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Scientific principles in psychodynamic interpretation.

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SCIENTIFIC PRINCIPLES

IN

PSYCHODYNAMIC INTERPRETATION

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Docte, doces pueros recte componere verba:
Ignoscas peto, si lapsa sit haece manus!

N.M.C.

SUMMARY

The interpretation of behaviour in psychodynamic terms serves both as a mode of explanation and as an agency of therapeutic change. Although there is some discussion of the relation between these roles, the main purpose is to investigate the way in which it carries out its explanatory function. This is done with special reference to some of those rational and empirical principles which are regularly said to characterise "scientific" procedures, and to be absent from psychodynamic ones. But since human behaviour is characteristically purposive 'intentional' and expressive, and since interpretation is typically concerned with what such behaviour means or represents (and with the method of communicating such inferences), some affinities are explored with artistic interpretation which also has elements of explanatory analysis and executive communication (chh. 2, 8). A second principal analogy is the understanding of language, where problems of decipherment, translation and textual criticism are argued to have important parallels, in their rationale and use of evidence, with psychodynamic interpretation (esp. ch. 7). This leads to the suggestion that many psychodynamic concepts refer to (or are in some sense 'models of') generative or transformative mechanisms relating underlying structures to particular behavioural episodes. This in turn reflects our main contention (esp. ch. 5) about "scientific principles", which is that the appropriate paradigm for human behaviour is that of structure-modelling (in the wide variety of ways used in real, rather than stereotypic, "science"), rather than that of hypothetico-deductive experimentalism. From these points of view, psychodynamic interpretation is defended against some familiar but misdirected criticisms to do with its supposed lack of precision and objectivity, and its reliance on contaminated evidence (esp. ch. 4). Suggestions are also made *passim* about meeting valid features of such criticism.

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INTRODUCTION

"Could he, whose rules the rapid comet bind,
Describe or fix one movement of his mind?"

POPE: Essay on Man, II, 35.

Few people would nowadays write an Essay on Man; but that is not to say that the "proper study of mankind" is neglected. Rather, he is treated differently. His actions, capacities and functions, his perceptions, responses and attitudes, his motives, relationships and organisations, his thoughts, feelings and dreams, have all become the subject of that motley group of inquiries known as the Behavioural Sciences. This title is intended, no doubt, to signal to the impressionable the transfer of such topics from the realm of educated literature to that of the laboratory, where numeracy has too often been substituted for both literacy and education. In Britain, we even have a government institution to mark the contrast: the Department of Education and Science. Some voices, however, are still occasionally raised which venture to ask, with the poet, whether Man really is, after all, a suitable subject for Science (Ayer 1964, Deese 1972).

Some of those who raise this question do so, to be sure, for the wrong sort of reason. There is, for example, the obscurantist view that the mysteries of the human soul are not to be explained.rationally nor to be treated in terms of law-like regularities; but this will not be our concern. Neither shall we give much space to the aetiological objection that to adopt an avowedly 'scientific' approach to emotional functioning is symptomatic of schizoid ~~im~~depersonalisation and over-intellectualism (Fairbairn 1952, p.6; Guntrip 1961, pp.249-253) ~~Sutherland 19)~~. But it may also be doubted, on more sober and rational grounds, that actions which are a function, in an admittedly problematical sense, of the agent's preconceived intentions and purposes, can be studied and understood adequately by methods that have proved fruitful with planets, chemicals and steam-engines.

Nevertheless, such is the prestige which the established physical sciences have derived from their spectacular achievements, that other studies, such as psychology, economics, archaeology, sociology and anthropology, which have been anxious to claim an intellectually respectable pedigree (in the belief that hope of progeny goes with pride of ancestry), have consequently devoted considerable energy to insisting that their methods are objective, to hammering their data into quantifiable form, and generally to sacrificing on the altar of Scientism. Even the ancient Muse of history has been seen to shuffle the dust of Helicon, cast an uneasy glance over her shoulder, and grasp ambivalently at the passing bandwaggon. So eager, however, were some psychologists to mount the vehicle, that she was brushed aside; and they, having clambered aboard, began calling out to those students of Man who were less precipitate, "Either you have a Science or you have nothing". Thus they revived the kind of sectarian exclusiveness which fortified theologians through the Dark Ages, for St. Augustine had similarly preached that there was no salvation outside the Church: <u>salus extra ecclesiam non est</u>.

The rejection of this attitude, so far as it touches psychodynamics in general and interpretation in particular, will be a recurrent theme in our discussion. We shall have to take account of the dispute which has divided psychologists and commentators into those who hold, on the one hand, that the study of psychodynamics can be significant and constructive, and those, on the other, who see nothing in it but misguided and disreputable crystal-gazing. The former, it is true, are often none too convincing when pressed to give an account of their methodology, while the latter parade the banner of 'Science' and dress the window with slogans about 'objectivity', 'hypothesis-testing', 'predictions', 'experimental method' and the rest of it. These guardians of the Popperian shrine attack the plumbers of the soul for being not only unscientific in their empirical procedures (a sufficient crime), but also illogical in ~~the~~ their argumentation. We try to identify and straighten out some of the muddles which confuse this controversy (chh. 5-7).

Our general defence will be that not all approaches to the understanding of human behaviour (or anything else, for that matter) need to be 'scientific', in the sense required by these window-dressers, in order to be rational, useful and concerned with factual truth. Specifically, we investigate the nature of the logical and empirical work done by certain kinds of statement about why people do what they do. These statements, and the arguments in which they play a part, are of the sort which typify 'psychodynamic' accounts of behaviour: that is to say, those which deal with the interplay of such mental states and forces as regularly influence, and sometimes dominate, our actions and experience. I mean, of course, the statements which are known as 'interpretations' because they are intended, in the first instance, to illuminate the meaning of some behaviour (and only indirectly, perhaps, to indicate what has caused it).

Our contention will be that, although the implied rationale which supports or generates such interpretive statements in a particular case is demonstrabley different in important respects from what is characteristic of procedure in many physical sciences, yet it is not necessarily any the worse for that. For it has features in common with the procedures and argumentation used by other learned men, such as historians, archaeologists, linguists or epigraphists, who also manage to arrive at true, factual, well-founded and sometimes spectacular conclusions about their subject-matter.

This is not to deny that extremists in the psychodynamic camp have shown a culpable disregard for standards of evidence, objective observation and philosophical acumen in their speculations about the emotional currents underlying people's behaviour. But it is important to convict them on the right charge. To wave the slogans of scientism at them may be to misconstrue the logic of what they are up to (or could be up to), and to accuse them of not being something which they have no need to be. And yet, in their concern ~~eagerness~~ to appropriate to the study of Man the supposed tools and tactics of some physical sciences, many psychologists over-reach themselves and seem to assume that only physicists had previously been concerned with observation, facts, evidence, accuracy and sound reasoning; and, further that the implied rationale of "scientific discovery" as spelled out by Popper in the 30's remains the alpha and omega of empirical truth.

Thus Eysenck tells us that either "psychoanalysis is a science, or it is nothing"; and again that "the answer to the question 'what is wrong with psychanalysis?' is simple: psychoanalysis is unscientific" (1963, p.68; 1953, p.241). The answer is complicated, however, by his telling us at the same time that you can judge whether a discipline is "scientific" or not "without value implications" (1953, p.226). For it follows from *that* that the value-free statement '*P* is unscientific' cannot be an answer (simple or otherwise) to the value-loaded question 'what is *wrong* with *P*?'; nor can it generate the inference that *P* is "nothing", for that is a *denial* of value. "Scientific statement", he also tells us, "is based on fact and rigonous logical reasoning" (p.227). Well, does he mean that *only* scientific statement is so based? If so, then history and literary criticism emerge as 'scientific'; if not, then it does not matter that psychoanalysis is unscientific, for it may still rest on fact and sound reasoning, which is all that matters. But even these "scientific truths" are not all that you might think: they have the disappointing property of not always being "correct". According to Eysenck, these "truths" (only) "*tend* to be correct", because of the method by which they have been reached (*ibid.*, italics added). A flexible conception of truth, to say the least. Psychodymanic ideas in general, he concludes, rest on "loose and wishful thinking". Evidently not *only* psychodynamic ideas.

The purgations of Positivism and Behaviourism were, no doubt, a therapeutic antidote, in the study of Man, to the romantic imagination of some specualtors; but fortunately for the understanding of behaviour, and for the specific contribution of psychodynamic interpretation, these latter can be based on rational principles other than those of the idealised laboratory. These principles are discussed from three main angles.

of some speculators; but one is to defend the business against traditional lines of attack, by asking from where, in terms of logical and evidential considerations, any account of behaviour draws its explanatory force. This leads to an inquiry into some uses of analogy in scientific theorising; and to the suggestion that psychodynamic interpretation can helpfully be seen, in its explanatory role, as a special case of analogical explanation. This way of seeing it carries implications for the grounds on which we assess 'evidence' as being supportive, refutatory or neutral ~~in~~ with respect to a particular interpretation.

Elaboration of a principal analogy, and of the way it functions, represents another angle. That the understanding of behaviour should be regarded as more like understanding a language than understanding a thunderstorm, is not a new idea. In his Introductory Lectures Freud makes it clear that psychodynamic understanding involves believing in "the sense of symptoms"; and Rycroft has argued this metapsychological standpoint explicitly, if rather negatively, in more recent times (1966, pp. 7-21). But perhaps the most welcome philosophical support for this attitude comes indirectly from a non-psychodynamic study by Harré & Secord (1972), who argue for an "anthropomorphic model of man", characterised by attention to 'powers' rather than 'causes', in which some Freudian concepts might have the status of models for the unknown mechanisms which generate (by analogy with psycholinguistic concepts of 'generative grammar' and 'deep structure') the subjective significances with which people invest objects and events. We explore this approach by reference to certain linguistic activities including translation

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and decipherment, and to the way in which their practitioners meet questions of evidence, explanation and validation.

The third angle is to make explicit, what we have so far assumed, that the explanatory role of intepretation can, for some purposes, be separated from its therapeutic one, and to ask what light each throws on the other. The job of interpretations in therapy is to get something done; but presumably what they are intended to do is a function of what, if anything, they are supposed to be saying. Another analogy is pursued in order to illuminate both the nature of the claim-content in what is being said, and the relation between that and the job-content. The analogy is that of the interpretation of musical structure, which has also two aspects: on one hand, the analytical description, or depiction, of the alleged 'structure' of work; and on the other the communication of a sense of that structure to an audience in performance. We shall see that the validation of these analytical structure-claims, often in the face of rival claims, raises similar problems to those encountered by the explanatory claims of psychodynamic interpretation; and that the executive aspect of musical interpretation resembles, if only in its (partial) dependence on the analytic aspect and its need to make a systematic effect on an audience, the job-content of therapeutic interpretation.

It will be apparent from this that the metaphors of meaning and structure, as well as reflections on the characteristics of science and rationality, will run through our discussion. They will keep cropping up, like the themes in a multiple rondo. But if there is no strict sonata form, there will at least be one continuous pedal note: namely, the insistence that the physical sciences and their methods are not the sole repositories ~~guardians~~ of rationality nor of fact; and that there is more to the study of Man than slogans about objectivity, measurement, prediction and controlled variables. Science, we shall say, was made for man, and not man for science.

"Trace Science then, with modesty thy guide;
First strip off all her equipage of pride..."
POPE: *op. cit.*, 43.

Chapter 1

Dramatis Personae

(a) What are we talking about?

(b) Some aspects identified.

(c) A glance at representation and expression.

(a) What are we talking about? Let us work outwards, by way of conceptual reconnaissance, from an example of a very general kind of psychodynamic interpretation, which does not presuppose any particular theory of personality or psychopathology, and which I take to be typical of many sorts of interview-transaction, such as may occur in social case-work, vocational guidance, marriage-counselling, commercial 'depth-interview', general medical practice, or outright psychotherapy. We shall then be able to see more vividly what kinds of question, comparison and distinction suggest themselves (or ought to be suggested), and we shall have forged some pegs upon which to hang the controversial issues which I want to discuss.

A patient who has been coming to see me regularly greets my habitual first phrase of our therapeutic session one day with "Ah, your conventional opening!"; and I reply something like, "Perhaps you feel that what you have to say to me today is not so conventional...". (The fact that some schools of psychotherapy self-consciously avoid any such first phrases is, of course, irrelevant.) With this single speech-act I am doing two, and perhaps three, things. That it may 'mean' many more things to the particular hearer is again another matter; except insofar as the contrast between what a speaker intends to do, logically speaking, in a speech-act and what effect he has on a hearer has proved important, as we shall see, in the analysis of 'meaning'.

(i) By indicating what may be on his mind, and that the associated feelings can be recognised and accepted, I hope to make it easier for him to express those ideas and affects. (ii) What I actually suggest, by way of identification, about just what is on his mind, namely that it is something unconventional-seeming, looks like a tentative assertion of fact, of what-is-the-case. (iii) And I also imply, because what I say derives from what he says, that there is a connection, between his categorisation of my behaviour as conventional and his hypothesised (by me) perception of his own, in respect of the same construct (to use Kelly's (1955) language), - that of 'conventionality'. The former is taken as a sign of, or pointer to, the latter.

The attempt, in (i) to loosen up communication is part of a general effort to move the patient's behaviour in a supposedly beneficial direction, and as such is hopefully therapeutic. Or at least, lest we assume too much, insofar as it moves or changes him at all, it is what Strachey (1963) and Rycroft (1968, p.76) have called "mutative" and Farrell "transformative". Now there are many ways, of course, of transforming people's behaviour by saying things to them: promises, advice, suggestions, threats, bribes, flattery and abuse are some.

The third aspect (iii) of my speech-act looks in itself like a relatively independent matter-of-fact claim that the patient said Y because (in some sense) he had X on his mind. That is to say, it looks like offering some sort of (causal) explanation of why he said Y; an explanation which would stand or fall on merits independent of the motive for which it was advanced (that being to help get him better). And there is more to it than this: because the traditional notion of interpretation implies that this relation between X and Y is something more specific than just a vaguely generally causal one.

It has some causal-looking features, to be sure, but we have to look a bit closer. For if the patient comes in limping and with his trousers torn, I might infer that he had fallen and hurt his knee on the way to the clinic. This, however, would not constitute an interpretation of the limp; not at least, of the kind we are concerned with. But if he seemed to be drawing attention to the limp, by fussing about it unduly or parading it, I might think that this total behaviour (the fussing-about-the-limp) meant, or was 'his way of saying' (as the phrase goes) "Look what risks of injury I take coming to see you; you cannot reject me now, can you?". This would be to interpret the situation. An interpretation proper, that is to say, does not merely assert, implicitly, that X is evidence for Y, or is an indication that Y is the case: it asserts that X is an expression of Y, admittedly in a problematical para-linguistic sense, and with the notion of (unconscious) intention to express or communicate not far below the surface. And insofar as what is said to be so expressed, is a feeling, thought, attitude, idea, anxiety, motive etc. (in other words, some 'mental' state, event, force, etc.), to that extent it is a 'psychodynamic' interpretation. Further, it is the combination of this particular kind of explanation-claim, as in (iii), with the intention of 'transforming' the patient by giving it, as in (i), that would seem essentially characteristic of a therapeutic interpretation. We shall have to keep returning to this idea.

The distinction between these two aspects of clinical interpretation is brought out by noticing that, as a therapeutic or transformative act, it invites such questions as 'Was it effective?' and 'Was it given at the appropriate time?' or '...in the appropriate way?'; whereas in its quasi-explanatory role it is presumably to be judged as true, false, more-or-less precise or more-or-less complete. But how, in the latter case, are we to assess this particular sort of 'explanation'? The varieties and principles of explanation have been much discussed, to say the least; and we shall have to refer to some of the studies which impinge most closely on our topic (chh. 5,6). Let us just notice, for the moment, that it is one thing to explain how-something-is-done, and another to explain what-something-means. Explaining what makes a refrigerator work is obviously a very different logical job from explaining what 'polytheistic', or a certain ~~meanscertain~~ Italian gesture, means.

Suppose that the patient in our example does turn out to have the required sort of thing on his mind (that is, something ("unconventional")); in fact he began to talk of wanting to be rid of his wife, and to marry someone else's. That does not in itself substantiate the implied interpretive claim that his first remark ("Ah, your conventional opening!") was a <u>function</u>, in terms either of cause-effect or of meaning, of what was on his mind. The two things, comment and preoccupation, might have been quite independent. But how could it ever be shown that there really was such a relationship as postulated: namely, that the comment was a sign, effect or expression of the preoccupation? (The objection that this is a pre-Rylean formulation can be waived for the moment.) That the patient should have such a thing on his mind is a necessary condition of accepting my explanation-claim as true; but it is surely not a sufficient one because the preoccupation might have been there without having generated the comment. I still need vindication for postulating the <u>link</u>, and it is a problem to know what form such vindication would take. For that depends on what <u>sort</u> of link I have postulated.

Several questions, then, are already raised by our example, and they give some idea of the kinds of problem we shall be concerned with. They are raised, moreover, by a somewhat mundane example, which is at once less complex than some florid dream-interpretation and more characteristic of everyday psychodynamic transactions. There seem to be a number of separable strands running through and making up even this rather prosaic sort of interpretation: meaning, communication, transformation, causes, explanation, and individual 'perspective'. Insofar as particular interpretations vary in type, and in the purpose for which they are made, they will vary as to which of these threads are present, and as to which of those present is or are the most conspicuous.

But should we not specify, in a more comprehensive way, what defines the class of proposition or activities, called 'interpretations', of which our example is an example ? Perhaps we should try to say what particular subgroup of what wider class of accounts of human behaviour is to count as that of interpretations. It may be a mistake to think this a necessary prelude to a coherent program of investigation, and there is certainly a danger that in trying to squeeze the concept into a definitive mould you will distort it by including too much or by excluding something that is wanted; or that you will stultify it by producing a formula which is logically circular. This was the fate of some well-known early definitions of 'learning' by science-conscious but philosophically naive psychologists. "We may define 'learning' as a change in the probability of response" wrote Skinner (1950, p.199). Well, you may do, at your peril. Because you will end up talking about something very different from what everybody else understands by 'learning'; and it will become appropriate, on your account, to designate as 'learning' certain behavioural changes which have nothing to do with learning. Thus, if I catch a cold, the probability of my 'sneezing-response' changes, but I have not learned to sneeze more often. Skinner's definition includes too much.

Hilgard had foreseen this pitfall, and tried to avoid it by stipulating what such a change in response must be due to in order to count as learning. So he introduced the proviso that it must be due to 'training' (1948, p. 4). But he is now faced with the task of specifying some independent criteria for what activities are going to count as 'training'; that is, criteria that do not themselves refer back to the definiendum 'learning'. His specification will have to exclude, for example, the effects of accident, surgery or medication without mentioning learning, Insofar as he fails to do this, perhaps because the task is logically impossible, the proviso about 'training' risks making a circle of the definition and rendering it worthless; ~~and~~ furthermore it would seem also to reduce the animal experimenters' concept of 'latent learning', which is characterised precisely by the absence of training, to incoherence. It is only fair to add that, in a revised edition of his discussion (1956, pp. 1-6), Hilgard treats the problem more cautiously.

(b) Some aspects identified. Let us see if some textbook definitions of 'interpretation' fare any better. There will be the difficulty that some will be concerned with particularities like dreams or symbols, rather than with behaviour in general; but it may be that there are only particular interpretations of particular sorts of behaviour (such as dreams, slips of the tongue, gestures, psychosomatic symptoms), and that it is a mistake to expect them all to function in the same way and exhibit the same range of logical properties. Concentrating on the practical use of interpretation in therapy, Levy gives an account which, like Skinner's of 'learning', is altogether too loose and unselective at some points. He writes (1963, p.5) that it consists typically in presenting "an alternate description of some behavioural datum" in order to "redefine" or "restructure" a situation for someone who is "in a bind". But only some such proferred restructurings of my view of an action or habit of mine are interpretations of it; and we need to mark off which they are. Christian preachers, including the poet George Herbert, have often exhorted bored housewives to 'restructure' or construe their household chores as a contribution to the will of God, to the Grand Design, to cosmic order or to the fight against entropy: "Who sweeps a room as for Thy laws/makes that and the action fine" (Herbert, The Elixir). But this is not interpretation, in the sense that Levy wants; it is preaching.

What Levy is trying to get at, unsuccessfully, is the idea that people are often "in a bind" because they are really (deep down, unconsciously) structuring some situation in a different way than they (consciously) recognise; and that interpretation consists in bringing to light, and presenting them with, this hitherto hidden (neurotic, infantile, anxiety-driven) mode of structuring. How far, and in what way, a therapist goes on to encourage an alternative (and hopefully more realistic, healthy or adaptive) structuring is a matter of therapeutic doctrine and technique. The material which is interpreted, however, is not necessarily or entirely "hidden" from the patient. Indeed, sometimes it is all too apparent, and the purpose of the therapist identifying and verbalising it is held to be to reassure the patient that his feelings etc. are not too terrible to be faced openly. This is sometimes urged as one point of interpreting to children, and we consequently find Money-Kyrle omitting from his characterisation of Kleinian interpretation any reference to how accessible to the child are the feelings referred to: "...she provided them with toys and encouraged them to 'play freely' ... . She then 'interpreted' their play, that is, she described to them the feelings and phantasies which seemed to be expressed by it" (1955, p.xii).

Rycroft (1968, p.76) offers a definition which is very much more to the point; but it does not quite avoid the difficulty of including too much, and it illuminatingly invites the charge of explaining obscurum per obscurius. After the comment that the general idea is that of "elucidating and expounding the meaning of something abstruse, obscure, etc.", he writes that psychoanalytic interpretations are statements in which the analyst "attributes to a dream, a symptom, or a chain of free associations some meaning over and above (under and below) that given to it by the patient".

But we need to specify more closely the kind of 'meaning' which the analyst atributes, and/or the relation between the interpretive statement and the phenomenon interpreted (that is, the method of deriving the interpretation from the data) in order that not any sort of fuller understanding or greater knowledge of X shall count as a psychodynamic interpretation of X. If Rycroft attributes the meaning of 'chicken-pox' to a patient's spots, this may well be "over and above" (not to say "under and below") that given it by the patient; but the diagnostic judgement is not of the same sort, nor is it arrived at by the same pattern of inference, as when he interprets a glove-anaesthesia as a hysterical conversion of some repressed anxiety about bathing a baby. ~~Just~~ What some of the differences are we shall examine below (ch. 6(a).). All that matters for the moment is to see that this sort of distinction would have to be built into Rycroft's definition in order to make it sufficiently clear-cut.

The possible objection that the chicken-pox diagnosis does not go "over and above" the patient's own judgement that there was something wrong with him, but only elaborates it on the same level by spelling out what is wrong, draws attention to this obscure metaphor of stratification which Rycroft uses. The interpreted meaning has to be at a different level from that apparent to the patient, but so loose is the figure of speech that it can be conceived as either 'above' or 'below', - as a superior or more profound meaning. Does the interpreter trade in mysteries that are supernatural or infernal? The quotation from Virgil which Freud put at the head of his Interpretation of Dreams suggest that he at least was prepared to think that it might be the latter: *Flectere si nequeo superos, Acheronta movebo* ('If I cannot prevail upon the gods above, I shall stir up the Underworld'). But what are these strata of meaning, and what is it for one to be above or below another?

Freudian dream-theory has, of course, made familiar the ~~to~~ concept of 'latent content'. There is a meaning or message, about repressed guilty wishes and feared phantasied punishments, as it were "lying hid" (*latere*) among the distorted and censored images which were 'manifest' to the dreamer. He who knows the code of symbolism (both general to humanity and idiosyncratic to the particular person), and understands the characteristics of 'primary process' thinking which Freud referred to as the "language" of the Unconscious, can work back, with the patient's help, from the manifest dream-image to the latent emotions, impulses, wishes and anxieties. These are thought of as "lying hid" partly because their identity has been actively concealed and obscured by various dream-agencies, and partly because they are in any case of a kind which belong to a lower level of mental functioning in the Freudian scheme. The level at which such forces function is "lower", with respect to those of the *ego* and the *superego*, not just by virtue of being represented as literally at the bottom of a diagram of the 'psychic apparatus' (1933, p. 542), but also because they are supposed to be typical of lower rungs in the ladder of psycho-biological development. They are primitive in the sense of being characteristic of relatively infantile or immature stages of mental and emotional development; that is, stages of irrational, pre-logical, fantasy-dominated thinking and of instinct-driven, crude and impulsive affects.

The notions of obscurity, as to how you discover what is *really* there, and of ontogenetic primacy, with respect to ~~in terms of~~ what *sort* of thing is there, have coalesced in this image of psychic humility. And they both have a firm place in the concept of psychodynamic interpretation. But I shall argue that whereas it is the nature of the forces which it purports to describe that makes an interpretation "psychodynamic", what makes it an "interpretation" is the *method* of penetrating the obscurity.

(c) *A glance at representation and expression*. Let us not be too embarrassed, however, that we have not succeeded in concocting a form of words which includes all the sorts of statements or stories that we are interested in, and which marks off clearly all that do not count. It may be an error to expect them all to share the same properties, so that they can be captured by a schoolman's definition *per genus et differentiam*. Perhaps, although they do a certain *sort* of job, and do it in a certain *sort* of way, what they all have in common is, like Wittgenstein's overworked "games", not a set of categorical properties but only a sort of 'family-resemblance' which shows up here in the line of the nose, and there in the colour of the hair.

When seeking how to pin down what would count as a work of art and what would not, or rather, how to distinguish formally those things that we *do* regard as works of art from those that we do not, Wollheim found himself forced into a similar corner (1968, pp89-120). Since aesthetic analogy is a kind to which we shall return now and again, it will be well to notice how he dealt with the problem. It might seem that all you have to do, as a first move, is to draw up a list of all the different sorts of art-work and then look to see what properties they have in common (and which non-art-works lack). The antecedent unlikelihood of finding any categorical property shared by e.g. a string quartet, Michaelangelo's *David* and a couplet of Theocritus is considerable enough. Add to it the tactical difficulty of knowing how to tell when your list of contenders is complete; and that of identifying the precise nature of the contenders, so as to know between exactly *what* you are seeking resemblances.

This latter problem arises because it is hard to see whether to identify the essential art-object in the case of the quartet, for instance, with the printed score, the composer's manuscript, the sum of all actual or possible performances or the ideas in the composer's mind. If the score, then whose copy?; if the manuscript, suppose it has been destroyed?; if performances, how do you tell that they are *of* that work?; if the ideas, they no longer exist. Even if we concentrate on art-works whose identity as works of art is intimately connected with that of tangible physical objects (such as paintings and statues), and which seem to be more or less representational, there is the difficulty that we want to attribute to the picture, qua *picture*, qualities which canvas and paint do not have, such as movement, piety or repose.

A second problem, which is closely relevant because clinical interpretations often speak of overt behaviour '*representing*' some unrecognised feeling or motive, is that there are many ways in which *A* may "represent" *B*, and it is notoriously hard to say in virtue of what, exactly, in particular case, *A* is a 'representation' of *B*. Taking up the latter point, Wollheim remarks that resemblance obviously will not do as either sufficient or necessary condition (1968, pp.32-36). Because if I draw a picture of Wittgenstein which in fact looks like Napoleon, that neither makes it a representation of Napoleon nor prevents it from being one of Wittgenstein. True, somebody could use it as a representation of Napoleon in a history class; but this only emphasises the role of intention, and I could still insist that it was really (intended as) a representation of Wittgenstein. My insistence on the intention is not, however, immune to all factual considerations: I should have to revise it if I had copied a picture of Napoleon which was mis-labelled 'Wittgenstein', and was consequently mistaken in thinking that what I was drawing was Wittgenstein.

Since there are many ways of 'representing' any one thing, (the first point above), some ~~ways~~ methods may have nothing by way of physical, or even relational, properties in common at all. The chemical formula for sodium chloride looks nothing like a ball-and-wire model of its molecule, but they are both effective representations of that substance (cp. Wittgenstein on 'picturing' the facts that constitute the world, in Tractatus). How much more is this true in expressive arts, where 'autumn' or 'devotion' may be represented in music, verse or paint. There may be little enough resemblance, indeed, between two artists' paintings of The Creation; but what could there possibly be in common between one such picture and Haydn's orchestral 'Representation of chaos' at the start of his oratorio The Creation?

This lack of categorical correspondences between artistic representations of the same thing, or, for that matter, between linguistic representations, will be appealed to later when we consider the consequences of the assumption that the same psychodynamic element may be expressed in different behaviour; a assumption from which it obviously follows that it would be idle to expect a one-to-one correspondence, over a variety of instances, between observed behaviour and interpreted psychodynamic content. More specifically, we do sometimes speak, of course, of a creative artist's representation of a fairly standard theme, such as 'the fall of Adam' or 'the four seasons', as his interpretation of it, and this is separate from the sense in which an executant performer gives an interpretation of a literary or musical text. But the latter analogy will recur in the context of the therapeutic <u>use</u> of psychodynamic interpretations (ch. 2(c)).

So multifarious are the obstacles to constructing an account based on the communality or exclusiveness of some particular properties, that Wollheim seems compelled to invoke the "aesthetic attitude" and to concede that anything created <u>as</u>, or in the spirit of, art-work has to be allowed to count. Indeed, some would extend the notion to include anything <u>perceived</u> in such a spirit: such objects of 'natural art' being rocks shaped by the sea or oddly-grown branches from a hedge. This is not to say that anything purported as an art-work <u>is</u> such, because its creator might be mistaken in thinking that he was working in the appropriate spirit; and the same presumably goes for a perceiver. Inebriation has been mis-identified before now as inspiration. But if the search for criteria is abandoned (pp.119-120), what is this spirit which is put in its place? Another Wittgensteinian concept, even more nebulous than 'family-resemblance': a "form of life" (Lebensform). Let us see if we can be more specific than this in respect of psychodynamic interpretation.

If we could get no nearer to a traditional definition of terms than the 'family resemblance' analogy brings us, would not this be thought a dereliction of duty on the grounds that systematic theorising depends upon the use of technical concepts whose fields of reference and implication are unambiguous? How else are theories to be tested against facts, revised and replaced? This is the first of many encounters we shall have with what Harré (1970) calls the "myth of deductivism". The myth has been sold wholesale by influential philosophers of science, and it has been bought in bulk by many teachers of psychology, whose students, lacking the equipment to assess it, are left mouthing clichés about hypotheses, predictions and experimental evidence, and are implicitly invited to believe that non-scientific enterprises (like histories of Queen Elizabeth, translations of Voltaire, discussions of 'causality' or commentaries on Tacitus) are characterised by imprecision, capriciousness and indifference to the facts. It is a mediaeval superstition, endorsed by Descartes' fantasy of encompassing all knowledge-of-the-world under a quasi-mathematical scheme of proposition and implication in which deductive logic is the only valid ticket of inference, that you need to be able to mark off complete and clear-cut boundaries of reference before a term can be useful to knowledge. The idea is that by such exhaustive referential circumscription, empirical terms are converted into logical counters suitable for playing the cartesian inference-game. ~~(a sort of metaphysical jeu des cartes)~~. But so far from the whole of scientific methodology exemplifying this schizoid fiction (in whatever up-dated Popperian form), it is not even true, in real-life science, that the use of particular terms necessarily conforms to the plan.

Scriven has made the point with reference to the astrophysicist's concept of a 'radio-star', in his reply to Skinner's critique of psychoanalytic concepts. Skinner had argued that Freud should have avoided some of the looseness and vagueness in his theories, and generally made them approximate more to the good old physical sciences, by defining the central concepts more closely from the start, if only by means of that well-known methodological deus ex machina 'operational definition' (1954, p. 305). Scriven answers that this is a misconceived requirement of scientific theorising because, when you are breaking new observational and explanatory ground (and perhaps even when you are not), you may need merely to sketch in some aspects of what looks like being a new 'concept', in order to focus attention on certain features and suggest connections, while leaving open the questions of what other features may also be important, and hence of what objects or phenomena may come to be subsumed under the new concept. For this sort of reason, "it is as wrong to suggest that Freud should have pinned his terms down to infant neurology or ... to physical and biological science, as it would be to insist that the founders of radio astronomy should have said whether a radio star was a solid body or a region of space. They introduced the term as a name for the hypothesised origin of short-wave electromagnetic radiation". (Scriven 1956, p.128).

That is to say, you may usefully call whatever-it-is that produces certain crucially interesting phenomena, or behaves in a certain way, a "pulsar" without knowing what other properties pulsars may turn out to have. Indeed some of these other properties may turn out to be more important and exclusive than the ones originally picked out, so that they eventually take over as the defining criteria of the concept. When Freud broadened the concept of 'sexuality' by declaring "sexual life comprises the function of obtaining pleasure from zones of the body" (1938, p.26), he was doing something of this sort. And when something crops up which meets most of the criteria, or meets them in a sort of way, doubt will arise whether to teat it as an anomolous pulsar, a true pulsar in freak conditions, or something quite different which is "mimicking" certain pulsar-like effects. But the possibility of such doubts, and the fact that they are not totally forestalled a priori, does not render the 'pulsar' concept useless. Lorenz's ethological concept of 'imprinting' was not made scientifically worthless by uncertainty about exactly what would prove to be the essential features of the sort of behaviour he was marking out, nor about whether, for instance, the human infant's "social smile" should count as an instance (Ambrose et.al. 1963). Such questions do indeed lead to finer distinctions being drawn, and to some aspects being emphasised rather than others, but that is a different matter. This chronic vulnerability of many empirical concepts to the "borderline case" is connoted by Waismann's well-known notion of the "open texture" of many concepts in physical sciences (1945, esp. p. 120).

However, even if we do not offer a formula which is proof against the seepage of uncertainty, any more than Rycroft does, we can surely agree with him that the main heritable characteristic which the family of psychodynamic interpretation exhibits is a concern for "meaning". They are statements which purport to provide understanding of certain behaviour by treating it as bearing obscure but potentially explicable signs, unrecognised by the agent, of the agent's feelings, motives, attitudes, unconsious processes and so on. What kind of signs they are, and what relation they have to the inferences drawn from them (if indeed they are "inferences") is left open at this stage (see ch. 6). I question in advance that these judgements are like 'inferences', because this term suggests too close a parallel with a different hermeneutic situation, namely that of the augur taking the omens (for example, by examining the state of a sacrificial animal's liver) and using them as evidence for predicting the fortunes of some momentous enterprise. There the enterprise is external to the omens in a way that someone's actions are not external to his feelings, motives and attitudes. The actions are not so much evidence <u>for</u> the feelings, and so on, as expressions <u>of</u> them: and there are those, of course, who would even seem to <u>identify</u> the mental or emotional states with dispositions to act in certain ways.

