Scientific Realism and the Human World: the case of psychoanalysis

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In this paper, I want both to defend scientific realism as the correct ontological assumption of the human as well as the natural sciences, and to make certain reservations about the epistemological status of the human sciences.[1] The main course of the argument concerns Freudian psychoanalysis, though I also refer to the other human science to the defence of which I am committed, namely Marx's materialist conception of history. But I think my conclusions have absolutely general application in this area.

The paper arose partly out of an attempt to defend Freud, both against his critics (particularly Timpanaro) and against his interpreters (particularly Lacan); and partly out of an attempt to tie up the loose ends I left in my essay 'In defence of epistemology'.[2] In that essay I was defending the realist epistemological claims of Marxism against a neo-Kantian critique, which itself arose out of an attempt to resolve certain contradictions in Althusser's work. I felt that those contradictions could be resolved, not by repudiating realism, but by reinstating the notion of an experiment as 'a question put to nature', and identifying this as the essential epistemic mechanism of all science. But in doing so, I neglected to mention that an experiment is not just *any* question put to nature; thus I undermined the distinction (which I nonetheless used) between observations made in the course of the practical application of a science, and experiments proper. I therefore greatly understated the insuperable difficulties confronted by the human sciences in their attempt to approach the rigour of the natural sciences. This paper then can be seen as an explication and correction of some passages towards the end of that essay (pp.94-96).

Throughout much of this paper, I shall counterpose scientific realism to empiricism. In doing so, I shall draw on Bhaskar's model of scientific realism, as set out in *A Realist Theory of Science*. So I shall start with a brief exposition of his view.

Bhaskar breaks with the ontology of classical empiricism by postulating three domains: the Empirical, which is inhabited only by experiences; the Actual, which is inhabited by experiences and events; and the Real, which is inhabited by experiences, events and mechanisms.[3] Thus his ontology is distinct not only from phenomenalism, which holds that only experiences are real, but also from forms of empiricism which allow that events can occur unexperienced, but which conceive of laws of nature as constructions made by us. The term 'mechanism' refers to that in nature which corresponds to the scientific law. It need not be mechanistic in the sense of the Newtonian paradigm (which Bhaskar argues is actually internally incoherent). Mechanisms are the structures of things and they explain the powers or tendencies of things. For instance, a hydrogen atom has the power to combine with a chlorine atom to form a molecule of hydrochloric acid; a dog has the power to bark; a human being has the power to act in accordance with reasons. In none of these cases is the power something that can be exercised arbitrarily; it depends on the structure of the 'agent', and can be exercised only in certain conditions.

The structures investigated in the different sciences will have very different properties; the use of a common terminology across the sciences ('structures', 'agents', 'mechanisms', 'powers', 'tendencies') does not involve generalising either mechanistic or anthropomorphic explanations; but it does draw attention to some common features of explanation, across the sciences.

The powers of things, then, are real, even if unexercised. Only if exercised do they enter the domain of the Actual. But they can also be exercised unrealised, that is to say, without producing the effects that one would expect from their operation, and which they would produce 'other things being equal'. Newton's apple was subject to the law of gravity all the time it stayed on the tree. Scientific experiment largely consists in devising ways of realising the powers of things, so they can be tested and measured. But the interest of the experiments is the fact that we can apply the knowledge we obtain from them to analyse the operation of the same tendencies in the domain of the actual, where they may operate unrealised.

This theory allows us to form a much clearer conception of the nature of an experiment than empiricism does. Thus Bhaskar says:

> These distinctions may be conveniently expressed by the formula Dr ⩾ Da ⩾ De, where the special case Dr = Da = De, assumed to be spontaneously satisfied by empirical realism, has in fact to be worked for in the social activity of science. (op.cit., p.229. Dr = domain of the Real, Da = domain of the Actual, De = domain of the Empirical).

The creation of such a 'special case', the elimination of variables irrelevant to the question at issue by laboratory conditions such that the mechanisms can

so to speak be made to appear, is referred to by Bhaskar as the production of conditions of *closure*. We isolate particular mechanisms as closed systems so as to discover the workings of these mechanisms, which also operate in the 'open systems' of the world outside the laboratory. But open systems, though they obey the laws of nature, don't present those laws neatly exemplified in constant conjunctions, as they appear in experiments.

This theory is realist then in that it asserts the reality of the world independent of us; in that it postulates the reality of 'generative mechanisms' which empiricism is apt to regard as 'logical constructions' or 'theoretical entities'; and in that it treats causal necessity as irreducible to constant conjunction, which is neither a necessary nor a sufficient condition for the ascription of causality in open systems. It rejects the empiricist assumption that conditions of closure obtain naturally ('actualism'), and the empiricist tendency to reduce ontological questions to epistemological ones ('the epistemic fallacy').

These positions are argued for in Bhaskar's book: I present them as results. But in the following two sections I put them on one side and discuss *empiricist* controversies about Freud; the discussion will illustrate the need for a realist position.

1 Critics of Freud's Scientific Credentials

Freud is often attacked for being unscientific. There are, broadly speaking, three lines of defence. Some boldly claim that there is no problem, that psychoanalysis is a science just like any other. Some claim that it is a science, but with a different method, and perhaps even in a different sense, than other sciences. And some grant that it is not a science, but claim that it is none the worse for that.

There are intermediate positions. There is the idea that all sciences have quite different methods, so that psychoanalysis is, so to speak, just like any other science in being unlike all the others. This view, which I take to be that of Louis Althusser, and which was my own until recently, falls between the first two defences. And there is the view that psychoanalysis is in some respects like a science, in some unavoidably unlike one; my conclusion in this essay can be seen as a version of this view.

To test the defences we must consider the attacks. I shall look at three - those of Sir Karl Popper, Professor Cioffi and Comrade Timpanaro.[4] In each case, my defence of Freud will not dispose of the difficulties for his theory, but merely shift the problem. But I hope that by the end of the discussion, the problem will have been shifted to its proper place.

Popper's criticisms both of psychoanalysis and of Marxism are well known, though they are much more fully worked out in relation to Marxism, and for this reason their underlying error is easier to see in this connection. But the two cases are point-for-point parallel, and so far as Popper's case against them is concerned, the two theories stand or fall together.

Superficially, Popper's criticism of these theories takes the form of a dilemma: if they are interpreted in a strong form, such that they have unconditional predictive consequences, they are easily refuted by the facts of history or biography;[5] if on the other hand they are interpreted in such a way as to escape such refutation, they lose all their content, for if any event whatsoever in the real world is compatible with a theory, that theory tells us nothing about the real world.

But this dilemma is not the real content of Popper's case, for it assumes that the only way Marxism or psychoanalysis *could* have a content is if they were to make predictions about what will in fact happen in the world; and if they did this, they would already be shown to be unscientific on Popper's criteria, because guilty of historicism. That is to say, they would not be resting their claim to be scientific on *conditional* predictions (if event E occurs under conditions c, event E' will occur), but on unconditional assertions about the inevitable course of events - in short on what Popper calls 'prophecies'. If Popper can show that Marx or Freud made 'prophecies', he has already proved his case, without having to show that they are vague or false ones; but he has marked his cards, for he never considers the possibility that they could make any sort of predictions other than prophecies.

