is just what it is not.' Although we observe that Popper does not regard a myth as necessarily static, there is some cogency in Farrell's asseveration that psychoanalytic theory does progress, as for instance in the later departure of ego psychology, and that this progression bears some relation to observation. However, it is clear that this progression does not involve a formal methodology of test by the seeking of refutation. Farrell goes on, unfortunately, to make statements in justification of psychoanalytic theory which are patently unacceptable; for instance 'it is also the case that (clinical psychoanalytic material) suggests that we should use the theory to talk about it'. This is justification by conviction rather than by scientific method. Farrell also feels that there are grounds for saying that the theory is at least an approximation to the truth 'because of the fertility of its application to the humanities'.

Cioffi (1970), in a debate with Farrell (1970b) which Slater (1975) describes as 'animated' but which might better be characterized as acerbic, offers the most detailed analysis of the irrefutability of Freudian theory in terms of Freud's own writings. He states 'a pseudoscience is not constituted merely by formally defective theses but by methodologically defective procedures' and goes as far as to say 'there are a host of peculiarities of psychoanalytic theory and practice which are apparently gratuitous but which can be understood . . . as manifestations of the same impulse: the need to avoid refutation'.

Although the debate continues about demarcation, it tends to be very much a rearguard action by the proponents of psychoanalytic theory. Some, however, have offered justifications which bypass this argument. Rycroft (1968) claims that the debate is ill-founded and that the objective of psychoanalytic theory is not causal explanation but an attempt at understanding: 'the procedure (Freud) engaged

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in was not the scientific one of elucidating causes but the semantic one of making sense of it.' This reiterates the views of Karl Jaspers and we must now turn to examine these.

Karl Jaspers. Causality and meaning

Jaspers himself seems to have been surprised at the effect of his 1913 article (Jaspers, 1974) and of his *General Psychopathology* published in same year (Jaspers, 1963). 'All I had done was to link psychiatric reality with the traditional humanities.' However, his distinction between causal explanation and understanding was seminal.

'In the natural sciences we find causal connections *only*, but in psychology our bent for knowledge is satisfied with the comprehension of quite a different sort of connection. Psychic events "emerge" out of each other in a way which we understand. Attacked people become angry and spring to the defence, etc' (Jaspers, 1963).

For Jaspers, these meaningful connections are merely *analogous* with 'real' causal connections. Understanding is something immediate and irreducible and, he claims, not acquired inductively by experience.

Hence 'the meaningful connections of psychological events have also been called "causality from the inside" and this term has characterized the unbridgeable gulf which exists between this which can be called "causal" only as an analogy and the real causal connections, the "causality from outside"' (Jaspers, 1974). Further, 'the rules of causality are obtained inductively and culminate in theories about what lies at the root of the given reality. Particular cases are then subsumed under them. Genetically understandable connections, however, are ideal, typical connections; they are self-evident and not inductively obtained. Because we note the frequency of a meaningful connection, this does not mean the meaningful connection becomes the rule' (Jaspers, 1963).

Jaspers does not make the mistake that psychic events are not open to causal explanation. However, he implies that psychic events may only be effects and that causation has to be 'extraconscious'. Attempts to explain a concrete effect in terms of a psychic cause, as in hysteria for example, Jaspers terms 'pseudo-understanding'.

Jaspers claimed that Freud's work is a psychology of meaning and not of causal explanation 'as Freud thinks'. He criticizes the tenet of psychological determinism: 'However, it is only the postulate of unlimited causality, not the postulate of unlimited meaningfulness which is justifiable' (Jaspers, 1974).

One can see that the separation which Jaspers has effected between causality and meaning might well be received thankfully by those concerned to protect psychoanalysis from the Popperians. But we are obliged not to accept this *ex cathedra*.