When my patient said "Ah, your conventional opening!" (the argument goes), that was not gronnds for <u>inferring</u> a concern with conventionality; it was an <u>example</u> of such a concern. Or perhaps it was both? Somewhat analogous is the misuse of the inference-paradigm in discussions of perceptual judgement. Early experimental psychologists used to speak of "inferences" made unconsciously from visual "cues" in depth-perception; and, in philosophy, Austin is well known to have argued against Ayer that it is misleading to construe everyday cases of seeing something as a house as a perceptual conclusion based on the "evidence" of raw sense-data. Actually seeing people having dinner is importantly <u>unlike</u> inferring, from the 'evidence' of the crumbs on the table, that people have been having dinner. (Austin, 1962, p.123; cp. Ayer 1967). So it is with the meaning of behaviour. To interpret some action as a sign of some conflict is not just to say that it is caused by some (separate, antecedent) conflict; it is to see <u>it</u> as conflicted behaviour.

Since interpretations sometimes look more as though they are identifying or conceptualising behaviour as being of a certain kind, rather than postulating causal antecedents for it, it is tempting to try to avoid treating them as any variety of explanation. It is true, of course, that the crucial move in some sorts of explanation seems to be to identify some element in the <u>explanandum</u> as belonging to a certain class: 'Why does Smith have fish for lunch every Friday?'; 'Well, he's a Roman Catholic, don't you know'. But this move has explanatory force only because there is a generalisation in the background, under which the identified class (Roman Catholics) is tacitly subsumed. However, even if everything that deserves to be called (strictly?) an 'explanation' conforms to this logical pattern, as disciples of Popper and Hempel (such as D.M. Taylor (1970)) still want to argue despite mounting criticism, I do not want to prejudge the question whether all accounts of behaviour which conduce to its systematic understanding and amelioration also necessarily do (cp. Ch.5, section (a)).

Acceptance of a formal doctrine about how empirical explanations function would oblige one to show either that interpretations really are <u>that</u> sort of thing in disguise, or that their function is different but nevertheless respectable in its own fashion. Reluctance to be restricted in this way is reflected in the rather more informal program of observing that the <u>raison d'etre</u> of all such accounts is to reduce puzzlement to vanishing point, and of examining how various puzzlement-reducing exercises achieve their aim. Now, puzzlement can be intrinsically of different sorts, can be about different sorts of things and can arise from different sources. It would be remarkable, therefore, if there were only <u>one</u> logical way in which it is dispelled, and if there were thus only <u>one</u> logical type for quasi-explanatory accounts of behaviour. But we shall return to this issue below (chh. 5,6).

On the other hand, I do not advocate the opposite view that psychodynamic interpretations, in therapy at least, represent merely a puzzlement-reducing way of looking at the material, which is independent of causal and propositional claims (cp. ch.3). The point of treating some behaviour as the unwitting <u>expression</u> of something, rather than the <u>effect</u> of something, is to draw attention away from the framework of causal analysis and covering-laws, in order to focus it on the question of how we do in fact manage to understand expressions and meanings of various sorts. If we can then show that such understanding, with the empirical consequences that follow from it, is regularly reached in the absence of certain conditions (of precision, control and watertight generalisations) which the priests of scientism worship, we shall protect certain potentially valuable accounts and judgements about human behaviour from unwarranted rejection; and we shall also be able to refine such accounts by clarifying the principles on which they logically rest.

For our purposes, there are at least two forseeable chinks in any would-be definitive form of words for the nature of interpretation through which "doubt may seep in" (to borrow Waismann's phrase): one is in the area of the analysis of meaning, and the other is to do with the tactics of therapy. The concept of psychodynamic interpretation trades, as we have seen, on the metaphor of 'meaning' and on the idea that some actions may be understood as an expression, in a sort of behavioural 'language', of otherwise covert feelings, thoughts, wishes and so on. Freud spoke after all of "the language of the unconscious", of the need to learn the symbolism of dreams, and of the "sense" of symptoms. But there are different ways, as we shall see, in which _X_ can mean, or come to mean, _Y_; and correspondingly various ways in which different sorts of _X_ can be inferred to mean their respective _Y_'s. It may be very hard to say in advance which ways are going to count for our sort of interpretation, and of course which sort (or sorts) are exemplified by a particular instance of making an interpretation. But it will not do to adopt a policy of "let them all come", and to allow any sort of inference _to_ how a person is feeling (thinking, etc.), _from_ observation of what he does, _by_ appeal to its alleged meaning, as psychodynamic interpretation.

If a Frenchman says "_J'ai peur que..._" and I consequently say to a child "That man is afraid that....", I should have

;

fulfilled these requirements; but I should not have made a psychodynamic interpretation. To object that the inferred meaning must be hidden to the agent, "over and above"what he can see (in Rycroft's phrase), still leaves two possibilities: what the latent meaning is may be hidden, or the connection between that and the behavioural 'sign' which is interpreted. If someone hitches me up to a machine that records the electrical conductivity of the skin, and correctly reports that I am feeling anxious, what is that? It seems no less of an interpretation because I knew very well that I was anxious; my feelings may well have been "hidden" from him. And it is very different from my simply telling him, in a semantic system which he understands, that I am anxious. It certainly looks 'psychodynamic', because it is about how I feel; and yet he seems to have started from the wrong sort of material for his conclusion to count. What if I had tripped over the carpet, or come late, or gone t to the wrong room, and he had said to his assistant, "Gee, this guy's nervous"?

Or, on the other hand, suppose I did not know that sweaty palms could be a sign of anxiety; then the judgment "You are anxious" would be "over and above" what I thought the damp palms meant. Does my ignorance of a connection between emotional state and physiological correlate turn someone else's judgement about the fommer into an interpretation? What seems to be important is the way the judgement is derived from the observational data, and what sort of data they are, and the support-relation between this evidence and the interpretive judgement. But here again, can you mark off different sorts of behaviour and relations in such a way that PGR's do not count and carpet-tripping does, and can it be
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done so reliably as to prevent doubt creeping in at this point?

Secondly, in the treatment-situation, remarks made by therapists may convey messages about "hidden meaning" more or less implicitly. This creates problems when a third-party observer tries to analyse a therapist's speech-acts with a view to examining some hypothesis about therapeutic technique ~~(refs.)~~ (e.g. Marsden 1971). Such an observer may want to start by allotting the interventions to the various categories which therapists claim to use, such as clarifications, questions, confrontations and support-noises (Menninger 1958, ch.6). A remark, for example, which sets out as a 'clarification' of what the patient has said about some feeling may be phrased in such a way that is suggests a parallel, comparison or analogy with something else the patient has said previously. That is, the therapist may deliberately repeat a phrase which he used on the previous occasion with the intention, or perhaps only the hope, that it will ring a bell with the patient and get him to associate '<u>Q</u>' with <u>P</u>. The therapist has not <u>said</u> that <u>Q</u> is another case of <u>P</u>, which might have amounted to an interpretation along the lines of '<u>Q</u> means the same as <u>P</u>, did you but know'.

But suppose that <u>P</u> had already been interpreted as meaning <u>X</u>. Then to suggest, hint or convey the impression, that <u>Q</u> is another case of <u>P</u>, or is analogous to it (in some way which is often unspecified), is tantamount to interpreting <u>Q</u> also as meaning <u>X</u>. What is the observer to say that the therapist has <u>done</u> in this case?; was his intervention an 'interpretation' or not?; if so, has the therapist given 'the same' interpretation (as given for <u>P</u>) again? And what <u>is</u> the interpretation, then: that <u>Q</u> means the same as <u>P</u>; or that <u>Q</u>, like <u>P</u>, means <u>X</u>? (Compare Wollheim's quest for the essential art-object, p.1-11 ~~PP~~ above). We cannot let the answers turn on whether the patient took it <u>as</u> an interpretation, or <u>what</u> he took the interpretation to be, because he may mistakenly see all sorts of things as interpretations which are not. And yet therapists themselves are often quite happy with the idea of leaving an interpretation implicit, so that a patient can take it if he is 'ready' for it but not be disturbed by it if he is not.

Chapter II

Diagnosis, Therapy and the Performing Arts

(a) Elucidatory features.

(b) Transformative aspects.

(c) The analogy of 'executive' interpretation.

We have seen that the metaphor of the 'meaning' of behaviour is central to the concept of psychodynamic interpretation; and we have encountered the contrast between treating actions as _expressions_, in some quasi-linguistic sense, of underlying states of mind as opposed to considering them primarily as _effects_ of antecedent causes. This contrast has been epitomised by Bennett (1964, p.14) in distinguishing meaning as 'symbol' from meaning as 'symptom'. We have also met a typical situation in which a judgement about such an alleged meaning is both made, by way of understanding someone's behaviour, and communicated to him for the therapeutic purpose of changing him for the better (howbeit indirectly and in the long run). What I said to the patient who construed my first remark as 'conventional' seemed to serve both these purposes, - elucidatory and transformative. Let us press this distinction futther, and introduce some of the difficulties to which it leads.

(a) _Elucidatory features_. It seems obvious that we can separate these two possible fnnctions of interpretation. I could clearly have made the elucidatory judgement, about the significance of his comment, without ever communicating it to him; and it would have had no chance to be transformative if it had not been communicated. Communication of many things in therapy, as in ordinary conversation, need not, of course, be verbal. But the fact that some ideas and attitudes may be communicated non-verbally (by nods, grunts, facial expressions and other 'meta-messages') is beside the point for the moment. We might just notice, however, that, although it would seem at first sight that only rather crude and unarticulated messages can be sent by that means, yet in the field of drama one thing which distinguishes the great actor from the others is probably his technical ability to convey a wide range of relatively subtle and specific meta-messages to his audience. Not perhaps to the extent burlesqued by Mr. Puff in Sheridan's The Critic (Act III, scene I):

> Sneer: ... Now, pray what did he mean by that?
>
> Puff: You don't take it?
>
> Sneer: No, I don't upon my soul.
>
> Puff: Why, by that shake of the head he gave you to understand that even though they had more justice in their cause and wisdom in their measures - yet, if there was not a great spitit shown on the part of the people - the country would at last fall a sacrifice to the hostile ambition of the Spanish monarchy.
>
> Sneer: The devil! Did he mean all that by shaking his head?

There is no doubt that head-shakes and the like can 'transform' in therapy, in the sense that they can selectively probabilify some sorts of behaviour rather than others, but they do not do so by conveying a message about the significance of the behaviour which they alter. Let us see how far we can separate making elucidatory or diagnostic interpretations (D.I.), simply for their explanatory force, from communicating T.I.'s to a patient to help get him better.

Although D.I.'s all share the common task of unravelling the significance, in a broad sense, of actions which are variously obscure, perplexing, inconsistent or portentous, they will do so in different ways because they will be directed towards different kinds of obscurity, perplexity and so on. Some are concerned with what an image in a dream stands for, some with why a person forgets a familiar name, some with why Mr. X ignores Mr. A. and talks to Mr. B, some with what a mother is trying (or not trying) to tell the doctor by saying <u>that</u> about the child, some with how a man sees himself <u>vis à vis</u> his workmates or boss, and some with what a response to a Rorschach card tells you about a patient. They aim to facilitate the understanding of the presented phenomena, and may be quite independent of any attempt to change them.

We have noticed that, in the ordinary way, people come to understand some sorts of thing or situations by appeal to causes (why has the car broken down?), others by appeal to purposes or intentions (why did he leave the office early on Friday?), still others by reference to ethical considerations like justifications or obligations (Why did you let him off?; Well, you can't kick a man when he's down). It is interesting that children have to learn what sort of explanation it is appropriate to expect in different cases, and to ask (at least according to Piaget) less often for justifications and more often for causes, -

the world being less anthropomorphic than they initially assume. ~~(but cf. Isaacs 19)~~ We shall contend below (ch.5) that there is no one logical framework under which these various appeals can be subsumed, in spite of the fashionable doctrine that they all work in principle by referring a particular statement about the puzzling phenomenon (whether to do with causes, purposes or duties) to a generalisation of the same kind, in such a way that what is to be explained can be read off as the conclusion of a deductive argument such as one finds in elementary geometry.

But philosophers of science are by now familiar with the idea that perplexity is dissolved even in the so-called 'physical sciences' in more ways than one. Sometimes for example we analyse the "fine structure" of something to expose "hidden mechanisms", and sometimes we design different sorts of model or anologue which replicate the functioning of what we are trying to understand. And in case it is argued that even these procedures depend logically on implicit universal generalisations, let us notice that in the fields of language or history, for example, we can arrive at factual explanations in the demonstrable absence of the allegedly necessary generalisations. We shall return to this argument when we consider theoretical objections to the possibility of constructing viable 'explanations' out of the sort of conceptual bricks which psychodynamic interpreations work with (ch.7). For the moment let us merely illustrate the point by reference to another field of human expression, which also serves to introduce certain analogies to which we shall appeal <u>passim</u>.

Literary critics speak of 'interpreting' a drama or the text of a drama. At once we meet a distinction between "the play" and "the text" (of the play), and the argument draws partly on the fact that plays, on the whole, are meant to be acted, to be translated into public human activity, whereas poems and novels are generally not. The same goes, obviously, for a musical score, the performance and 'interpretation' of which will also furnish some analogies. In the case of the play, there are interpretive questions about what the text means, and sometimes even about what the text is; and if we cannot establish even the latter, there is little hope of success in Wollheim's quest for the identity of the essential art-object (p.6, above). There are questions, too, about how an actor conveys his view of the significance of some passage or situation to the audience. Thus Olivier's 'interpretation' of Hamlet consists, of course, in what aspects of the character's behaviour he emphasises, what he communicates about Hamlet's thoughts and feelings, and how he communicates such things. I shall call this sort of interpretation, through the active performance of a text, an 'executive interpreation' (E.I.), because to call it "performative" (as Wollheim does) would suggest too close a parallel with Austin's "performative" "utterances", (which are touched on below (ch.3, Section (c))).

One set of questions which now arises concerns the relation between such E.I.'s, on the one hand, and critical interpretations of the nature and significance of the text, on the other. The latter are interpretations because they have to do with questions of the general type 'What did X (the author, composer) mean or intend to convey by using the symbol-system (words, notes) in such a way?; let us call these 'CI for 'critical interpretations'. Now CI's obviously correspond rather closely to DI's in psychodynamics; and the question of what relation EI's bear to them runs parallel to the question of how TI's relate to DI's in clinical practice. It would seem at first that the relation is one of asymmetrical dependence: for what Olivier does on stage depends largely on what he believes to be true about the text, whereas CI's about the text take no account of what Olivier does on stage. Except that, occasionally, a textual point (CI) may be settled by appeal to what is practicable or customary for actors in general to execute. Thus scholars might reject a reading in a Sophocles manuscript on the grounds that you could not, or did not, do that sort of thing on the tragic stage at Athens. The way in which Olivier expresses in a performance his CI of some passage takes into account also, of course, various conventions and contingent features of stage and audience. But this too has a clinical parallel in that a therapist might communicate the same DI in the form of rather different TI's to different patients; and Levy actually provides a prescription of the forms to be used with different categories of patient (1963, p.80).

The main point here, however, is that CI's themselves do not form a homogenous class of judgements, because they are judgements about a wide and varying range of problems. A systematic study of the various sorts of question to which CI's address themselves in literary criticism has been made by Weitz, taking Hamlet as his main point of reference. He distinguishes three broad types of question, arguing both that the kind of 'evidence' or consideration which is appealed to to settle those of one type will differ from that appropriate to others, and that the relation of logical support between such evidence and proposed answers will also be different for the various sorts of questions (1964, epp.chh. 12 and 13). Whether or not we accept Wollheim's criticism (1968, pp. 106-107) that Weitz's categories of question s? cannot consistently be separated in the required way, there can be no doubt that questions like 'Does this phrase allude to Polonius?', 'Is Hamlet in love with Ophelia/his mother/Horatio?', 'Does Hamlet say "... this too too sullied flesh"?', 'Does the Queen know that the wine she drinks at the end is poisoned?' are different in some important way. To say that the text should read "sullied flesh" and that Hamlet does not love Ophelia, is to assert two very different sorts of fact; and these are probably different again from claiming that in King Lear "Cordelia is a death-symbol", or that Aristophanes' Acharnians is "a plea for peace" (Freud ...; Forrest).

In the case of the textual reading we appeal to MSS, early editions, copying errors and the like; but we also invoke other sorts of consideration such as contemporary pronunciation (would not 'solid' have sounded much more like 'sullied' then anyway?), and the likelihood that one image rather than another is dominant in the pun. In the case of Ophelia, it is a question of how you 'take' certain of Hamlet's utterances, and of whether you think he would say or do these or those things if he did love her; and this meets with the difficulty, of a kind familiar also to systematic psychodynamics, that there is little behaviour which could not be taken as consistent with the true, but theoretically stultifying, proposition that love, like God, moves in a mysterious way. But what sort of evidence counts in favour of the thesis that Cordelia really is a death-symbol? Especially in this sort of case, though also in the ~~ot~~ others less often, we may disagree not just about how much support a consideration gives to a thesis, but even whether it bears upon the question at all. This is no doubt partly because the 'logical grammar' of the proposition "<u>X</u>" is a <u>d</u>-symbol" is not sufficiently clear: does it or does it not, for instance, entail that Shakespeare <u>intended</u> <u>X</u> to represent <u>d</u> (and does it make sense to ask whether, if so, he succeeded?)?

Still less shall we be able always, or even usually, to <u>predict</u> what particular eventualities, whether of manuscript readings, historico-cultural observations or subsequent events in the play, will count for or against a thesis. And how, logically speaking, do these various sorts of evidence <u>support</u> the thesis which they support? Not, certainly, by enabling us to construct a deductive syllogism from which the controversial thesis can be read off as the exclusive conclusion. Nor are these questions and limitations peculiar to what some would regard as the loose and woolly reasoning of the humanities: they arise also, in principle, where any empirical hypothesis is put to observational test, even in the case of so-called controlled experimentation (see ch. 6, sections ....). That they provide well-known difficulties for the corrigibility of D.I.'s and psychodynamic propositions generally, does not, therefore, in itself mark off these latter as methodologically different from other hypothetical accounts of empirical phenomena.

Now, it is true also of interpretations which are actually given to a patient in therapy that they vary as to what they are 'about', and therefore as to what kind of thing they are saying. Accordingly, insofar as they aim to "transform" him, T.I.'s do so by saying different sorts of thing about his behaviour. As speech-acts, not only do they make some sort of assettion, however implicitly, about the patients behaviour, but also they do this precisely in order to influence his future behaviour in some respect. It would be tempting to take a cue from Austin (1962) by calling the former aspect 'locutionary' and the 1 latter 'perlocutionary'. For it is this similarity to a kind of speech-act which intrigued Austin (namely his notorious 'performative utterance', which seems actually to perform the action that it denotes), which Farrell has marked by calling some therapist-interventions 'transformative'. So, in order to avoid suggesting a closer logical parallel than perhaps exists, we use Farrell's term and partner it with 'propositional' (in place of locutionary'). We have seen that my reply to the patient who apperceived a remark as 'conventional' was aimed at getting him to communicate further, and that it carried some propositional and quasi-explanatory implications about what was on his mind. The nature of this fusion of diagnostic with transformative aspects will concern us shortly; but we must emphasise first that the immediate raison d'être of a T.I. is the therapeutic consequences, because this is the source of some confusion.

(b) Transformative aspects. In order for them to have such consequences they must necessarily take the form of communications to a patient, whereas a D.I. could, in theory, be filed away and forgotten. Much communication in therapy can be made, as we have seen, by the non-verbal agencies of grunts, nods and glances, which are regularly used (sometimes unintentionally) to convey 'messages' about the acceptability or value of something the patient has said. The same goes for ordinary conversation too, of course, but therapeutic skill consists partly in seeing when and how such support can be given most effectively, and in avoiding the artificial encouragement of some sorts of topic to the neglect of others. But we have noticed that such modes of cummunication seem too little structured to be able to articulate the sort of message, purporting to elucidate the meaning of some behaviour, which a typical T.I. conveys, and by means of which it aims to transform.

There are situations, however, in which the significance of an unarticulated action may be (perhaps has to be) given articulation by the context in which it is performed, and not by the manner of its performance; this much can be said in defence of Sheridan's Mr. Puff, the head-shake interpreter, introduced above. In an obvious way, asking a question creates such a context. If my dentist nods when I go into his surgery that means something vague and inarticulate; if he nods immediately after I have said "Do you need to take out all my teeth this morning in order to avoid recurrent oral infection?", there is nothing vague and inarticulate about what that nod means. Rather more subtly, players of cricket can identify a simple gesture by the batsman (hand poised over ball on the ground) as meaning "May I pick up the ball (contrary to Law OO forbidding the batsman to grasp the ball) and return it to the bowler, (in order to save the time and effort consumed by a member of the bowling side running in to do this, without being accused of 'obstructing the field'?".

It will be said, of course, that all that the two gestures mean of themselves is, respectively, "Yes" and "May I ...?", and that the context in some sense 'supplies' the rest of the message. But it is with messages that we are concerned; and the fact is that "Yes" does not convey the same message (never mind how, in this vehicular imagery of semantics) in answer to both "Do you like Tolstoy?" and "Are you a policeman?", And this taking of the context for granted is typical, not exceptional, in human interaction. Wittgenstein even seems to argue that it is a <u>necessary</u> condition of getting linguistic communication, of the sort which we in fact use, off the ground (1953, paras. 1 - 65). Consequently, it is commonly argued, against the behavioural atomism which seeks to analyse human activity into sequences of elemental 'responses', whether conditional or operant, that such an approach will never reach <u>understanding</u> of human actions, because the latter depend for their significance as much upon what the agent's reference-community is known to do, to expect, to believe and to understand as upon what the agent himself 'objectively' does. But this is another issue which will be treated more fully later (ch.8, section (a)).

In therapeutic practice, the necessary semantic context may be provided by the patient being half-aware what feelings lie behind some anecdote he has reported, so that the therapist needs only to feed back some crucial phrase of the patient's own, without further comment, in order to bring these preconscious feelings to bear upon the reported material and thus generate an interpretation of its dynamic sigificance. Fromm-Reichmann (1950, pp. 91-92) gives a clear example of this technique, and even implies that this sort of thing can sometimes be done without words at all, when she goes on to discuss the technical indications for using "nonverbal interpretive response" in contrast to "worded interpretation" (p.95). This takes us back to the difficulty (noted above, p.00) which an 'objective' ~~difficulty~~ observer would have in deciding whether some particular speech-act of a therapist <u>really was</u> an interpretation or not.

Given that T.I's are essentially special communications intended to affect a patient's behaviour, and not merely to account for it, the sort of technical question which they invite are to do with whether they were successful in achieving therapeutic progress, how you assess such progress, how you tell whether what they said about the patient was true, whether they would have been more or less effective if given at a dfferent time or in a different way, and soon. But the answers to these questions will vary because T.I.'s address themselves to different aspects of behaviour, and will differ according to whether their message is predominantly quasi-explanatory (propositional) or ~~quas~~ quasi -directive (transformative) in its force. One may interpret a dream to a patient in order (mainly) to give him insight into the nature of his underlying and perhaps unrecognised emotional conflicts, or one may inteppret a neurotic defence, after careful preparation of course, in order (mainly) to get him to abandon it and develop a more adaptive and ego-adjusted one in its place.

The trouble with this contrast, however, is that the pressure to make clinically <u>effective</u> interpretations, that is, to maximise their benign directivity, may draw attention away from the nature and status of their propositional content, with the result that almost any suggestion about how the patient might usefully "see" himself and his problems is treated as an interpretation, regardless of the fact that the elucidatory contribution to the suggestion which the therapist is making has dwindled to nothing. I shall argue below that an important discussion by Levy (1963) sometimes amounts to this (ch.3, sections b,c).

It is clear that traditional psychodynamic theorists have always been prepared to distinguish the various targets at which therapeutic interpretation may be directed, thus acknowledging that T.I.'s do a range of different jobs (or at least that the same kind of job may be done on different material). There are those, such as Bibring (1954) and Menninger (1958, pp. 129-131), who, perhaps in order to sidestep these questions prefer to speak not of making particular interpretations in particular speech-acts, but rather of an "interpretive process" to which various kinds of "intervention" by the therapist will contribute. Menninger, however, seems confused about the relation between such interventions (which do not themselves "interpret" but are "precursors of", and "lead up to", interpretation) and what he calls "interpretation proper". He writes that "interventions which prepare for .... interpretations (of unconscious material, defence-mechanisms, etc.) should be considered a part of interpretive action". And yet, if they "constitute the final act itself", instead of merely preparing for it, then "they cannot be considered interpretive in the analytic sense ...". But he has just given us to understand that such a final act would be "interpretation proper". At all events Menninger seems to hold that speech-acts may play various roles in the overall "process" of interpretation.

Many of his colleagues are less cagey. They will discuss, for example, interpretations of the symbolic content of dreams and fantasies, of 'the transference', of 'resistance', of slips of the tongue and motivated mistakes generally, of the dynamics of personal interactions within a group, and so on. What these sorts of interpreations have in common, no doubt, is the use of certain behavioural phenomena as signs of feelings, attitudes, wishes, etc. which are more or less unrecognised by the agent. But they differ among themselves in at least two relevant ways.

One difference is that some of them depend upon a technical theory in the way that others do not. Those that do may draw on such technicalities either in what the interpretive message says about the (meaning of) the patient's behaviour or in the way that that message is derived from the raw data. Thus the judgement that someone's solicitude for his father's welfare is a reaction-formation against oedipal jealously (however such a judgement might be expressed to a patient) takes it for granted in the form (not just the words) in which it is conceptualised, that people do have certain feelings for certain developmental reasons, and that certain patterns of defence against guilt and anxiety are regularly adopted. On the other hand, a dream-interpretation to the effect that the dreamer wants to outdo some disliked rival would intrinsically express no more than a commonsensical message; but it might have drawn on dubious technical hypotheses about dreams being disguised wish-fulfilments, and about certain dream-images symbolising competition.

By contrast, with both of these, however, to identify what someone conflictedly wants to tell you (such as something 'unconventional') from the fact that he chooses to comment on the conventionality of something else, depends only on the most general dynamic principles of selective perception; principles so general, indeed, that they are almost part of what we would call "educated common sense". This distinction, which may at first seem trivial, will matter in assessing the validity of what an interpretation is saying: crudely, the question of checking whether someone really is "projecting" his "oedipal guilt" raises special problems that do not arise in trying to find out whether he really has something "unconventional"to tell me.

A second way of subdividing T.I.'s, which is not unconnected with the first, is to contrast interpretations of "contents" with those of "dynamics". Broadly speaking, the latter are statements which identify behaviour in the here-and-now situation (especially in the highly-charged 'transference-relationship'), as indications of what kind of thing a patient is anxious about, pleased about, angry about, sad about, trying to achieve or trying to communicate at the time. The former,on the other hand, use particular idiosyncratic features of present behaviour as clues to understanding the causal origins, and specific form taken by, the patient's disturbed feelings and actions in general. And it is perfectly possible, as Fromm-Reichmann illustrates, to make the fommer sort of interpreation of specific material without being able to make the latter. That is, you may be able to identify the dynamic significance of for instance, a delusional idea or obsessional thought (by seeing in what emotional conditions it recurs to the patient and perhaps what sort of purpose it serves in his psychic economy) without knowing how it comes to take that particular form (why <u>that</u> thought, <u>that</u> image, <u>those</u> words); and even, exceptionally, without knowing what the thought etc., is (1950; pp. 85-96, 19).

A three-fold classification, which is treated as traditional by Menninger (1958) also, contrasts such content-interpretations separately with those of 'transference' and of 'resistance'. Fromm-Reichmann pursues the distinction by saying that psycho-analytical therapists have come to pay proportionately more attention to interpretation of dynamics than to that of content, and she discusses the various sorts of therapeutic material that invite the former sort of interpretation (transference, resistance, blocking, acting-out) before considering dreams, hallucinations and fantasies, which require the latter sort. Menninger (1958, pp.135-150) shares her concern to abjure the notion that therapeutic interpretation consists mainly or typically in the sort of hermeneutic virtuosity which Hollywood used to attribute to its technicolour psychoanalysts. True, an analyst's job compels him from time to time to assume the mantle of a latter-day Daniel and translate the sign-language in which the moving finger of the unconscious writes on the wall of overt behaviour. But Menninger prefers to start the discussion of interpretation in terms of helping the patient to see how his maladaptive relationships and emotional reactions are reflected in his behaviour towards the therapist (transference), and how his reluctance or inability to see this (in the analyst's terms!) affects the present activity of unconscious pressures (resistance). These are both "dynamic" forms of interpretation in Fromm-Reichmann's contrast, but they have been separately distinguished from content-interpretation in a traditional tripartite categorisation which we have just noted.

Although his discussion of resistance-interpretation seems to concentrate on identification of forces allegedly at work (that is, of dynamics), and almost to avoid deliberately any talk of elucidating the 'meaning' of what the patient says or does, it becomes apparent on closer inspection that the principles of elucidating dynamics (whether in respect of transference-behaviour or of resistance-defences) are not as different from those of interpreting content as both writers seem to suggest. For, on the one hand, he writes (p.136) that the second stage in "the interpretation of the resistance" is that "one points out" to the patient "how it manifests itself" (after having told him that, as a matter of fact, resistance does exist); but this amounts, whatever language may actually be chosen, to treating some of the patient's behaviour as 'signs' or 'expressions' of particular kinds of resistance, and indeed eventually (third stage) as expressions of particular, largely unconscious, motives for resistance. And, on the other hand, although he starts discussing content-interpretation in colourful metaphors of decipherment and epigraphy, which contrast strongly with his way of talking about transferences and resistance, he soon tells us that not only all therapeutic interpretation but even all psychoanalytic "technique" is based on the Freudian theory of dreams (pp. 148-150). The interpretive aspects of that theory, however, would seem to be almost entirely a matter of 'content' and 'dynamics', and not necessarily to involve 'the transference' at all.

In an exposition of psychoanalytic theory and practice which Freud himself underwrote, Nunberg (1955) distinguishes <u>id</u>-interpretation from <u>ego</u>-interpretation and refers to them as two kinds of interpretation". But again the difference emerges as one merely of the sort of material or question to which interpretation is addressed: for in the latter "we demonstrate to the patient the reactions of his ego in relation to his problems" (and are consequently much concerned with "resistance"), while what we show him in the former is "the stirrings of the id", (pp. 343-348). Since the pattern of neurotically disturbed behaviour is woven by an interplay of both 'primary' (id-) and 'secondary' (ego-) psychic processes, according to Freud's conceptual schemes, many interpretations will perforce concern themselves with both, and so be of both "kinds" at once. Thus he writes of an example (p.346) that it was "an id interpretation insofar as it concerned his repressed sexual life, and an ego interpretation insofar as it involved his defensive attitudes towards it". Nunberg describes the basic strategy of both these kinds of psycho-dynamic revelations as that of bringing "order out of chaos" by "reading sense into" or reconstructing the meaning of, material which has been "distracted" by the agency of the "primary process". Jung's complicated analysis of the various forms of interpretation both overlaps and contrasts with these ideas (cp. 1935, pp.288-300; 1943, pp.80-89).

This looks like a clear admission in that there are not separate types of interpretation but rather different uses to which interpretation may be put, in some of which the basic notion of sign-reading has been driven further underground than in others; and that the paradigm form of psycho-dynamic interpretation is what we are calling diagnostic. This conclusion seems justified, at any rate, from analysts' theoretical discussions of therapeutic technique, even though what they do in practice (and what they say in justification of that practice) may not always agree closely with the avowed theory. To say this is not to imply that the practice is inferior to the theory. On the contrary; there are those who would argue a priori that the theory is so bad that any departure from it in practice would constitute a welcome improvement; and others contend that close observation of what analysts actually do in therapy turns out to be more coherent than the theory on which the therapeutic activity is nominally based. We must return, however to the logical relationship between the paradigmatic D.I's and the T.I.'s which, in principle (according to the present argument), implicitly express them for therapeutic purposes, whatever else they may also do.

Two possible misunderstandings, of the contention that T.I.'s are essentially communicated D.I's, should perhaps be anticipated. One is that it does not follow, of course, that any D.I. which is communicated to a patient necessarily constitutes a T.I. In a 'diagnostic' interview, or in a diagnostic part of a clinical interview, one may confront the patient with a D.I. in order, not primarily to move him in a more healthy direction (typical T.I.), but to find out more about him by testing his insight, sounding out the strength of resistance, or investigating the nature and quality of his defences in a particular area. This information would be held to be necessary to benign transformation; but the fact remains that a particular 'diagnosis' may be put to the ? patient mainly in order to diagnose further the nature and extent of his disturbance, occasionally even at the risk of making matters temporarily worse for him, such as by tipping him into a frankly psychotic episode or frightening him away from treatment.

(Therapists sometimes cover themselves against this and similar risks by saying that a patient may have to get worse before he can get better.) It may turn out to have transformative side-effects, as it were, but its aim is exploration.

The other point is that, in insisting that a TI (as distinct from other transformative interventions like suggestion, confrontation or clarification) implicitly communicates a DI, I do not deny that it may also offer the patient, or may be felt to offer, a new way of looking at himself, - a new 'perspective' on his porblems. But it is not any potentially beneficial belvedere that is offered. The The only new perspective which an interpretation is entitled to offer, if it is to be a significantly different enterprise from suggestion, advice, persuasion or encouragement, is one which derives from understanding the signs which the patient gives of his 'latent', as opposed to 'manifest', attitudes, strivings, anxieties and so on. For the theory is that a false perspective on them is at least contributing to his disturbance, and perhaps even precipitated it in the first place. Some such understanding of signs is central to diagnostic interpretation; and I shall argue below (ch. 3) that it is only by resting logically on implied propositions about what is a sign or expression of what, which assert identifiable and checkable states of affairs, that TI can be rescued from the charge of arbitrariness and from the possibility of misuse.