The nature of Popper's mistake can be seen better if we use Husserl's distinction between abstract or theoretical sciences which 'are *nomological* in so far as their unifying principle, as well as their essential aim of research, is a law', and concrete sciences in which 'one connects all the truths whose content relates to one and the same object, or to one and the same empirical genus'. Husserl goes on to say that 'the abstract or nomological sciences are the genuine, basic sciences, from whose theoretical stock the concrete sciences must derive all that theoretical element by which they are made sciences' (*Logical Investigations*, pp.230-31). Physics and chemistry would be among the abstract sciences, geography and astronomy among the concrete ones. In Bhaskarian terms, the abstract sciences operate with closed systems, the concrete ones with open systems. (Ted Benton critically analysed Bhaskar's distinction of open and closed systems in *Radical Philosophy 27* - though the present article was written before the appearance of that analysis - Ed.)

Now it seems to me that when Popper comes to deal with the human sciences, he assumes that they can only be concrete sciences, and then blames them for not being abstract ones;[6] and it used to seem to me that, once this error had been identified, nothing was left of Popper's attack. Everything was in order: The materialist conception of history, for example, was an abstract science and behaved like one, making only conditional predictions and so on. Marxist conjunctural analysis (or, in the past tense, historiography) was on the other hand a concrete science, and behaved like one; it used the concepts of historical materialism to analyse concrete conjunctures, about which it made only probabilistic predictions, and, just like a concrete natural science such as meteorology, was not refuted when its predictions were falsified. Likewise with Freudian metapsychology, and case studies.

But the identification of Popper's error does not abolish the problem: for Popper's placing of the human sciences in the concrete slot was not arbitrary; there seems to be nowhere else that they could be tested but in their concrete applications; in the natural sciences, on the other hand, the conditional predictions of the abstract science can be tested experimentally, independently of its application in a concrete science. In Bhaskarian terms the abstract parts of natural sciences can be tested under conditions of closure; those of human sciences cannot.[7] This displacement of the problem can be made clearer with reference to Cioffi's argument that psychoanalysis is a pseudo-science. Cioffi tells us:

> It is characteristic of a pseudo-science that the hypotheses which comprise it stand in an asymmetrical relation to the expectations they generate, being permitted to guide them and be vindicated by their fulfilment but not to be

discredited by their disappointment.
(op.cit., p.474)

The trouble with this formula is that it is also true of a *science* once it comes to be applied to concrete realities outside the laboratory. 'The concrete is a union of many determinations', said Marx,[8] and that particular concrete reality which is an individual psyche is subject to influences that lie outside the field of psychoanalysis as well as those that lie inside it: biological and social influences, for example.

Once this point is recognised - that in any 'open system' (i.e. outside of artificially 'closed' experimental conditions) confirming instances support but disconfirming instances do not refute - the whole of Cioffi's case collapses. Even his most striking example, intended to damn Freud all the way to hell, loses its sting.

The example is that of little Herbert and little Hans. Little Herbert is referred to in Freud's paper on the beneficial effects of enlightening children about sexual matters. His parents' liberal attitude is said to have led to healthy development.

> On the other hand the unfortunate Hans was a 'Paragon of all the vices' - his mother had threatened him with castration before he was yet four, the birth of a younger sister had confronted him 'with the great riddle of where babies come from' and 'his father had told him the lie about the stork which made it impossible for him to ask for enlightenment upon such things'. Thus, due in part to 'the perplexity in which his infantile sexual theories left him' he succumbed to an animal phobia shortly before his fifth year.

Cioffi then gleefully informs us (Cioffi, op.cit., p.485) that 'Hans and Herbert are the same child, the account of Hans written *after* and that of Herbert *before* he had succumbed to his animal phobia (but not before the events to which Freud later assigned pathogenic status).'

But just what does this example prove? That Freud, who had been led by his theories and his observations to the very plausible and by now well documented conclusion that it is harmful to threaten and lie to children about sex, used as an example a child who appeared to be healthy and whose parents he believed to be enlightened. Later the child had problems and it came out that the parents were not so enlightened. Cioffi later alludes to the episode as a 'counter-example'; to what? Freud's omniscience?

What we have here is a failure on Cioffi's part to distinguish between the testing of a theory in experimental conditions and the application of the theory in an open system. Freud's error and later correction of it fall into a perfectly ordinary class of everyday explanations and corrections. Compare the following: a house has been burgled; a detective examines the house and says 'the burglar could not have got in by the door, as it was bolted; so he must have got in by the window'. Later it emerges that it is quite impossible that the burglar got in the window. The detective will consider all sorts of possibilities; he will no doubt show great 'adjustability in relation to counter-example', as Cioffi says of Freud. You can bet your boots on one thing: he won't say 'the laws of physics have been refuted! The burglar must have de-materialised outside the door and re-materialised inside it!'

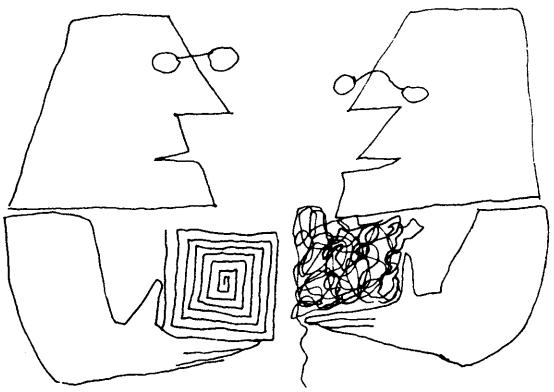
But the fact remains that our knowledge of the laws of physics does not depend exclusively on our knowledge of the art of burglary. We can argue with confidence from abstract to concrete because we can test the abstract sciences experimentally, independently of their concrete applications. If a concrete science has no such experimentally tested abstract science from which to derive its confidence, this confidence may be misplaced, and this is the crucial problem of the human sciences: they are, so to speak, concrete-bound.

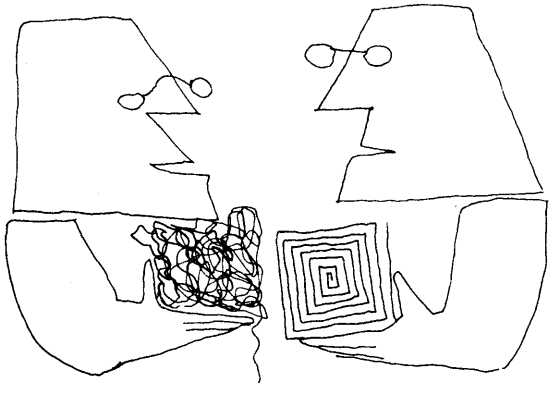
Now suppose someone were to say: there is no problem after all. To be sure, Freud's abstract postulates can't be tested experimentally; we cannot isolate and measure the forces which operate in the unconscious. But we have to postulate them to explain the phenomena, and the therapeutic practice which both gives rise to and uses them does not require any higher level of exactness. This might be all very well if there was only one theory in the field; but as our next critic illustrates this is not so.

Timpanaro claims that many of the examples given by Freud of parapraxes - 'Freudian slips' - can be explained quite adequately in terms of the concepts of textual criticism. The main part of his book *The Freudian Slip* is devoted to giving examples. This is an altogether more serious and pertinent critique of Freud than Popper or Cioffi can be bothered to provide Popper originally thought that one counter-example scuppered a whole theory; if there ever were any scientists who proceeded by this method, their names have not come down to us, for the simple reason that they never discovered anything. It has now been accepted by people working in the Popperian tradition - e.g. the late Imre Lakatos - that refutation involves not two elements (a theory and a counter-example) but three (two theories and a test between them). You do not abandon a theory until you have got a better one.[9]

Timpanaro claims to have a better theory; it is a theory which is already in use in other contexts than explaining parapraxes (as of course is also true of psychoanalysis); and it undeniably has a bearing on the phenomena. Different people will no doubt assess the relative plausibility of the two theories differently.