Firstly, there are inconsistencies in what Jaspers himself says about the distinction. For instance, he talks of extending our understanding 'to remote connections which at first sight perhaps seem incomprehensible' (Jaspers, 1963). In this he allows the importance of hypothetical considerations in generating understanding, which belies the self-evidence of genetically understandable connections. Moreover, one could argue that self-evidence is in fact the belief we have in those commonsense models of the mind which to some degree we all possess. Further, his concept of interpretation (the imaginative leap necessary to encompass understanding when not all relevant data are available) suggests that understanding is quantitatively related to data, which again belies self-evidence.

Secondly, the appeal to the metaphysical concept of 'ideal types' in order to justify the distinction between psychic and causal connections is not acceptable to the writer. Again, the distinction does imply a reductionist point of view which would now be regarded as old-fashioned (Jessor, 1958). This is best exemplified by Jaspers' insistence that causal links can only proceed *up* the hierarchy of concepts.

Psychoanalytic theorists would not be put off by the role of hypotheses in generating understanding. However, this is an admission which to the present writer seems to point to a different interpretation of Jaspers' dichotomy, for if hypotheses can generate understanding, why cannot all hypotheses do so? And if all hypotheses do so, are there alternatives to psycho-

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analytic theory in our understanding of psychic events? Once we have come this far, we are obliged to regard Jaspers' distinction in another light, for if understanding is something which attaches to theories it may be that there is no psychology of meaning separate from theories. Looked at in this way, psychological theories cease to be in a different category from other scientific theories. For, as we shall demonstrate, understanding is an aspect of meaning which attaches to all theory. Meaning contains two elements, namely import and conviction. Jaspers' *Verstehen* is an example of the latter—we have conviction of the validity of a psychic connection when we understand it empathically. The role of conviction in non-psychological theories has been elaborated by a number of writers. Hence, Einstein, in his address on Max Planck's 60th birthday (quoted in Popper, 1959), talks of 'the search for those highly universal laws from which a picture of the world can be obtained by pure deduction. He goes on 'there is no logical path leading to these . . . laws. They can only be reached by intuition and empathy.'

Popper himself (1959) has this to say in describing the importance of conviction to both logic and science: 'Yet whether statements of logic are in question or statements of empirical science, I think the answer is the same: "our knowledge, which may be described vaguely as a system of dispositions and which may be of concern to psychology, may in both cases be linked with feelings of belief or conviction: in the one case, perhaps with a feeling of being compelled to think in a certain way, in the other with that of perceptual reassurance." ' Medawar claims for both myths and science the necessity of 'the property of being believable in'. It is of interest that Lewis (1949), in his address to the British Institute of Philosophy, took as his subject the psychological problem of conviction and its relation to philosophical activity.

Kuhn (1971), however, has made a concept of scientific theorizing as a language central to his explanation of scientific revolutions. He claims 'an apparently arbitrary element, compounded of personal and historical accident, is always a formative ingredient of the beliefs espoused by a given scientific community at a given time'. 'Theory must be chosen for reasons that are ultimately personal and subjective.' Some sort of mystical apperception is responsible for the decision actually reached. The choice between Kuhn's 'paradigms', then, rests not merely upon their causally interpretative qualities but upon conviction of meaningfulness. This leads to a problem of translation between paradigms: 'to translate a theory or world view into one's own language is not to make it one's own. For that one must go native, discover that one is thinking and working in, not simply translating out of, a language that was previously foreign.' Now this sounds remarkably like the process of empathic understanding as described by Jaspers. The writer is led to conclude that this process is not qualitatively different when applied to persons or to causal theories in the physical sciences. The implication is that we need not reject the language of causal theories when approaching the problem of understanding a patient. However, before we go on to talk of this possibility we must turn our attention to a further argument in support of non-scientific psychoanalytic theory.

Causality and purposive systems

A distinction between mechanism and meaning is the cardinal point of Sir Denis Hill's Ernest Jones Lecture of 1970. He uses this to separate simple mechanistic systems from the teleological character of human activity. Hill (1970) claims that models of the mind are not amenable to the type of scientific testing required by Popper because of (1) the nature of the perception involved and (2) the fact that they refer to a purposive system. It is not clear that these are in fact separate arguments, as Hill employs the word 'meaning' in three different uses without this being readily apparent.