Are we to say, then, that TI's are logically parasitic upon DI's? In a sense, yes. But not in a sense which implies that TI's do no more than slavishly transport a kind of Pooh Bearish 'Idea' from the therapist's mind to the patient's. They take as it were their mandate, both theoretical and ethical, from such plenipotentiary propositions; but the way they carry out that therapeutic commission is their own technical business. For this reason analysts will theorise both about what such mandates should ideally contain (that is, about how they should be drawn up and what their terms of reference are), and about how the commission should be executed, in the sense of how it is to influence the patient's mental state. Thus we find Ezriel (1953), on the former point, spelling out the psychic ground that should be covered by a full-dress (dream-) interpretation, and saying that it should first identify the repressed wish, secondly indicate the feared punishment (if the wish were to be indulged), and thirdly show what defence-mechanism is used against the guilty wish. And Menninger takes it for granted (1958, p.) that Freud's Interpretation of Dreams (ch.7) is the *locus classicus* for what such interpretation consists of, though he might have mentioned also part of the Introductory Lectures (1917, pp.100-239).

On the other hand, Yorke (1965), for example, addresses himself to the question of how TI's take their effect. Essentially the TI, by virtue of being an interpretation as opposed to suggestion, exhortation or persuasion, alters the patient's balance of mental and emotional forces, that is his 'psychic economy' in Frued's terminology, in a different way from that in which these other kinds of communication alter it; for the latter, insofar as they also are 'perlocutionary', are likewise intended to do something rather than just assert something. By this token, a TI, even though derived from a DI, might fail to function as an 'interpretation' for a particular patient at a particular time. If the DI content is not perceived (or perhaps perceived but 'resisted'), it may have the psychic empact of a mere suggestion; or worse, even that of an accusation or a threat. Whether, and how, you can tell of what sort the effect of a particular communication on psychic economy has actually been, is, to say the least, another question. But the idea that a TI might be partly defined also by the manner of its influence on the mind and behaviour of the recipient is not an incoherent one. Like Wollheim's art-work, an interpretation may owe its essence partly to the spirit in which it is perceived.

(c) The analogy of 'executive' interpretation. Since my argument is that DI is logically primary to TI's, we shall mainly be concerned with what sort of thing the former class of interpretation is saying, with what sort of illumination or explanation they provide, and whether they can be evaluated in familiar terms of truth and falsity. Their relation to TI's, whose job is essentially to affect people, will be illustrated by elaborating the analogy, already introduced, of 'executive' interpretation (EI) in the performing arts, and principally in music. Although this theme is not developed until later (ch.9), we need to make its acquaintance here, so that we can bear it in mind during the intervening discussion. We saw that a performer's EI of Hamlet or the Waldstein sonata, although representing his point of view or perspective on the work, is not merely a matter of taste and opinion, because it is grounded in an admittedly elusive way, on various sorts of propositional judgement (CI) about the nature of the work. That ism he does certain things in a certain way on stage or at the piano because he believes certain things about the play or the sonata. Such beliefs, of course, may not be explicitly formulated, and the performer may be unaware of how they are influencing what he does; they are also, as we have seen, beliefs about different sorts of thing. Consequently performers have the concept of passages in plays, poems and music which "speak themselves" or "play themselves", where what is meant is that the text conveys to an ordinarily sensitive executant a strong but inarticulate impression of 'how it should go'. One consequence is that the only way of conveying, expressing or reproducing that impression may be to do the passage, that is, to give an E.I. of it. Another is that all that the performer may be able to say in justification of his E.I. of the passage is "I just feel it that way", or "Well, you can't play it any other way"; and if you press him to articulate his implied C.I., he may reluctantly say "Well, I suppose I see it as a sort of ...." or he may just dismiss you as an importunate philistine.

But we need to tread a middle way (which has its corollary in psychodynamics too). For there are some features of the music or the text which the inarticulate, or even articulate, impression of the executant cannot be allowed to overrule or be at variance with. He may not add words to clarify Shakespeare's syntax; nor may he suddenly start playing at half-speed with no direction from the composer, as Liszt did when performing his piano reduction of the 'peasants' merry-making' section from Beethoven's Pastoral symphony, explaining that he felt that the passage represented the old people joining in the dance. On the other hand, it would be absurd to suppose that all passages have enough sufficiently objective properties to determine precisely how they should be executed. And the idea, that the musical significance or import of a passage sometimes cannot be expressed otherwise than by actually playing it, has a well-known literary parallel. The story is told of Eliot's The Cocktail Party, as it has been, no doubt, of other works before and since, that, when the author was asked what it "meant", he answered that if he could say that he would not have had to write the play.

The psychodynamic corollary is that we have to strike a balance between over-subjective views of T.I and those that are too categorical. On one hand, some therapists are reluctant to distinguish very closley between what are mere 'associations', on their own part, to the patient's material, and what purport to be relatively objective statements of what it 'really' means, so long as the patient seems to be moved in the right direction (Winnicott 1971, p. 178). It would be unrealistic, on the other hand, to insist that everything which a T.I. communicates derives, or should derive, from an implied D.I. which is independently verifiable. The inter-dependence of T.I and D.I. is more subtle than this; as it also is with E.I. and C.I.

With other passages again, a performer may not be able to decide how to play them until he has decided what he believes about them, and hence what he <u>feels</u> about them. Obviously some lines of a speech or bars of music do not make dramatic or musical 'sense' for a player until he arrives at a view of what their function is; for example, of how they relate to other parts of the work (as opposed to what they 'stand for' in themselves). The musical amateur can supply his own examples of this. For instance, a pianist friend points to a couple of bars on the last page of Chopin's <u>Fantasy</u> in F minor, saying "I don't know what to make of this bit", and one says, "Well, surely it's a reference back to the beginning of the slow B major episode in the middle". With luck he may then say, as happened in this particular case, "Oh, I see; now it makes sense"; and the implication of this 'making sense' is that he now sees how (he wants) to play it. The whole point of functional analysis in music is of course to expose the structure of a piece, and to get people to grasp that structure both conceptually and emotionally, so that the nature and significance of the various parts of the work become evident or are enriched. For in music most meaning is 'syntactic', rather than 'denotative' or 'semantic', as Langer has argued (1942, pp), in the sense that a phrase derives its aesthetic significance and value from its relations to other phrases, as opposed to standing for, or referring to, something in its own right. On the whole there is no vocabulary: one does not have to be an anti-Wagnerite to find the language of the <u>Leitmotif</u> relatively artificial; and it is not for nothing that we regularly contrast 'program' music with 'absolute' music. However, this syntactic view, which the player arrives at, may or may not be correct; clearly, I might simply have misinformed my friend about the structure of the Chopin piece. And, anyway, depending on what kind of question is at issue in the <u>CI</u> (and we have seen Weitz, in another context, insisting on their variety), the propositional judgement on which the <u>EI</u> is based is more or less corrigible: that is, there are more or less firm grounds for calling it right or wrong, and there may even be no 'correct' view at all.

Both these features of the musical <u>CI</u> (the involvement with syntactical properties of the data, and the problem of its definitive description) have clear analogues in the psycho-dynamic field. For, on the first point, it is evident, as we shall elaborate below (ch.7), that the significance which a <u>DI</u> attributes to a behavioural datum depends as much on its relation to other elements in an apparent pattern as to its own intrinsic properties. Thus a particular action, image or idea may be said to 'mean' different things in different behavioural company, just as a given word or linguistic form has different meanings in different verbal contexts. And, on the second point, the problem will arise of how to characterise, in a way which does not prejudge its significance, the behavioural material which is to be interpreted (ch.4). In meeting this, we shall have to try to reconcile the elusiveness of such a definitive and theoretically neutral description with accepted standards of objectivity and validity.

It might be supposed that the latter difficulty is not so acute in respect of *CI* and *EI* in the arts. Surely there is less of a problem here about *what* is to be interpreted? Usually we know at least what the text is, what notes are to be played, and are consequently in a position to say that some performances must be categorically wrong in this respect. The study of exceptions to this situation, however, where we have to infer from inadequate or false data what the text ought to read, will also be found illuminating below (ch.4 (c)). There is more room for doubt, obviously, about the *way* the notes are to be played; whether fast or slowly, loudly or softly, roughly or smoothly and so on. Broad indications are often given, to be sure. But how *much* should you slow down for a *rallentando*?; just *how* loud is *mezzo forte*?; does this degree of *rubato* lose sight of the 'basic tempo' or not?; and what does Beethoven mean by *andantino* or *allegro assai* anyway? (We know, indeed, that he did *not* mean some of his metronome speed-markings, according to Schindler; cp. ch.8c). And one man's *con espressione* is always another man's *schmaltz*. However, even if you could not tell whether some particular tempo was really too fast for andante con moto, yet I am simply wrong to take very slowly a passage marked prestissimo; and it is only some kind of joke for me to play very softly where fortissimo is asked for (as Chopin did when he was physically too weak to follow his own dynamic markings). I am not at liberty to play it any way I please.

There are, indeed, more general constraints, to do with musical and dramatic plausibility, whose criteria are even more difficult to identify and apply. If your opera-house orchestra lacks, say, two of the six solo cellos which Verdi asks for at one point in Nabucco (introduction to Act II scene 2), you compromise by giving the top two parts to violas; you do not give the bottom two to trombones. Even in opera, executive interpretation is the art of the plausible. It may be asked whether such a compromise, since it involves a departure from the text, invalidates the EI of the passage. Well, it does indeed seem to be the view of Goodman, for example, (p. 8(c) below), that this sort of procedure would not even qualify as a 'performance' of the passage, let alone as a valid interpretation. But, less extremely, the argument need only be that some departures from the text are categorical grounds for rejecting the EI which incorporates them; there are departures and departures, as the illustration shows. And, once again, we have to face a precisely analogous problem in psychotherapy. In trying to get a patient to 'take' an interpretation 'p', what latitude in the choice of phrasing, imagery and so on can the therapist use without inviting the objection that he has not really given interpretation 'p' after all? In practice this would often be an artificial question; but it might well matter in a therapy 'workshop' situation where techniques are under intensive review (cp. Malan 1963)

If the C.I. is concerned with the character and structural role of a passage, which is sometimes used as evidence for an appropriate way of playing it in terms of speed and dynamics, there is some scope for uncertainty. Suppose someone disputed my C.I. of that section from the F minor _Fantasy_ of Chopin, saying that the two phrases just _happen_ to be somewhat alike and that the later one does not _refer_ to the earlier at all. What'evidence', or what considerations of any sort, are relevant to deciding between the rival hypotheses (the conflicting C.I.'s)? Whatever they would be, and we shall look below into what _can_ be said when experts disagree widely over the form or internal structure of a work (ch.9 (o)), they will illuminate what precisely it is that I am claiming when I say "Phrase B refers to phrase A"; and they will be illuminatingly different from the sort of thing we should appeal to in order to tell whether this speed really is too fast for a Beethoven _allegretto_ or not.

At this point we are back to familiar difficulties in psychodynamic interpretation. For it is certainly a characteristic feature of D.I.'s, or of explanatory narrative which incorporate them, that they trade on _allusion_. Their plausibility often rests heavily on hypotheses such as that this memory, misperception, free-association or parapraxis alludes to that alleged motive, conflict, wish, anxiety etc., so that apparently illuminating links and patterns are revealed in the material. The objection that any revelation so based is bogus and illusory is discussed below (ch.5(a)). Moreover, since there is much diversity in the kinds of claim that DI's make, whether implicitly of explicitly, there is consequently a great variety of observations or considereations which bear, in different ways, upon their validity. But if there is a variety in the sorts of thing that DI's assert, there is also no simple, direct and invariable relation between DI's and TI's; and it is perhaps this fact, or rather the combination of these two sources of diversity, which makes some TI's look as though they do not rest on any DI at all but are merely idiosyncratic expressions of 'perspectives' or points-of-view on the patient's behaviour. As if the therapist were saying "This is the way I see it", with no more categorical implication than the musician who says "This is the way I feel it" when asked to justify his EI of some passage.

This diversity of relationship between DI and TI springs from the fact that the latter communicates or expresses the former to someone, and in such way as to have a hopefully therapeutic effect on him, just as the executive artist wants to make a particular aesthetic impression on his audience. In order to convey the same message individually to several different people (of different education, intelligence, age, culture or personality) you will not be able to stick to the same form of words, and you will emphasise different aspects of the message in different cases. In order to have "the same" effect upon the different people severally, you may well actually say different things to them. Therefore in order to give the same interpretation (DI) to different patients, in the sense that they can take it and use it, you may need to utter different words, make different allusions, introduce or avoid certain emphases, and so on. This leads to the problem, just noted, of knowing whether we really are giving interpretation '<u>p</u>' or not. And since the form of the T.I. is adapted to the condition and circumstances of the patient, it also runs the risk of seeming to be no more than a suggestion about how he might usefully look at things from his point of view, merely recommending a new 'slant' on his behaviour, without resting on any categorical claim about his psychic make-up. But there is an assortment of ways, once again, in which utterances express to a hearer the ideas of judgements which they convey. Without investigating the concept of 'expression' very far, and equally without trying to prize away hypostasized 'propositions' from the particular sentences which are held to affirm them, it must be clear that the key word is denoting rather different semantic relations when a lyric by Raleigh is said to "express" the romantic melancholy of the Elizabethan age, or a sigh to "express" relief, or '<u>Vive la France</u>' and '<u>Long live France</u>' to "express" the same thing in different languages. The form in which a judgement is expressed will depend partly on the purpose <u>for</u> which it is expressed; and since both executive and therapeutic intepretations exist for a purpose, their form will be a function partly of variables affecting the achievement of these purposes.

The vehicular metaphor of conveying a message from A to B perhaps suggests a picture of A thinking of his message, sealing it up in an envelope of words and popping it into the mind of B as into his letter-box. I do not mean that a T.I. is simply a convenient verbal wrapping for a D.I. in this way, and that that constitutes their dependent or parasitic relationship. I mean that a T.I. <u>expresses</u> some D.I. <u>in</u> a therapeutic way, <u>for</u> a particular person <u>of</u> a certain psychodynamic make-up. Consequently these factors may serve to vary the form taken by a T.I. on different occasions in expressing one-and-the-same D.I. Conversely, too, one-and-the-same phrase may "express" very contrasted judgements. The story goes that a music critic went to a performance of <u>Parsifal</u> at Bayreuth and was bored rigid for six hours. At the final merciful curtain, his neighbour, overwhelmed by the noble drama and consumed with religious ecstasy, exclaimed "Thanks be to God". "Amen to that", said our critic.

Suppose that a Shakespearean actor wants to convey to the audience a point about Hamlet's readiness to think ill of his mother. What he will actually do on stage (the speed, emphasis, inflexions, pauses of his speech, the movements and gestures, all of which constitute his E.I.) will depend upon the nature of the audience, the stage, the production, the way the other parts are being done, and so forth. Sometimes it will be hopeless to try to make some points at all, because the audience is too young or too old, too urban or too rustic, has or has not been through a war, and has or has not seen Hamlet ten times before. Sometimes the point will be conveyed by making an implicit contrast with Horatio; other times, by suggesting similarity with him, depending on how Horatio is being played. Obviously an apron or round-house stage may allow him to say the same thing in a quite different way from what is possible in proscenium-arch conditions; and a twitched eyebrow on television may suffice to convey the same message as a raised fist does on stage. In all these cases, I want to say that the varying E.I.'s are all expressing or conveying the same C.I. And since the ~~raison d'etre~~ rôle of the former is to communicate the latter, there is a one-sided dependence in the relation between them. But an E.I. is not a function exclusively of its C.I., because it is adapted to other variables as well; so this dependence is not that of a talking parrot upon its instructor.

The same goes for the sense in which a T.I. depends upon, expresses, conveys or communicates a D.I., We have already come across the idea of a therapist tailoring a T.I. according to the degree of insight or ego-strength of the patient, and we have met the argument that therapeutic interpretation consists not in uttering certain sorts of phrase but in altering a patient's 'inner world' in a certain sort of way by saying a certain sort of thing (pp.00,ooabove). That the extent to which a patient is "ready" for an interpretation (D.I.) may influence the verbal form in which it is expressed to him (T.I.), illustrates the contention that T.I.'s do not merely restate in second-person grammar what the D.I. states in the third person.

What a therapist actually <u>says</u> to a patient, in order to get him to grasp a particular <u>DI</u> in a way that is not too threatening to be used beneficially by him, will be determined not only by the propositional content of that <u>DI</u> (the 'message' which is being conveyed) but also by what the therapist believes about the insightfulness, vulnerability, defensive techniques, unconscious anxieties and patterns of association of the patient himself. If two patients differed markedly in these repects (and we all differ in them to some extent), it would be idle to expect the same phrase to convey the same message equally benignly and constructively to them both. We are beginning to realise that even in cognitive testing, the so-called 'standard instructions' of intelligence test items do not guarantee that children from different sub-cultures will, on hearing them, be confronted subjectively with the 'same' task. Teachers report that if you draw a sheep on the blackboard in Canterbury, England, and ask a class of 6-year olds "What's that?" they will instantly say "A sheep"; if you do 'the same' thing in Canterbury, New Zealand, they will puzzle long and hard, and then ask hesitantly "Could it be a two-year old short-horn Merino?". Hence Piaget's so-called 'clinical' approach to cognitive research, which lets the next question be determined by the previous response, in an attempt to standardise not the auditory input but the subjective situation for the child. But Bryant's recent insistence on the need for corrective 'experimental' check on the fruits of such research must be noted (1974, pp.171-181).

Another variable which effects the significance of an utterance or action, and which hence may determine what interpretive force, if any, an intervention has, is the context <u>from</u> which it is drawn, - as opposed to the context <u>into</u> which it is introduced. Consequently this is a further means whereby one-and-the-same T.I., in terms of the therapist's actual verbal behaviour, might well serve to express a different D.I. on different occasions. Let us approach the point deviously, by appeal to the question of the "significance" of what an artist does when he uses certain visual effects.

Wollheim (1968, pp. 72-75) adapts from Gombrich the following argument, to which we shall need to return from time to time.

> "An artist expresses himself if, and only if, his placing one element rather than another on the canvas is a selection out of a set of alternatives: and this is possible only if he has a repertoire within which he operates. Knowledge of the repertoire is a presupposition of a spectator's capacity to understand what the artist is expressing: but the existence of the repertoire is a presupposition of the artist's capacity to express himself at all".

Consequently, when a painter puts a blob of blue on his canvas, we cannot begin to know what it 'means' or what he is 'expressing' until we know what other colours he had on his palette. Is it blue-rather-than-red, or blue-rather-than-black; or is it indeed blue-rather-than-any or all of red, black, yellow, green, white, etc.? More concretely, a black line that turns out to be part of a monochrome sketch expresses something different from one that features in a Goyaesque orgy of colour. The same harmonic device conveys cosmic agony in Haydn, in Mahler a jaded piquancy.

What goes for the aesthetic 'meaning' of a blob of paint goes also for whole styles of painting; it goes analogically, too, for any significant human action. So that when Van Gogh paints like Delacroix, as in his <u>Crucifixion</u> of 18.., it means something different from Delacroix himself painting like Delacroix. And this is at least partly because when Van Gogh does it, he is painting like the Frenchman, <u>rather than</u> like Van Gogh; just as, when the blue blob is blue-rather-than-black-only, its significance is different from that when it is blue-rather-than-green,-red,- yellow-or-white. (There is something odd, of course, but not necessarily useless, in the idea of someone painting <u>like</u> himself; though perhaps all it can be is self-caricature.) There are a variety of reasons for adopting someone else's style or artistic language, and thus a variety of possible significances in the adoptive product: technical exercise, personal flattery, forgery, obsession, atavism, joke.

Sometimes, at least, you may use the language of another person, time or place because it just happens to hit off ideally what you want to express. Why should one not, in twentieth century England, have mediaeval, Indian mystical or Athenian <u>feelings?</u> And if one does, he will naturally, like Betjeman or Eliot or Brooke, break into the appropriate ancient language here and there in his English verse. It is very common for educated young people, in the questing identity-diffusion of adolescence, to come across an attitude, manner or <u>Weltanschauung</u>, typical of some bygone age or alien culture, which seems to express precisely their own feelings about themselves, the world and life. As a student, the British composer Vaughan Williams became hooked, as we should now say, on the historic harmonic language of the "modes"; this system derived from the Greeks, permeated mediaeval music and was generally supplanted, from around the sixteenth century onwards, by that of "keys" which predominated from Bach to Richard Strauss. Consequently, every piece the young musician wrote at this stage turned out in the Dorian, Lydian, Aeolian or cognate mode, instead of in E-flat or G-minor. Thinking to free his pupil from this retrospective rut, his teacher, C.V. Stanford, told him at the end of one tutorial to go away and compose a waltz. Vaughan Williams came back next week with a modal waltz (ref.   ).

And it works the other way as well. When a piece of music has originally expressed a feeling, idea or message of some kind in the artistic language, historical context and cultural setting of a modal age, the executive interpreter of today is faced with the problem of conveying that same message to his contemporary audience, by making 'the same' aesthetic impression upon them as the work made upon people for whom modes, galliards, galleons and virginals were part of everyday experience. But playing the thing exactly as it was originally written (or played) will not necessarily achieve this aim; indeed, it is rather unlikely to do so, because of the wide difference in musical language and experience between the sixteenth and twentieth centuries. Here, then, is a chance to drive a wedge between critical and executive interpretation (C.I. and E.I.), and to illuminate by analogy the relation between D.I. and T.I.

For it follows, from the fact that the aesthetic effect of an E.I. is relative to the audience's musical experience, that a given way of playing would mean something different in the musical atmosphere before, say, Beethoven and Wagner had transformed the medium out of all recognition compared with what it would convey to us. A piano piece by Mozart which was rich, daring and brilliantly powerful to his contemporaries cannot hope to make that same impression on people who know the Hammerklavier and the Liszt sonata; by comparison it just is a feeble tinkling. At one time it was the most exciting piano sound available, and as such could express things that nowadays require a totally different range of harmony, complexity and bravura to get them across. In order to receive the same impression from Mozart, we try to tune ourselves in to the 18th-century idiom, which means trying to understand and empathise with the musical "palette" on which he was drawing. But the fact that any such readjustment or correction should be necessary underlines the point that we are having to learn, in effect, a new aesthetic language, in order to grasp and appreciate the message.

The same difficulty besets the performer and his E.I. When a Bach Brandenburg concerto is played today on out-of-tune straight trumpets, piffling recorders and all the other paraphernalia of so-called "authentic" performance, this is no way to convey the same rich and thoughtful message to us, who are used to in-tune valve trumpets and clean-sounding flutes, as was conveyed to the fortunate Margrave to whom it was dedicated. To play Bach thus in 1720 was the obvious and only thing to do; to do so in the 1970's is to play sourly and breathily rather than sweetly and clearly. Historical and aesthetic authenticity do not necessarily go hand-in-hand.

Let us translate these observations, about the influence of cultural and sub-cultural contexts upon interpretive communication, into psychodynamic terms. In Kleino-Freudian circles of group-psychotherapy it is customary to suppose that, when a member absents himself from a session without notice, the other members will feel anxious and guilty that they may have driven him away by what they said or failed to say (generally, how they treated him) at the previous session. This is often assumed to furnish the current session with a group-problem about the feared destructive consequences of aggressive and rejective feelings, and about the possibility and means of 'undoing' them or making 'reparation' for them. Given this background, if someone mentions the missing patient without even referring to his absence, the therapist can convey to the group an 'interpretation' along the lines just indicated (their presumed anxiety about what they have done to Mr.Smith) by simply saying "And of course Mr. Smith is away today". This remark is capable of conveying such an interpretive message because the therapist's "palette" of associations to the idea of "being away" is known to contain (even to consist exclusively of) these particular theoretical notions. But the remark would not be expected to have anything like the same effect on an audience in another social, cultural or historical context, where, as it were, the relevant palette contains a different range of colours. It might actually convey a specific but conflicting interpretation to a Jungian, Presbyterian or Mafia group, where the background assumptions and expectations are equally strong but rather different.

These considerations make it absurd to expect that third-party observers can form worthwhile judgements as to whether the therapist has made three or seventeen interpretations in a particular session; or whether, if he is said to have given seven, the second was 'the same' as the fifth. Indeed, an intervention may carry interpretive force for some group-members but not for others, just as only some members of a theatre audience may "take" a classical allusion, say, in one of Hamlet's speeches, while others do not. Nor will it do to object that the therapist has still <u>given</u> the interpretation, as surely as Shakespeare has <u>made</u> the allusion, regardless of who takes or misses it. For interpreting is not an all-or-none process, like releasing a food-pellet into a Skinner box. A therapist may deliberately leave some interpretive interventions no more articulate than suggestions, adumbrations or hints, so that the patient may take the point if he is "ready" for it, but does not have it thrown at him inescapably if he still needs to resist perceiving it. In which case, it is not a question of patients having blurred perceptions of clear-cut events: sometimes the facts themselves are blurred. This is not to take refuge in a smoke-screen of obscurantism. It is to indicate that there is a problem of data-description; that some ways of characterising them will not do; and that we are obliged to look into the reasons for preferring some ways to others (cp. ch.8).

CHAPTER III

NON-PROPOSITIONAL THERAPEUTIC INTERPRETATION

(a) Means and ends.
(b) Irrelevance of truth
(c) Could interpretation be claim-free?

(a) Means and ends

If we grant, then, that there are two main strands, explanatory and transformative, running through the fabric of psychodynamic interpretations in general, and that those which are dominantly transformative are intended to get patients into a "better" state (however characterised), we may be tempted to concentrate entirely upon whether such would-be therapeutic interpretations succeed in this aim and to turn a blind eye to the truth or falsity of what they actually say about the patient. To succumb to this temptation would be to open the door to an apology, based on the end justifying the means, which could be used to cover all manner of dubious, and even frankly unethical, practices carried out in the name of interpretive therapy; and it plays into the hands of those critics of psychotherapy, such as Sargant (1959), who argue that the therapist 'transforms' his patient (in so far as he succeeds) by exerting emotional pressures which are both independent of the truth of what he says and essentially the same as those manipulated by the religious preacher or the political brain-washer. For the suggestion is that all three activities (preaching, brain-washing and psychotherapy) function by trading upon, if not actively producing, the common elements of dissatisfaction, anxiety, insecurity, guilt, disorientation, dependence and suggestibility in their victims, and then presenting them in this disorganised state with a new belief-system in whose cognitive structure they may rediscover a sense of security, orientation and identity. Systematic attacks along these lines by Chinese Communist agents upon the personalities and beliefs of western political prisoners have been carefully described and examined by Lifton (1961, esp. pp. 84-160), and it is not difficult to find parallels of a sort, as Lifton himself does (pp. 447-536), between the various stages involved in them and various aspects of education, religion, therapy, propaganda and advertisement in western society.

This type of argument, however, needs to be kept on a short rein, for it can easily get out of hand. In the first-place, it will not do merely to show that one object (or activity, or whatever) has certain features in common with another, and then to conclude from that that they are both "the same sort of thing really", or that one is no more than an instance of the other. For, despite all that they have in common, one may differ from the other in having some unique property which puts it in quite another class and makes it quite another sort of thing. It may be true to say of Einstein, Mozart and yesterday's soup that they were all ninety per cent water, but that's no reason for thinking of them as alike or valuing them the same. Some things which are black and have legs are grand pianos; others are cats. Secondly, a good deal depends, of course, on just how close are the parallels which can be drawn. For if you give a sufficiently imprecise specification of the features to be sought as common, you may enable yourself to demonstrate all manner of specious similarities and show that almost anything is a case of almost anything else. You may assimilate a speech of Hitler's to a poem by Keats, by saying that they were both using words; but nobody wants to say consequently that they were doing, in any useful sense, "the same thing".

And yet this is a game which psychologists notoriously play. If you define the crucial concepts of your system broadly enough (or, some would say, circularly enough), you may give superficial cogency to sensational views such as that neurotic illnesses are "merely wrong habits which have been learned" (Eysenck 1957, p. 268), or that all human behaviour is under the "control", in some alarming RUBBISH! sense, of environmental agencies (Skinner 1953, pp. 437-449). In the former case, the technical concepts of 'learning' and 'habit' have been expanded from their everyday image to include processes and activities which we would ordinarily <u>contrast</u> with e.g. learning to drive or taking tea at four'o'clock; so the technical statement, which masquerades in ordinary language, is not saying quite what it seems. In the latter example, the fact that much of what people do can be <u>represented as</u> producing "operant responses" (of a sort), in the context of "discriminative stimuli" (of a sort) and meeting with "reinforcement contingencies" (of a sort) does <u>not</u> justify the conclusion that people going about their daily lives are in the same case, to any interesting extent, as pigeons in a Skinner -box pecking at spots for a food-pellet. But it is this kind of coarse-grained thinking which leads to the provocative attempt to consider psychotherapists <u>merely</u> as agents of selective 'reinforcement' (cp. Krasner 1958), or even, indeed as a "reinforcement machine" (Krasner 1962). The same trouble occurs, unfortunately, in psychodynamics too. It is a well-known difficulty in testing psychodynamic hypotheses that, when they involve such concepts as 'denial of insecurity' or 'hypomanic overcompensation', there are far too many sorts of behaviour which can be seen as instances of such denial or overcompensation, and far too few ways of telling whether they really are such instances.

The over-facile denunciation of processes which seem to have some elements in common with some aspects of brainwashing has misled critics such as Laing and his associates into intemperate onslaughts against traditional methods of psychiatric treatment. But doubts have also been stirred in more moderate minds about how far the psychotherapist is doing the same sort of thing as Lifton's political thought-reformers. Let me, at this point, simply state five important differences and return to the question below: (1) the patient (usually) comes of his own accord, asking to be helped, that is to be changed in some way: (2) the nature of the desired change is agreed in advance: (3) the methods by which the change is to be effected are also discussed and agreed: (4) the patient also agrees, perhaps implicitly, to accept the therapist's judgement about how those methods are to be deployed from minute to minute in the course of treatment; (5) the therapist is under a moral obligation to be benevolent about points (2), (3) and (4), if not literally honest, (for there may be good ethical, and even clinical, reasons for not telling the patient, at once anyway, how little change can be expected or how long it will take). It would be naive, however, to suppose that these can be worked out in practice without running into difficulties.

Certainly they are not trouble-free: with respect to (2) and (3), for instance, the therapist may disagree with the patient about what sort of change is necessary, and he may perhaps, regard some rigid "moral" scruple or element of religious conviction as part of the patient's pathological condition.

These obstacles aside, none of the five conditions obtains in the case of that forcible eradication and substitution of particular political and social attitudes which the term 'brainwashing' originally denoted, and upon which it still trades for its emotive overtones. The fact remains, however, that by the criterion of therapeutic efficacy alone, we would seem to be justified in saying something positively false to the patient so long as it got him better; that is, not just made him <u>feel</u> better (for a while, perhaps, or even for ever) but actually <u>made</u> him better (by some criterion independent of his temporary feelings). And this looks like the thin end of a sinister wedge which alarmingly resembles the ruthlessly pragmatic procedures of brainwashing proper. In this case, the fact that giving the (false) interpretation proved to be therapeutically advantageous obviously would not render its content true, though it might render the giving justifiable. Hence, it could be described as "valid" (a term often used in this context), in the loose sense that you have no right to complain. On this score the clinician is likely to have different priorities from the theoretician, and Levy (1964, p. 130) blandly concedes that a therapist may "legitimately" ignore whether an interpretation is true or false so long as it does a good job. Let us look, then, at some ways of resolving this conflict.

One way is to stand, with Levy, by the idea that therapeutic interpretations do typically make or imply factual claims about the nature and origins of the patient's behaviour, but that it does not matter whether these claims are false so long as the interpretation, when communicated, is benignly transformative. And there need be no special problem about what transformations are "benign", because in practice it means those that are consistent with the second of the five principles listed above (p.70). Another way is to deny that such interpretations necessarily have any propositional content which can be regarded as true or false: that is, to hold that they are not really saying _anything_ matter-of-fact or categorical (in spite of grammatical appearances) about the patient's present state or past development; so that they cannot, logically, be saying something false. The problem for this view is to show how anything worthwhile is being said that is different in principle from reciting incantations, uttering threats or giving encouragement; or indeed, to show that what is being done differs essentially from manipulating people into more desirable behaviour by the application of drugs, operant conditioning or subliminal persuasion. In its defence, one may argue that what such interpretations do is to offer the patient a new way of looking at himself, and in particular his difficulties, which will be helpful (in a sense which we shall have to examine), but which makes no more claim to be _the_ one-and-only correct view of the data than does an actor's interpretation of the role of Hamlet or a pianist's of the _Waldstein_ sonata. We have already seen, however, that even such 'executive' interpretations in the arts are not independent of factual claims as the argument would require, and we take up the question again in ch.10.

(b) <u>The irrelevance of truth</u>

To begin with we must, as always, distinguish things which differ. It is one thing to say "You were right to give that interpretation, because it helped to make Smith better", and quite another to say "You were right in making that interpretation, because it was true". In the latter case, indeed, many therapists would want to say that some interpretations ar e too true to be given; or, at least, to be given before the patient has reached a certain stage of therapeutic development. Menninger, for example, is italically firm on this point: "One thing which we certainly <u>never</u> do ... is to tell the patient what is in his unconscious long before he has any capacity for grasping the significance of such oracular diagnostic incisions" (1958, p.136). More recently, Winnicott has underlined the point in discussing his use of interpretation in therapeutic consultations with children built round their projective drawings (1971, pp.9-10): "an interpretation that does not work always means that I have made the interpretation at the wrong moment or in the wrong way and I withdraw it unconditionally. Although the interpretation may be correct I have been wrong in verbalising this material in this way at this particular moment."