For example, there is the case of Freud's acquaintance who forgot the word '*aliquis*' in a Latin quotation; in a long string of associations he moved via 'liquifying' and 'St. Januarius' (whose blood is supposed to liquify periodically), to his fear that his woman friend had missed her period.[10] Timpanaro gives the alternative explanation, which he describes





as 'pedestrian (but true)', in terms of the unfamiliarity to a German-speaker, and strangeness even in Latin, of the construction of the sentence in which '*aliquis*' occurs.

Timpanaro wisely refrains from claiming to have refuted Freud's theories; after all the account of slips is only one aspect of the theory, and Timpanaro unfashionably (but I think correctly) regards Freud's essays on sexuality as being on solider scientific ground than his 'interpretive' work such as that on dreams or slips. But Timpanaro does claim that his alternative explanations 'are pertinent to any overall judgement of psychoanalysis'. He clearly believes that he has rendered certain Freudian hypotheses redundant.

Once again Freud can be defended, but once again the defence shifts the problem rather than abolishing it. The defence is that Freud himself pointed out the presence of mechanisms such as those which Timpanaro thinks explain the slips, but he claims that there is *also* an unconscious motive. One of the peculiarities of Freud's theory is his notion of overdetermination, according to which many thought processes converge to produce the symptom. Within the psychoanalytic context, many explanations may be given of the same event, and it is recognised that the 'over-interpretation', as it is called, can never be known to be complete. In relation to the other human sciences too - biology, linguistics, social science - Freud likewise leaves open the possibility that their laws might have made their contribution. (See the replies to Timpanaro on *New Left Review* No.94.) But it is precisely in this possibility of the peaceful co-existence of theories that we encounter the real problem. For if no explanation can be pronounced adequate, it would seem that no limit can be set to possible speculative explanations, and no criteria set up for sorting out the true from the false. If on the other hand we accept that Freud's and Timpanaro's accounts *compete* as explanations of slips, the question arises how we can decide between them.

Of course there are parallel cases in the application of natural sciences to open systems, where we may never in fact find out which of several explanations is true; but the concepts from the abstract natural sciences which compete in these cases will have received independent experimental justification. It is important to recognise both the relevance and the non-relevance of parallels between the concrete human sciences (which have no independently tested abstract foundation) and the concrete natural sciences (which have). Ontologically they are relevant, epistemologically non-relevant.[11] This is a question to which I will return, but as I have already raised the issue of overdetermined processes and the possibility of knowledge of them, I shall try to make this matter clearer by using this distinction. Ontologically, the theory of overdetermination is all in order; it is perfectly logically coherent and intelligible; there are analogous processes in other fields; it is quite compatible with what we know of the structure of the real world. Empiricists who reject it *a priori* as contrary to Ockham's razor are rightly accused of dogmatism; they commit the epistemic fallacy, i.e. they reduce ontological questions to epistemological ones.[12] But the distinction of ontology and epistemology does not in itself save Freud insofar as he also makes an epistemological claim: to have a science. In short: the empiricist accuses Freud of being slippery; Freud may justly reply: the slipperiness is not in the concept but in the object - it is not my fault if the contents of the unconscious are slippery fish; but the empiricist may retort with as much justice: no, but if the fish are too slippery to catch, don't claim to have an aquarium.

It is not enough to show that the mechanisms of the unconscious might very well be just as Freud says; any reputable theologian can claim as much for his theory. It must be shown that Freud's theory is epistemically better founded than its rivals. It must be shown that there are no rival theories without the problem of 'concrete-boundness'; and that there are criteria for distinguishing epistemically better and worse theories of this kind - and that Freud fares well by these criteria. The first task, then, is to examine the claim that we can have experimental access to the processes in question after all.

2 Freudian Theory and Experimental Psychology

Let me set out my aims in this section with reference to a quotation from Trotsky, which Timpanaro also quotes. Trotsky, like the other Bolshevik leaders, was inordinately impressed by the work of Pavlov, but, unlike some, he also had an interest in and respect for Freud. Here is how he compares the two:

> The idealists tell us that the psyche is an independent entity, that the 'soul' is a bottomless well. Both Pavlov and Freud think that the bottom of the 'soul' is physiology. But Pavlov, like a diver, descends to the bottom and laboriously investigates the well from there upwards, while Freud stands over the well and with a penetrating gaze tries to pierce its ever-shifting and troubled waters and to make out or guess the shape of things down below. Pavlov's method is experiment; Freud's is conjecture, sometimes fantastic conjecture.
>
> (*Problems of Everyday Life*, p.234)

Let me say at once - without stopping to ponder on the possible Freudian motives of Trotsky's metaphor - that I think that the metaphor he applies to Freud is an apt one. But he is over-optimistic about Pavlov. Even if Pavlov's experiments with dogs bear some resemblance to conditions of closure, this is no longer the case when they are generalised to humans, whose behaviour is affected by a lot of variables of which there is no concept within Pavlovian theory. And if we are comparing Pavlov with Freud, we are presumably talking about humans. You can't psychoanalyse dogs - they aren't talkative enough. Pavlov's psychology is just as concrete-bound epistemically as Freud's, its abstract parts just as speculative.[13] What then is the difference between the two? I would suggest - they were looking down different wells.

We need to ask why they were doing so, and was one of them wrong. If they were both 'psychologists', ought they not to have been looking down the same well? Husserl characterised the concrete sciences as ones in which the unifying principle that constituted the science was 'the same object' or 'the same empirical genus'. But what can this mean in the present case? Not the individual person or the human species, for both Freud and Pavlov were studying people, as are the practitioners of many other sciences. The constituting factor in each case is a *practice*: in Freud's case, the practice of analytical theory, in Pavlov's, of conditioning. These different practices turn up different sets of phenomena - different wells to look down. In Freud's case the phenomena were the words, silences and symptoms by which people talked about their problems in the analytical situation. This 'phenomenology' was the starting point, though not of course the content, of his theory itself. The theory was the set of 'conjectures' by which Freud explained the troubling of these waters. The semiotic and conflictual nature of Freud's theories stems from his starting

point in a different set of phenomena than Pavlov's, rather than from a desire for short cuts (as Trotsky seems to be suggesting), or simply from different speculative dispositions; the different phenomena in turn are determined not by theoretical but by practical considerations.

Perhaps, having said all this, I should immediately make clear that no sort of pragmatist, subjectivist or relativist theory of knowledge can be derived from it. Certainly, practical criteria determine a 'selection' of facts (in a certain sense of 'selection'). But in the first place, from a fruit bowl containing apples and pears, you can select an apple or a pear, but not a plum. Insofar as either Freud or Pavlov succeed in discovering facts, those facts had to be there to be discovered. And of course, insofar as both discovered facts, these facts are equally objective, and cannot conflict with one another. The fact that one may find conditioning distasteful, or politically dangerous, or morally objectionable, does not invalidate particular findings of Pavlovian research. Moreover, the term 'selection' is misleading insofar as it suggests choice rather than discovery - as if Columbus chose what to find on the American continent, because he chose to sail west rather than east. And finally, it is not at the stage of theoretical speculation that the 'selection' is made, but in the 'choice' of practice; any intervention of value-judgments at the theoretical stage would be inexcusable.

The elements in the human sciences then are (1) a practice, (2) the empirical phenomena turned up by that practice, (3) speculations about the mechanisms generating those phenomena. It is the practice which comes first and constitutes the science, i.e. designates its object and marks it off from other sciences.