Now the first argument is a reiteration of Jaspers and lays stress on the differentiating value of the act of identification involved in setting up models of understanding. Hence 'Psychic reality which cannot be directly perceived through the physical senses is only directly experienced in the self, but can be communicated by language and can be inferred by observation. Physical reality can only

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be perceived through the senses.' He adds 'we can only know "others on the inside" by an act of identification; we can only know them "on the outside" by acts of perception. The former is a method entirely absent from the physical sciences, the latter is their only method'. He says of Medawar 'he is speaking of physical science and the methods at its disposal. The scientific method . . . is dependent upon observation of what is perceptible through the senses . . . it cannot from its nature be used to disprove what is not so perceptible'. As might be expected, the writer would argue that our perception of the physical world is an interpretation relating to our meaningful models of it, that no one can perceive directly someone else's psychic reality, and that our grasp of it is based on the interaction of our sense data and our models of the mind. Hence, both the outside world and the psychic realities of others are perceived 'in the self' and through our senses.

The second line of argument uses two further separate uses of the word 'meaning'—behaviour has meaning in the sense that it is goal-directed and goal-perceptive. Hill (1970) proceeds as follows: 'the answers which scientific activity provides are always to questions as to "how things occur" and not answers to questions "why things occur". The latter are questions peculiar to human experience and are of a different order of abstraction. The first is concerned with mechanism: the second with meaning.' Now it is certain that there is a difference between a linear mechanistic system and a teleological system involving feedback. However, it is a long way from the conclusion that 'the methods of science are only applicable to questions of the first kind', and it seems hardly tenable that 'why things occur' should be a question peculiar to human experience. Hill continues 'If this is so, psychoanalysis should admit to being a causal theory in the teleological sense rather than in the mechanistic sense, and the hypothetico-deductive model is not logically applicable, except in a very limited sense.' Unfortunately, Hill does not expand in detail what is meant by 'a very limited sense'.

The debate concerning the appropriate mode of description of teleological systems has involved others (Toulmin, 1970; Peters, 1970; Taylor, 1970; Borger, 1970; Malcolm, 1968). In a sense, as Taylor (1970) points out, the abstraction of purposive descriptions from the methodology of science is as much an activity of behaviourists as it is of defenders of psychoanalysis. For they have been concerned to attempt explanations of goal-directed behaviour purely in the linear linking of events, in other words to explain purposive behaviour in terms of controlling antecedents. Taylor (1970) claims 'we can only derive the thesis of mechanism if, like most theorists of the behaviourist persuasion, we assume it beforehand . . . the *a priori* argument is shown to be bogus'. Pure behaviourists, he says, seek to explain goal-directed behaviour in terms of 'a non-teleological antecedent, that is, one which makes no mention of that property of the response which is its being appropriate, that is, required for the goal in question'. As Borger (1970) says, the distinction between teleological and non-teleological antecedents is difficult to specify.

There seems really to be some confusion between system-types and methodology. If by mechanism we are merely referring to systems which are not goal-directed, this is solely a description of a system type. We may, however, mean by mechanism a methodological decision that we should attempt to describe all natural systems in terms of causally related antecedents in a way which would eliminate the possibility of involving a description of feedback and corrective action. Now, as Borger (1970) points out, this reflects a limited view of mechanism, a view which Hill (1970) shares. A wide view of mechanism might be said to be any description which is compatible with methodological adherence to the principle of causality. Now it is perfectly possible to specify the function or 'groundplan' of a goal-directed system involving feedback in a manner so compatible, whether the system be a simple thermostat or the complex purposive behaviour of animals and humans. It would seem that Hill is confusing these two views of mechanism when he claims that scientific methodology is inappropriate to purposive systems. The present author would aver that there is nothing idiosyncratic about goal-directed systems which precludes us from attempting a causal explanation in testable

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terms, and that an explanation in teleological terms of, for instance, feedback and corrective action need not in principle evade refutation. Hence we conclude that Hill's (1970) contention that 'the hypothetico-deductive method is not logically applicable' to purposive human behaviour is not supportable.