Conversely, some analysts will admit that an inaccurate, or only partially true, interpretation may be therapeutically beneficial (Glover 1931, Fromm-Reichmann 1950, pp.84-85; cp. Yorke 1965, esp. pp.27-28), and this concession obviously debars them from making beneficial consequences a test of the truth of an interpretation. This consideration (of benign <u>sequelae</u>) is, however, regularly used as one criterion of the "validity" of therapeutic interpretations (Farrell 1962; Wisdom 1966; Rycroft 1968, pp.76-77). By a slide in the meaning of 'valid', therefore, we could construct the paradox that a particular interpretation may be both clinically valid and epistemologically invalid at the same time. And the general problem, of whether the conditions of observation in psycho-dynamic therapy are such that ordinary standards of scientific validity cannot be applied to its findings, will concern us below (ch.4).

In an attempt to pinpoint just where and how the question of truth does arise, Levy (1963) distinguishes two component elements in interpretation and argues that if we are clear about how they differ we shall be able both to check, correct and refine our making of interpretations and to teach the skills of the business in a hard-headed, mystique-free way.

According to Levy's analysis (1963, ch.2), there are two logically separate stages in the construction of an interpretation, whether diagnostic or therapeutic, and questions of truth or falsity are admissable (broadly) at the second but not at the first (this primacy being, of course, that of implicit logic and not of temporal sequence). The reason for this is that at the first stage, designated 'semantic', the raw behavioural observations which are to be the subject of interpretation are merely processed or encoded, according to a certain sheme of classification which Levy calls a "language-system". This code is held to be, to an important degree, arbitrary, since it derives merely from the form in which the empirical hypotheses to be invoked at stage two are cast.

The analogy seems to be that, in the physical sciences, if you wish to apply a theory concerned with weight you must process your observational data into grams or some other weight-units; if the theory you want to use is to do with colour, you must measure the wavelengths of light reflected by the things you are interested in. But precisely because this classification is arbitrary (the argument goes), statements couched in grams are no more or less "true" than those expressed in millemicrons: they are only more or less relevant or appropriate. Perhaps in order to avoid the objection that, in this sort of case false statements clearly <u>can</u> arise as a result of <u>mis</u>-measurements, Levy sticks closely to the ideas of linguistic or semantic coding and insists that the only sorts of error that can occur at this stage arise from either failing to follow the rules of the language-game (moving the rook diagonally at chess is not a false statement), or using an inappropriate code (recording grams instead of millemicrons). Levy repeatedly compares this stage with linguistic "definition", by which he means the sort of <u>prescriptive</u> definition that is done when setting up a symbol-system <u>de novo</u>. If I decide, for some purpose like the Hanfmann-Kasanin test of visuospatial concept-formation (Semeonoff & Trist 1958, Ch.2), to <u>assign</u> the nonsense-syllable 'nev' to low-volume objects regardless of shape or colour, so as to provide a cue for correct classification, I am obviously not claiming that such objects <u>really are</u> "nevs" in the same sense that they really are made of wood (nor even in the sense in which whales are 'really' <u>mammals</u>). But, once you have decided to administer my version of the test, it would be a <u>mistake</u> to call high-volume as well as low-volume objects 'nevs', because we should no longer be able to apply scoring procedures and test-norms to your report of a subject's performance, and we should consequently be unable to <u>use</u> your observations.

For this sort of reason, Levy argues, the semantic stage "adds" nothing to the raw behavioural data, and is to be contrasted with the second ('propositional') stage, at which particular theoretical hypotheses, which may indeed be true or false, are brought to bear upon the encoded behavioural data.

The contrast, in practical terms, would be as follows. It is a matter of <u>coding</u> or 'definition' that, according to a certain conceptual scheme, hoarding bus- tickets is correctly classified as 'anal' behaviour; but it is a matter of fact whether hoarding bus-tickets is (always or generally or in this case) causally connected with emotional conflicts of a particular sort, and generated at a particular stage of childhood as required by the theory of 'anal fixation'. The truth of the former (classifactory, or 'semantic') claim is of course independent of that of the latter (empirical, or 'propositional') one; and this same logical distinction between aspects of taxonomy is sometimes made in the language of 'analytic' versus 'synthetic' exemplaries of a concept (cp. Miles 1966, <u>passim</u>). I mean: even if ticket-hoarding is never, in point of fact, so caused, it would still be true that such hoarding <u>is</u> (in the former sense) 'anal behaviour'. Confusion arises, however, because the mere statement that something "is" anal behaviour is ambiguous as to which of these two different claims is being made. A thorough investigation of these and associated complexities would have to touch upon the metaphysical foundations of scientific classification in general, which are clearly beyond our scope; but they have recently been the object of Harré's scrutiny (1970, pp. 203-233).

Classification without empirical implication, however, runs the risk of being another "empty ceremony". For if the procedure is to be other than a disguised form of <u>naming</u> (with only one member to a class), the class must have members other than the one being classified, which in this case is 'hoarding'. That is to say, there must be types of behaviour other than hoarding which will also be assigned to the class of 'anal' traits; and in practice such other members are suspiciousness, meanness, love of precision and concern for order. But now, the only heuristic justification for classifying actions <u>X</u> and <u>Y</u> as 'A-type' behaviour (and not so classifying <u>Z</u>), when not postulating common properties, is the assumption that <u>X</u>, <u>Y</u> and other class-members occur in each other's company, or cluster, more often than they do in the company of _3_ and non-members. This, however, is a definite empirical hypothesis, and as such it is at variance with the concept of the 'semantic stage'; for it both makes the business unlike definition and naming, and renders it vulnerable to factual investigation. Does such clustering as is required by the rules-for-use of the class-name, 'anal' actually occur (regardless of 'propositional' hypotheses about what causes the alleged clustering)? This is precisely one of the questions to which Kline has addressed his recent survey. His answer is that there is good evidential support for the sort of clustering denoted by this syndrome, though not so much for the corresponding 'oral' one; but that the 'propositional' hypotheses about the altiology of both syndromes, receive very little confirmation from available data (Kline 1972, pp. 44, 94.).

There is another count, also, on which the required distinction does not quite hold water; mainly because the choice of "language system" is not as free of empirical content as Levy suggests. The choice of what system is "most useful for one's purposes" (Levy 1963, pp. 38-39) depends upon what sort of thing you want to code _for_; and _that_ decision must depend, externally to the code, upon what aspects of a person's behaviour you hold to be important for understanding him. Levy seems in a way to recognise this but at the same time to deny that it undermines the empirical neutrality of the logical operations at the'semantic'stage. This last decision, (that concerning 'importance' or 'significance') is represented as being made, internally as it were (and hence non-empirically), by the code itself. Thus Levy speaks of some "attributes" rather than others, of the behavioural data, being "considered significant by our ... language system" (1963, p. 39). But it is *people* who "consider", not languages. What matters for the moment, however, is that here we have serious suggestion about how one aspect of interpretation-construction is non-propositional and consequently independent of truth-considerations.

But it raises a general problem about the nature of the raw data in this context, and how to represent them. For, if you think in terms of collecting raw observational data and applying hypotheses to them, then there is a familiar difficulty in the behavioural sciences about how to characterise such basic data. For, in so far as much human behaviour is purposive, intentional, expressive and symbolic, the quest for a behavioural atomism which would confin itself to supposedly 'objective' description, without imputing contextual significance (Weber's *Sinn*) or emotional content to people's actions, runs the risk of missing their whole point. In Bartlett's classic example (1950, pp. 247-248), two men raise their right hand in"the same" way; but one is greeting a friend, the other stopping the traffic. A content-neutral data-language which described the identical arm-movement in space-time coordinates alone, as a basis for the subsequent application of some behavioural calculus, is going to let the real nature of the behaviour slip through the mesh of a pseudo-scientific net. It is "pseudo-" not because its descriptions are inaccurate, but because they accurately describe the wrong things (cp. Polanyi 1958, pp. 3-17).

This theme and its relevance to the logic of interpretations will tend to recur (see Ch. 8); but what counts at this point is that a certain doubt is cast on whether there <u>can</u> be useful description, even at this preliminary, 'semantic' level of a neutral data-language, which is free from empirical implications and hence not vulnerable to error, bias or coloration. It has often been argued, to be sure, by anti-scientists that <u>any</u> scientific system-atisation, even of non-behavioural, inanimated material, necessarily concentrates on some features rather than others, and consequently had no special claim to non-directive neutrality. Bergson warned against supposing that philosophy could take over "the facts" from empirical science and then go on to apply its own critique of logical analysis or metaphysical inquiry to them; for the data had already been contaminated by the metaphysics (often unspoken, and therefore the more pernicious) of scientism. "The metaphysic or the critique that the philosopher has reserved for himself he has to receive, ready-made, from positive science, it being already contained in the descriptions and analyses, the whole care of which he left to the scientists. ... Let us not be deceived by an apparent analogy between natural things and human things. Here we are not in the judiciary domain, where the description of fact and the judgement on the fact are two distinct things, ..., Here the laws are internal to the facts, and relative to the lines that have been followed in cutting the real into distinct facts". (Bergson 1911, pp. 238-242). Poincare went even further to argue that methodology in science was, to an important extent, precisely a matter of <u>choosing</u> one's facts ("<u>la choix des faits</u>";)cp. 1912, pp. 15-24).

Now we are no longer particularly bothered by these questions, I dare say, as far as physical sciences are concerned. Certainly we are not as confused as Eddington was about how to reconcile his notorious "two tables": the one composed (according to the story of physics) of whirling quasi-particles, electric charges and void; the other made up (by the evidence of his senses) of hard, firm, brown wood (Stebbing 1937, ch. 3). But when we try to press the study of behaviour into the mould of the established physical sciences (or some of them), wise men still dispute about what should be regarded as the basic data and hence about what form the neutral data-language should take. This is an old and large controversy, which becomes polarised into the various forms of so-called behaviourism, on the one hand, and phenomenology on the other (Mace 1948; Wann, ed. 1964). We return to some aspects of it, insofar as they affect our subject, in chapter 8. But we must notice here how Levy works out, in his analysis of interpretation, his concern to avoid mentalistic mystery-mongering, and to put his argument on as concrete a behavioural foundation as possible.

One of his objects is to bring the business down to earth, and to divest it of the mantle of mystique and occult obscurity which it too often wears. And only by breaking it up into its components, so that their precise nature and function can be understood, will the process become susceptible of rational scrutiny and be put into a form in which its claims can be checked, where appropriate, against facts, its methods improved and its techniques coherently communicated. For, as long as a skill is represented as being available only to people with a certain flair, intuition, perceptive faculty, clinical experience or whatever, so long will there be no doubt whether there is any skill there at all: whether, that is, the emporor has any clothes. It fosters the suspicion that it is all no more than an in-bred tradition, by which pupil-therapists have learned merely to do what theacher-therapists approve. Even Menninger, unfortunately, perpetuates such doubt on the topic of timing when he writes, "The question constantly arises ... how the analyst can be sure when the optimum level of frustration-tension is being threatened. ... Practically, after some years of experience, this comes intuitively"; and later, "... if he keeps 'in tune' with the patient's unconscious, he <u>knows</u> when to speak" (1958, pp.133,136). Now, how do you tell whether you are keeping in tune in the required sense? Menninger realises, of course, that this is unsatisfactory advice, and he has a good deal else to say as well. But it is a pity that this note of obscurantism should be allowed to sound at all. I do not want to deny that a certain constitutional or acquired sensitivity may help in interpretive psychotherapy (a central European accent also does); but if there <u>is</u> a system, which is really a <u>system</u>, then it can be expressed and taught.

The other comment, on the general problem of characterising the raw behavioural data, is that, since there are such radically contrasted ways of looking-at or talking-about one action as are represented by "physical-thing language" on the one hand and phenomenology on the other, is there not a whole range of different, but less sharply conflicting, perspectives between these two extreme points of view? Or even, are there not several alternatives bunched within any segment of the spectrum? That is to say, even if you opt for a particular <u>sort</u> of approach, there may still be several different ways of describing and structuring the data from that one general point of view.

Suppose you decide to take a <u>geometric</u> view of the top of my desk, - as opposed to the view of a carpenter, a still-life painter or an economist. There are still various ways of conceptualising its properties even from that cognitive angle. You may regard it as a straight-sided surface; in which case it is the same sort of thing as the ceiling or the floor, and hence liable to be 'classified', if occasion arises, with them. You may see it as a four-edged object, which groups it with my ruler or the door. Seen as a horizontal plane it is like the ceiling but not the door; while, taken as an area of so many square feet, it resembles the door but not the ceiling. Now, it is notorious that even psychodynamic accounts of human behaviour differ among themselves in this kind of way. That is, patterns examplifying the 'constructs' of different (and perhaps conflicting) theoretical schemes can sometimes be seen in, or picked out of, the same set of observations. The clinical student at a Kleino-Freudian case-conference, for example, may have the disturbing experience of hearing the only Jungian in the room plausibly reallocate certain behavioural data, which had dutifully been identified as Oedipal, variously to the Collective Unconscious, Mandala symbolism and the myth of Persephone.

This does not justify, however, the nihilistic conclusion that, since different structurings are possible, the whole business is arbitrary in the sense that there are no rational grounds for preferring one account to another. For we <u>do</u>, as a matter of fact, prefer one account of why the sun moves across the sky to others, even though (indeed, precisely because) it is selective about what properties of the sun, the earth and space it deals with. What it does mean is that we need to bring to light exactly what are the background assumptions which generate one way-of-looking at the data rather than another. As far as therapeutic interpretation is concerned some of these assumptions will be not only about the nature of emotional disturbance in general but also to do with what constitutes improved adjustment and how it is to be achieved. If you hold, with some of the 'behaviour-therapists', that symptom-loss equals cure (Eysenck, ed. 1960, pp. 4 - 83; Bandura 1969, pp. 1-117), then not only will you be aiming at a different end-state from that sought by a clinician who believes that symptom-freedom is consistent with continued disturbance (Malan et al. 1968), but you will also do different things about the symptoms. It is important to be clear as to where and how this sort of assumption impinges on the way the behavioural data are encoded at Levy's stage of semantic processing. For it would seem, as we have argued above, that the kind of 'code' used, and what is selected for coding, must depend upon synthetic (that is, fact-stating and not merely analytic) premises about what is teleologically relevant.

This appeal to the possibility of different 'perspectives' on the same data may lead, when its arbitrariness is stressed, to the idea that T.I.'s are non-propositional (in a fact-stating sense) in so far as they merely provide the patient with another way-of-looking at his problems, without asserting even implicitly that it is the correct way. Levy invokes this argument, which we are about to take up (section (c), below) quite explicitly at times (1963, esp. pp. 20-21); but it is not the defence he has in mind when he says that the therapist "may legitimately not even be concerned with error in interpretation, if it appears that the interpretation will have its desired effect" (1963, p.130). That would appear to be no more than a forthright pragmatic claim that it does not matter (in a sense) if you say something palpably false so long as you get the patient better. Since the TI is post-propositional according to his scheme, and not non-propositional like a semantic-stage move, it cannot escape having a truth-value.

We must be concerned with this truth-value for theoretical purposes in general, and certainly for the particular purpose of correcting both background theory and therapeutic techniques; and we have seen that Levy aims to facilitate such correction by his demysticising analysis. To hold that a practice is legitimate if it seems to work is both pragmatically attractive and the thin end of a dangerous wedge (cp. p. above). One defence is to contend, as with the appeal to 'perspective', that nothing false is really being said, for the reason that nothing is really being said at all. Such assertions, it might be argued, are 'propositional' only in form and not in content or spirit. Let us look more closely into whether some therapeutic interpretations might be claim-free in these senses.

(c) Could interpretation be claim-free? A statement which was truly free from any propositional claim could not (logically) be treated as true or false, because it would not be asserting that anything in particular is, was or will be the case. Not being susceptible of verification (in practice or in principle), it might even be regarded as "meaningless" in a much-discussed positivistic sense (Ayer 1946, pp.7-21; Hempel 1950); but it could still play the role, for instance, of an exhortation, a recommendation, a threat, an encouragement, a warning or a command. In psychotherapy, therefore such statements could be used to offer, and to facilitate the acceptance of, a different way-of-looking-at the patient's predicament from the one he has adopted hiterto, without involving the categorical claim that this is the correct perspective.

Austin is well known for having discussed a type of utterance, or linguistic performance (the 'illocutionary act'), which functioned, not as a report on the speaker's own mental scene or behaviour (as perhaps with "I am afraid...", "I expect", or "I am reading Doctor Zhivago"), but by actually doing what they said: e.g. "I promise you ...", "I warn you ...", "I suggest ...". These he called 'performatives', because, instead of referring to an action or whatever, they perform it, as it were, themselves (Austin 1958; 1961, pp.220-239; 1962, passim). Critics have argued subsequently that the constrast is not as clear-cut as all that (Strawson 1964; Fann (ed.) 1969, part 4; Searle 1969, ch.2); while Austin himself did not doubt that there would be borderline cases, and soon saw that the original idea needed revision. But we shall come across some analogies to the distinction that he was making. Indeed Turner (1971) has argued that although the use of 'performatives' might be expected to provide a significant parameter for the sociology of verbal interactions, an understanding of the "total speech situation" is necessary even to identify a speech-act that is functioning in this way, since mere "syntactical or lexical correspondences" will be misleading. We do not need, however, to fish the contentious streams of linguistic philosophy for everyday examples of recommendations or exhortations dressed up in the grammar of assertions or predictions.

"This will put you right", says the doctor, handing out a placebo to a patient whose stomach-ache he judges to be psychosomatic thereby deliberately trading as part of the treatment on the patient's supposed suggestibility. The doctor is not making a simple prediction: what he says is *itself* intended to have transformatory force. And insofar as he is predicting anything, it is not that the tablet *per se* will cure the ailment, but that *taking* that tablet *in the belief that* it will cure is a sufficient condition for relieving that patient. Similarly, we often try to get the best possible performance out of people by manipplating their morale. Consider a school sports-day. To a boy running a race you might say "Come on, John!"; or, if you think he's over-anxious, you might say instead "You're doing fine!". Grammatically speaking, the latter makes an assertion where the formèr does not; but its real force, of course, is to *alter* a present state-of-affairs, not to describe it. The same sort of altering may also be done by quasi-predictive statements, as when, to boost the confidence of a diffident high-jumper who has already failed twice at a particular height, you might say "You'll do it next time". The form is predictive, the content transformatory.

Now the justification for saying things like this does not lie in how accurate the quasi-descriptions are, or how well-grounded the quasi-predictions. They are appropriate or otherwise according to whether you said the sort of thing that would get the desired effect. But once you make efficacy the sole criterion, and say that it does not matter that John was not in fact "doing fine" when you said he was, you seem to open the gates (as we have been above, pp.   ) to all sorts of malpractices; and *TI* becomes merely a special form of benevolemt persuasion, suggestion or manipulation whose apparent explanatory content is no more than a means to an end, - like the distracting patter of a conjurer or the white coat of a 'scientific' hypnotist. But perhaps even those who reject the criterion of 'true-implied-explanation', for approving one interpretation rather than another, are tacitly relying on <u>some</u> background theory which needs to be brought to light.

Consider another class of quasi-categorical statements that are easily translated into perspective-recommendations. Teachers of any skill know that, for most learners, some ways of 'looking-at' the constituent elements are more advantageous than others. Here "advantageous" means conduc~~t~~ive to quicker and/or better mastery (sometimes the distinction matters); and "looking-at" covers many different modes of conceptually organising the operations involved, including which features you attend to first,which ones you group together and even how you 'feel' about them. Accordingly, teachers often make manipulative statements in order to get their pupils to develop~~e~~ the required ways of 'looking-at'. These statements may take the form of categorical~~y~~ propositions about the skill, without being 'true' in a straight-forward sense. Indeed, some are deliberately paradoxical and others clearly false!

Trainee riflemen used to be (and maybe still are) told, "You don't <u>pull</u> the trigger: you <u>squeeze</u> it". Put still more categorically, it might be, "A rifle is fired not by pulling the trigger, but by squeezing it". Now, how would an observer tell whether Smith was really pulling, as opposed to squeezing, the trigger?; and, indeed, where does a smooth "pull" merge into a jerky "squeeze"? The paradox is of course that the movements are overtly so similar that the required difference in performance is most effectively marked, and achieved, by getting the pupil to <u>feel</u> it <u>as</u> one sort of action rather than another: that is to say, by attending to their phenomenological, subjective qualities. Thus unwanted muscle-contractions, which might be jerky and disturb the aim are avoided by appealing (indirectly) to the imagination and not by attending (directly) to the minute movements themselves. It is the spirit of the thing which counts, and which has to be trained for. The question whether it is <u>really</u> a squeeze or a pull is both ontologically odd and beside the point.

Now an actual falsehood. The soloist's first entry in Liszt's E-flat piano concerto involves a rapid sequence of two-handed chords which lie alternately low and high on the keyboard. The low point is constant, but the distance to the high point gets progressively wider. The hands therefore have to 'jump' from base-camp to high-point, back to base, up to a higher point, back to base, and so on in a two-way travel. But a veteran teacher at London's Royal Academy of Music used to tell his pupils studying this work,"Remember, you jump in one direction only". Now it seems obvious to any player that he has to 'jump' <u>up</u> the keyboard and then <u>down</u> again; but the point of the remark is to get him to <u>think</u> of it as a series of one-way leaps, <u>all</u> taken from base-camp (that is to say, you don't <u>jump</u> back: you just start again!). Subjectively, a series of leaps in the same direction from a constant feference point (A to B, A to C, A to D etc.) seems much less hazardous to control than leaping alternately in opposite directions. And what seems less hazardous will be less prone to anxiety-induced error. In this case, the recommended mental perspective may also have the physiological effect of dissipating muscle tension on the downward journey and thus facilitating controlled attack on the next leap upwards.

An anecdote about Arthur Schnabel makes a similar point. Coming across a pupil who was working herself into an anxious frenzy over the speed at which a rapid passage had to go, he said "You do not have to play it quickly, my dear: just play it slowly in a fast tempo".

In both these examples, the quasi-categorical statements may easily be converted into recommendations plus reasons. In the first it would be, 'Think of it *as* sqeezing, rather than pulling, because then you will make a smooth movement and not jerk the gun out of line'; in the second, 'Think of it *as* a series of upward jumps from the same point, because ... (as above)'. Whether the advice is good or not is obviously another question, and one which we know how to answer because we know what the advice is supposed to achieve. But the original remark about squeezing and not pulling the trigger is not making a true (or false) statement logically parallel to 'Rifles are fired by percussion, not by sparks'; nor does the one concerning the Liszt passage correspond to saying of the first page of Schubert's A-flat impromptu from *opus* 90, 'You play broken arpeggios downwards only' (Schubert simply not having written any that go upwards). How then can this be applied to a 'perspective' theory of therapeutic interpretation?

Our firearms-instructor and piano-professor might well have said, respectively, "It is *as if* you squeeze rather than pull ...", and "It is *as if* you jump only one way". Now, this is precisely a formula beloved of psychotherapists when introducing interpretations. "It is as if you are afraid that ...", "It is as if you cannot let yourself ...", "It is as if you resent ...", "It is as if you avoid ...", "It is as if you are trying to ...". The flies on many a consulting-room wall must know these phrases by heart. But the logic of such 'as if' statements is complicated, and they do a range of different sorts of job. (cp. Wittgenstein 1958, pp. 193-214). It is one thing, for example, to say, "Smith is acting as if he were a tin god" (and we all know he is not one); and quite another to say, "Smith is acting as if he were reluctant to ..." (and for all I know he may be reluctant). Sometimes acting-as-if-you-were-doing-X is not to be doing X at all; other times it is to be doing X in a special way, or (which is different) to be doing it in a manner of speaking. The latter alternative leads to propositions like "It is as if you were preventing yourself from succeeding" being used as grounds for (or even being treated as identical with) the claim, "You actually are, in a special way (e.g. unconsciously), preventing yourself ...". And it is worth noting that although Freud chided Janet, in one of his Introductory Lectures, for treating "the Unconscious" too much as a figure of speech and too little as a categorical reality, yet he was forced elsewhere to admit that there could not really be "unconscious affects" in the same way as there are "unconscious ideas", and later that different psychic subsystems are "unconscious" in different senses (Freud 1917, p. 257; 1915, p. 178; 1933, pp. 57-72; and cp. Miles 1966, pp.76-89; MacIntyre 1958). This elision of the "as if", which turns a plausible simile into a dubious categorical, is very close to what has happened, according to a currently fashionable view, to engender the "mythical" concept of 'mental illness'. Truncated, part of the argument is that there has been a slide from something like, 'This man is behaving *as if* there were something wrong with his 'mind' (just as people are physically hampered by being ill)' to, 'This man *is* ill, but in a special way: namely, 'mentally' rather than physically' (Szasz 1972).

But let us return to the question of how legitimate the original interpretative similes are; that is, to a therapist saying to his patient something like, "It is as if you were trying to make yourself fail". The troube is, of course, that there is *some* point of view from which almost anything can be seen *as if* it were almost anything else; to say that 'X' is really more like 'Y' than 'Z' both depends upon and betrays one point of view rather than another. The question 'But which is it *really* like?' cannot often be answered in the abstract. A coffee-table may be like a cat in having four legs, but like a tree in being made of wood. If I choose to say that it is really more like the tree, I indicate that I regard substance-resemblance as more important in general than shape-resemblance. But we can all think of some purpose (e.g. teaching a child the number '4' or the extended concept of 'leg') for which the table parts company with the tree and goes along with the cat. That is why Bergson was so concerned about the potentially contaminating and tendentious effect of *any* method of representing basic observational data. Accordingly, we have to ask (in the following chapter) whether the point of view from which psychotherapeutic observations are made necessarily 'contaminates' them in a way that renders them useless as an evidential basis for explanation.

Now, in the case of 'as-if' interpretations, this purpose or background theory which justifies them is too often formulated only very vaguely, so that it is not at all clear why one 'as-if' is more appropriate than another. It is a notorious problem, as we have seen above (p.00), that one background theory in psychodynamics will generate quite different 'as if' comparisons from another, depending upon its focus, conceptual structure and so forth. Very occasionally a 'resemblance' which is pointed out can be shown conclusively to be an illusion, and we shall discuss one such case below, that of the "vulture" in Freud's study of Leonardo da Vinci (pp. 00-00). We need to bring to light both the general background theory *and* the specific hypotheses which generate from it the individual *as if*-propositions. This is what Levy seems to be doing much of the time, when discussing, for instance, whether an interpretation is approporiate to its governing theory (*op. cit.* pp. 14-15). On the other hand, however, he *also* wants to say that the only test of its appropriateness is whether it rings true, or perhaps can be made to ring true, with the particular patient concerned. I shall return to this inconsistency below.

If I pick up something gingerly, who is to say whether I pick it up 'as if I thought it was hot', or 'as if I thought it was poisonous', or 'as if I thought it was alive', or just 'as if I was afraid of it'? And is not the question "Well, how did he *really* pick it up?" oddly misconceived? - though it is no simple task to say precisely what is wrong with it. It is for this kind of reason that perspectivists would disclaim the idea that an interpretation of theirs offered the correct 'as-if' story about a patient's behaviour. Thus Levy asserts that interpretation consists essentially in "bringing an alternate frame of reference ... to bear upon a set of observations or behaviours, with the end in view of making them more amenable to manipulation" (1963, p.7). The term 'manipulation' may strike a sinister note, but all that is meant is that the patient has developed a way-of-functioning in certain circumstances (or a way of 'responding' to certain 'stimuli') which is maladaptive, which makes him unhappy, which disturbs others etc., and which he himself wishes to be rid of. Consequently, situations in which psychological interpretation is offered have, according to Levy, one feature in common, namely that "someone is in a bind"; and the way in which it aims to get him out of his "bind" (fix, rut, mess or hang-up) is by a "redefining or restructuring of the situation through the presentation of an alternate description of some behavioural datum" (1963, p.5). (The English reader should perhaps understand "alternate" as meaning 'alternative').

A little later, Levy explicitly denies that the therapist is formulating a "truth" about the patient which he has discovered: he is (merely) matching one point of view against another and asking the patient in effect, 'Can you see it in this way instead?'. So that, when we inquire about his 'insight', "we are simply asking about the extent to which the patient's construction of events matches our own, not whether he too has become privy to the truth" (1963, pp. 20-21). But, plainly, our therapeutic object is to get him to see things in a new way, because we hold *ex hypothesi* both that his present troubles arise from, or are intensified by, looking at things in a certain way, *and* that "an alternate construction of a given situation *will be of more use to him* than that which he has come up with on his own" (p.21, italics added). So there *is*, after all, an implied claim: not, indeed, that the new perspective is right, but that the patient will get on better with it regardless of that.

Now there are, of course, a variety of background hypotheses about what sort of perspectives are conducive to good adjustment or "getting on better", and a variety of theories about what 'good adjustment' consists in. But we have seen that Levy tries to avoid the embarrassment that such hypotheses and theories might in fact be wrong, by insisting that the way in which an interpretation draws upon them for the new perspective which it offers, is only like using another *language*. "In psychological interpretation", he writes (p.5), " ... one language system is pitted against another".

Plainly, however, to give the patient an "alternate construction" of a situation, which will hopefully be more useful to him than his existing view, cannot be merely to express his existing view in a different language. An alternate *construction* is essentially a different *view*, arising from a different conceptual breakdown of the data; it is not the *same* conceptual breakdown put into another language, like saying the same thing in French or Morse code instead of English. This fresh construction or conceptual scheme will consist precisely in seeing some elements of the problematical situation <u>as</u> exemplars of other classes than those to which they had been attributed hitherto. This is not merely to translate: it is to reconstruct the data; or, in Kelly's terminology, which Levy has in mind (cp.p.9), to 'reconstrue' them, by suggesting different contrasts, different groupings, different 'as-ifs' from those which the patient has so far seen. What matters for the perspectivist is not the difference in symbol-system, like calling the inhabitants of a field <u>vaches</u> and <u>moutons</u> instead of cows and sheep: it is the change in conceptual scheme analogous to grouping my desk-top with the ceiling, on the ground that they are both horizontal planes, instead of with the door (as hitherto, because <u>they</u> are the same shape and size). This Levy sometimes seems to concede: for example, when he writes of interpretation as "the application of an alternate construction for a given event" (p.73).

Let us take up his example of the concept of 'authority-figure' (pp. 25-26). There are obvious reasons, on one's view of some situations, for grouping brothers, sisters and parents together as 'family', in contrast to teachers and employers, who are not. But an interpretation might well depend upon grouping parents, teachers and employers together, as 'authority-figures', as opposed to (younger) brothers and sisters, who are not. The change of <u>language</u>, that parents are now called "authority-figures", does not matter tuppence of itself: it is the changed scheme of construing the data, by which a certain role is selectively emphasised in the re-grouping, that might be thought, on certain theoretical assumptions, to be potentially beneficial to a patient. Levy recognises, to be sure, that "the classes we have available in our language-system ... make a difference" (to the possible interpretations which can be constructed), and that "one must ultimately choose the language-system ... that one feels will be most useful for one's purposes" (pp.38-39). From which it is clear that the therapeutic offering of new perspectives is not claim-free at all. For, when an interpretative perspective treats respect-for-the-opinion-of-others, vacillation-in-decision-making, conforming behaviour and not-volunteering-for-authority-positions as all instances of 'dependency', it may not imply the causal claim that such actions and dispositions really do spring from one-and-the-same basic attitude (p.38). But it must be generated, in so far as it rests on any rationale, by the pragmatic or prognostic background proposition 'Seeing these things as all instances of "dependency" will help you, more-or-less indirectly, to get better'; or perhps, even by the broader claim, 'Seeing these things as ... "dependency" helps people in general'.

Certainly, if its helpfulness for the particular patient is the only criterion of the rightness of a new perspective we should be entitled to interpret 'the same' behaviour (causally speaking) in one way to one person and in another to another, on the ground that 'Seeing it this way helps people like you', but 'Seeing it that way helps his sort'. This must be distinguished from giving different interpretations on the ground that 'the same' action means one thing (phenomenologically speaking) to one agent and another to another. It would simply be to take the pragmatic criterion seriously, and to divest therapeutic interpretation of any last shred of explanatory objectivity.

But even that would not quite put an end to our questions. For (1) how do you tell whether a patient has found a given perspective helpful?; (2) how long do you wait before deciding that it will not be helpful and offering him another?; and (3) is it not better for patients to work round to their new perspectives for themselves, anyway, rather than take them over from somebody else?

Question (1) cannot be answered without appeal, however tacit, to some propositions about good adjustment 'recovery', or 'cure'. That the patient is now symptom-free, feels better, is holding down a job, etc. will not do for theorists like Malan who argue that a patient may achieve that sort of result by having learned how to avoid situations that trigger neurotic reaction-patterns rather than by having improved in ego-strength and adjustment so that he can now deal with them maturely (Malan et al. 1968). For similar reasons it will not do that the patient himself thinks he has now got a better perspective, that he has developed more insight, that he understands himself better or that his troubles are at an end. We do not allow, for instance, that the cyclothyme who has swung out of his depression into hypomania, feels fine and wishes to discharge himself from hospital, is cured. And yet this criterion of self-satisfaction or ontological comfort is used more-or-less explicitly by Carl Rogers, as we shall see shortly. There is also, for some psychotherapists, the strange concept of 'flight-into-health' to contend with (cp. Rycroft 1968, p.53). It is a well-known defence against the distressing business of facing up in therapy to his unconscious conflicts (the argument goes) that a patient produces a specious improvement in functioning, which does not derive from a genuine working-through of his anxieties and ego-control over them, but from some pathological mechanism such as intensified repression, dissociation or manic denial instead. It is important, therefore, for both therapist and patient to recognise such pseudo-recoveries for what they are in order not to be taken in by them. Cynics would suggest, of course, that it is a mechanism whereby patients are sometimes taken in by the therapist and induced to continue what is often expensive treatment beyond the point at which they no longer need it. But if there is such a thing as 'flight-into-health', the distinction between it and 'genuine' improvement is a highly technical and theory-laden business which must be left to the expert judgement of the therapist, <u>provided that</u> the distinction can be expressed in a coherent way which admits of systematic judgements being made about it.

Levy seems to deny, however, that the therapist knows best in this kind of way (see above p.00). In offering his new perspectives and restructurings, the therapist is merely saying, in effect, "Try this way of 'looking-at' your situation; it may get you out of your 'bind'". But he will never know whether to make that recommendation again if he does not know how to tell whether the patient is really <u>out</u> of his "bind" or just in a different one (which is perhaps more subtle and less conspicuous).