The concrete natural sciences may share this feature of being constituted by a practice, as when we speak of scientific medicine, cookery or gardening. But they differ in that they can make deductions from the abstract natural sciences in quite a different way from what is the case with human sciences. In place of these deductions, the human sciences have what Trotsky called 'conjectures', and I referred to as 'speculations'. These form the abstract part of the human sciences, to which belong for instance Freud's metapsychological writings. The following section will be concerned with the status of these. For the present it is enough to note that these abstract parts of the human sciences, being (unlike their natural-scientific equivalents) parasitic on the practical disciplines which give rise to them, reflect a practically determined demarcation between these sciences.

Now let us look at experiments designed to test Freudian hypotheses. These experiments reflect the empiricist tradition in psychology. If they succeed, my hypothesis would be false and Trotsky's would be correct; it would be possible to get down the well, and the sooner we stop staring and start pot-holing the better.

The experiments are by no means without their own interest and value. Yet what strikes me about them is that they usually (not quite always) seem to miss the point in relation to psychoanalysis, and that it is not immediately obvious why this should be. After all there is nothing magic about the psychoanalytical situation: discoveries made within it could also be made by other means, as Freud notes in connection with infantile sexuality.

Let us look at three experiments designed to show the 'scientific credibility' (or otherwise) of Freud's theories.[14] The first two experiments appear to confirm hypotheses of Freud's. The first was designed to test Freud's theory of slips, using post-hypnotic suggestion to set up the requisite mental conflict. The subject was told under hypnosis that someone would come in and start talking to him; he would be bored to tears, but must at all costs be polite and not show his boredom. Sure enough, when the occasion arose the subject went and shut the door, and when asked what he was doing, said 'Why, I just shut the bore.'

This would be a satisfying example to tell Timpanaro, but it really adds nothing to Freud's examples. We know that post-hypnotic suggestion can't hermetically seal its effects off from other influences, for subjects who have been told under hypnosis to dream of a certain event and nothing else generally dream that or a similar event, but worked into a complex story which clearly has other significant determinants. The purpose of experiment in the natural sciences - the establishment of a closed situation in which causal laws manifest themselves in constant conjunctions - has not been achieved. It is an 'experiment' only in that it creates artificially a conflict of the type that Freud studied in their natural habitat. But artificiality is not what constitutes an experiment - it is necessary precisely because closure does not occur naturally. When it fails to establish closure, an experiment establishes nothing that cannot be established in a natural open system.

In this case, we know that the subject made the slip while in a state of conflict between boredom and politeness - but then we also know that Freud's acquaintance made his '*aliquis*' slip when trying to put from his mind the possibility that his woman friend was pregnant. The experiment makes no qualitative addition to the evidence for Freud's theory.

The second example is a study which 'sought to "kill" two birds - the validity of Freudian sexual symbolism and the kind of psychosexual stages in children - but confirms both'.[15] Children of various ages were asked to give preferences between pairs of drawings which had been designed to contain male and female symbolism in their shapes. The Freudian theory of sexual stages was deemed to predict that children under four, being at the oral or anal stages, would show no preference; that children of four to six, being at the phallic stage, would show preference for the symbols of the opposite sex; children aged seven to eleven or twelve, being in the latency period, would prefer the symbols of their own sex; and children over twelve, being in the genital stage, would again prefer symbols of the opposite sex.[16] It was further predicted that in the final stage, preference would be weighted towards male symbols, as America (where the test was carried out) is a male-oriented culture. (An opposite bias was predicted for a matrilineal culture in Ghana.) This bias was not expected to occur at the phallic stage, as it was assumed that children of that age were not capable of discerning the ascendency of the male sex in their culture.

All these predictions were confirmed. But the fact of cultural bias towards one or the other sex should immediately emphasise the absence of a closure here, for it is a fact of sociology, not of psychosexual development. How can we know that the whole process is not sociologically determined? Or for that matter, genetically programmed? Freud's observations may have been confirmed, but his explanation is precisely where it was.

Finally, we come to the theory of dreams. Fisher and Greenberg conclude that the hypothesis of a 'latent content' to dreams is superfluous. They claim that it is not necessary for the dreamer to give private associations. Instead they propose that the manifest content of a dream should be sorted into

elements which can then be directly related to other aspects of the dreamer's life. The symbols that occur in dreams are said to be 'widely shared'.

Let it be granted that this approach may pay off; by asking a certain set of questions about the manifest content of a dream, one may find something out about the dreamer. This does not rule out the possibility that there is also a true depth interpretation. By ignoring private meanings which symbols may have, as well as the leads given by free association under conditions of transference, the possibility of coming across a latent content is avoided. Inevitably, a method based on statistical comparisons neglects private symbols, but that doesn't prove their unreality. And a study which does not try to dig beneath the surface of a dream should not be surprised to discover nothing but the surface. The phenomena discovered are different because the practice leading to their discovery is different.

These examples show, I think, that the relative (not absolute) irrelevance of such 'experimental testing' to the claims of Freudian theory is due to the differences in the kinds of phenomena looked at. They are not absolutely irrelevant because the paths of those different types of investigation do cross.

But more importantly, I think they illustrate the fact that any appearance which such 'experiments' may have of superior scientific rigour to psychoanalysis is quite illusory. It stems from a misunderstanding of the value of experiment in science, due to an empiricist philosophy of science. These so-called experiments do not go any way towards creating conditions of closure, any more than Freud's clinical work did.[17] 'Experiment' turns out to be no more than observation, plus mathematical accessories.

Without then impugning the veracity and interest of such paths of psychological inquiry, we can dismiss their claims to the sort of superiority that would put them in a position to make or break Freud's theory, i.e. to put it on a scientific footing or refute it. Their bearing on its verification or falsification is quite marginal.

It would be unfair not to mention that Fisher and Greenberg are far from unaware of some of these problems. Yet they remain trapped within an empiricism which leads them to draw the wrong line of demarcation between science and non-science. In reply to psychoanalysts' claims about overdetermination making experiment impossible in the strict sense, they say that exactly the same problem arises in other areas of psychology, and they instance studies of intelligence. I am tempted to say 'Thou hast said it, my Lord General, and not I'. So-called experimental psychology labours under just the same problems of concrete-boundness as psychoanalysis. Yet these authors repeatedly use the word 'scientific' to describe the methods of such psychology in contrast to psychoanalysis. The titles of their books bear witness to that.

A word is perhaps necessary to prevent a hasty conclusion, and will also serve as a transition to the next section. There are of course procedures designed to approximate to closure in 'empirical psychology' - controls aimed to ensure that 'experiments' test what they are meant to and not something else. It might be thought that, by denying the possibility of closure in the non-laboratory sciences, I have discounted these controls and reduced the human sciences to a purely anecdotal level. Surely statistical controls on the one hand, or the peculiarly disclosive nature of the analytic hour on the other, do count for *something*? And is not one important function of the human sciences precisely to dispossess the unrepresentative anecdote? It certainly is, and politically this is extremely important. We have all experienced the way in which every capitalist, sexist or racist lie, from 'scroungers on the welfare' to the Protocols of the Elders of Zion, is backed up by anecdotes which are based on 'personal experience' and are therefore irrefutable, though evidentially worthless.

In fact I do not intend to deny intermediate degrees between laboratory conditions and anecdote. After all, compare the situation in the natural sciences: we can never establish 100% perfect closure; and yet we are not reduced to anecdote outside the laboratory. If the purity of one's materials is an important condition of closure in chemistry, that does not mean that we cannot (for the time being) distinguish sea water from crude oil without the equipment requisite for analysis.