Epistemology and the psychiatric formulation

It appears possible that the origin of the particular methodology of psychoanalysis and the problems arising from it derive from its concern with the individual patient. The general psychiatrist is likewise involved with the individual patient, and his knowledge and conjectures about the patient are summed up in a formulation which is the basis for therapeutic intervention. This formulation may use the language of science at different levels—at a biochemical and neurophysiological level and at a psychological level. It may involve concepts derived from psychoanalysis and from scientific psychology. It will also use commonsense concepts. These concepts together make up the language of the psychiatrist, and the formulation is an attempt at a global understanding of the patient. We will conclude this essay with a brief consideration of how this process relates to the process of scientific investigation and how the 'knowledge' of the patient relates to scientific knowledge. Hill (1970) points out the analogy of the two processes: 'Every clinician, psychoanalyst or scientist approaches a new patient or a new problem by setting up some provisional hypothesis . . . which gets progressively modified as a result of the process of interaction between the observer and the object'. Lewis (1958), however, points out, of the individual, 'Because he is unique and extremely complicated, he does not seem wholly catchable within the scientific net.' The writer would argue that the formulation is a complicated entity of statements of differing epistemological value and this is why it differs from the mere setting up of scientific hypotheses. The first element in the procedure is one of the naming of phenomena—the provision of statements like 'this is an example of . . .'. This is the equivalent of the observation of basic data, and such statements can only be *objectified* by consensus. We may not necessarily use these data except to promote our understanding, and we may then be using the language of science merely as that.

Some statements we make may take the form of 'existential statements' (Popper, 1959), e.g. 'there is a psychogenic cause for this pain'—these statements differ from hypotheses in being verifiable but not falsifiable.

Further statements are in the form of scientific hypotheses, e.g. 'this is the psychogenic cause for this pain'. Such statements are in principle refutable. In practice this may not be possible, and the statements then become an effort at understanding.

This complicated procedure of formulation is psychologically necessary to the work of the psychiatrist. It derives its practical value from the work of science but it is not purely a scientific process. When it is mistaken for such there is a danger that the knowledge so derived may be regarded as scientifically valid without critical evaluation. But although the conjecture arising from the idiographic study of the individual may lead to valid knowledge it can only do so by a collateral process of scientific investigation.

Science and psychiatry

We have argued in favour of Popper's thesis that the growth of knowledge is a function of the observation of error, and its extension that the most effective methodology of science is that which sets up hypotheses and seeks to refute them. This is a methodology which has been contested in its applicability to psychiatry. We have examined and rejected the argument that the concept of understanding necessarily encompasses knowledge which is not amenable to this scientific method. Likewise, we have not been convinced that the nature of the system of human activity necessarily prevents its application. The word 'necessarily' is of importance in these two preceding sentences: for our arguments all devolve on a methodological issue—if we can delineate that method which renders our knowledge as sure as possible, then should we not stipulate that this is the method we should use, whatever practical difficulties may hinder us? There are, of course, other methodologies —the methodology of history is of critical

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appraisal of mutual and internal consistency of sources (Popper, 1963)—and some have advocated these (e.g. Farrell, 1970a). But where there are questions of the appropriate methodology, we should take the best.

It seems likely that the application of the principle of refutability to psychoanalysis would involve major change in the form of the theory. Local rather than global propositions would be tested, and it might seem appropriate to jettison whole 'metapsychological propositions' (Rappaport and Gill, 1959), as indeed Bowlby (1969) has done with libido theory. Although, as Slater (1975) emphasizes, this represents an enormous task, the author would feel that if the insights afforded by the conjectures of psychoanalysis are to be of full value they must be subject to test.

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