A decision on question (2), about how long to wait for improvement before you conclude that a proffered perspective is not helpful and suggest another in its place, will have to take account of the well-known mechanisms of 'resistance' and 'secondary gain'. In classical psychoanalysis, an interpretation, even though correct, may be resisted, and thus prevented from inducing therapeutic progress, because it is offensive to the superego or otherwise psychically uncomfortable. Accordingly, a potentially helpful interpretation (whether correct or not) might fail for the same reason to produce short-term behavioural dividends. This leads to a dilemma. Either you must short-circuit the concept of 'resistance', on the grounds that, if a perspective is disconcerting enough on presentation to evoke resistance, then it is not 'helpful' in the required sense; or you must have some theoretical way of distinguishing between non-progress which is due to resistance to a potentially valuable perspective, on the one hand, and non-progress occasioned by an intrinsically unhelpful suggestion, on the other. The latter alternative is obviously theory-laden, and consequently at variance with Levy's position. The former is either tautologically true, by some strangeddefinition of therapeutic 'help' which makes temporary discomfort inconsistent with it, or just empirically false (if we are to stick to ordinary ideas about what is helpful).

A second reason why even the (potentially) most helpful interpretation may not produce behavioural improvement for som- time is that some patients, hysterics notoriously, have built such a life-style round their maladaptive behaviour that they get a great deal of emotional satisfaction from other people's protective, indulgent, supportive and solicitous attitudes, and stand to lose all of this 'secondary' gain if their symptoms were relieved as a result of adopting the new perspective offered in the interpretation. In which case, it may be said, the therapeutic task would be to find a perspective according to which healthy independence is <u>seen as</u> preferable to pathological dependence and passive manipulation; the fact is that the 'working-through' of resistances has been since early days a central task in interpretative psychotherapy. Certainly; but this depends upon the <u>theory</u> that 'help' consists in moving the patient away from that sort of dependence and passivity.

Carl Rogers is well known for answering question (3), as to whether patients should work out their new perspectives for themselves (without having them suggested by the therapist), in the affirmative. It is a determining feature of his so-called 'non-directive' and 'client-centred' psychotherapy that the therapist does not offer interpretations, explanations or new perspectives <u>to</u> the patient, but makes it his business merely "to hold", as 'twere, the mirror up to nature" in such a way that the patient comes to restructure his own view of himself, <u>for</u> himself, as a result of what he sees reflected therein (Rogers 1951, esp. chh. 1, 2, 4, 5). This is done in practice by the therapist picking up and repeating, more or less verbatim, some of the things the patient says about himself of his own accord in talking about his problems.

Surely here at last then, we have a truly claim-free therapeutic technique? Not, to be sure, a form of claim-free <u>interpretation</u> as such, for Rogers is at pains to disavow the use of such authoritarian impositions, as he sees them (1951, pp.219-228); but a means, nevertheless of changing a patient's way-of-looking at himself for the better, without the therapist making <u>any</u> propositions or recommendations (however implicit) about it.

Because he asserts nothing <u>about</u> the patient's predicament, but only reflects back certain features of what the patient himself says about it, the therapist cannot be accused of distorting the relevant behavioural data (as he may be when using theory-laden classification schemes), nor of influencing, by his own expression of opinions, what the patient says, or refrains from saying, next. So Rogers specifically says that when a therapist enters a therapeutic relationship "making interpretations, ... his distortions enter with him" (1951, p.42). And even Levy concedes (1963, p.248) that giving a conventional interpretation betrays something of the therapist's view of the patient and of therapy. This furnishes, of course, a standard objection to the idea that the validity of an interpretation can be checked by appeal to its "eventual" acceptance or beneficial outcome: namely, that anyone in an emotionally-charged relationship is predisposed, for the sake of security, to accept suggestions from the prestige-figure on whom he is dependent, regardless of their truth, and to feel better for having acquired <u>some</u> coherent structure or perspective where he had none before. (As if the giving itself tends to transform in the direction of what is given.) Whereas with Rogers it is a point both of honour and of technique that the patient should not be able to tell what particular views the therapist holds about anything. Thus a patient reports:

"A lot of times I walk out with a feeling of elation ..., and of course at the same time I have the feeling that 'Gee, he must think I'm an awful jerk' ... But ... those feelings aren't so deep that I can form an opinion one way or the other about you". And Rogers comments: "There was not only no evaluation, but no standards expressed by which evaluation might be inferred" (1947, pp. 104, 108).

But this principle of non-directed self-seeking on the part of the patient turns out, on examination, to be no less theory-laden than some Freudian doctrine about how to use the 'transference' in therapy. For it must rest on the claim that views-of-yourself that you arrive at without direction (assuming that you _are_ undirected) are more adaptive, more stabilising, more accurate, more egosyntonic or generally more _something_ (desirable) than those to which direction by a therapist has contributed. This claim in turn would have to derive either deductively from some more general theory about the nature of personality-integration, or inductively from empirical comparisons of the relative effectiveness (according to agreed criteria) of directive and non-directive techniques in comparable cases. Here and there Rogers does seem to appeal to observations which purport to make the latter sort of comparison, and (more dubiously) to his own clinical experience (1947, p. 109; 1951, pp. 213-225). But for the most part he tends to take it as almost self-evidently true that a self-discovered re-organisation of one's self-perception is better than one derived from someone else's suggestion; and at one point he actually refers to the assumption that conventional interpretations increase dependency in the patient as "a reasonable _a priori_ hypothesis" (1951, pp. 214-215). Thus it is important not merely that one reorganises one's self-perceptions, but also that in so doing one becomes aware of one's "capacity for reperceiving experience".

However, doing things differently at someone else's suggestion also provides scope for discovering one's own (perhaps despaired-of) flexibility, - especially if 'resistance' has been worked through and overcome _en route_. Rogers would counter, as we have just seen, that a therapist should not encourage the patient to be dependent on someone else's ideas and promptings. But it would require an enormous and very theory-laden investigation to show empirically that this is always bad; and, in any case, psychotherapists have long since recognised the need to wean patients off their temporary transference-dependence.

Certainly, in his example of the young wife who had been behaving in a violently disorganised way towards her maid but who was no longer disturbed, according to her own account, after "I ... discovered it was nothing more than that she (i.e. the maid) resembled by mother", he advances no specific reason for thinking that her recovery would have been less effective or speedy if the patient had been given that interpretation in the conventional way (Rogers 1947, pp. 111, 110). Rogers apparently assumes that traditional therapeutic interpretation represents an unwarranted imposition of someone else's point of view, and tends to identify interpretation _per se_ with what most analysts would regard as bungled interpretations. Thus, although he asserts, on the one hand, that in his system of therapy "the clinician brings to the interview no pre-determined yardstick by which to judge the material", he nevertheless recommends against telling the patient "authoritatively ... that he is governed by certain factors or conditions beyond his control" (note this caricature of traditional 'interpretation'), on the ground that "we have frequently observed that ... it makes therapy more difficult". This latter observation evidently amounts to a regulative principle or "yardstick" for therapeutic technique, which is summed up in the doctrine "it is only when the individual discovers for himself that he can organise his perceptions that change is possible" (_op. cit._ pp. 105, 109). That is a sizeable background claim for a claim-free procedure of perspective-revision, and the contrast drawn by the caricature is in any case false.

It is just not true, to begin with, that an interpreting therapist typically makes statements about the patient's feelings, motives, conflicts etc. in an ~~authoritative or~~ _ex cathedra_ manner, if that means without regard to what the patient thinks, feels or understands. Indeed, we have already seen Menninger insisting that analysts always try to avoid dazzling the patient with incomprehensible insights for which he is not ready (p.00 above); and other therapists specify that most interpretations consist ideally in verbalising notions which the patient is on the verge of conceptualising for himself (cp. Yorke 1965). Secondly, it is grossly misleading to suggest that psychoanalysts typically imply (let alone, assert) that what they interpret to a patient is "beyond his control". For the ultimate, and perhaps only, purpose of the exercise is to identify "certain factors" precisely _in order_ that the patient may eventually bring them under control, instead of having them influence, (and usually disrupt), his behaviour _malgré lui_. Freud summed up this therapeutic objective in his well-known epigram "where Id was Ego shall be", and it is remarkable that Rogers should have chosen to forget it.

The doctrine of non-directivity, if taken seriously, also involves the claim that the patient knows what his problem is (that is, what it really is), or at least that he can be steered round to it, non-directively, before too long. Most clinicians will think, however, that they have come across cases where the patient's 'presenting problem' was no more than a smoke-screen, decoy, buffer or cry-for-help with respect to what was really bothering him. This is often referred to as 'the "by-the-way...." syndrome". On such occasions classical DI plays a major role in identifying the underlying conflict from clues given in what the patient says about the presenting problem. But according to Rogers' scheme, either this situation never in fact occurs (and must therefore be an iatrogenic delusion, like 'symptom-substitution' in the eyes of hard-line behaviour-therapists), or the therapist must collude in the patient's self-deception. Consistently enough, Rogers also lets the patient rather than the therapist be the judge of when he is well enough to conclude treatment. This rests in turn (as we saw above) on an implicit denial of the claim, made by classical psychoanalytic theory, that feelings of well-being, or the conviction that one can see things in their proper perspective now, may themselves be pathologically generated.

But students of epileptiform altered-states-of-consciousness are familiar with the phenomenon known as a 'vision of clarity' which some patients experience as a prodrome to an epileptic episode, rather like a cognitive counterpart of the more usual sensory 'auras'. In this particular altered state of consciousness, the patient has a vivid sense of having found the solution to some problem, of everything having clicked into place coherently, of having grasped the meaning of life in a flash, or something of the sort. Here is one situation, then, in which a patient's relieved and euphoric impression of 'seeing-it-all-in-perspective-now' cannot be trusted. Another neurological condition characterised by reduced tension and by relatively carefree unconcern about one's attitudes and impulses is the post-leucotomy syndrome. Indeed, the words of one of Roger's patients, with which he illustrates his criteria for improved adjustment, might well have come from the mouth of a newly leucotomised subject experiencing disinhibition, reduced sense of responsibility and mild dissociative euphoria. "I find that when I feel things, even when I feel I hate, I don't care. I don't mind. I feel more free somehow. I don't feel guilty about things ... It's suddenly as though a big cloud has been lifted off. I feel so much more content."

Rogers comments, "when an individual permits all his perceptions of himself to be organised into one pattern, the picture is sometimes more flattering than he has held in the past, sometimes less flattering. It is always more comfortable" (1947, pp. 112, 114). This sense of freedom and contentment *is* used, however, as a sign that the new perspective is "realistic". There *is*, therefore, the background claim that some ways of looking at one's self are more accurate than others; and also the claim that accuracy conduces to comfort, with its converse that comfort does not attend inaccuracy or unrealism. Commenting on the self-report quoted above Rogers writes, "Note that ... the willingness to perceive herself as she is, <u>is</u> to accept herself 'realistically', ... This realism seems to be accompanied by a sense of freedom and contentment" (<u>ibid</u>). But if the therapist can judge after the event that some perspectives are more "realistic" than others, he will also have some ideas in advance about the direction in which realism lies. If, however, he still fails to steer the patient in that direction, by directing his therapeutic mirror here rather than there, is not the therapist wilfully keeping his client ill and failing in a moral duty? And if he does so direct it, then obviously he is not being 'non-directive'.

These considerations lead to further claims about what the therapist should focus his mirror on (and why), and about why his attitude of benevolent acceptance helps the patient to restructure his self-perceptions and thus conduces to his 'self-actualisation'. The therapist, we are told, "assists the client in bringing from background into focus his own self, making it easier than ever before for the client to perceive and react to the self". He becomes "only an alternate expression of the client's self", and "By providing a consistent atmosphere of permissiveness and understanding, removes whatever threat existed to prevent all perceptions of the self from emerging into figure." This security from attack, and this "assistance" in focussing upon "the perception of self", are what conduce to "a more differentiated view of self and finally the reorganisation of self" (1947, p. 118).

In his more recent advocacy of so-called "encounter groups", Rogers (1970) takes to an extreme this idea that a non-directed self-discovery emerges from a supportive, permissive, non-authoritarian, theory-free interaction between participants. While not denying the liberating and self-actualisting potentiality of <u>some</u> such experiences, we need also to recognise that, contrary to what is claimed, quite specific theories and authorities are implicitly being invoked. On the one hand, a dangerously naive cathartic theory of ego-development and mental health dictates the method; and on the other the <u>mores</u> of the group's subculture determine what is to be accepted as the "true" personality of the participant (Cheshire 1973c).

What looked at first as though it might be relatively a claim-free, self-generated revision of his perspectives on the part of the patient, achieved by the therapist playing the role of non-directive sounding-board, turns out to be riddled with implicit technical claims about the psychodynamic function of the therapeutic interview, the nature of maladjustment, the means of bringing about re-adjustment and the criteria of recovery. The therapist's "non-directive" mirror may not be tinted or distorted, but it certainly is pointed in one direction rather than another, and at some things ~~and~~ more often than at others. All the same, it will be said, the mirror never shows the patient anything which he has not produced of himself. But even this will not do as a defence against the charge of systematic manipulation.

For, when a Skinnerian operant conditioner trains a rat to turn three circles and press a lever with its left paw to get food, he never shows or imposes on the animal (according to the theory) any behaviour which it has not produced of itself.

Yet it ends up doing something which the experimenter wants it to do and which it was not at all inclined to do previously; and it also "feels better" in so doing, because now it keeps itself fed rather than starved. The theory is, of course, that by selectively rewarding some elements, and not others, in what the animal does of its own accord, you can gradually pick out those elemental actions which are relevant to some purpose of yours, and string them together in such a sequence that the animal ends up doing something which it would not otherwise have dreamed of doing.

Now, if you allow that, when the warm, understanding, accepting father-figure of a therapist reflects back a idea or attitude to a client, this serves to reward and reinforce that particular way of looking at things even though no evaluation is expressed, the analogy between Rogers and Skinner becomes apparent. Indeed, it has become traditional to refer at this point to a pioneering study by Greenspoon (1955), who showed that you could make a patient unwittingly use a certain kind of word more frequently in therapy merely by giving an accepting-sounding grunt whenever he uttered an instance of it. It is immaterial, of course, that Rogers' patients could not tell what views and attitudes he held, because in Greenspoon's demonstration also the subjects altered their verbal behaviour without being aware of his experimentally-assumed 'preference' for plural nouns (and a fortiori without realising that they were changing in response to that preference).

Since, however, the purpose of psychotherapy is not (usually, if ever) to get people to use certain words more often in the company of certain person, the Greenspoon phenomenon is not quite as alarming as is sometimes assumed. But if the same goes for ideas, attitudes, ways-of-looking-at-oneself and perspectives as goes for words, then the possibilities for surreptitious directivity in superficially claim-free techniques are considerable. And how much greater is the scope for overt directivity when interpretative techniques are admittedly theory-laden. We must now ask what effect this has on the evidential status, for explanatory purposes, of observations from therapeutic interviews.

CHAPTER FOUR

Data and Discovery in Psychotherapeutic Material

(a) What therapists say

(b) Observation and distortion

(c) Decontamination in other disciplines

(d) The nature of psychotherapeutic 'discovery'

(a) What therapists say. Having looked at some of the things which are typically said and done by workers who practice PDI, we are now in a position to ask openly some rather fundamental questions already hinted at, about whether it is systematically possible for a clinician to observe and describe his behavioural material in such a way that the observational data can provide a basis for any sort of objective account of what the people observed are doing and why they are doing it. For, in reporting their work, therapists of a psychodynamic orientation in outlook and technique certainly do make what appear to be discovery-claims about their patients, and sometimes, by extension, about human nature in general; and they do appear to back up their claims by appealing to evidence. That is to say, they will claim to have found out, for example, that a patient has a conflict about rivalling his father; and they will point, by way of evidential support, to the fact that, for example, he fails at, or avoids, certain typically masculine and adult achievements, and that in therapy he says this and that sort of thing, omits to say another sort of thing, seems tense on this occasion, relieved on that, and aggressive on the other. To some critics these interpretive discovery-claims seem far-fetched and ill-founded at the best of times; to others they sometimes seem uniquely plausible in their ability to construct a coherent pattern out of otherwise odd, pointless and perverse behaviour.

But nobody doubts that their empirical status, as evidentially-based discoveries of a para-scientific sort, is somehow precarious and embarrassing. It often seems, to put it midly, as though some <u>other</u> discovery-claim might equally well be constructed out of the same observational data; and indeed we are often told that therapists of different theoretical persuasions <u>do</u>, as a matter of fact, find different discoveries in the same evidence. That is to say, the Jungian finds his Persephone myths, the Adlerian his organ-inferiority, the Freudian his Oedipus-complex, and the Laingian his double-bind in one-and-the-same set of observations (cp. Marmor 1962, p. 289). But if such discoveries are allowed to be relative to a particular point of view, is not that the very antithesis of objectivity (and objectivity, we are taught to believe, is a "good thing")? Worse, however, is to come. For not only can the observational material be structured in different ways, between which it is hard to choose on an external criterion of validity, but also the material itself is contaminated by the <u>method</u> of observation. We have seen above that therapists can be expected to focus their patients' attention on different kinds of material: (a Rogerian on identity-feelings, an Adlerian on inferiority-conflicts, and so on); and even that they may selectively reinforce the production of such material without realising that they are doing so (ch.3 (c)). If, then, the claims made by therapists on the strength of such observations may be said to have any validity at all, they are valid only in a doubly relative, and therefore doubly weakened, sense: their validity is relative both to the therapist's theoretical perspective and also to his practical methods.

Now these objections are well known. What is by no means so well known, however, is what can be done about them by anyone who might wish to do something. And discussion of them has recently been revived by Farrell (1972), in a forceful and provocative manner, which although admirable as a ruthless analysis of difficulties which any scientifically sophisticated psychotherapy has to overcome, nevertheless succeeds paradoxically in exaggerating the obstacles and presenting an unnecessarily gloomy view of our capacity to overcome them.

After setting up the problem in the foregoing way, Farrell draws out two consequences, the one about the logical status of such interpretive discovery-claims, and the other about how therapeutically-generated 'evidence' might contribute to externally valid discovery-claims about human behaviour, whether in psychotherapy or outside it.

(b) Observation and distortion. Let us take the second conclusion first, and try to show that it is unnecessarily sceptical and demoralising. Farrell argues that psychotherapists, and especially psychoanalysts, tend to assume that, in order for psychotherapy to be valid in any useful sense, its theoretical rationale and its practical observations must be such as to be capable of generating discovery-claims which are true in the "objective" and "external" sense characteristic of natural science; and that whatever objections serve to undermine this assumption serve also to deprive psychotherapy of any validity whatsoever (pp.157-159). It is as if Farrell assumes that they have been brow-beaten by Eysenck's dictum that "psychoanalysis is a science ... or it is nothing (1963, p.68), and are consequently committed to defending its para-scientific honour. It suggests also that he is recommending that they ought rather to defend it along different lines; namely, by establishing it as a tertium quid which, on the one hand,is not indeed a'science' but, on the other, is "not a nothing either" (to adopt a Wittgensteinian stance). Now, however, the problmm arises of saaying what sort of facts and discoveries are being dealt with. If not those of the physical world, then perhaps those of subjective experience and phenomenology? Happily, such nebulous extravagances are not necessary, for we have seen (p.0) that Rycroft, for one, has taken as his tertium quid a kind of semantic approach. Farrell's middle way takes a different course.

Since straight-forward defences of para-scientific honour are futile (because the behavioural evidence is method-distorted, and discovery-claims about it are perspective- distorted), he lifts psychotherapy off the Eysenckian dilemma by preserving the concepts of 'discovery' and 'validity' at the expense of hedging them about with such restrictions that their force is intentionally weakened. This provides a logical imprimatur for saying, as it often seems reasonable to say, that psychotherapy can make discoveries of a sort; and that a therapist's interpretive discovery-claim may be valid in a way, or certainly that some are more valid than others. Roughly speaking, it seems to amount to a licence to translate the statement "Interpretation 'D' of evidence 'E' is valid" as meaning "Interpretation 'D' is an appropriate way of structuring† in a Kleino-Freudian therapy-situation". This may sound disappointingly weak; and yet it is not a licence to say

† in Kleino-Freudian terms these observations ('E') of what transpired

anything about anything according to taste, as cynics will sometimes suggest. Because one test of whether a particular interpretation is 'appropriate' is that it helps to "produce a coherent narrative" about a large body of data, - and there are many possible statements which would not pass this test (p. 164). It also leaves open the possibility of applying "internal" hypotheses, about how people in a Kleino-Freudian situation react to being given 'valid' interpretations of a Kleino-Freudian sort, to the admittedly contaminated data, with a view to seeing if there is any evidential support for the D.I. content of a particular interpretive move. But since evidence can in any case support empirical discovery-claims (that something factual is the case) in a variety of ways and to varying degrees, and since many empirical claims are valid, because bearing a certain relation to relevant evidential considerations without being in any narrow sense 'scientific', why do we need to operate with diluted notions of discovery and validity? Could we not in some way allow for the distorting effect of the therapist's method on the data, and of his perspective on the discovery-claims, so as to reconstruct the situation with decontaminated evidence and perspective-free claims?

Farrell does indeed compare the situation with that of an historian trying to assess the dubious evidential value of a damaged manuscript containing an evidently prejudiced report of some event (see below, p.00). Certainly, the document is some sort of evidence, but what sort? Previously, however, Farrell has entertained the possibility of "allowing for" the two sorts of distortion, but concludes that, from the point of view of current knowledge and techniques, this would be in the realm of "science fiction". To illustrate what it would logically involve, he constructs an allegory which adapts Plato's simile of the Prisoners in the Cave (pp.160-163).

> "Suppose that the prisoners ... were each supplied with a mirror and a light-emitting device for detecting the features of the floral landscape behind them. Suppose that each mirror was so made as to produce its own special type of distortion, (and) ... that in focussing light on the landscape ..., the light so focussed progressively changes the colour and growth of the flora ...; and that the devices for emitting light do not work in a uniform way. In this situation, any one observer ... would find out about the landscape with his own mirror and hence from his own perspective. He would also find out about it in a way that was dependent on his own light-emitting device".

In order to make the necessary allowances for the ways in which what he actually "sees" has been distorted by his angle of regard, his mirror and his torch, and thereby to reach "objective" and "externally true" conclusions about the flora being observed, a prisoner would need to know a good deal about the characteristics of his particular mirror and torch, and about the laws of optics and photochemistry. Now, Farrell hints that, in the case of some empirical inquiries which have to cope with the fact that their observations ~~and~~ are method-dependent, those who pursue them do know enough about their corresponding tools and background-laws to make a start at applying the necessary corrections. But when he spells out what the analogous corrective knowledge would look like in the case of psychotherapy, he argues that it would involve a great deal more information than we now have, both about how patients of a given psychological make-up interact with therapists using a particular technique and about how people in general function when not being subjected to psychotherapy. This would be especially so when we consider using observations of psychotherapeutic interactions as a source for propositions about personal interactions in general.

I want to suggest that one way in which Farrell underestimates the strength of the therapist's hand is by seeming to assume that any background-laws which he might invoke are themselves therapy-dependent: for this is what corresponds to the prisoners trying to work out the laws of optics from their contaminated visual data <u>alone</u>. But a therapist may very well be armed with all sorts of extra-therapeutic information, which would correspond to the prisoners' having independent knowledge about the pre-observation nature of their flora. For they usually have access to detailed case-histories, biographies and self descriptions for each patient; and they may even have psychodynamic descriptions and analyses based on projective tests (like TAT, ORT and Rorschach). These latter are especially relevant because they investigate to a considerable extent the same aspects of mental and emotional make-up as the therapist does, and, although their findings are reported from a conceptual 'perspective' similar to his, the 'method' of observation is very different. Their importance, in terms of the allegory, is this. Let us accept that the same sort of observations, namely evidence of particular conflicts or particular defence-mechanisms, are made <u>both</u> in the situations of therapy and projective testing <u>and</u> also in day-to-day non-clinical interactions, such as those on which Freud based his <u>Psychopathology of Everyday Life</u> (1924), and in experimental studies done as far apart as Ghana and Sweden (Kline 1966, 1969; Blum 19..). Now, this corresponds to the prisoners still seeing the same sort of shapes and colours if they substitute less, or differently, distorting mirrors and less, or differently, destructive torches; and if the same sort of images keep gropping up, even with different methods of observation, they may reasonably conclude that there must be something in what they see after all.

Moreover, given that they may thus have some sort of before-and-after observations, therapists may (and of course do) construct some sort of admittedly weak and folklore-like generalisations about how certain sorts of people respond to certain sorts of therapies and therapists, even if such generalisations are so crude that they can justify saying nothing more specific than, for instance, that this patient is not suitable for a group, or that one should be treated by a woman. They may also be entitled, when they see reactions characteristic of therapy-situations (such as dependency, regression and projection) occurring also in close, anxiety-laden dyadic relationships in everyday life,to conclude that people in general are susceptible to such 'transference' behaviour in specifiable kinds of circumstance. But it seems a persuasive objection that these primitive descriptions of base-line states, these before-and-after comparisons and this lore about the way people react in therapy, would have to be articulated, particularised and tightened up out of all recognition (and thereby transported into the realm of what is now "science-fiction") before they could have sufficient corrective power to do any significant decontamination of psychotherapeutic evidence as it now stands; at any rate, before it can do enough to allow us to claim valid discoveries in his stronger sense of "valid" and "discovery".

My suggestion, however, is that if we examine how other empirical disciplines contrive to "allow for" the contamination which sometimes creeps into their evidence, as a result of certain methods and perspectives having been used in its compilation, we shall see that they often appeal to generalisations about content and background which are no tighter than those available to psychotherapists; but that even such generalisations as these, taken in combination with other considerations (often themselves common-sensical and non-scientific), can serve to justify factual discovery-claims which are valid "beyond reasonable doubt", - in the phrase which Farrell adopts.

The metaphor of 'perspective' may be, in some cases, no more than a figurative reference to selective attention. An industrialist who manufactures indoor tennis courts may well have a different perspective on a particular tennis-match than a spectator who has wagered his life savings on the unfancied player. They will consequently notice different things about how the game goes, and give different reports; but those reports will not necessarily conflict: they may not even impinge on one another, but rather "pass each other by". In this way they would be unlike pictures of one and the same incident in the game taken simultaneously from two different camera-angles. But we do not have to say, therefore that the two stories are true only "internally" to the industrialists' or the gambler's <u>Weltanschauung</u>. On the contrary, they could both be true <u>de facto</u>, but be concerned with different facts, or (perhaps more likely) with different relationships between facts. The manufacturer sees a bad bounce as the defective reaction of a new surfacing substance to television lights; the gambler sees it as the last of many adversities which cracked his man's morale and lost him the game.

It is tempting to argue that clinicians who adopt apparently conflicting conceptual perspectives are doing the same sort of thing. They are like cartographers using different systems of topographical projection to depict the surface of the earth in two dimensions, or like Eddington wondering which of his "two tables" was the true one, the permeable swarm of colorless moving particles or the solid, static, red-brown lump of furniture. In Eddington's case, we may help to resolve his paradox by distinguishing different 'levels of discourse' and insisting that predicates which denote the macroscopic, perceptual properties of something ('solid', 'heavy', 'red-brown') carry no implications for, and therefore cannot be inconsistent with, what is said about their sub-microscopic characteristics. And yet cartography seems to provide a clear case of a representation which is "true", as it stands, only internally to a particular system, but which is nevertheless capable of yielding empirical statements that are externally valid.

In Mercator's projection, and to a given scale, it may be true that Omsk is three inches at an angle of four o'clock from Tomsk, whereas on another projection the former town is correctly placed four inches away at 3 o'clock from the latter. These two cartographical 'statements', although superficially conflicting, are of course both equally true relatively to their own projection-system: each is as true as the other in Farrell's "internal" sense. But this apparent restriction of their scope should not lead us to suppose that the statements have no general, objective, 'external' validity. For they certainly have, in the sense that, by applying the transformation-rules of their repective systmes, they can both generate, or be converted into, one-and-the-same 'externally' valid statement about the objective distance and direction on the earth's surface from Omsk to Tomsk.

In general terms, then, we may not necessarily conclude, from the fact that the immediate validity of a statement is sytem-dependent, that it is not a reliable source of independently valid descriptions of the world. To get rid of the negatives: a proposition may both be system-dependent and yet rigorously entail objectively true empirical statements.

It is one condition, however, of the Cave dilemma that the prisoners are not in possession of the relevant transformation-rules, because they can never compare 'reality' with their visual images and thus can never infer the principles for reliably converting (internally valid) statements about the latter into (externally valid) ones about the former. But in this repect the Platonic simile is too pessimistic for the situation of psychotherapy. and Farrell's extension of it underrates the therapist's resources.

For hundreds of experiments have, of course, been done in the general area of 'social perception', which yield systematic information about how observers working with different assumptions, knowledge, expectation, recent experience and so on, will tend to misperceive, misinterpret or misjudge other people's behaviour (cp. Livesley and Bromley 1973, pp. 1-71; Argyle 1967). As a consequence, people, such as schoolteachers, personnel managers and social workers, whose job involves making assessments of others on the basis of rather unstructured observation, can be made aware, albeit at a very basic level, of certain likely sources of error in their judgements. Some well-known examples are: the tendency to assume that a subject has further qualities that you approve of if you have already elicited evidence of <u>some</u> of which you approve. ('halo judgement'); the tendency to assume that certain attitudes, traits and abilities go together, so that having established that the subject is athletic, say, the observer assumes, without particular evidence, that he will also be, say, tolerant and non-authoritarian ('implicit personality theory'); the tendency for interviewers' ratings of candidates to be more favourable in proportion to the amount of talking done in the interview by the interviewer himself; the tendency for an observer who realises that he dislikes a subject, or finds him annoying, to 'overcompensate' and attach more favourable weight to a given observation than he usually would.

I do not see why psychotherapists should not be, or learn to become, at least as insightful as this, and consequently make allowances for their own <u>explicit</u> personality theories, psychopathology and counter-transferences (initially, perhaps, by identifying and counteracting their own use of 'projection'). There are after all some quite specific tactics which other clinicians regularly deploy in order to avoid making spurious observations. A psychologist, for instance, who is using a combination of 'objective' tests and clinical judgement to assess aspects of someone's mental abilities and personality, often has to anticipate or counteract the effects of 'cognitive set'. He has to recognise when the subject who does not answer an intelligence-test item is failing because he is looking for a higher-level answer than is required, rather than because he cannot see how to answer at all. The tester then says or does something that will break this 'set' without directly helping with the particular question. (This problem is regularly encountered when testing bright subjects on the 'Similarities' subtest of the Wechsler scales).

Again, if the tester moves on to projective techniques, like Rorschach or TAT, after administering cognitive tests, he may need to insist when introducing them that the subject's task is no longer to find the "right answer" and so on, but instead to describe his personal impressions of what the stimulus-material looks like, on the understanding that there is no question of right and wrong answers now. In doing this, the psychologist is making use of two admittedly vague but entirely factual generalisations: one, about the likelihood of a particular 'perspective' being transferred from one test-situation to another; the other, about the way in which such a transfer of perspective would contaminate the second set of test-data. Even more generally, when a clinician (or detective, for that matter) phrases a question in such a way as to avoid contaminating the response by prestigious suggestion, we do not need to know how a particular suggestion would have contaminated a particular repponse in order to conclude that *this* response at least was *not* so contaminated (as it otherwise might have been), because no such suggestion had been made. In this sort of way we can and do control, or at least limit the range of, possible sources of M-distortion.

But even if perspectives themselves can to a certain extent be allowdd for, it is a more sinister feature of the Farrellian Cave that the therapist's perspective is brought to bear on data which have already been distorted in unknown ways by the very method of data-collection. This seems to invite alarm and despondency, for we are perhaps inclined to think of the standard case of indeterminacy in sub-atomic physics: the properties of light-waves, we are told, are such that, if someone sets up the conditions necessary to 'observe' the location of an electron at a given moment, he is bound to disturb its directional velocity; while, if he concentrates on determining the latter, it becomes impossible to ascertain the former. As a consequence, therefore, of the nature of 'M', one can never discover *both* the position *and* the directional velocity of a given electron at a given moment. It may not be very relevant that sub-atomic theory seems to survive this epistemological deprivation, but it will certainly be instructive to see that, in the less extreme cases of some other empirical inquiries, it does seem to be possible to use judgements about how the given observational material has been contaminated, by M's and P's, in order to reconstruct what the original data must (or, sometimes, should) have been.

In a commonsensical way, we allow for unknown 'errors' of observation every time we pool the scores of one group of experimental subjects, who have done some task under one set of conditions, and compare their mean value with that of another group who worked in different conditions; or when we draw a line not through, but between, plots on a graph, on the assumption that it represents a function of which the actually-observed values are but a blurred or distorted reflection. In these cases we suppose, no doubt, that the factors which introduce distortion here will cancel each other out, over a large enough sample of subjects or time, because they are just as likely to raise a recorded value above the 'true' level as they are to reduce it; that is to say, because they are assumed to have a strictly non-systematic, or 'random', effect. But even here, it is well known that conditions which produce 'experimental error' do not always do so randomly, especially perhaps in psychophysical or psychometric studies. Those which affect perceptuomotor reaction-times, for example, are not as likely to reduce them as they are to increase them; and the same goes for the variables of a test-situation which influence a child's IQ score. Such results, therefore, are more likely to be under-estimates than over-estimates. But the point about observation-conditions in Farrell's Cave is that they are even less manageable than these, because they can be expected to distort both systematically and in an unknown (and unknowable) direction.

(c) Decontamination in other disciplines. How do we deal with the distorting effects of 'P' and 'M' in other empirical inquiries? We have already noticed Farrell's allusion to an historian's problem of assessing the evidential weight of a damaged document written by a biased reporter. In this case the M-distortion (getting information from a damaged and reconstructed report) occurs after P-distortion (prejudiced selection and description of 'the facts') has already been brought to bear on what actually happened. But here the possibility sometimes exists of neutralising M-distortion by reconstructing the text, according to principles broadly agreed by palaeographers and historians; in which case there could emerge a consensus of expert opinion about what the documentary 'evidence' was, in spite of its original M-distortion. The implication of the example is that no similar consensus could be achieved in the present state of psychotherapeutic observation. And even for the historian, the second-stage problem of what to make of the evidence, in view of its P-distortion, would still, of course, remain. Now, all this raises the question whether there is such a thing as a complete and objective account of 'the facts', entirely free from such distortions, which would be available in an historian's heaven, and to which all actual reports are more-or-less poor approximations.