Statistical controls can certainly be used as an approximation (however distant) to conditions of closure. But before they can do so, it is necessary that some speculative hypothesis has been made which would explain the possible results. In the absence of this, even well established statistical correlations which have some predictive power have, as Roy Bhaskar has pointed out, no explanatory power. And if the speculations guiding a questionnaire, for instance, are screwy, the results will have no value. A particularly blatant example is provided by Eysenck's test on politics and personality.[18] Communists and fascists are 'proved' to share certain attitudes backed by liberals, by the simple expedient of asking some questions which will be answered in the affirmative by communists but not fascists or liberals, and some which will be answered in the affirmative by fascists but not communists or liberals, and marking the affirmative answers up on the same axis.

On the other hand, once we know how to speculate in a scientific manner, anecdotes are far from valueless. A single case study such as that of 'Anna O.' can yield quite a lot of knowledge (e.g. of the *possibility* of psychogenic physical symptoms, of unconscious ideas, of therapeutic abreaction of memories - see *Studies in Hysteria*).

3 Freud's Realism

So far this has got us no further than to put Freudian theory and so-called experimental psychology on

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an equal footing. If the latter has come in for more stick, that is only because it makes unfulfillable claims to superiority for itself.

But I do think that the Freudian theory is superior, and the superiority lies not so much in the sort of evidence adduced as in the ontology assumed. This ontology - which is perhaps what Freud gets criticised for most - is a scientific-realist one: he takes it for granted that there must be underlying mechanisms generating the phenomena he encounters in analysis, and that it is his task to speculate as to their nature. This task, I would argue, is necessary despite its conjectural nature, because the alternative is not to abstain from speculation, but to speculate in a way guided by another ontology. There are several contenders here: some form of phenomenalism, for which the data are what they are, and not the symptoms of another thing - I include behaviourism in this category. Or some form of voluntarism, for which all the data are results of free choices, equal as to rationality, and assessable if at all only in moralistic terms. Or some form of reductive materialism for which the data are mere epiphenomena of a purely physiological process. The fact that these speculations may be negative in character - denying the need for explanations, rather than offering alternative ones - does not make them any less speculative.

Furthermore, all these ontologies have the common feature of covering the traces of irrationality, making the symptom invisible as a symptom, i.e. as a sign of something wrong. If Freud gravitated spontaneously towards a realism of the unconscious, at a time when phenomenalist and constructivist theories of science prevailed, this is because his practice was that of a therapist, and hence the phenomena he encountered were pathological ones. Once again, this fact has been used against Freud, i.e. it has been said that his theory only applies to neurotics. But a machine-operator can very well be a 'behaviourist' as long as the machine is working - all he needs to know is what happens when you pull which lever. It is when the machine goes wrong that a mechanic - that is to say, a realist - is needed. Freud is predisposed in favour of realism because he observes the methodological primacy of the pathological.[19]

It might be alleged at this point that I beg the question by assuming the reality of symptoms and of the phenomenon of irrationality and accusing behaviourism and voluntarism of being blind to them, when of course the consistent behaviourist or voluntarist will accuse me of seeing things that are not there. And in replying to this I can make no use of appeals to pre-theoretical givens or to the sanctity of common sense. For Freud himself is highly revisionary in his account of the data. He finds irrationality not only where 'common sense' finds it, but also where there appears to be rationality (rationalization), and where the phenomena had previously been ascribed to non-rational processes (physiology).[20] If one accepts this revisionary function of science in relation to appearances in one direction, one cannot rule out the possibility of revision in the other direction. If good enough reasons could be given for regarding what appears to be irrational as really rational or non-rational, we would have to drop that concept - and with it psychoanalysis. But a serious project of eliminating the concept of irrationality would have to include an account of the appearance of irrationality, just as Freud explains appearances of rationality in terms of displacement and rationalization. So far, the attempts that I have seen to eliminate the concept of irrationality - whether behaviourist or voluntarist - leave me with the impression of a mentality that would smash the microscope rather than admit to the presence of bugs in the holy water. For instance, Sartre's comment on an example from Jane:

> A young bride was in terror, when her husband left her alone, of sitting at the window and summoning the passers-by like a prostitute. Nothing in her education, in her past, nor in her character could serve as an explanation of such a fear. It seems to us simply that a negligible circumstance (reading, conversation etc.) had determined in her what one might call 'a vertigo of possibility'.
> (*The Transcendence of the Ego*, p.100)[21]

The position of Freudian psychoanalysis in relation to other theories in the psychological field, then, is as follows:

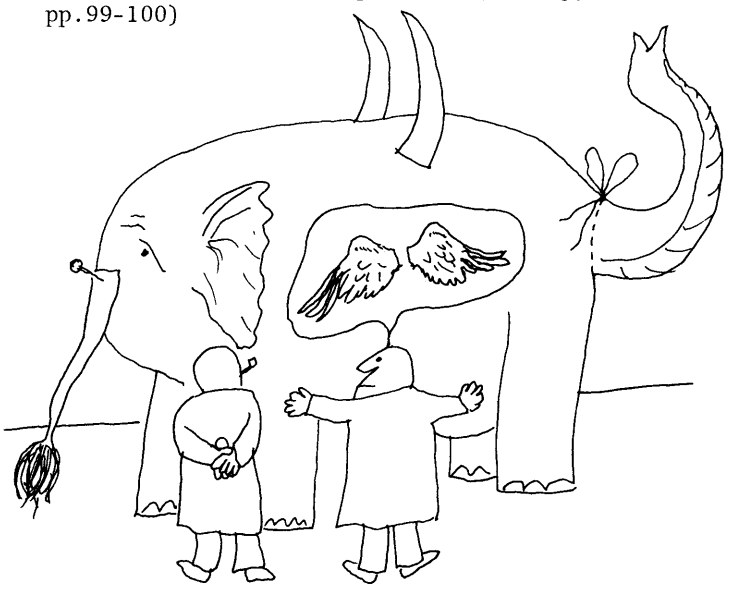
1 It is differentiated from non-analytic psychologies by being constituted by a different practice (the talking cure, rather than, say, the statistical survey, behaviour-modification and so on).

2 It consequently takes as its raw material a different set of phenomena - i.e. precisely those phenomena produced by free association under conditions of transference.

3 As an effect of the 'pathological' nature of its raw material, it makes 'realist' assumptions about the generation of these phenomena. This makes it appear more speculative than the various 'empirical psychologies' constituted by other practices, in that it makes more positive ontological assumptions. However, the empirical psychologies neither possess an experimental alternative to such speculations, in the way that Trotsky's metaphor suggests, nor can they avoid the guilt of speculation by virtue of the negative nature of their speculations - in that their theories claim to have explanatory power without postulating explanatory mechanisms.

These points can be illustrated with reference to a discussion of the conceptual issues involved in the debate between behaviour therapy and psychodynamic therapy, from the conceptual standpoint of 'operant psychology' by Harzem and Miles. They refer to the (essentially realist) distinction between those medical terms which are 'nosographic' and those which are 'nosologic'.

> A nosographic concept is one which describes the course of an illness but makes no claim about underlying causes; a nosologic concept is one which both describes the course of an illness and entails certain views as to its origin. Thus in the present state of knowledge *fever* is a nosographic concept, while *tuberculosis* is a nosologic one.
> (*Conceptual Issues in Operant Psychology*, pp.99-100)



The behaviour therapists' view that in psychiatry 'the symptoms *are* the illness', i.e. that they are not signs of something else, is then interpreted as the claim that, at present, psychiatric concepts are nosographic not nosologic. The behaviour therapist's case against the psychodynamic therapists is then set out; the latter

> have assumed that clinical concepts are nosologic and have therefore made the 'medical'-type assumption that one should treat causes and not symptoms. Since no ordinary cases have been discovered, such as biochemical abnormalities, they have had recourse to mythological causes such as 'conflicts of forces within the psyche', and it is these forces which they have supposed to be in need of realignment. This mythology has led them into believing that even when manifest symptoms have been removed substitute symptoms will appear.
> (*ibid*).