So far as utterances, or movements from place to place, or numbers of troops on either side are concerned, this seems reasonable: we could (both logically and causally) have had a verbatim report of what Socrates said to his judges, instead of what Plato puts into his mouth according to surviving texts of the *Apology*. But it is doubtful whether there could, logically, be a similarly complete, objective and perspective-free account of what he was trying to do in the speech: was he 'really' seeking martyrdom, or trying a double-bluff that misfired, or inciting his followers to civil defiance? Perhaps he was 'doing' nothing so precise, or switching from one to the other; the facts themselves may be blurred or unstable, and consequently the most faithful way to represent them may be by an imaginative analogy: the therapist's standby, "It's a bit as if you were trying to ...". A full and true account of Socrates' actions would seem to require an imaginative empathy with the man himself. If such an account could in principle be given, then it would scarcely be 'objective'; but if an objective, distortion-free account is a chimera, then of <u>what</u> are our admittedly distorted reports a distortion? It is hard to resist the feeling that it is not just a question of a "neutral data-language" in which to report "the facts" non-tendentiously (cp. ch. 8, section (a)): it seems more a matter of a neutral viewpoint from which to conceptualise, significantly but non-tendentiously, what counts as what-is-the-case. After all, what Wittgenstein sought in the <u>Tractatus</u> was not an atomic <u>language</u> but "atomic <u>facts</u>".

It would seem necessary, in formulating the problem of Farrell's Cave, to assume that a corresponding distortion-free account of a patient's behaviour in psychotherapy <u>is</u> possible. But we have already met the difficulty that, as soon as you stop merely recording data and start bringing some conceptual scheme to bear on the data, you risk giving selective attention to some events, or features of events, rather than to others (Bergson's problem, p.00 above). That is, you stop measuring 'arm-movements', and start treating some of them as 'greetings' and others as 'threat-gestures'; this you start to do because the explanatory generalisations to be involved are concerned with such questions as what makes people express friendliness or hostility in particular conditions. So we are faced with the prospect of constructing an account that is both significant, for the purpose of a particular sort of explanation, and yet at the same time free from whatever pernicious aspect of conceptual processing it is that arouses Bergsonian anxiety. Accounts are clearly not to be thought of as either significant or not, in some absolute way: an economic and physiological explanation of a conversation with my bank-manager do not necessarily draw on exclusively different data, but may differ in the way they conceptualise some of the observations common to them both (cp. the "bad bounce" in the tennis match, p.   above).

If, however, we take seriously the possibility of tracking down the nature and source of distortions, and of trying to reconstruct a <u>less</u> distorted (if not entirely un-distorted) picture of what happens in psychotherapy and of what can be discovered from it, the example of the historian with his damaged text can be instructive. For it is one of many non-psychological discovery-situations where observers are faced with material distorted by 'P' and 'M', and yet manage to do something about it, in such a way that they can sometimes eventually say "This must be how it was". What sorts of thing, then, do they do?

The methods used in an archaeological excavation, for instance, may well distort the evidence, in the crudest sense of causing an object to be discovered in some place or condition <u>other</u> than that in which it 'should' have been. And, although at the time the excavator does not know which particular feature has been disturbed in what particular way, yet the majority of the evidence often falls into such a clear pattern that he can confidently conclude, retrospectively, that some inconsistent observation, such as finding a particular object in a particular stratum, must be 'wrong'; wrong, that is, in the sense that locating it in that stratum would entail attributing it to a level of occupation, or historical period, other than its true one.

He is then entitled to look for a truly *ad hoc* explanation of how the errant thing came to be in the wrong place; that is, of how that observation came to be M-distorted. Such an explanation will be strictly '*ad hoc*', in that it disposes of a particular anomaly in order to preserve a more general thesis; but it will not necessaily be so in any pejorative sense. For it may appeal to no more than well-established causal generalisations, about what sort of thing actually does happen. Sometimes, indeed, it is specifically testable. The archaeologist can sometimes look back and detect signs of the insect-burrow or slippage by which the object moved from one spot to another. And here we see, incidentally, the possibility of some loose controlling generalisations about the nature of M-distortion: gravity being what it is, misplaced objects are more likely to have fallen down from later strata to earlier, than to have been worked up from earlier to later. But even when such an hypothesis cannot in practice be checked, that deficiency may carry little weight against the knowledge that it postulates the kind of thing which could well have happened, given that people and the world have changed only so much. When the question of Stonehenge having been some kind of astronomical computer was revived in detail by Hawkins (1965), his argument depended in part on assuming that certain discrepancies, between the present-day stone-positions and the mathematical pattern which he claims they originally exemplified, are due to some of the stones having been moved out of position in the course of time. This amounts to arguing that the presently observable data reflect not just the system which generated them, but system-plus-'noise' (in the engineer's sense of that term). In which case some of the observational 'readings' have to be adjusted before the total data-pattern will represent pure system. If there is no independent way of telling which readings are noise-distorted, and therefore in need of correction, the choice of which ones are to be corrected is at the mercy of the general hypothesis about what the system is and of an *ad hoc* hypothesis about the source of the noise. The explanatory narrative which underpins a D.I. often takes precisely this form (ch.6, section (b)), but it is a difficult form to control, and to judge the effectiveness of, in a particular case.

We can keep a certain grip, to be sure, in the archaeological case, on the appropriateness of the main hypothesis and on the plausibility of the *ad hoc* mechanisms. Stonehenge just does have certain elementary properties of solar orientation (so why not more complex ones?); and stones, even large ones, just do get moved in the course of three-and-a-half millennia (so why not *this* one?). But where are the controlling generalisations about how much noise we are entitled to postulate in such a system after such a length of time? That is to ask, what value of 'signal-to-noise' do we expect, in order to judge whether Hawkins (or Freud, in the analogue' is postulating too much noise? Obviously there are no such generalisations. Some comparable systems have been totally destroyed, giving an S-N ratio of zero to infinity; others are intact, with the inverse ratio. This is why we often find it hard to tell whether a particular argument of this type is plausible or not. Some are wildly out of court, such as Cioffi's examples of pseudo-interpretation (1970, pp.490-494), and some seem inescapably cogent, such as Hoyle's revision (1966, a, b) of Hawkins' theory of Stonehenge; but there is a large class of borderline cases, in respect of which we do not, or would not, know which way to jump, because we would not know what principles to invoke.

Whether we accept, without being able to check, that a particular stone has been moved from a particular spot, depends, of course, on the strength of the grounds for thinking that such-and-such a pattern originally existed; and indeed on the proportion of 'corrected' to uncorrected observations necessary to restore the supposed pattern. Thus, if Hawkins had supposed two stones to have stayed in place, while thirty-five had been shifted randomly out of their original pattern, we should feel our credulity strained. (And yet, what about systematic shifting, with subsidiary evidence of the direction, distance and cause of shift?) So it would be also with 7 to 30, and 12 to 25; but what of 27 to 10? What leads us to regard some such reconstructions as plausible or even cogent, and others as too speculative to be taken seriously, is something to do with an intuitive balance of likelihoods (cp. ch.7 (d)).

What Hawkins' ad hoc hypothesis must not do, of course, even though it is reasonable in principle, is to make particular counter-factual assumptions. But, according to Atkinson (1966) and contrary to what was implied above for the sake of argument, there is evidence that some of the allegedly misaligned stones have not been moved as required; and this is where Hoyle's (1966 a,b) adaptation of Hawkins' theory is particularly instructive. For, on his different hypothesis about how the structure was used to take azimuthal bearings, he can explain why many stones are off-line, can predict the direction and extent of misalignment, and can provide a complete 'fit' with theory if you allow him to assume 'noise' only in that part of the system which is dubious on independent archaeological evidence. Specifically, the fit breaks down on readings involving either the uncertainly reconstructed location of a missing 'station stone' (no.94) or the doubtful marker-hole 'G' which has long been thought by some not to be a man-made feature of the structure. But Hoyle insists that he can claim statistical significance even without postulating this noise; and he makes a quaint obeisance to the totem of prediction when he assures us: "What happened was that the logic of measuring the azimuthal extreme occured to me before I worked on the data" (1966 b, p.271).

Now, there is no doubt some alignment of London chimney-stacks which points directly from Nelson's column to the sunset on Trafalgar Day. But the reason why we do not suppose that they were so positioned in order to trace this line is partly the absence of any background consideration linking chimney-pots with Trafalgar-commemoration, and partly that there are so many constructional features in the immediate vicinity that there is a strong likelihood of some such line-up having occurred by chance (if you do not specify in advance precisely what line-ups are relevant). When the repertoire of possibly relevant features is smaller, however, as when this argument is turned against the pseudo-archaeological concept of a 'leg', we again enter the no-man's-land of borderline cases. What sets the Stonehenge story far apart from chimney-pots and legs is the enormous <u>un</u>-likelihood of it happening to embody certain mathematical features if it had not been intended for certain uses. Specifically, it is wildly unlikely, if the Aubrey holes had not been meant for use as a lunar clock <u>inter alia</u>, that they should just happen to be able to generate, by the simplest arithmetical means and within an error of 0.3%, an obscure value (18.61) that would be crucial to such a use (and <u>only</u> such a use); and again, it is wildly unlikely, in the absence of such an intention, that subsidiary structures (a row of posts) should have existed of just such a kind and location as to allow the calculation of just that item in Hoyle's eclipse-predicting model which you would not know from other sources, and without which the model would be unworkable (Hoyle, 1966 a).

Such background unlikelihoods are a feature of empirical discovery-arguments in many fields of study, and we return to them below (ch.7) because they figure conspicuously in the implied rationale of many a D.I. When Freud (1924, pp. 1-6), in interpreting a famous memory-lapse, appeals to the facts that the same syllable keeps recurring in a train of associations (Bosnia, Botticelli, Boltraffio; Herzegovina, Herr), and that, if you allow a transformation from German to Italian (Herr-

Signor), which is apt for the context, you can reconstruct the forgotten name (Signorelli) and the reason for its being forgotten, he is also trading on the assumption that such 'alignments' within the data are so unlikely to occur fortuitously that the pattern must be determined. A notorious difference, however, between the Freudian and the Hoylean case is that, whereas Hoyle can specify minutely in advance what alignments and what values he needs to find, Freud can say only (rather loosely) what sort of patterns he expects, and then demonstrate post hoc that the actual data are of the required sort. This is clearly brought out by Hempel (1964, p.63) when considering whether such a parapraxis-explanation can be forced into a nomothetic mould. In the context of perspective-distortion, what matters about these judgements, of what the background likelihoods are and how they balance out, is that they follow precisely from an observer's 'perspective' and could not be made without it. Consequently this perspective, so far from hindering, actually helps him to identify and unscramble instances of M-distortion in his data.

The archaeologist, then, is able to tell that this coin or piece of pot does not really belong in the place where it was discovered, precisely because, and insofar as, he has a certain view about the nature and chronology of the site. Analogously, it may be that a therapist's perspective helps him to infer that, when patient X says 'p' in a given situation, what he was really trying to say (or not to say) was 'q'; and that therefore we shall be barking up the wrong tree if we concentrate on explaining 'p' when we ought to be trying to explain 'q'. In practice, this is a common, even stereotypic, more in interpretive strategy. 'You say that you are *afraid* of *X*; let us suppose that (in a sense) you *want* *X*, and see if that makes things fall into place'. 'You say that you are trying to *achieve* *Y*, but let's suppose you are "trying" to *avoid* it ..."; 'you say you *love* J, but let's suppose you are *ambivalent* (and consequently guilty) about him ...'; and even, as in our original example, 'you describe *my* behaviour as conventional, but let's suppose you are concerned about how conventional *your* *own* behaviour is'. In general terms, 'perspective' may help us to avoid looking for a data-pattern which 'p' fits into, when we really need one which accommodates 'q'. Consequently, we shall examine below (ch.0 (o)) a famous case of explanatory interpretation which neatly accounts for a pseudo-datum that demonstrably ought *not* to be accounted for.

To take another archaeological example, it seems inescapable that Ventris was able to crack the notorious Mycenaean script called "linear B", only because he adopted certain technical perspectives on the data. Contrary to the prevailing and fruitless assumption of scholars that the language could not be a form of Greek, he took the view that that possibility had not been adequately discredited. The other crucial angle from which he worked was that of supposing that the characters represented *syllables* rather than letters or pictures: that is, that the script was neither alphabetic, as in that of Greek proper, not ideographic, as were some associated Minoan scripts. His subsequent observation of the relevant groupings, similarities, parallels, contrasts and patterns among the data was thus highly and very specifically P-dependent, But, so far from this detracting from the objectivity and external validity of his discoveries, it would have been utterly *impossible* for someone operating <u>without</u> these perspectives (from different ones or from none at all) to 'see' the necessary characteristics of the data and make the consequential discovery that the first word of Pylos tablet P641 reads 'ti-ri-po-de' and consequently means "something with three feet", - that is, a 'tripod'. And to find this word actually paired on the tablet with a drawing of a tripod, and other such correspondences, provides a vindication, of the linguistic hypothesis, which depends upon the fact that "the odds against getting this astonishing agreement purely by chance are astronomical" (where 'astronomical' hints at unquantifiability; Chadwick 1967, p.82). This kind of knock-down pay-off is probably, however, untypical of palaeographic decipherment, and in his description of the logic of the Linear B enterprise Palmer is quite explicit that, although some of the background likelihoods are calculable, once you know the range of symbols and possible combinations, yet there are other unquantifiable questions which have to be left to the informed intuition of the experts. Thus, on one such point (how to judge the antecedent likelihood that this particular unknown language forms its plural nouns in this way rather than that) Palmer writes (1961, p.66): "Success in this vital point of grammatical procedure cannot be expressed mathematically, and we must leave its assessment to the collective common sense of the scholarly world".

The situation is sometimes complicated even further by the need for an observer to reconstruct, by means of his perspective, how a corrupt (M-distorted) text ought to read, before bringing his perspective to bear again in order to translate it. Textual critics of ancient manuscripts, and even readers of badly-written letters, regularly hav e to do this. For, although it may be the case that the writer of that particular Pylos tablet wrote down the correct characters for what he wanted to record, so that we have a linguistically sound document which can be taken at face value for translation purposes, there are other times when we want to say that the writer of a text cannot have meant to put p at some point, or ought not to have put q at another.

When a manuscript of the Aeneid has been copied from an earlier copy which was itself several copies removed from what Vergil, or his amanuensis wrote, it will contain errors. Some of them will be straight-forward scribal mistakes attributable to the machanics of copying: slips of the pen, ommissions, dittographies, false assimilation of work-endings and the rest. They make our 'observations' of what Vergil wrote very concretely M-dependent; and when they result in a reading that is evidently nonsense or non-Latin, we try to pin-point the contamination and to infer how the original, supposedly perfect, version read. But in order even to recognise these blunders for what they are, we need to have a tather precise view of what, for example, is a possible wor-form, possible grammar or possible scansion: perspective is necessary. How much more so is it needed in our corrective reconstructions.

Textual corruptions may also occur, however, as a result of 'perspectives' held by previous observers of the data. This means that the evidence of our present text is both P- and M-dependent, just as with Farrell's therapy-material. For occasionally a copyist will incorporate into his version of the text a phrase or word which was really only a marginal comment on the text, made by a previous reader, and thus accidentally introduce somebody else's 'p' into the data. But sometimes the change is not accidental, and a scribe will deliberately depart in his own copy from what is written in the version he is copying (his 'exemplar'), thinking that he is correcting a mistake, when in fact what the exemplar had was correct but unusual or obscure. In this way he contaminates the data with his own 'P'. All these contaminations, by both 'M' and 'P', are at least sometimes identifiable and reversible, and the controlling generalisations which the textual critic invokes, in his recognition of particular errors and reconstruction of an archetypal text, illuminatingly reflect the different sources of distortion.

We have seen that where there has been a failure in the sheer mechanics of copying, like letter-substitutions, gaps, repetitions and homoioteleuta, we are dealing with what Maas (1958, p.15) calls "scribal blunders" which "normally produce obvious nonsense". Deciding that something has gone wrong depends on knowing what does 'make sense', in terms of word-forms, grammar, scansion and the rest, as well as on assuming that Vergil's archetype did not contain such nonsense. (This is not an entirely trivial assumption, because it did of course contain at least one sort of anomaly, - that of incomplete lines). Reconstruction of what has gone wrong, and consequently of how the text should read, draws on generalisations about what letter-confusions actually occur in various styles of script, and about what sorts of eye-slips are likely. Knowledge of M-conditions can be used to counteract M-dependent distortion.

The trouble, however, with P-dependent distortions, as when the scribe unwittingly incorporates a gloss, or knowingly 'corrects' a puzzling reading in his exemplar, is that they will most often produce perfectly good sense. It may be only when it is possible to compare two MS witnesses, one of which lacks the interpolation or the pseudo-correction, that the contamination can be recognised. The more economical text shows up the corrupt one as repetitious or pleonastic. And yet perhaps the more concise reading owes its economy to omission; that is, to a merely M-dependent scribal blunder. We are tacitly assuming that a mechanical lacuna is unlikely to correspond, just by chance, to a syntactic unit, in such a way that a possible sentence-structure is preserved. This throws into relief the question what exactly the controlling assumptions are, and what they are based on. Obviously the textual critic cannot conjecture that *any* instance of repetitiousness, pleonasm or hendiadys reflects a corruption: because some writers just *are* repetitious, pleonastic and prone to hendiadys. Thus the critic is clearly not armed with the tight, exclusive generalisation 'All A's are B'. As Maas puts it (*ibid*.), "in texts where such an interpolation has been demonstrated, much becomes suspect simply because it appears to be superfluous ... And yet there is undoubtedly superfluous ... matter in every original."

Nevertheless, the critic is driven to consider, especially in the case of the second source of corruption, what kinds of mistake are most likely to occur on what Maas calls "psychological grounds": that is, the balance of likelihoods mentioned above. This is evident in the traditional rule of thumb for deciding which is likely to be the more accurate copy, when one 'witness' has an unusual or puzzling reading at a particular place while another has a straightforward one (say, different forms of the same verb or noun). 'The harder reading takes preference', decrees stemmatic folklore: *praestat lectio difficilior.* But why? Because a scribe is more likely to treat an unusual expression as a mistake, and put something obvious in its place (thus trivialising the expression), than he is either to have wilfully obscured a straightforward exemplar (P-dependent error) or to have substituted a difficult but possible reading by chance as a result of a merely mechanical slip (M-dependency).

It seems, then, that there is a range of empirical situations where observations and reports become contaminated by observation-methods ('M') and observer-perspective ('P'), but where such contamination can nevertheless sometimes be identified, and some quasi-archetypal account of the data be reconstructed. Accordingly, if we regard a therapist's observations, of successive behaviour-samples from his patient, as different (but not independent) 'witnesses' to a behavioural, rather than textual, tradition, there may be greater scope than Farrell suggests for detecting, and making allowance for, such distortion as 'M' and 'P' introduce into the data. And there is one final variation, on this theme of text-restoration, which can be mentioned instructively here, even though it is not played until later: even the archetypal text may itself be wrong.

We noticed above (p.00) that a therapist trying to interpret his clinical material needs to recognise that the most 'objective' account of what a patient said and did may be misleading. Although the patient actually says or does 'p', he <u>should</u> (in the absence of displacement, denial, reaction-formation etc.) have said or done 'q', in the sense that 'q' would have expressed what was really going on in his mind. In interpreting a slip-of-the-tongue or parapraxis, the relevant 'data' are as much (or more) what was <u>not</u> thereby said or done as what was. The same happens with texts too. Sometimes we know how the archetype reads, and yet have good grounds for concluding that it should read otherwise. We do this sort of thing every day when we silently correct for ourselves misprints in a newspaper; and it is done rather more technically when, for example, a musicologist decides that what Beethoven actually wrote in his original manuscript of the <u>Hammerklavier</u> sonata or the <u>Diabelli</u> variations must have been a mistake (cp. ch.8(c)). But the nature of the assumptions and generalisations which constitute the grounds for these conclusions (that is, conclusions which look behind the distorted datum to the pristine intention) will be examined at a later stage in the argument (ch.7 (c)).

(d) <u>The nature of psychotherapeutic 'discovery'</u>. For the moment it is enough to have shown, by appeal to analogous problems about archaeological and linguistic evidence, that observations from psychotherapy do not necessarily have to be rejected, as a potential souce of objective discovery, simply because they are subject to contamination from the therapist's methods and perspectives. Given that these defects can at least sometimes be overcome in other empirical disciplines, our attention is focussed on two questions: what sort of discovery-claim does our interpretation purport to make, and how are such claims related logically to the evidence on which they are based? In respect of the former, the argument will be that the discovery has more to do with structure and relations than with objects and events; as to the latter, I emphasise the role of analogical patterns rather than that of causal generalisations. In order, therefore, not to lose sight of the fact that the peculiar mode of understanding which psychodynamic interpretation generates depends upon claiming to demonstrate significant relationships between features of a person's behaviour, we shall find a convenient fusion of ideas in the notion of 'explanatory discovery'.

We noted above (p.00), however, that the first of Farrell's main contentions is precisely that such explanatory discoveries as a therapist purports to make about his patients are often 'about' them in only a curiously indirect way: a way, indeed, such as virtually disqualifies them from being either explanatory or discoveries. It also seems to involve casting doubt on the insightfulness, and even almost on the good faith, of the interpreting therapist. For Farrell argues that such interpretive claims, do not "function primarily ... as descriptive statements to state hypotheses". On the contrary, "their primary function is to do things such as orient the therapist himself in respect of the current situation; reassure him that 'he knows what is going on' at the time, and so help him to feel secure and in command of the situation" (p.157 f.). This is all rather alarming and alarmist. It suggests that the therapist is doing, either knowingly or negligently, something different from what he claims to be doing; and it thus (unintentionally, no doubt) makes him out as some sort of impostor who says what he does say in order to camouflage his own ignorance, and to make himself, not the patient, feel better. No doubt Farrell does not mean to appear quite so cynical. But equally he does not intend merely to make the innocuous point that some interpretive statements provide a preliminary structuring of the data which can generate hypotheses to be checked later on; because even the interpretations advanced after such hypothesis-checking would still be said to serve "primarily" the same sort of purpose and thus to have the same logical character. It is not like a code-breaker assuming that the code he is faced with is type 'T' simply in order to have something to work on, and some reason for trying one technique before another (rather than vacillating between this approach and that). For if his interpretive claims are primarily self-directed in this important way, the therapist, unlike the cryptologist, never reaches the stage of being able to say 'So it really was that type after all'. Like Man in Pope's poem, he "never is, but always to be blessed"!

But perhaps we are wrong to take umbrage at the idea of a therapist doing something 'for' himself, and to assume that if he is doing it for himself he is not doing it 'for' the patient as well. If I complain to my doctor of feeling ill, he may elicit all sorts of behavioural evidence from me and then announce, what is undoubtedly a 'discovery-claim', "You've got a touch of gout". Well now, let us ask 'For whose benefit is this assertion made; whom does it help?' Evidently not me: I do not find it at all reassuring. Who then? Why, my doctor, of course! He's the one who now feels better, feels "in control", feels "he knows what's going on"; because now he knows what treatment to give whereas when I first walked into his surgery he did not.

It would not be unduly Pickwickian, then, to contend that factual, objective, medical diagnosis-statements are made, in an important sense, "primarily" for the benefit of the doctor. But saying this does not imply that they are not also made 'for' and 'about' the patient. The constrast which Farrell draws therefore turns out to be less startling than it seems at first. And in any case, if interpretive claims can usefully structure and organise the behavioural data for the therapist (without being intended as categorical descriptions of what is actually the case), it is hard to see why they should not do the same thing for the aptient himself. The need for structure (whether that structure is "valid" or not) is not confined to therapists; and the view that interpretations function mainly, if not merely, to offer the patient a new, potentially helpful, 'angle' on certain of his feelings, attitudes, incapacities and reactions is, after all, widely canvassed, as we have seen above (ch. 3, passim.). And if interpretive claims are stripped of their pretensions to objectivity, and credited only with a kind of tactical structuring role, it is not clear why the "pragmatic" force with which they are left should be thought to serve primarily one party to the therapeutic dyad rather than the other, when both parties are trying, for different reasons, to get a conceptual grip on the behaviour in question.

We have seen reasons above, however, for rejecting an exclusively, or even mainly, tactical view of TI (let alone of DI), and for insisting on their categorical, structure-depicting function (ch. , section). But there is a danger that this idea of 'explanatory discovery', which we shall relate to that of the 'psychoanalytic narrative' (Sherwood 1969, ch.6), may be too glib a fusion of problematical concepts, (namely 'explanation' and 'discovery'), and may consequently beg more questions than it illuminates. We must accordingly inquire now into some aspects of how explanations of events and behaviour are ordinarily constructed, and into the variety of ways in which evidence is ordinarily used to substantiate matter-of-fact discoveries. That is the very general brief for Part Two.

Part Two

Aspects of Understanding and Confirmation

CHAPTER V

Patterns of Explanation

(a) The hypothetico-deductive paradigm
(b) An historical paradigm
(c) Analogues and structural relations

(a) The hypothetico-deductive paradigm. Discussion of the rational and empirical status of PDI tends to run up against, among other things, some conventional assumptions in the philosophy of science. We have just looked at the way questions about observation and objectivity are raised by the nature of the empirical material involved. Now we must consider some objections which spring from views about how, logically speaking, any matters-of-fact are in principle to be explained, once 'the facts' have been established. For some time now, there has been a two-fold tradition about the implied logical structure of such explanations. And the exclusive validity of this tradition has come widely to be taken for granted by commentators who wish to insist on the 'scientific' status of methods and theories in psychology.

One aspect of this tradition is the so-called 'covering-law' theory of explanation. This depends on the thesis that an event, or factual state-of-affairs, is explained by showing that a statement reporting that particular event (etc.) is strictly entailed by the combination of (a) a statement (or statements) of some law-like empirical generalisation(s) with (b) statements about particular conditions, properties etc. exemplified in what is to-be-explained (the explanandum). In its simplest, paradigmatic form, this consists of constructing a deductive syllogism which assigns the individual, whose property or behaviour is to be explained, into some class whose members invariably show that property or behaviour. Problem: 'Why do sections of railway-track get longer in summer?' Explanation: '(a) Metals expand when heated; (b) (i) railway-track sections are metal, and (ii) in summer they are heated by the sun'. Thus the problematical behaviour of the track getting longer in a certain circumstance (summer) is resolved by showing that the track belongs to, can be subsumed under, a class of objects (metals); that the particular circumstance can be subsumed under a class of circumstances (rises in temperature); and that there is a general law linking these two classes.

Of course, you may go on asking for more and more minute explanations, - 'why does this metal expand more than that ...?'; or for more and more general ones, - 'why do metals expand at all ...?' But the argument is that the logical form of the answers would be the same. And the level of generality or specificity at which one's puzzlement stops is logically arbitrary, but determined in practice by the purpose of one's initial request for explanation. Of course also, not all the logical ingredients of an explanation are spelled out in a particular instance; in practice, indeed, probably most instances are in this way elliptical. So that, when the crucial generalisation itself is suppressed and taken as read, reference to a particular fact (condition, event, property, etc.) may seem to carry the explanation on its own shoulders. For instance, depending on what aspect of the situation I think you are puzzled or uninformed about, I may explain why Smith is having fish for lunch either by reminding you that today is Friday or by telling you that Smith is a strict Roman Catholic. And the suppressed, but logically crucial, premise may impinge on the *explanandum* in more than one way.

Thus, to adapt a deliberately many-sided example from Austin: Problem, 'Explain how can you tell that that is a bittern'; explanation, 'I was brought up in the Fens' (cp. Austin 19.., p.00). Here the suppressed empirical generalisation is obviously something like, 'People brought up in the Fens can recognise bitterns', though it would perhaps be nearer the mark to insist that it should read, '... can *be expected to be able* to recognise bitterns'. In which case we introduce a new aspect of the business, which should serve at least to remind us that it is not be cut and dried so easily (see Section (b) below). Yet further complications are raised, of course, by another of Austin's explanatory answers to the same question: 'Well, I heard it booming'. A range of less problematical examples of elliptical explanation has recently been reviewed in a psychodynamic context by Sherwood (1969, pp. 7-22). But even if this 'covering-law' story fails to cover positively *all* empirical explanation, *pace* those philosophers who seem to urge that it need not fail (Hempel 1965, pp. 412-425; D.M. Taylor 1970, *passim*); it could still be held naturally to characterise all truly 'scientific' explanations, and it is to Popper (1935) especially, that we owe the popularisation of this view.

(ii) The other main feature of the tradition concerns the grounds on which the required law-like generalisations rest. Since no number of confirmatory instances will conform the universality of an affirmative generalisation (because you can never know that there is not a disconfirmatory instance just round the corner), and since a single disconfirmatory observation is enough to refute its universality, two consequences follow: one, that such generalisations are established not directly, by being confirmed, but indirectly by resisting refutation; the other, that the evidential weight of scientific observation is essentially refutatory. Given, then, that empirical observation is necessarily both specific (dealing with instances, not generalities) and negative, the general hypothesis which is being examined by a particular program of observation will have to be cast in such a form that specific and negative observation has some general and positive force. To cut the story short, this requirement has led to hypothetico-deductive methodology, with its emphasis on testable prediction and on crucial experiment directed at a 'null hypothesis'. If you wish to test the law-like thesis that 'All A's are B', you draw off some 'prediction' (P) or consequence such that, if it were false that all A's are B, you would not observe P in specified conditions x, y, z; this P usually being not a separate event but a statistically significant value (or difference between values) of some experimental parameter. You then set up these crucial conditions, or look around till you find them instantiated, and proceed to show that P is to be observed. Thus you have converted the general to the particular by inferring from it a specific hypothesis which would hold if, and only if, the generalisation were true; and you have formulated that hypothesis in such a negative way that it is borne out when this negative form is refuted by particular observation.

The rationale of this engaging methodological charade is being called into question more openly nowadays (Cohen 1970, Harré 1970, Harré and Secord 1972), and some of the considerations relevant to our purpose will be noticed below (pp. 00-00), and ch.6 passim); but it is fair to say that commentators who are concerned for the 'scientific' status of psychological inquiries and theories still tend to assume that strict generalisations, deductive argument, and observationally refutable predictions or implications must characterise a respectable empirical explanation and the investigatory procedure behind it.

Critics of psychodynamic accounts of behaviour often argue that such accounts simply do not meet these criteria, and therefore "there's and end on't". Thus Eysenck has asserted that if the propositions of psychoanalysis are intended to be about matters of fact, then either they are "subject to the usual dictates of scientific argument and scientific evidence" or they are nothing (1963, p.68). For him there is only one sort of argument about matters of fact, that is "a scientific " one. One part of his ensuing argument is to claim that, in point of fact, those methods of psychotherapy which are deducible from the theory just do not work (refutation of prediction); another is to complain that propositions about symbolic meaning, which are essential to much of the theory and which give it much of its apparent explanatory power, are too loose and elastic to be able to generate precise statements about what observations to expect in specified conditions, and hence fail to meet the requirement of refutability. Elsewhere Eysenck makes it clear that he supposes the one-and-only paradigm of scientific methodology to be hypothetico-deductive, so that the absence from psychoanalysis of tight generalisations and testable predictions renders it simply "unscientific", - which leaves it as no more than some sort of religion or myth (1953, esp. p. 241). More recently he has gone so far as to identify explanation, "in a scientific sense", with hypothetico-deductive predictivity (1970, p.408). An associated way in which psychodynamic explanations fail to meet hypothetico-deductive criteria is by arguing back from present behaviour to inferred causes, on the basis only of loose, unquantified generalisations, according to an essentially fallacious logical scheme (1957, p.247 S& N); but this question is taken up below (ch.7).

However, the view that psychodynamic theory is useless as a basis for empirical explanation because it is intrinsically untestable and hence irrefutable, runs into a conspicious difficulty which has been remarked on before (Cheshire 1973 a). It is simply, that very many 'scientific' efforts have in fact been made to test or refute it. Eysenck himself once admitted that there is an "experimental literature dealing with psychoanalytic concepts", which, in the interests of consistency, he ought to regard as systematically misguided and irrelevant. Instead, he summarised its import as showing that "for every hypothesis supported there are at least two where the evidence is doubtful or clearly contrary to expectation"; and this proportion he descrived as "by no means a bad average as scientific hypotheses go" (1953, p.232). More recently, the now greatly expanded literature has been surveyed by Kline (1972), whose conclusions in their turn have been criticised by Eysenck & Wilson (1973). Kline not only comments illuminatingly on which parts of this "premature synthesis" of hypotheses have survived best (and which not at all), but also insists that, although there must be some relatively clear-cut empirical core to the 'theory', it is nevertheless of a kind which requires rather more subtlety in its evaluation than the demand for concrete predicitions at which 'the evidence' is to be thrown. Not only do facts not speak for themselves; but when they do speak it is not necessarily by addressing themselves to predictions.

On the other hand, Cioffi (1970) brings the charge that much of this apparent refutability is illusory, because it is in the nature of psychoanalytic methods, as well as concepts, deliberately to evade independent empirical check. Some hypothses (e.g. about the relation between infantile sexuality and adult neurosis) seem specific and concrete enough; but, as soon as there is a whiff of apparently contradictory evidence, they are adjusted to become more vague, more nebulous, more elusive and more tentative. Thus they "lead a double life", and contribute to what is necessarily a "pseudo-science". It is not easy to see, however, how Cioffi establishes that this tendency, if admitted, is in the nature of the theory itself rather than in the human nature of the theory's exponents. He can compile, to be sure, an alarming rogue's gallery of examples showing that Freud and others were guilty of this practice from time to time. But it is worth remembering, in order to preserve a certain balance, that Freud himself undoubtably propounded his theories _as_ a science, and specifically recognised the need for refutability. Thus he explicitly contrasted his own "illusions" with those of religion, for example, in respect of their checkability (1927, p.51).