The psychodynamic therapist, then, is accused of (a) thinking that psychiatry is in a more advanced state than it is (I would say: in a scientific rather than a pre-scientific state, explanatory rather than merely descriptive), and (b) postulating 'mythological' (i.e. mental) causes in the absence of 'ordinary' (i.e. biochemical) causes, and (c) predicting on this basis a possible clinical phenomenon (symptom substitution) in advance of the empirical evidence.

It seems to me that this 'psychodynamic' procedure can be justified as follows: (a) its only *a priori* assumption is that psychiatry ought to be scientific. Hence it takes the speculative risks necessary to any transition from descriptive pre-science to explanatory science. It assumes a similarity in explanatory form between psychiatry and other medical sciences. (b) It abstains from the *a priori* assumption of the behaviourist that mental causes must be mythological, and hence that psychological medicine *must* ultimately *give way* to physiological, which alone can be explanatory. (It certainly does not assume mental causes to be ubiquitous; psychoanalysts have never denied the existence of areas of psychiatry where a physiological explanation and cure are appropriate.) (c) Clinically, it is no more rash than the behaviourist therapy; the latter's choice of the practice of symptom elimination over psychotherapy is equally a gamble on a speculation - i.e. that symptom-substitution will not occur.

If this is right, there are no counts on which the psychodynamic approach comes off worse; and on two counts it looks in better shape, namely that it is less dogmatic, and that it has the *possibility* (if correct) of being an explanatory science. Naturally (as Harzem and Miles point out) this discussion leaves all the empirical facts to be discovered.

I am certainly not claiming that it is impossible to work in a scientific way with the data provided by non-psychoanalytical practices in psychology. The 'facts' of the 'empirical' psychologies are as good as the 'facts' of psychoanalysis. But the *tendency* of psychological disciplines whose data are of a non-pathological nature is to theorize them in a non-realist, empiricist way.

4 Freudian psychoanalysis shares its constituting practice and therefore also its raw material with a number of other theories whose speculative content is at odds with it. If there were no means of deciding between these, we would be left with an indefinitely large set of theories, and conceivably all could be consistent with their empirical input.

Here I think we must appeal to another feature of Freud's outlook: not only is he *realist* in postulating real mechanisms generating the phenomena, he is *materialist* - in a sense akin to Marx's - in his conception of these mechanisms. Perhaps this is best understood in terms of a phrase familiar in both Marxist and Freudian traditions: the scientific world view.

To elaborate on this: in the first place, the scientific knowledge of our place in the universe, our subjection to natural laws, our continuity with other animal species, our dependence on our material environment and so on, has a debunking effect on the pretensions of human vanity to such things as individual autonomy, the self-determination of consciousness, free will; the processes which constitute us as the beings that we are, occur independently of our will (as Marx also says of the relations of production in which we enter, and which determine our social positions).

This imposes on anyone who would adopt a scientific world view in relation to the human world the Spinozist imperative - to renounce judgements of praise and blame along with the metaphysical assumptions which they rest on 'neither to laugh nor weep, but to understand'. Freud's whole work is shot through with this approach.[22]

The other side of this anti-voluntarist coin is the strict determinism which was Freud's consistent assumption and the first theoretical premiss of all his work. We are physical beings, and if determinism holds in the physical world, our mental life, which is ontologically dependent on it, cannot be exempt from determinism - though the causal chains which include our mental life will not be purely mentalistic. Once determinism is granted at the physical level, the denial of mental causality implies, not the autonomy of mind, but its epiphenomenality. (In the unlikely event of theories about sub-atomic particles being shown to have any bearing on this, we can safely say that they will not in any way help the defenders of free will.) Freud's assumption of a partly mentalistic determinism is the only theory that can be described as applying the scientific world view to the study of mental life. The alternative (if we leave out the miraculous) is to leave mental life in magnificent inexplicability, and to regard physicalistic explanations as the only ones. It then matters little whether one puts the stress (idealistically) on the 'mystery' of mind, or (reductivistically) on the physical determinism. If we accept this scientific world view, we can exclude at the outset theories such as 'existential psychoanalysis', or the attempt of phenomenological or linguistic philosophers to excise causality from psychoanalysis and transform it into 'hermeneutics without energetics'. Why should we accept it? Simply because, whatever else human beings are, we are also beings subject to natural laws discoverable by the natural sciences, and as Marx put it, 'The idea of *one* basis for life and another for *science* is from the very outset a lie.'[23]

A more specific feature of Freud's theory which, given his raw material, is indicated by his scientific world view, is the role that he assigns to sexuality in symptom formation and character formation. Of this matter, Trotsky comments that Freud's school:

> is materialistic, if you leave aside the question whether it does not assign too big a place to the sexual factor at the expense of others, for this is already a dispute within the frontiers of materialism.
> (*op.cit.*, p.233)

However, though the origin of this theory of Freud's was perfectly empirical (the correlation of neurotic symptoms with disturbances of the sex life, on the one hand - 'actual neurosis' - and with infantile sexual phantasies, on the other - ' psychoneurosis'),

it is nonetheless difficult to see what other theory could have been developed consistently with the clinical findings, and given the scientific (or as Trotsky would say, materialist) world view within which he was working. The instincts postulated by Jung, Adler, or later therapists with a mystical turn of mind - instincts which make up in edifying potential what they lack in physiological origin - can be ruled out; for they would give rise to a psychology situated in a biological and social vacuum, which Freud's is not; Freud's well has a bottom to it.

On the other hand, the other instincts which have got materialist credentials - hunger, thirst, etc. - differ from sexuality in a way suggested by the title of an essay by Freud: 'Instincts and their Vicissitudes'. It is only the sexual instincts that can properly be said to have vicissitudes. The only fates which can befall hunger and thirst are satisfaction or frustration, and prolonged frustration of them is fatal; hence as Freud says, there can be no question of *repression* in these cases. The only other case in which repression is possible is that of an external stimulus which, because of special circumstances, cannot be removed by action - for example, fear, as experienced by a soldier at the front. Here indeed repression can occur and produce symptoms, as Freud recognises. But such conditions could not possibly be the normal ones in which our personalities are formed.

In short, granted a symptom-interpreting practice, a scientific approach, and a few well-known facts about human biology, the conclusion that sexuality is the unique source of symptom-formation and character-formation is unavoidable. The only question that arises is whether Freud did not actually understate the influence of sexuality, at least once he started speculating, in a less than scientific fashion, about a death-instinct.

4 Implications for the Geography of the Human Sciences

If these sciences have not got 'theoretically constituted objects', but practically constituted ones, yet do *discover* objective facts in the human world, then we cannot expect them to be like 'continents' or 'islands', such that knowing which one you were on would help you to know where you were. Rather, they are like criss-crossing forest paths, cut for practical reasons, and communicating only at their intersections. One may know a path without being able to map the forest.

To make this slightly more concrete and less metaphorical: if each of the human sciences had a theoretically constituted object, clearly marked off from those of others, there could be no problem about the legitimacy of using a science in an area far removed from its associated practice; one would simply have to ask whether the area in question fell within the definition of the object of the science in question. So that, given that art (for example) is a social practice, one of the ways in which human agents reproduce/transform their societies, and that Marxism is the correct theory about such practices, a comprehensively Marxist science of aesthetics would be known to be possible in principle, even in advance of any work being done in this area. Some excellent work - e.g. Della Volpe's *Critique of Taste* - has been motivated by such an assumption. But if the human sciences are essentially adjuncts of practices, we may expect Marxist theory to get out of its depth once it is removed from the context of working-class politics - and likewise with psychoanalysis outside the context of the talking cure. Such 'applied' uses of the human sciences are doubly speculative: they lack not only experimental proof, but any kind of practical check. I conclude that these uses can indeed never be scientifically legitimate; it does not follow that they can never be legitimate in any way. If the human sciences do not belong to different continents, it may well be that concepts derived from one practice may throw light on others. The metaphor of paths crossing is meant to indicate this.