> "... I hold fast to one distinction. Apart from the fact that no penalty is imposed for not sharing them, my illusions are not ... incapable of correction. If experience should show ... that we have been mistaken, we will give up our expectations".

There are particular occasions, indeed, when he does announce that his subsequent observations have compelled him to change his ideas and "expectations": a major instance is his restriction of the 'pleasure-principle' in the face of the need to account for 'repetition-compulsion' (1920; and cp. 1933, pp.494, 566-572; 1915, p.263). He may still, of course, have failed _in general_ (whether through incompetence, negligence, or self-deception) to formulate his ideas in such a way as actually to allow of empirical check. And it could be argued that the considerations on which he bases his incidental theory-modification are not necessarily the right ones; or, if they are, that they are not invoked consistently throughout the theory. But we should not lose sight, on the other hand, of the fact that what is propounded _as_ a science may be useful as something else. After all, ECT was advanced as a cure for schizophrenia, but turned out to be good for endogenous depression. And even though Faraday, according to Maxwell, represented electromagnetism as being mediated by "elastic cords of ether", which was a mistaken, not to say absurd (as we might think(, representation, this does not lead us to dismiss the concept as a last-ditch botch-up of an unscientific myth when it is understood in a <u>different</u> way by subsequent workers (cp. Toulmin & Goodfield 1962, pp.287-293; Hall & Hall 1964, p.297).

So long, however, as one practical implication has been strictly inferred from the theoretical corpus and put to systematic observational test, as in the six-hundred-or-so investigations reviewed by Kline, it cannot be the case that all elements in the synthesis lead an irredeemably "double" life all the time. Against this, Cioffi makes two moves. He affirms, first, that refutability is only a necessary and not a sufficient condition for genuine science; a view which conflicts, incidentally, with Eysenck's assertion that a particular "sentence" is "scientific" because it is amenable to falsification (1970, p.409). Secondly, he seems to contend that, since certain basic concepts, axioms and practices which typify, or define the identity of, psychoanalysis are defective in the way suggested, then the whole superstructure must be scientifically bogus.

A central, and identity-determining, feature of psychoanalytic accounts of behaviour is that they rely directly or indirectly on the concept of 'interpretation'; and since this operation is essentially "allusive" (rather than causal-predictive), and therefore "illusory", the explanatory system which depends on it must also be scientifically bogus (Cioffi 1970, p.473). The phrase in brackets, however, represents a gloss on Cioffi's argument, for he does not clarify what precisely he thinks is necessarily wrong with allusive explanations. He produces some "blatantly spurious" instances from other contexts (pyramidology, and Dante interpreting the significance of the date of Beatrice's death), which consist merely in showing that there is some quantitative relation between the numerical values of certain given events or situations. And he implies, by reference to Pareto's analogy (p.491), that D.I. also takes the defective form, in principle, of tracing or constructing a "route" between two given points (that is, between present behaviour and allegedly determining event, motive, conflict) in the absence of any check as to whether that route was actually taken, or even as to whether it is a route at all.

This may be a fair representation of the undertaking in Freud's study of Leonardo da Vinci, to which Cioffi appeals; and yet,-if it were all that Freudian D.I. ever does or seems to do (as it is all that pyramidology ever does), then Freud's "interpretive transactions" would not have made such a mark as they have. For what makes them of interest, evidently, are the occasions when the allusive steps seem to lead from one given point (observed behaviour) to another one (psychic event, etc.) whose existence and location were previously unknown to the interpreter, and even sometimes to the subject; and when confirmation of the latter point, for example by the subject's admission or recollection, seems to confirm the validity of the steps taken. This is the form exemplified by the D.I. of parapraxes, slips of the tongue, misrememberings and so on, many of which appear to constitute remarkable explanatory discoveries on Freud's part (Freud 1917, pp. 24-78; 1924 passim).

But let us return, with Cioffi, to the weaker sort. Nobody doubts the difficulty of setting up general empirical criteria for assessing the validity of a D.I.; and the fact that one psychic conflict may find expression in a variety of actions, while one action may be the expression of either this, that or the other conflict, leads the sceptic to complain that there is no given behaviour which could not be linked in a (speciously) system-supporting way to a given psychic determinant. If the law-like generalisations of the system link almost anything with almost anything then, the argument goes, they (almost) cease to be law-like at all; when the same set of premises can generate the conclusion 'Therefore not-*x*' as an *explanandum*, just as strictly as it can 'Therefore *x*', explanatory power collapses. When a *D.I.* trades on the claim that behaviour *B* alludes to conflict *C*, the 'laws' invoked to establish the connection are so elastic that *any* behaviour (even 'not-B') could lawfully be seen as an 'allusion' to *C*; thus the *D.I.* produces only an "illusion of intelligibility" with respect to this particular *B* (Cioffi *op. cit.*, p.000).

Now it is a mistake, as I have pointed out before (Cheshire 1964) and shall elaborate below, (Ch.7), to suppose that law-like statements which are loose, elastic or even mutually contradictory, necessarily suffer from explanatory impotence: it depends how, and in conjunction with what, they are used. If Cioffi were to run out a formal syllogism explaining how the English version of his extract from the *Vita Nuova* gets to be what it is, he would sooner or later have to use a general premise of the form, 'The word *W* sometimes means *a*, sometimes *b*, and even sometimes *n*'; and indeed also, '*W* sometimes means *p* and sometimes not-*p*'. But his account can still be rationally coherent and empirically valid in spite of this. So it is not only Freud's 'primary process', the thought-language of the unconscious, which can waive the principle of contradiction and allow that 'not-y' as well as 'y' may proceed from 'x'. Nor does this semantic licence necessarily lead, as Cioffi suggests, to epistemological anarchy and illusory understanding.

Another mistake, often made by critics eager to make the charge of explanatory impotence stick, is to exaggerate the degree of elasticity displayed by the law-like statements actually, or necessarily, used in psychodynamic theory. For there is a tendency for such critics to move from the observation that Freud links, for e example, both under-restrictive and over-restrictive patients with the d evelopment of a sever superego in the child, to the conclusion that the implicit theory about superego-development must contain the (explanatorily impotent) generalisation that "if a child develops a sadistic superego, either he had a harsh and punitive father or he did not", (op. cit., p.485). This conclusion, however, simply does not follow from the premises. The premises are, schematically: (i) high values of x (paternal punitivity) produces y (severe superego); (ii) low values of x also produce y. The g eneralisation necessry to cover these two hypotheses is the limited one, 'Extreme values of x produce y'; and not 'Any value of x produces y'. The latter (false) inference is, of course, consistent with the given premises, in the sense that it also succeeds in including them, but it is by no means necessary in order to do so. Converting this back to the example, it does not follow, from what Freud ways, that 'middlingly restrictive parents produce ...'.

It is certainly important, in order to make the stony about extreme values of punitivity refutable (and thus potentially 'scientific'), that Freud should set up antecedent criteria for identifying these extreme values in particular cases. But even in the absence of these, the story in itself is not incoherent. Indeed, we regularly use, in everyday empirical explanations, exactly parallel ~~(ambivalent)~~ generalisations, which are capable of jumping both ways, as it were, and which might consequently be styled 'ambivalent'. Consider the generalisation expressing what lies behind the fact that my car starts badly *both* when the engine is cold *and* when the engine is hot. It is not that '(some) cars start badly all the time" (i.e. at *any* engine temperature): for in *that* case, the proposition 'The engine is cold/hot now' would have no explanatory force in a particular case. The reason is that '(some) cars start badly at *extremes* of engine temperature'; and that is why *both* "Well, the engine is cold now" *and* "Well, the engine is (too) hot now" can serve as explanations of poor starting on different occasions.

A more important problem with this allusive route-planning which is said to characterise *D.I.* when it purports to show that behaviour *B* arises from conflict (etc.) *C*, is that in eliciting the intermediate stages and linking them together, the implied argument is something like: *C* gives rise to *x*, *x* is connected *y*, *y* is expressed in *z* and *B* is a form of *z*; so there you are. The trouble here is that the linking-relations, between the elements in the storyoor the stages on the route (*x*, *y* and *z*), are of different *sorts*, and Cioffi usefully reminds us of the variety of metaphors in which Freud speaks of these postulated links. Some allude to causal conditions and consequences, some to forms of 'expression', some to intentions and purposes, some to various kinds of sign-significate and type-token relationships. This can be seen in miniature in one aspect of the Leonardo analysis (Freud 19...).

The proposition towards which we are trying to find a "route" in the sense of establishing a chain of inference which generates it from sparse data, is that Leonardo had a pathogenically intense relation with his mother. One supposed datum is that he was separated from her after a period of exclusive possession in the absence of a father; another is the (bogus) references to a "vulture" in the memory of a childhood fantasy and in a crucial painting. From the former datum Freud moves directly to the goal-proposition, on the strength of the semi-technical causal claim 'Such a situation regularly produces such an outcome'. But the route from the latter data passes through such diverse claims as 'Childhood recollections and concealed figures have pathognomic significance', and 'In Egyptian mythology, the vulture is a mother-symbol' (cp. Farrell 1963).

This heterogeneity complicates the question of observational refutation a great deal. For what sort of observation, or evidential consideration, is relevant to assessing the validity, variously, of '_x_ produces _y_,' '_y_ is a sign of _x_', '_y_ is a form of _x_','x includes _y_', _x_ is the opposite of _y_', and '_x_ is expressed by _y_'? And worse: what is the compound status, as it were, of the synoptic proposition which results from such heterogeneous links, namely that 'Behaviour _B_ proceeds from (etc.) conflict _C_'? We need the answer to this question before being able to tell which of 'the facts' or what kind of 'evidence' to invoke in support or refutation (see Ch. 6). Small wonder that Freud, seeing the difficulty, or (if you will) wishing to throw up a smokescreen, was reduced to referring rather indeterminately to the "structure" of mental life and behaviour. We shall try, in Part III, to clearssome of the smoke from this metaphor.

We noticed above (p. 00) that the doctrine that all scientific explanation has a hypothetico-deductive framework is not accepted in contemporary philosophy of science as unquestioningly as Eysenck, for example, is wont to assert (1953, 1963, 1970). Nor is this simply because the advocates of a soft-headed, "humanistic" <u>Geisteswissenschaft</u> are being given more space, in contrast to those who favour a tough-minded <u>Naturwissenschaft</u> (cp. Eysenck 1963, p.67). It is rather because such crude disjunctive contrasts are themselves coming to be recognised, in this field as elsewhere in philosophy, as at best misleadingly clumsy and at worst corruptingly false. A number of confused and overlapping disjunctions of similar crudeness contribute to the more general contrast: either a theory or practice is 'scientific' or it is not; no science without measurement; no 'scientific' explanation without prediction; 'scientific' theories are testable by observation; either a proposition is about a matter of fact or it is not; either a proposition is testable by 'evidence' or it is not; and so on.

In his analysis of the "myth of deductivism", of which such disjunctions are symptoms, Harré distinguishes three broad assumptions which can be seen to underlie it and to give rise to particular contributory superstitions such as these (1970, pp. 3-29). The broad assumptions are: (i) that propositions are the only vehicles of thought; (ii) that scientific theories are like mathematical proofs; and (iii) that the objects of empirical, natural knowledge are events and their contingencies. Not all the arguments with which Harré disputes each of these logical substrates concern us here. But, whatever its origins, the mythical status of the view that deductivism provides the exclusive logical imprimatur for scientific explanation and theorising can be exposed on both internal and external grounds.

Internally, it can be shown to lead to a number of paradoxes, artificial contrasts and counterintuitive dogmatisms, concerning explanation and confirmation (op.cit. ch. 1. esp. p.29). For the apologist, these are problems to be solved by dialectical ingenuity (cp. Ayer 1972, pp. 54-88), even at the expense of what amounts to the redefinition of what is supposedly being explicated (Hempel 1965, pp. 247-251). ~~Harre op. cit. p.18).~~ For Harré, they are reductions to absurdity of an untenable scholasticism which needs to be replaced: to be replaced by a view that should be derived more closely from the varied realities of scientific argument and theory-construction, and from a more representative range of scientific contexts.

The external grounds, to which we draw attention below (section (c)), also have both a negative and a positive aspect. On the one hand, we shall see, negatively, that not all the explanation-generating observations and formulations of the paradigmatic 'hard' sciences do, as a matter of fact, exemplify a hypothetico-deductive scheme; and on the other, that some such observations and formulations are, positively, "allusive" in character (pace Cioffi), in the sense that they draw their explanatory force from analogy. This last point has been stressed by other critics also; such as Toulmin (1958), Hanson (1958) and Hesse (1966). Our next step, however, is to outline an approach to empirical explanation which has been both contrasted with and assimilated to the 'covering-law' story.

(b) An historical paradigm. When historians make discoveries about matters of fact, and use them to explain some pattern of events or actions in the past, the accounts they produce may often be true, evidentially-based inferences about the physical world. And yet, if we were to reduce these successful explanatory accounts to syllogistic form, they would rarely if ever include the universal law-like generalisation ('In conditions C, people always do X'; or 'when Z occurs, B always follows') necessary to guarantee deductively a specific explanandum, and thus to 'explain' conclusively why Napoleon did 'X' in a particular situation, or why 'B' happened after 'A' on a particular occasion. No matter, argues Hempel (1964): the deductive paradigm also covers the case where an incomplete or statistical 'law' gives a high degree of credibility, or (confusingly) "inductive probability", to an explanatory argument. Thus we can derive the likelihood of Brown's hay-fever attack subsiding within ten minutes of taking a certain dosage of a certain drug, from the combination of the major premise 'Most hay-fever attacks subside ...' with the minor premises 'Brown had a hay-fever attack' and 'Brown took such and such a dosage ...'.

But the ostensible preservation of the deductive paradigm is specious on two counts. Firstly, what we want to explain is not the general likelihood of Brown recovering, but his actual recovery at a particular time. And secondly, the paradigm cannot be adapted without running into absurdity. For it is not the explanandum which needs to have "a degree of rational credibility" conferred upon it (as Hempel puts it, p.60), but the explanation. We do not need grounds for believing what we have actually observed, namely that Brown recovered; what we need grounds for is the belief that he recovered because of this and that. This much, however, presumably does follow: whatever logical framework is held to justify the scientist in constructing explanations from merely statistical law-like premises is available in principle also to the historian working from the generalisation that 'In condition C, most people do X'. Indeed, historical discoveries or observations often seem to be used to show that the relevant "conditions" were these and these (rather than those and those), or that this (hitherto ignored) condition also obtained, with the result that Napoleon's doing 'X' or the occurrence of 'B', which was previously puzzling, becomes understandable (cp. Gardiner 1952, pp. 65-112). But it is an odd sort of explanation of Napoleon's action, in a way, to show that it is, statisticallysspeaking, what anyone would do. The spectre of truism seems, for example to Scriven (19), to lurk not far round the corner.

A rather more perceptive approach, which brings us closer to our psychodynamic theme by involving aspects of 'intentionality' and "the operation called Verstehen" (Abel 1948), is the elaboration of a covering -law paradigm made by Dray (1957) and to some extent Walsh (1967). The idea is that, since human behaviour is not just like the reaction of a billiard-ball to impact but is often conceived and executed in such a way as to bring about an intended consequence out of a perceived situation, these features should be built into the 'covering-laws'. An historical action is to be explained, on this view, by showing that it was a reasonable thing, or the appropriate thing, for someone in those circumstances and with those aims to do. It takes for granted that the agent knows (or thinks he knows) what his circumstances are, and understands what kind of action is likely to bring about his purpose. It also presupposes that an observer can have enough empathic understanding of human nature (Verstehen), as opposed to adequate statistical records, to be able to see that a given action is likely (or would be thought likely) to produce the required results in given circumstances. Dray's paradigm, somewhat elaborated, for explaining action X of person P, accordingly runs:

1. In a situation of type C, the appropriate thing, for someone with motive M, to do is X
2. (a) P was in situation type C
   (b) P had motive M
3. Therefore P did X

Now, apart from anything else (and there is plenty else), even if we admit the premises, they still do not of themselves generate the required conclusion (3). A third minor premise, 2 (c), is needed to link the particular agent P with "the appropriate" or reasonable thing-to-do. For, in the absence of 'P is a reasonable man', as 2 (c), we have no ground for expecting him to do what is in fact appropriate to his circumstances and motives (hempel 1964, p.74). But this addition would indeed suffice to force the explanatory enterprise into the "nomological" mould of a "covering law" scheme. Consequently the historian's investigation and deployment of evidence could be seen as directed in principle towards establishing, for example, that the relevant circumstances of some action or trend were $C_{b....e}$, rather than $C_{a...d}$ as previously assumed, or towards showing that the protagonist's main motives were $M_{a,d}$, rather than $M_{b,c}$; and all this to the end that an otherwise puzzling action turns out to be recognisable as 'reasonable' and 'appropriate'.

This approved framework, however, can support a realistic argument only at the expense of further modification of the propositional content, in a direction which takes it a long way away from descriptions of particular event-complexes. It is a direction which leads towards the thorny territory of the subjective perception and understanding of actions, as opposed to the movements or noises of bodies. The issue is given away in Dray's phrase, "When in a situation of <u>type</u> $\underline{C}_1$, $\underline{C}_2 \ldots \underline{C}_n$"; and in Hempel's, "the empirical circumstances, <u>as seen</u> by the agent" (<u>op</u>. <u>cit</u>., p. 73; italics added).

Two problems arise at once. First, it is notoriously difficult to specify individually and categorically the properties of a <u>type</u> of human situation. There is no list of specific events etc. which exhaustively defines all possible situations in which, say, a Prime Minister's leadership is threatened. So the premise, 'The Prime Minister was in a situation of threatened leadership', as 2(a), is not to be established against a check-list of categorical criteria as in the parallel, non-intentional, premise 'The metal strut was in a situation of increased side-thrust and raised temperature'. For the former depends upon the Prime Minister being able to recognise and conceptualise other people's actions, remarks and so forth for what they are: that is, to understand their behavioural significance, Weber's <u>Sinn</u>. And one-and-the-same action, externally specified, may of course have a quite different <u>Sinn</u>, in one context of perceptions and intentions, from what it has in another. Even within the artificially restricted <u>milieu</u> of a chess-game, the very same move may represent, or rather actually <u>be</u>, in different contexts a genuine threat, a bluff, an insightless text-book reply, a gesture of despair or a mistake. So, on the one hand, there is no finite inventory of dispositions-of-pieces which 'operationally' defines my king's bishop being threatened; and, on the other, my reaction to a given disposition-of-pieces cannot be explained until you realise that I mistook my opponent's tired slip for a cunning innovation.

This is really the second problem. It is pointless for an historian to show that a situation actually *was*, objectively "of type *C*", and to appeal to what is appropriate to *that*, if it was not recognised as such by the protagonist. What any useful explanation must show is what situation he *thought* he was in; that is, his conceptualisation and apperception of his circumstances. Now the evidential considerations appropriate to *that* demonstration are very different from those which would indicate what his 'objective' situation was. So much so that, for some critics, an account involving such 'intentional' concepts, and their problematic validation, cannot be counted as nomological at all, but is properly seen as belonging to the contrasted category of 'idiographic' explanations (cp. Donagan 1963). It begins to look, then, as though Dray's formula for a covering-law paradigm can be applied in practice only by representing the "conditions" of the major premise in a way which turns out to be 'open-textured' in Waismann's phrase (1945), 'intentional' and idiographic.

And what goes for the open-texture and intentionality of the circumstance-specification applies also to the specification of appropriate action. For there is, again, no finite list of categorically particularised 'appropriate things to do', given the *sort* of circumstances which obtain.

Consequently one can say in advance only that certain <u>sorts</u> of things-to-do are appropriate, and then argue retrospectively that what was actually done was indeed of that sort. Consider the insecure Prime Minister again. Given that he sees his leadership threatened, and that he has certain private motives and public policies, it may be reasonable for him to make a show of strength, to bribe some sector of the electorate, to discredit the opposition party, or to distract attention toward some impending national crisis (real or imagined). But these are ~~all~~ all <u>kinds</u> of action, not particular acts; and the range of particular acts which can exemplify these kinds of action is boundless, if only because there are innumerable ways in which the Prime Minister may weigh up the actualities of the situation to which his individual action has to be adapted.

But further, if we have to investigate the <u>minutiae</u> of how the agent perceives his environment, and of what he wants, expects and fears, then we shall end up conducting a full-scale case-conference on the agent before we can put Dray's paradigm into practice; and we shall be admitting, by implication, that, at a macro-behavioural level, there <u>are</u> no usable generalities for the historian to invoke. Or at least, that such generalities as there are are not such as to allow the <u>explanandum</u> to be derived from the <u>explanans</u> by reference to a law-like statement covering that particular action. This is not to deny that such accounts may have explanatory force, that they may be matter-of-fact, or that they may be true; it is to say that they, like many other valid empirical explanations, may possess all three qualities without owing them to a rigorous deductive framework.

What is of interest is to notice how Dray's paradigm, spuriously rigorous as it is, does apply in general to the psychiatric case-conference, and in particular to the psycho-dynamic case-history. For the underlying logic of the case-conference is often to collate a varity of observations, about the patient's family background, socio-emotional development, work-situation, abilities, personality, neurophysiology etc. with reports about how he apperceives the world and the people about him, in such a way that his disturbed behaviour comes to be seen as 'the sort of thing' you would expect a person of that psychological make-up, with that developmental history, from that sub-culture, under that particular stress etc., to do. The residual diagnostic problem may then be concerned with what psychopathology we need to postulate in order to show that what was actually done was 'the appropriate thing', for someone like that, to do.

But, in the forst place, all this is conspicuously idiographic; and, secondly, the 'laws' invoked (insofar as laws are invoked at all) are not macro-behavioural links between circumstances and reasonable appropriate actions. Much of the behaviour is highly unreasonable and inappropriate, <u>except</u> when seen as the consequence or expression of a very specific personality-structure, psychopathology, neuropathology or whatever. And the 'laws' relevant to this operation of 'seeing as', are the micro-behavioural considerations of developmental psychology, neurophysiology and the rest. It is perhaps in the light of this, the fact that there <u>is</u> no 'appropriateness' or law-like tendency at a macro-behavioural level, but only in the informed eyes of the relevant technical expert, that philosophers sometimes seem to deny that there are any laws at all in voluntary, self-conscious behaviour (Wittgenstein, 19 ; Ayer 1964); and Wever's unclarity on the point can lead, according to Winch

(1958, p.47), to uncertainty about how to use his concept of Sinn.

Certainly the immediate job at case-conference seems more like looking for patterns of forces and states, in both the patient himself and his environment, of which the problematical behaviour is a consequence or (more especially for our purposes) an expression. There will have to be, no doubt, some kind of law-like links between the patterns which emerge from the multi-dimensional collation of observations; but, although these link-statements, in cases where the links are propositional, do have a certain generality and contribute to explanation, they may play many roles other than that of covering-law. And indeed it may even be a mistake to suppose that the conceptual links, between an explanatory pattern or 'structure' and what it explains, are necessarily propositional at all. For that supposition is arguably one of the contributory superstitions in the 'myth of deductivism'.

(c) Analogies and structural relations. We have encountered above (pp. 00-00) the general idea that, so far as real-life science is concerned, the purpose of a theory often is, not so much to run out law-like general propositions, but to depict, in various ways, a plausible 'structure' which can be related to the phenomena of observed events and states (the subject-matter of traditional contingency propositions) in various rationally and empirically cogent ways.

Two emphases here are central to our argument: that on structure, and that on variety. For I want to urge inter alia three main contentions. One is that the part played by explanatory discovery in D.I. is not so much, or in the first instance, that of bringing to light causal sequences of events, as that of elucidating the patterns, structures and functional relations which obtain within a person's behaviour and experience. Secondly, since the logical and ontological status of these structures is diverse, they will stand in different relations to the phenomena they illuminate as well as to evidential considerations relevant to their validity (just as do the various types of theoretical 'model' in the physical sciences). The third point, which is double-edged, is that we can get a clearer idea about <u>how</u> such structures illuminate the behaviour to which they refer, and about how they are related to various sorts of factual observation, by comparing them on the one hand with the nature and functioning of some structural models in the 'hard sciences', and on the other (Part III) with the way in which structural claims about the more 'expressive' material of linguistic and aesthetic ~~musical~~ activity can conduce to its understanding and be related to 'the evidence'.

First we need to remind ourselves specifically about the diverse properties and explanatory functions of scientific models and analogues (or "allusive" structures), and notice some questions about the nature and uses of evidence which they raise. It is one of Harre's basic contentions that a principal way, perhaps indeed <u>the</u> principal way, in which physical scientists try to understand and explain something is to seek (or, if necessary, postulate) a structure capable of producing the phenomena to be explained. In the case of going to look, the direction and nature of the search (where and at what you look) will be determined by the known regularities of the physical world, and to that extent the search is 'governed' by natural laws; although, if the combination of conditions in which you are working is novel you may not know how, or whether, to extrapolate from existing laws. Similarly, in the case of postulation, the supposed method of 'production' has to be consistent with this known <u>natura rerum</u>, though not necessarily confined to it, and the end-product of the postulated system will often be only analogous to, not identical with, the <u>explananda</u>. That is to say that a model is not necessarily a design for an underlying mechanism; but that, where it is not, a model, unlike a metaphor, will necessarily be furnished with 'transformation-rules' for converting descriptions of its relevant features and states into statements about the world. It is notorious that some so-called 'models' of psychodynamic functioning such as those which feature in the 'object-relations' theory of Klein or Fairbairn, do lack this provision, and consequently turn out to be mere metaphor (Cheshire 1966, pp. 128 - 147).

The explanatory enterprise of actually going to look for a mechanism has been called 'exposing the "fine structure" of a system (Harré 1964, p. 82), and is most concretely exemplified in such pursuits as anatomy, dissection, chemical analysis, microscopy and chromatography; or, for that matter, in a child's taking a toy engine to pieces to see how it works. Even in these cases, however, there is still the question of the <u>logical</u> relation between identifying the underlying structures and processes, on one hand, and 'understanding' the surface phenomena, on the other. It is entirely possible to <u>mis</u>-understand how the parts of system work together, and consequently to think one understands when one does not. The exposure of find structure explains nothing of itself in the absence of empirical generalisations about what to expect from such structures, given that the world works in the way it does.

It is not uncommon for archaeologists literally to expose physical objects or structures whose discovery contributes nothing to the understanding of the site because it is not known what they are for (or, in some cases, whether they are 'for' anything). This is to say that their functional relation to the other component elements of the system-to-be-explained (the archaeological site) is unknown. There is indeed a rag-bag concept, that of 'ritual significance', which is invoked precisely to indicate that the functional relation between one find and the rest is not understood! Or again, the discovery that a substance is composed of this and that chemicals will not explain to me why it burns with a blue flame, if I do not know how such chemicals can be expected to behave.

If the relation between descriptions of observed categorical structure and the explanations in which they figure are complicated, the part played by descriptions of <u>envisaged</u> functional structure in their explanations is even more so. To begin with, the forms in which such structures may be envisaged and depicted are varied. They range, in solidity at least, from physical replicas of various kinds, through 'black-box' diagrams to sets of algebraic equations. And even within the group of solid three-dimensional constructions, the way in which a golf-ball model of the solar system can explain eclipses is plainly unlike that in which a ball-and-rod model of an amino acid crystal contributes to explaining phenylketonuria.

Harre's analysis and classification of the diverse properties which explanatory models and analogical structures may have, and of the diverse logical relations in which they stand to their respective *explicanda*, makes it clear that explanation by 'allusion', of various kinds, is very far from being intrinsically unscientific; and that the explanatory concepts and tactics involved in *D.I.* can often be shown to belong to the same analogical category as accepted theoretical procedures in 'respectable' sciences. This does not, of course, guarantee the validity of those forms of *D.I.*, because the empirical assumptions linking analogue to reality may be inadequate or false: but it does mean that they cannot be dismissed as capable *a priori* of leading only to illusion.

When we try to explain the working of a puzzling system, or the production of puzzling phenomena, by envisaging what functional characterisitcs the generating system must have, we can draw on a wide range of properties, processes and relationships which are familiar from the way other systems work. The range tends to be restricted in practice by consideration of what seems empirically apt for a particular case. But this restriction can be both misleading and misunderstood. Misleading, because it may narrow the conceptual outlook too far, so that a productively original way of looking at a problem is missed; misunderstood, because critics sometimes reject a theorist's explanatory analogy on the ground that the actual system cannot have *some* of the properties of the proposed analogue.

The former point has been urged by students of 'creative' or 'original' thinking in science, who emphasise that many theoretical advances seem to have sprung from breaking away from conventional and apparently 'realistic' patterns of thought, about a particular issue, towards the combination of certain ideas which had been separated by the inhibiting assumptions of habitual 'cognitive set'. Thus the form in which the crucial combination of ideas comes to mind may seem singularly unfitted for its theoretical task. Kekulé's derivation of his model of the benzene ring, from a day-dream about a snake, is an oft-quoted example; and Koestler (1964) has popularised many others, though his collection has been criticised as inaccurate by Toulmin (1964).

But even if we try to use a criterion of empirical likelihood, in the search for analogues, it is hard to apply. For, not only may the puzzling phenomena be more or less <u>sui generis</u>, so that nothing quite 'like' these processes etc. are found in other empirical conditions. What happens at extremely high speeds, and over extremely great or extremely small distances, just does not happen at all, we are often told, at speeds and distances which we ordinarily observe; and the fact that some sub-atomic goings-on consequently have no parallel in either common experience or commonsense, makes them very difficult for the physicist even to talk about. For our purposes, then, what should we expect the workings of mental and emotional forces to be <u>like</u>?

On the other hand, once we admit the "logic of analogy", it is not easy to specify, in general and in advance, the parameters of similarity by which to distinguish potentially fruitful likenesses and correspondences from useless ones.

But in a particular case, confusion is averted by indicating which features of the analogue are being treated as corresponding to properties of the explanandum and which are not: that is to say, by specifying the areas of 'positive' and 'negative' analogy (Hesse 1966, p.8). And we have seen that a fully developed model will be equipped with 'transformation rules' enabling propositions about those features of the analogue which come into the 'positive' area to be converted into ones about features of the world. What is wrong with some of Freud's notorious hydraulic and anthropomorphic analogues is not that 'mental energy' cannot in many ways be like a current of water, or that superegos cannot, in many ways, be like committees; for it is equally true that the plastic-covered wire in a model of the cardiovascular system is not, in many ways, 'like' blood-vessels. The troube is rather that Freud too often does not mark off, even informally, this area of negative analogy.

There is nothing necessarily confused about using such model-sources as hydraulic systems and committee behaviour, provided that their use is disciplined. The need is not to outlaw allusion per se, as Cioffi implies, but to insist upon controlled allusion; and to notice how it is in fact controlled in scientific usage. But it proves to be no straightforward matter, on investigating living examples, to identify those logical features which differentially characterise successfully controlled uses of allusion in the sciences.

Hesse, it is true, tries out two criteria (1966, pp. 86-87).

But the one, to do with similarity, has to be kept so unspecific that it is almost unhelpful; and the other, which requires that the causal relations obtaining in the system alluded to shall be "of the same kind" as those in the *explicandum*, seems clearly too restrictive. For, in the latter case, there does seem, for example, to be a place in the study of the nervous system, which is known to function by electro-biochemical processes, for allusions to inorganic hardware circuitry. You *can* explain, or at least illuminate, the findings of Lashley's classic cortical ablation studies 'on the model of' a telephone switchboard (Broadbent 1960, pp.   ); and Miller *et.al*. have commented (1960, p.00), in presenting their TOTE theory of adaptive perceptuo-motor integration, on the general problem of relating such inert hardward analogues to their organic "software" counterparts in people.

Further complications, not to say *impasses*, are encountered, of course, when we contemplate the possibility of analogues whose causal structure is to be of the same 'kind' as that of mental (as opposed to neural) processes and of emotional experience. And yet Lewin's causally incongruous transfer of certain "dynamic" concepts from the physicist's electromagnetic force-field to the psychological "life-space" of an individual, with the idea of emotionally polarised areas which induce positive or negative behaviour in respect of them, seems to have paved the way for fruitful reductionist investigation of some aspects of conflict and regression, and even for the experimental validation of specific psychodynamic hypotheses (Lewin 1935, pp. 0-00; Murphy and Kovach 1972, pp. 264-267).

So far as D.I. itself is concerned, it is already evident that the analogues most conspicuously invoked are those of 'expression', whether semantic or affective, and its understanding or translation. The discrepancy of causal structure here is between that exemplified in the (largely) conscious and voluntary communications of the analogue, namely some linguistic system, and the (largely) unwitting and involuntary quasi-communications of the explicanda, namely the interpretable behaviour.

Harré, however, is less insistent on causal homogeneity. He even suggests, for example, that the notorious Bohr-Rutherford model of the atom still has some explanatory force in spite of containing a known counter-empirical requirement: which is that a particle of finite mass should move a finite distance, from one orbit to another, in zero time. After all, if such a model is sufficiently illuminating in other ways, it may persuade you to change your conception of an electron; or even, in the absence of a preferable alternative model, to maintain a mixed wave-particle conception, the two aspects of which embody each other's negative analogy (Harré 1970, p.44; Hesse 1966, p.91).

And certainly it is not at all uncommon for a less extreme causal unreality to be built in from an analogue to a model, in the form of the idealisation of some property of the known system to which it alludes. Thus the corpuscular theory of gases involved conceiving a volume of gas as an agglomeration of minute and perfectly elastic solid balls. The fact that there is actually no such thing as a solid sphere that is 'perfectly' elastic does not necessarily vitiate the analogy, so long as the conception points to the need to combine, in the model, some properties of solid balls with the notion of perfect elasticity, in order to represent what gases must be like.