In other words, there is no conceptual mistake involved in using Marx's or Freud's conceptions in, say, literary criticism, but there is no *a priori* warrant for doing so. Put that way, this may seem obvious, but I think it is very easy, given the Althusserian metaphor of continents, to assume that there are whole areas of virgin territory just waiting to be opened up for colonisation by psychoanalysis or Marxism, and which, by virtue of belonging to the appropriate continent, have been assigned to the science in question by Manifest Destiny. We should remember Lenin's warnings:

> Whenever any Marxist attempted to transmute the theory of Marx into a universal master key and ignore all other spheres of learning Vladimir Ilyich would rebuke him with the expressive phrase 'Komchvanstvo' ('communist swagger').
>
> (Trotsky, *Problems of Everyday Life*, p.221)

It goes without saying that one should be equally wary of psychoanalytic swagger. This does not mean that we should dismiss works such as *The Future of an Illusion*, *Civilisation and its Discontents* *Group Psychology and the Analysis of the Ego*, or *Leonardo*, nor that we should regard them as fictions like *Moses and Monotheism* or *Totem and Taboo*; but the most we can say for them is 'that sounds plausible; that seems to throw light on such-and-such a phenomenon'; we should not expect from them knowledge of society, or evidence for Freudian theory, or grounds for new speculations within that theory.[24]

Finally, I want to show that the view of scientific practice in the human world that I have presented is not incompatible with a view that I also hold, namely that, as Roy Bhaskar puts it, 'there is an ontological hiatus between society and people' (*The Possibility of Naturalism*, p.46).

My view may seem at first sight to make all categorial boundaries within the human sciences artificial, to support the notion that 'in itself' the human world is not ontologically differentiated. In fact, I have not as yet said *anything* about different ontological regions in the human world - only about different scientific disciplines. My 'practical demarcation' operates at the level of epistemology, not of ontology. But the question remains, how can we *know* of this ontological hiatus, given that we cannot in principle establish closure - i.e. study de-socialised people or a de-populated society? The answer must be along the following lines: it is no accident that we have the practices that we do; the fact that the practices of the talking cure and of working-class politics are possible, and are distinct, tells us something about the nature of the realities on which they act. A Bhaskarian transcendental argument for the ontological stratification of the human world is therefore possible, but it will be an argument from the possibility of certain practices, rather than of certain bodies of abstract knowledge, as in the case of the natural sciences.

This requires a bit of elucidation. If we look at Bhaskar's argument for the ontological hiatus,[25] we find that it has to do with the 'transformational model' of the society/person distinction. People reproduce and transform society, usually unconscious-

ly, in the process of engaging in conscious activities with some other aim; and the manner in which they can do so is fixed by their places in existing society. The two aspects of their activities must be kept distinct: 'we do not suppose that the reason why garbage is collected is necessarily the garbage collector's reason for collecting it' (*ibid.*, p.45).

Now scientific socialism and psychoanalytic therapy are practices which aim *consciously* at the self-transformation of society or people (respectively). And it is not *logically* inconceivable that the structure of society could be changed simply by changing people. Utopian reformers such as Winstanley, Godwin and Tolstoy have always thought in those terms. Likewise, it is not *logically* inconceivable that people could be nothing but passive mirrors of their social positions, which could develop only insofar as the social structure changed. Some of the Italian fascists believed something of the kind, and you occasionally hear it from 'Marxists' in moments of *Komchvanstvo*.[26]

Scientific socialism is premissed upon the rejection of (at least) the former as a historical possibility: it is no use converting the boss to socialism, the constraints of competition will make him act like any other boss. Of course, in order to change the social structure, it is necessary to convince a lot of people that the change is needed; but these activities of persuasion in which the vast bulk of socialist political activity consists are not an end in themselves, but constituted entirely by the need to change the structure. This applies to here-and-now politics as well as the revolutionary goal: the aim is not to change people but to build an effective party, strong trade unions and so on; at every turn, scientific socialism directs activity away from individual change towards structural change - pointing out, for instance, that having leftist individuals as trade-union officials solves nothing, while altering the trade union structure so that officials are accountable to and dismissable by the members solves a lot.[27]

The non-reducibility of structures to people then can be seen as a condition of the possibility of scientific socialist politics. Conversely, the non-reducibility of people to the social positions of which they are bearers is a condition of the possibility of psychoanalysis. If people were wholly passive products of society, conditioning would be possible, but not psychoanalytic therapy or rational argument. If they were wholly self-determining rational agents, rational argument would be possible, but not conditioning or psychoanalysis. But all three are possible.

The possibility of psychoanalysis indicates that people are agents capable of rationality, but subject to inner as well as outer constraints, partly of a biological nature, but partly also autonomously psychological ones - structures precipitated by a personal history.

It seems then that we know more about the broad ontological structure of the human world than of the more abstract parts of existing human sciences, or of the conditions of the applicability of their concepts. Our familiarity with the forest paths enables us to make sound generalisations about the geology, fauna and flora of the forest. But it leaves us permanently without serviceable maps.[28]

Footnotes

1 The same task is undertaken with many of the same assumptions and in a systematic fashion by Roy Bhaskar, in his book *The Possibility of Naturalism*, which was published while I was in the process of writing this essay. But I had not had time to fully assimilate the findings of that work, and my references to Bhaskar, except in the final section, are to his earlier work *A Realist Theory of Science*.

2 Abridged version in *Radical Philosophy 20*. Page references are to the full version - stylistically mutilated by a copy-editor, sometimes to the point of literal nonsense - in *Issues in Marxist Philosophy*, Vol.III (eds. Mepham and Ruben).

3 Represented thus on page 13 of *A Realist Theory of Science*

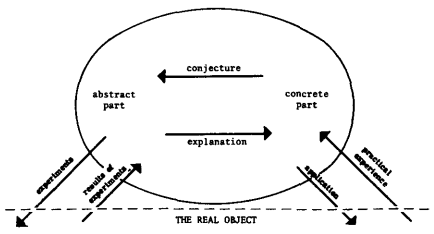
	Domain of the Real	Domain of the Actual	Domain of the Empirical
Mechanisms	✓		
Events	✓	✓	
Experiences	✓	✓	✓

4 All quotations from Cioffi are from his essay 'Freud and the Idea of a Pseudo-Science' in *Explanation in the Behavioural Sciences* (eds. Borger and Cioffi).

5 Though most of the predictions attributed to Marx by Popperians are fictions. Marx would have burst his ulcers laughing at the idea that Britain would be the first country to have a workers' revolution.

6 That is to say, Popper assumes that any laws, explanations or predictions postulated in the human sciences must be claims about the open world of history or biography; then he attacks them for failing to recognise the abstract nature of the laws of science, and the conditional nature of its predictions. Likewise with the accusation of unfalsifiability, which only appears to stick to Marx and Freud because they are assumed to be working in purely concrete sciences, and it is a characteristic of all concrete sciences - including natural ones - that counter-examples don't falsify them. Unfalsifiability is only an objection in an abstract science.

7 The structure of science can be represented thus:



The arrows on the left side are absent in the case of the human sciences. This is what I mean by calling them 'concrete-bound'.

8 In the 1857 Introduction. See *Grundrisse* (Penguin, 1973), p.101, where the actual words are: 'The concrete is concrete because it is the concentration of many determinations, hence unity of the diverse.'

9 See 'Falsification and the Methodology of Scientific Research Programmes' in *Criticism and the Growth of Knowledge*, eds. Lakatos and Musgrave, Cambridge University Press, 1970.

10 See *The Psychopathology of Everyday Life*, Chapter II.

11 I am sorry about the cryptic nature of this remark, which I hope will become clearer in the light of the final two sections of this essay. But roughly speaking, what I mean is: if someone says of a hypothesis in the human sciences 'that it couldn't be true', then pointing out the truth of a hypothesis in the natural sciences postulating structurally similar realities is a useful reply; but if the objection is not 'that couldn't be *true*' but 'that couldn't be *known*', such an appeal to natural-scientific parallels has no force. The human sciences really do labour under epistemological handicaps.

12 See John Mepham's criticisms of Popper in 'The Structuralist Sciences and Philosophy', in *Structuralism*, ed. David Robey, p.109.

13 I admit that I do not argue for this conclusion *re* Pavlov - but I shall do so in connection with the 'experimental' testing of Freud's theories, and the argument can easily be generalised to 'experimental' psychology in any form.

14 They are taken, respectively, from *Freud and Psychology* (eds. Lee and Herbert), *The Scientific Evaluation of Freud's Theory and Therapy* (eds. Fisher and Greenberg, and *The Scientific Credibility of Freud's Theory and Therapy* by Fisher and Greenberg.

15 *The Scientific Evaluation of Freud's Theory and Therapy*, pp.237-248. Researcher: Paul Cameron.

16 It was fully admitted that only approximate results could be expected, as the ages at which the various stages are reached vary from individual to individual.

17 Indeed, it might be argued that the 'fundamental rule' to free-associate *does* establish a sort of closure, as the pressures of reality, conscience etc. are removed so that wishes can be studied in their nakedness. But this would depend on the unrealistic assumption of a perfect patient.

18 Eysenck: chapter on 'Politics and Personality' in *Sense and Nonsense in Psychology*.

19 Compare Heidegger: 'When its unusability is thus discovered, equipment becomes conspicuous' (*Being and Time*, Blackwell, 1962, p.102) - though I do not know what Heidegger would think of my thus generalising from the manner in which the being of the ready-to-hand is disclosed, to that of Dasein. For Heidegger, it is in *Angst* that the being of Dasein is disclosed, but from a Freudian perspective, *Angst* appears as one way among others in which, if I may so express it, Dasein becomes un-ready-to-hand for itself.

Freud is quite explicit about the special epistemological status of the pathological: 'neurotic human beings offer far more instructive and accessible material than normal ones' (from *The Question of Lay Analysis*, quoted by Paul Roazen, *Freud and his Followers*, p.151).

20 If it is wished - which perhaps it should not be - to designate 'the object' of psychoanalysis in one word, I suggest 'the irrational' is the best we can do. It covers a larger area than the unconscious, and it is a concept which, as distinct from the rational and the non-rational, had never been adequately theorized before Freud.

It is paradoxical in the extreme that some Freudians have thought that Freud showed that there is no such thing as the irrational, presumably on the ground that he showed irrational phenomena to be *interpretable*. It is exactly as if someone were to say that Marx, by showing that surplus value is produced in the case that all commodities exchange at their value, had shown that exploitation did not exist under capitalism.

Of course, it is also true that psychoanalysis tells us something about the *conditions* of rationality in thought and action - but not about its *content*.

21 Similar - though more serious - examples are quoted by Juliet Mitchell in her counter-critique of Freud's feminist critics - see her *Psychoanalysis and Feminism*.

22 Compare Marx: 'My standpoint from which the development of the economic formation of society is viewed as a process of natural history, can less than any other make the individual responsible for relations whose creature he remains, socially speaking, however much he may subjectively raise himself above them' (Preface to *Capital*, Vol.I).

23 In the 1844 Manuscripts, *Early Writings*, p.355.

24 A great many misplaced attacks on psychoanalysis - and as many blind-alley developments of it - stem from an inadequate grasp of the different statuses of Freud's various works. On the basis of considerations put forward in the present essay, I suggest the following categorisation (in order of diminishing scientificity):

(i) First-hand case studies such as *Studies in Hysteria* and the cases of Dora, the Rat-man and the Wolf-man. (These are of course themselves by no means theory-free, and neither could they be.) The nature and treatment of the empirical material in *Three Essays on the Theory of Sexuality* and some of the early clinical papers put them in this class too.

(ii) Abstract scientific theorizations such as the meta-psychological papers and *The Ego and the Id*.

(iii) Non-clinical interpretive work (dreams, jokes, slips) and second-hand case studies (little Hans, Senatspräsident Schreber). These use theoretical concepts derived from clinical work, in contexts (i.e. individual psychopathology) where they are known to have application. Nevertheless, the openness of the systems involved, and the absence (except in the case of patients' dreams, which really belong to class (i)) of clinical checks (resistance-analysis, abreaction) make all the interpretations highly problematic.

(iv) Applied psychoananalysis (as discussed in the text).

(v) Fictions, whether concrete (*Totem and Taboo*, *Moses and Monotheism*), or abstract (such as *Beyond the Pleasure Principle* or the draft 'Project for a Scientific Psychology').

25 See his *The Possibility of Naturalism*, pp.34-47.

26 I would suggest that the slogan 'the personal is political', when used by non-Marxists, generally implies the former error, and when used by Marxists, generally implies the latter.

27 Perhaps it is necessary to point out that nothing I have said implies the absurd view (sometimes attributed to the Marxists of the Second International or to Althusser) that social structures can be changed other than by human agents - who of course will change in various ways in the process. But the fact that changes in structures involve changes in people does not imply that the two kinds of change can simply be equated. A personal very important change in an individual's life can be politically irrelevant and *vice versa*.

In short, one who thinks there is an ontological hiatus between societies and people need not be disconcerted to find that structures do not take to the streets; I suspect that those who disregard this hiatus will never be able to explain the fact that, for the most part, workers do not either.

28 A note where perhaps another paper is required:
If my model of the work of the human sciences is correct, it might go some way towards explaining the fact that, whereas the natural sciences progress exponentially, the tendency is for any human science, after an initial burst of discovery, to mark time. Thus, psychoanalysis can hardly boast a discovery in the last sixty years, of the same magnitude as half a dozen or so made by Freud in his first ten years of analytical work. Since then, development has largely taken the form of adaptation to new human subject-matter, and the rejection of various false paths. If my view is correct, this would not be surprising. Radically new knowledge of the human world is unlikely to come from already-established sciences; it is more likely to arise from the production of materialist explanatory speculations in connection with some *practice* which has hitherto not been theorized scientifically.

This view of the development of the human sciences, taken together with the fact that these disciplines are heavily subject to ideological pressure, may also account for another singular phenomenon: that precisely the most rigorous and critically intelligent practitioners of the human sciences will often appeal to the early sources of the science, as if to an 'orthodoxy' against 'heretics'. This need not be due to dogmatism; granted the small progress which takes place beyond the initial discoveries, and the ease with which those discoveries are mutilated by and re-absorbed into the ideologies which they had replaced, such defence of 'orthodoxy' may be a condition of the survival of socially critical knowledge.

This does not mean that there is no real danger of dogmatism, but the form it generally takes is failure to comprehend new departures in the subject-matter (e.g. the changing culturally-induced images of masculinity and femininity).

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