Nor is there anything intrinsically disreputable in a model drawing on more than one analogue-source at a time. In the class of models which Harré calls 'paramorphs', because the source for the properties of the model is some system other than that which is being modelled, there are several sub-classes which are defined by the way in which such a model uses its source or sources. One of these sub-classes is that of the 'multiply-connected paramorph', which, again like the Bohr-Rutherford atom with its mechanical and electromagnetic features, combines properties and processes derived from more than one sort of system. The variety of analogy, therefore, in which Freud depicts the working of, say, the defence-mechanisms (Madison 1961) is not of itself reprehensible. Such variety, however, may be (and in this case probably is) symptomatic of a cavalier attitude to that need for marking off, even implicityly, the area of 'negative analogy', which has just been noted.4

One further question, about the varied relations in which scientific models may stand to causal generalisations, predictive deductions and observational check, concerns whether and in what sense their theoretical value depends on their being (usable as) specifications for hypothetical mechanisms. For it seems inescapable, on the one hand, that a D.I. often purports to explain its data by showing them to be the product of the activity of some psychic 'mechanism', such as those of "repression and defence" or the semantic scrambling of the dream-censor; while, on the other hand, it is tempting to seek an analysis of D.I. which avoids categorical causal implications as much as possible, precisely because of the ontological dubiety of whatever categorical postulates might be required. In this latter vein, I have tried to show elsewhere how even some of the more exotic concepts of Kleinian psychodynamics may to some extent be rescued from ontological absurdity, by recasting them partly into the quasi-dispositional reductionism made familiar by Ryle, and partly in terms of the speculative neurophysiological constructs of Hebb (Cheshire 1966, pp.108-127, 148-167).

This should not be taken to suggest, however, that the non-categorical interpretation of theoretical models is merely a defensive exercise undertaken to avoid philosophical embarrassment. For there is a whole class of conceptual models which serve to represent fhe functional relationships characteristic of a system without, as it were, saying anything categorical about the underlying mechanisms which mediate them.

An extreme case is the 'mathematical model', which expresses algebraically how the values of certain output-parameters of a system vary as a function of input-parameters and concomitant conditions. The terms which feature in such algebraic formulations <u>may</u> serve merely to hit off these interrelations as economically as possible; consequently they do not necessarily refer individually to (nor are they convertible into propositions about) separable parts or structural features of the system being modelled. Physicists are familiar with this argument from Dirac (cp. Harré 1964, pp. 95-97), and its logic can be generalised to define a broader class of modelling-relation, which is not confined to models using mathematical methods of depiction.

This more general type of modelling-relation, which Harré calls a 'modal transform' in contrast to its disjunctive type known as the 'causal transform', is what obtains between descriptions of model-states and descriptions of states-of-the-world when the former are not independent causal conditions for the occurrence of the latter, but are instead another way of looking at, or conceptualising, them. For in this case, unlike 'causal transforms', the model-descriptions and world-descriptions do not refer to separate sets of entities but (in a sense) to the same things in a different way. Thus:

> "In the case of the modal transform, there is no separate question as to the existence of the hypothetical mechanism and its states which the model represents, for they are the same states of the world looked at from a different point of view" (Harré 1970, p.54)

It is important to realise that a theory, and especially one like psychoanalysis which is arguably no more than a "premature synthesis" of disparate theoretical elements (Farrell 1963, p.24), will often contain both modal and causal kinds of transform; and that there is a consequent danger of mistaking the one for the other.

Harré juxtaposes illustrations from the molecular theory of gases and the wave theory of light with ones from psychodynamics. The relation between sentences about the impact of molecules and those about gas pressure, or between those about repressed conflicts, on one hand, and about 'hysterical' behaviour on the other, are <u>causal</u> transforms. It is a <u>modal</u> transform, however, from statements about a surface reflecting light of a certain wavelength to those about its being coloured a certain hue; or between talk of a (Freudian) 'slip of the tongue' and 'admission of guilt'.

Couched in these terms, then, it is part of my thesis that the explanatory allusions made by D.I. are often to be understood more in the spirit of modal than of causal transforms, and that this distinction has fundamental implications for the means by which we seek to confirm or validate them. It raises, to be sure, some intricate and elusive epistemological problems; but, without pursuing them much further here, we can at least take care not to try to validate 'modals' by methods appropriate to 'causals', a mistake which experimental psychologists seem to have made before now (Harré op.cit., p.50). It also serves to emphasise that the logic and methodology of explanatory discovery in 'the sciences', let alone in psychology, is a great deal more complicated than some experimentalists assert.

If this talk about modal versus causal analogues in D.I. seems itself to be another way of looking at (a 'modal transform' of) Levy's distinction between 'semantic' and 'propositional' stages in the enterprise (pp. 00-00 above) then it should be reassuring that, after starting from a quite different source and travelling a more circuitous route, the argument has led to a similar place. The logos is, after all, unitary, as Parmenides taught. And if, like the observers in Farrell's Cave, the same logical forms and properties continue to show up when we look into different mirrors, then we may reasonably suppose that those forms and properties have something in them (p.00 above).

One further point of affinity between Harré and Levy is instructive for our purpose. We have seen above (pp.00-00) that, in Levy's analysis, the "semantic stage" merely categorises or redescribes the behavioural data in technical concepts, which are relevant to the hypotheses to be adduced at the 'propositional stage', but which do not in themselves carry any empirical implications. This a priori, and implication-free exercise is held to contribute nevertheless to explanatory discovery, by paving the way for the subsequent appeal to emprical hypotheses and causal generalisations: we process the explicanda, as it were, by identifying them as examples of 'obsessional' or 'over-protective' behaviour (or whatever), so that we can bring to bear on them our theoretical propositions which deal in such concepts.

But in the case of modal transformation, we seem to have a correspondingly analytic procedure without a correspondingly empirical, or synthetic, second stage. How then can it be conducive to explanatory discovery? The answer is, as we have already noticed, that "mere" redescription is not necessarily analytic, and emprically claim-free, as Levy argues; and we found that his argument compelled us to make a brief expedition into the metaphysics of taxonomy (p.3-10.). If we observe three action-sequences, say of a mother towards her child, and process a, b as 'obsessional' and c as 'over-protective', we are implying at least that there is some explanation-relevant way in which b goes with a but not with c. It is to say something about the structure of the mother's behaviour; and it is at least a "mongrel" semantic-propositonal assertion (by analogy with Ryle's "mongrel hypotheticals", which have a bit more of the categorical in them than do pedigree hypotheticals) insofar as we are inclined to think that the sense in which one item "goes with" another must be that they have either some categorical property or some causal antecedents in common. This amounts in practice to implying either that behaviour-items are phenomenally alike (as going back to check the windows is <u>like</u> going back to check the gas-taps), or that they are a product of the same anxiety, conflict, wish, fixation etc. (as with obsessionality and suspiciousness).

We return to the problematic logic of such structure-claims below (ch.9 (a)); but here we need finally to observe how Harré regards the laternative, non-causal, conceptual scheme used by a modal transform as helping to advance empirical understanding. It turns on arguing, as we did against Levy, that taxonomy is not teleologically neutral: it is not the <u>prelude</u> to a line of directed emprical inquiry, but the first stage of it. Thus Harré indicates that, when we describe crystals of common salt as "cubical lattices of sodium and chloride ions", we set up "a modal transform between the shape of the crystal and the structure of the lattice". The point of the "lattice" description is to invite us to classify salt along with other electrovalent compounds rather than with "peppercorns, bay leaves and parsley".

The way in which this particular reclassification gives direction to further understanding of the substance is obvious (though, once again, the <u>general</u> questions which it raises about the metaphysics of taxonomy are profound); but it should not be assumed that the technically advanced, micro-structural mode of categorisation is the one which necessarily leads to the most fruitful re-alignment of the explicandum. For Harré immediately reminds us that, in the study of human behaviour, for example, the informal phenomenal description may be the heuristically significant one. If you make a noise near me, you can describe my 'response' in terms of neurophysiological activation; but if you want to understand my reaction, you had better describe it as 'hearing a noise', or even as 'thinking that I hear a voice', if only because that will lead you to align it (fruitfully) with other perceptual experiences rather than (misleadingly) with playing a tennis-stroke.

I want to suggest that DI often uses its informal analogues (like 'projection','internalisation', 'denial' and'splitting') as modal transforms in this kind of way, to bring about an imaginative and potentially heuristic re-grouping or re-alignment of behavioural data. And the question of what groupings and alignments are characteristic of a particular individual's behaviour (the question, that is, of what are the theoretically significant taxonomies in his case) can be answered only in the light of the "fine structure" of his experiences, subjective outlook, relationships, acheivements, fantasies and the rest of it. Case-conferences and therapists try to collate this kind of information precisely in order to tell what taxonomic analogues are appropriate to this particular patient: in order to decide, for example, whether this particular mother's tense insistence on doing up all the child's coat-buttons is best interpreted as 'over-protective' or 'obsessional' (or, of course, something else, like 'denial of rejection').

It is the same spirit of constructively paradoxical revisionism in taxonomy which leads both Kekulé to think "Look: benzene molecules are like tail-swallowing snakes", and Freud to say "Look: accidents are like wishes". Whether they really are, furthermore, is perhaps as much a matter of tactics as of fact; but I do not see why what is sauce for the goose of physics is not also sauce for the gander of psychodynamics.

CHAPTER VI

Perplexity and the Uses of Evidence

(a) Puzzlement and story-telling

We have looked at some of the ways in which more-or-less formal logical schemes can be drawn up to account for the explanatory force which some observations, propositions and arguments have; and we have asked how DI can be, ought to be, or might be fetted into such schemes. The topic cannot be left, however, without a final glance at a less formal approach which has been adopted by some commentators on psychodynamics, and which perhaps owes something to a more general distrust of formalised framewords for metascience.

This approach is prompted both by the concern that, *pace* Kuhn (1962, pp.43-51), the formalised paradigm may as easily immunise one against the rule-testing exception as sensitise him to it, and by a consequent ethological metaphysic which insists on observing the characteristics of empirical explanations as they go about their business in their natural surroundings. It is to adopt what Harré & Secord have called the New Paradigm of "scientific realism" (197, p.). This enables us to see some of the actual situations in which explanations are sought and offered, and some of the characteristics actually displayed by those that prove acceptable or useful. As Sherwood puts it (1969, p.188), it is better to proceed "by studying just such an explanation that, if true, would be adequate, rather than developing a priori criteria and then trying to decide if a given explanation fits them". It becomes apparent that the search for an explanation is provoked, as has often been remarked, by a situation of puzzlement, anomaly, discretancy, 'cognitive dissonance' and the like. These 'dissonances' themselves may have a variety of sources, to which the make-up of the observer may contribute as much as what he observes; so that a request for explanation may spring from a combination of observation with ignorance, false assumption, anxiety or closed-mindedness on the part of the inquirer. It would therefore be odd if there were only one paradigmatic way of resolving such puzzlement.

It has long been recognised, indeed, that there are different kinds of explanation, in the sense that the explanans may relate the explananda to the inquirer's knowledge, assumptions, intentions or ignorance (and thus resolve his 'dissonance') in a variety of ways. But we must not treat this sense-of-dissonance as a necessary condition of the logical need for explanation: we may fail to perceive the dissonance in a set of observation or beliefs which we ought to be puzzled by. It is sometimes the mark of the revolutionary, paradigm-breaking theorist that he sees incongruity and anomaly, and the consequent need for explanation, in data which had hitherto been taken for granted in unperceptive equanimity (cp. Kuhn, 1962, pp. 35-65; Sherwood 1969, pp.9-22). It takes a Newton to ask 'why should apples fall?'; or rather, perhaps, 'why should apples fall?'

It is tempting to think, with Bridgman (1927, p.37), that we regard such incongruities and anomalies as resolved insofar as we can show that they are but special cases of some familiar situation, state or process. But this, Sherwood contends (1969, p.10), is to confuse "the psychology of persuasion with the logic of explanation": obviously, we may rest content, thinking that we understand, when we really do not. And he goes on to demonstrate how, in a particular case, a typical "psychoanalytic narrative" (PAN) contains "explanations of quite varied types" which are aimed at "incongruities of very different kinds" (p.23).

The idea that the interpretive explanations of psychoanalysis depend essentially upon constructing a relatively extended series of interrelated observations and hypotheses, which serves eventually to amplify, illuminate and make coherent the puzzling sample of behaviour which intially faced the clinician, is implied in the title which Klein (1961) gave to one of her case-studies: Narrative of a Child-Analysis. What we need is a story, not a syllogism. But what kind of a story, and how does it work?

When Farrell discusses this question (1961, 1963), he emphasises that "telling more of the story" often has the effect of showing up patterns and relationships in the data which render them understandable, coherent and no longer puzzling. This applies both to how we come to 'understand' the apparently incongruous behaviour of our new neighbour, in Farrell's example, and to how Freud accounts for that of Leonardo da Vinci. In the former case, we reconcile the discrepant facts, that his luggage includes a lot of gardening equipment but that he neglects the garden, by finding out that he used to do the gardening jointly with his wife who has, however, recently died. Thus the 'dissonance' between my belief that he is a keen gardner and my observation that he does not do the garden is resolved by the discovery that the circumstances are such as would lead even a keen gardner to neglect the garden: the paradoxical behaviour becomes 'the sort of thing you would expect ...' (cp. ch.5(b)).

What matters, however, is not so much that the dissonance or "psychological puzzlement" among my beliefs and observations is dispelled, for that might be done as much by another false belief (e.g. 'He is a Sikh; and Sikhs give up gardening at age forty on religious grounds') as by valid information: how it is despelled is what counts. Sherwood's point seems to be that there should no longer be a logical discrepancy between the two propositions 'he is a keen gardner' (full stop) and 'he does not do the garden'; and this is achieved by expanding the first propostion to 'he is a keen gardner who has just lost his co-gardening wife', and showing that this expanded version, which "tells more of the story", is not inconsistent with the second proposition.

Another main way in which 'telling more of the story' works is by showing how one fact, or properts of the facts, relates to another, so that some trend, pattern or structure is seen to emerge among the data. It is tempting to see this as being like turning up more pieces of a jig-saw puzzle so that it eventually becomes clear that what you thought was a picture of a house is after all a ship; or even like plotting more points on a graph, so that you could not make sense of as a linear function turns out to be a distorted ogive. Thus, in the case of Freud's Leonardo, odd bits of biographical data are used to fill out a sequence of events inwwhich certain trends (obsessionality, maternal overstimulation, obstruction of sexual identification, fascination with forms of energy, emotional detachment) can be discerned, and certain relationships observed (e.g. between slow, unfinished work and 'secret' handwriting, or between psychosexually destructive mothering and the 'emigmatic smile',

But, although this PAN certainly tells more of the story, some of which is simply wrong as it happens, it would be a mistake to think that it is logically parallel to plotting more of the graph or finding more pieces of the jig-saw. For a necessary and characteristic feature of the pattern-weaving here is to recommend that we look at some elements of the data in a different way from what is usual, on the ground that if we do so (and perhaps only if) then certain coherent patterns emerge which we can recognise and identify. This more analytic form of 'perspectisism' turns in general, as we have seen (pp. 00-00), on being ready to construe the manifest fear as a latent wish, the manifest accident as a letent intention, or the manifest solicitude as latent rejection; and, specifically, in Leonardo's case, on seeing, for example, the manifest anatomical curiosity as a defence against latent revulsion from physical heterosexuality or whatever. And it has to face the question whether the theoretical price, in terms of conceptual innovation and willingly suspended disbelief, is too high to pay for the ostensible integration of the data which it buys.

The main point, however, is that this analytical perspectivism, which depends upon looking at some elements of the data from a different angle and in a different light, is not like 'telling more of the story' merely in the sense of picking up some jig-saw pieces which had fallen on the floor: it is more like realising that some pieces can be fitted, and the puzzle consequently solved, only if you allow that they be turned face downwards. In this way, for instance, the occasional insightlessly dictational action of the ultra-mild man can be 'fitted in' by seeing his habitual ultra-mildness as a (not very egosyntonic) form of aggression-control; or, to borrow an example from Levy (1963), Don Juan behaviour is seen as expressing not sexual confidence, but corresponding insecurity. But you still have to decide which pieces to turn. Story-telling helps with this decision because the more pieces you have on the table, the more easily you can tell which ones need to be turned over in order to construct a coherent pattern, and thereby to produce a solution. This is one way in which what the PAN tells is not so much <u>more</u> of the story, as a different <u>kind</u> of story.

Sherwood has elaborated this point, as we shall see; but we must first take account of a notorious consequence of this approach to behavioural data. Since there is nothing which corresponds to being able to tell, by reference to an independent, external criterion, whether this particular piece should be turned over or not, the procedure assumes that reliable judgements can nevertheless be made both as to whether this is a 'coherent pattern' or not, and as to whether it is the right sort of pattern for the data. All this is reminiscent of Weber's problem about the role of the expert in identifying behavioural <u>Sinn</u> (pp.   above). For perhaps in a particular case there should not be one coherent pattern running through the data: perhaps, that is to say, the graph-plots reflect two relatively separate functions, not one uniform curve; and perhaps what we have on the table are the pieces of two separate jig-saw puzzles. In which case, if we manage, by turning jig-saw pieces over or by ignoring the signs of the plot-values, to construct a 'coherent pattern', then we are deceiving ourselves with a spurious artefact. But there are other empirical inquiries, as we have seen (ch.5 b.c.), which face this same difficulty, and manage to deal with it by appeal to contextual considerations of coherence and without reference to any independent check.

Now, in the case of the analogue-situations, two kinds of consideration usually safeguard us from this mistake. One is that we know what degree of internal coherence of 'goodness-of-fit' to expect in our jig-saws, and can even express numerically, for our graphs, the way two possible curves differ in this respect; but there is still the problem of deciding whether the ESP receiver is guesssing randomly or doing something significant, howbeit with a lot of 'noise' in the system and one card out of phase. And suppose that our jig-saw pictures were reproductions of unfamiliar 'modernist' abstract paintings; if art galleries can hang such things upside-down, we should be forgiven for not knowing whether we had one or two of them carved up before us. The other general consideration is the possibility of external check. We can draw off differential implications from the rival graph-functions and devise, or at least specify, critical observation-conditions; and the jig-saw puzzles can normally appeal to the picture on the box, even if many a child's jig-saw has long since parted company with its external criteria.

In the case of PAN, however, it is often objected that both these controls are missing: the latter, because it is unclear what 'external' observations are considerations would bear upon its validity, and how they would do so; the former, because it is sometimes insufficiently passimonious in its multiplication of explanatory <u>entia</u>, and because it can occasionally be shown that its unifying pattern is frankly spurious. Eysenck (1965, pp. 95-131) has argued, not without cogency, that Freud's PAN about the 'Little Hans' case is an instance of such over-elaboration, on the grounds that the principles of aversive conditioning exemplified by Watson's 'Little Albert' can explain just as much as the more exotic and speculative 'Oedipus complex' can. And Farrell's critique of the <u>Leonards</u> study is concerned precisely with the question whether Freud's overall picture (even if time) should be preferred to the piecemeal explanations of particular points of perplexity which the art historian can give.

One function, for example, of Freud's PAN is to relate the oral bird-fantasy, the enigmatic smile and the errors in anatomical drawing to a common psychopathological theme; but perhaps they should not be so related. Perhaps the explanation of the 'enigmatic' smile, for instance, is quite unconnected with that of the other puzzling phenomena. Specifically, if it can be shown that there was a current cultural craze for creating such smiles on living faces as well as on canvas (as there have been subsequent crazes for the yo-yo and platform shoes) and even that one women's beauty-manual of the times actually gave instructions about how to produce them, then is not that enough? For the question why da Vinci's enigmatic smiles are especially effective can then be answered by observing simply that he was an especially skilful painter. The bird-fantasy and the anatomical errors also have individual explanations at a similar level (Farrell 1963, pp.35-46).

At this point some apologists would invoke the problematical psychodynamic principle of the 'overdetermination' of symptomatic behaviour. They would say that such behaviour regularly arises from more than one set of causes, or serves more than one purpose, each of which separately would be sufficient to produce or explain it. Consequently there is no anomaly in accepting both the socio-cultural piecemeal explanation and the one in terms of idiosyncratic psychopathology as true alongside each other: it is not a matter of having to choose between them. A person's psychpathology may well find behavioural expression through those socio-cultural conditions and usages which impinge upon him; it does not necessarily run counter to them. Thus, it would be said, sadism may be expressed in culture-consistent activities, such as voting for capital punishment or in doing certain kinds of animal experiemtns, though it may be possible of course, to do both these things for motives quite independent of sadism. The point is that they can be used as a vehicle for it, and that it does not necessarily lead to culture-dissonant practices like bear-bating or cock-fighting. But if the appeal to 'over-determination' is to seem, in a particular case, any more substantial than transparent special-pleading to save a superfluous hypothesis, it is especially important that there should be some way of checking 'expernally' on the apparently unnecessary explanation.

Whether or not this can be done for some such all-embracing PAN's, it is an instructive feature of the Leonardo story that it demonstrably falls foul of that other hazard of comprehensive pattern-weaving which was mentioned above: it embraces too much. For it successfully incorporates into the grand design some pieces which can be shown not to belong at all. Freud makes symbolic capital out of the fact that (in the German translation, at any rate) da Vinci's bird-fantasy involved a vulture, and he seems to accept Pfister's observation that a vulture can be seen , in the manner of a puzzle-picture, in the drapery of one of the 'double-mother' paintings, the Madonna with Child and St. Anne of c.1510; he also reads psychopathological significance into anatomical errors in sketches of the human reproductive system.

Unfortunately for these ingenuities, a valid account of da Vinci's psychopathology does not have to account for vulture-images but for kite-images, because "vulture" was a mistranslation of the Italian word nibbio which really means "kite". Now, kites are not mother-symbols in Egyptian mythology, as vultures are; and if there is a vulture in the drapery, it has got there entirely by chance. Again, the anatomical errors, and consequent pathogenomic slips or 'resistance' which Freud attributes to da Vinci, can be shown to exist in contemporary texts on which da Vinci was relying.

We are forced to conclude, in the light of this, that these links in Freud's chain are not links at all; and, to take up Pareto's metaphor (p. above), that the path from A to C via B does not really exist. What, then, becomes of the whole 'chain' or the whole 'route'? Sceptics seize the opportunity, as we have seen (ch. 5a), of dismissing them as artefactual illusions, and they must certainly be regarded as a dire warning against using such interpretive methods in an under-controlled way. Freud did so in this instance with his eyes open, to be sure, knowing that he was chancing his theoretical arm with such sparse data and without the control of concurrent feedback from the therapeutic transference-situation. For it is precisely such contextual information which enables a more typical PAN to be monitored and revised as it is being developed, and Sherwood has shown (1969, pp.69-124) in some detail how Freud did this in the famous case of Paul Lorenz.

But the normal use of such contextual observations (whether from therapy or elsewhere), in the light of subsidiary background generalisations, still relies largely on the appeal to coherence, rather than on external validation: that is to say, on convincing us that a 'significant pattern' has been constructed, rather than on showing that there is independent evidence for postualted elements or aspects of the PAN. It may emerge below (section (c), (d)), however, that this contrast is not as clear-cut as is usually supposed. And, in any case, the demonstration that coherence-appeals can come to grief when evidence is sparse or defective, as in the Leonardo study, does not justify the inference that they necessarily constitute a faulty method in any conditions. The epigraphist, for example, who is trying to decipher a strange language or script, may have to decide precisely analogous questions about what is part of the pattern or whether there is more than one sub-pattern, on grounds of internal coherence alone. The pioneering 'linear B' workers had to establish, for instance, whether a particular symbol was another letter-syllable or a space-marker, and whether this particular mark represents a new symbol or is a variant from of a known one. A scholar knows very well that when the data are limited or repetitive (as in the Samothracian language, whose surviving 'texts' consist largely of recurrent identical three-word legends) he will make mistakes over such decisions. But he can also take heart from the fact that, perhaps even without a providential "tripod tablet" and certainly without such an external criterion as the known language on the Rosetta Stone, he can sometimes show an internal semantic coherence which defies "astronomical" odds (ch.4 ( ) above). We llok more closely in the following chapter into the implied rationale which supports such exercises in linguistic interpretation, and argue that much behavioural DI can be shown to rest on a similar logical framework.

(b) <u>The totem of prediction</u>

One way in which we decide between rival suggestions about what pattern or patterns really underline an ambiguous set of graph-plots is, as we noticed, to draw off differential predictions from these suggestions and to make further observations in conditions which would be crucial to that difference. The fact that it is rarely possible to submit <u>PAN</u>'s to this hypothetico-deductive procedure is sufficient to persuade those critics for whom prediction is a <u>sine qua non</u>, if not the be all and end all, of scientific explanation that such narratives can provide no genuine empirical understanding. A further word about this totemic attitude to prediction, which certainly seems to have become established in the catechism of experimentalist psychology, will serve to highlight some logical features of the way the <u>PAN</u> operates; in particular with respect to how it tells a different <u>kind</u> of story, rather than just more of it, and to what follows from this for the treatment of 'evidence'.

It is readily understandable that the prophet or soothsayer should have had an honoured place at the right hand of the kings and pharoahs of pre-scientific cultures. For they could dispel 'psychological puzzlement' about what was happening elsewhere or going to happen. He who can read the future must understand the Creator's plan. But Plato argues in the <u>Theaetetus</u> that 'true opinion' does not presuppose understanding, and that, when bereft of such <u>logos</u>, it is not to be confused with 'knowledge'. It comes as some surprise, consequently, to find the cult of predictivism still flourishing; and fostered most assiduously, perhaps, in the philosophy of psychology. We may think it symptomatic, indeed, that in Kelly's 'personal construct' theory the drive to anticipate events has ousted the instinctual <u>libido</u> as the mainspring of individual personality development: thus does 'man the scientist' replace man the adaptive pleasure-seeker.

The contrary view, that the generation of predictions is a contingent (rather than necessary) property of some forms of scientific understanding, has been canvassed now for some time (Hanson 1958, pp.7092; Kaplan 1964, ch. 9). And two lines of thought from Toulmin's treatment of the topic (1961, pp.18-43) serve our purpose particularly well. The one drives a wedge between 'prediction' and scientific 'understanding'; and the other shows that the notion of prediction has to be weakened so much, if it is to retain an intimate connection with explanation, that the contrst (which gave some concern above) between evidential appeals to specifiable 'external' implications of a PAN, on the one hand, and the mere construction of 'internal' coherence, on the other hand, begins to dissolve.

On the former point, the history of astronomy illustrates the plain matter of fact that you may be able to predict very accurately on the basis of observed regularities, while being quite ignorant of how those regularities are produced, and even while being quite confused about the difference between phenomena which you can thus anticipate and those which you cannot. The Babylonians, as we all know, could predict eclipses and so on; but they betrayed a lack of scientific understanding in expecting to be able to do the same for locust-plagues and earthquakes. The Ionians, however, were able to predict very little, but nevertheless understood for example, the relation between the sun and the illumination of the moon in a way that never struck the Babylonians. Nor when theoretical understanding does emerge, alongside predictive skill, does it necessarily lead to more accurate forecasting. It seems that post-Newtonian astronomical calendars and tide-charts were still for a long time compiled most accurately by actuarial (that is to say 'Babylonian') methods.

As to the second point, the sense in which Newton 'predicts' the diverse observations and relationships (about planetary orbits, tides and apples) which he explains, is a curiously elasticated one which has to embrace hypothetical prediction, 'predicting' what we already know and even 'predicting' the past. The same goes for that other great system, Darwinian evolutionary theory. But if 'prediction' now covers 'showing that what we know to be the case is a logical consequence of a theory' when that theory itself is necessarily derived from what we know to be the case (and not exclusively from other things that we know to be the case), then the idea of predicting something separate from what we know, in order to test a hypothesis about what we know, has given place to showing that

<u>many</u> things which we know are consistent with a particular hypothesis about <u>some</u> things that we were trying to explain.

The latter is what the analyst does, notoriously, when he shows at case-conferences that additional observations (from psychological tests, social workers, teachers) exemplify the same psychopathological patterns as outlined in his own clinical <u>DI</u>. Now, the experimentalist critic encourages us to believe that, in the case of a genuine theory, by contrast, this demonstrable consistency derives from the supporting evidence being shown to be precisely what is entailed by that particular theory (... what that theory 'predicts'). But this is misleading, because it presupposes knowledge of all relevant conditions. Darwinian theory can surely show in retrospect that some phylogenetic trends are the sort of thing you would expect, or what it would 'predict', in certain circumstances (cp. ch.5 (b)); even perhaps that they are precisely what it would predict if the circumstances had been <u>b</u>, <u>d</u>, <u>f</u>, rather than <u>a</u>, <u>c</u>, <u>e</u>, Since, however, we usually do not know whether the relevant conditions <u>were</u> in fact those rather than these, we do not know what precisely we should have predicted. But this does not of itself prevent the theory being explanatory, in spite of Wittgenstein's remarks as to its general character.

Another obstacle to predictivism, in the case of <u>PAN</u>, is that its component observations and generalisations are about many different sorts of thing. Sherwood has shown in detail (1969, pp.185-202) how Freud's <u>PAN</u> about Lorenz's behaviour involves four or five different <u>kinds</u> of explanation. Some are concerned with events, situations or reactions which are the "source" in time of certain behaviour, feelings, attitudes etc. ('origin'); some with how certain feelings and so on come to produce certain symptoms, habits, fantasies etc. ('genesis'); some with the 'motive' or 'purpose' for actions; some with how certain attitudes, beliefs and fantasies serve to keep a balance of emotional forces within the personality ('psychic economy'); and, finally, some with the more-or-less symbolic 'significance' of actions, phrases, images and the like. But some of these types evidently depend on hypotheses which have no predictive implications. Thus to explain Lorenz's compulsion to diet, that is, to get rid of fat (dick in German), as 'signifying' his jealous wish to get rid of Gisela's cousin Richard (called "Dick", à l'anglaise), is self-suffieient and carried no implications about further symbolic actions. Likewise the 'psychic economy' account of the resurgence of Lorenz's belief in an afterworld, in terms of mentally 'undoing' his father's death in order to balance the guilt of once having wished him dead, implies only the general contention that such mechanisms are used and can be recognised; and this makes good enough sense, without entailing anything about how they will be used by particular people in particular situations.

These are but extreme cases of the general diffeculty, noticed above (ch.5 (b)), that a PAN typically has to rely or rather loose tendency-statements by way of law-like generalisations. And a reason why one should not expect to be able, even 'inprinciple', to apply Braithwaite's correction and convert 'a's tend to be b's' into 'All a's, provided that C, are b's', has been advanced by Sherwood (1969, p.215). Suppose that we did have a generalisation, which we proposed to test predictively, to the effect that all people with a certain conflict, motive, anxiety or whatever, designated by a, would in specifiable conditions ($C_{f, q, r}$), act in a certain way (b). This presupposes that we can identify conditions f, q, r, independently of, and prior to, observing behaviour b; but it is a feature of the kind of material with which a PAN is concerned ('intentional', Sinn -laden and even symbolic actions) that the occurrence of action b is often the first and only evidence that there is as to whether f, q, r, were, or had been, in fact operative. Again it may have been something like this consideration which prompted Wittgenstein's seeming denial that law-like regularities could be formulated for thoughts and actions of any human significance. Sometimes the problem is still more radical, in that the action needing interpretation is the only evidence we have both of the underlying conflict itself and of the conditions, psychical and environmental, which have influenced the form in which it comes to be expressed. This would amount to having to identify, as it were, both the message and the code in one set of data without independent check either as to whether P did send message x or as to whether P does use code y. But that should give no cause for alarm: we do the same kind of thing everyday. If I switch on the radio and happen to catch the last phrase of a broadcast talk, I may hear some speech-sounds which could be taken either as "oil-taker" spoken in a 'standard'

English accent or as "I'll thank her" spoken in a heavy Irish accent of some kind. We do not need, however, in order to settle the question to phone up the speaker or the radio company and ask what he actually said, thus checking directly and externally the hypothesis that what he said was _p_ and not _q_. We can usually get enough information for a decision from the contextual observation of what the announcer says afterwards. If he goes on to say "That was Seamus O'Leary talking about his childhood in Cork", this supports one interpretation rather than the other. To go on to ask _how_ it does so would be to anticipate the argument of ch. 7. The present point is simply _that_ it does; and that other announcements giving a less definitive pay-off, would still provide differential support in their various ways. In this respect they are all members of a nebulous class of observations which provide retrospective contextual evidence about the questions of the speech-content and of the accent. Consequently it is a mistake to suppose that we need to be able to identify the 'medium' directly and independently (let alone antecedently) in order to be able to recognise the 'message'; or _vice versa_. The two unknowns _can_ both be evaluated concurrently in the same set of data (cp. ch.4 ( )).

A final difficulty for predictivism, and one which raises problems for the assessment of evidence, is that _PAN_ depends, as we have already remarked, not so much upon telling _more_ of the story as upon telling a differeht _kind_ of story. Thus it fits together, and makes sense of, a sequence of the patient's actions by seeing them as, for instance, an exercise in preserving his 'psychic economy' in the same sort of way as the anthropologist, in Sherwood's example (1969, p.15) explains the aboriginal tribe's habitual migration-route, which makes not geographical, ecological or climatic sense as a way of travelling from A to B, by showing that it all fits together if you _see_ it _as_ a re-enactment of the migration-route taken, in the tribe's creation-myth, by their totem spirit. Wittgenstein, however, contrasted Freud's reliance on "redescription" and "simile", as he regarded it, with explanation proper; and he seemed to concede no more than that Freud, like Darwin, had shown how to organise or "arrange" a great variety of material which still remained to be 'explained' (Moore 1955, p.316).

But we have argued above that a good deal of the most proper 'scientific' explanation depends upon such redescription, simile and analogy, especially when the subject-matter to be explained is relatively inaccessible to observation and conceptualisation (ch. 5 (c)). And the theoretical function of the 'modal transform', to which we appealed at that point, is not merely to suggest re-groupings and re-alignments of data in an autonomously "redescriptive" way: to suggest, that is, that we put accidents in the same bag as wishes. It is to do this in order that (negatively) we may avoid the mistake of assuming that the behavioural significance and underlying mechanisms of appointment-forgetting are quire different from those of daydreams and phobias, and (positively) that we may be pointed towards the kind of mental 'deep structure' and 'transformational' principles, if we may now allude to Chomsky, which are mecessary to generate all these sorts of behaviour. There can be no doubt that Freud's system, and psychoanalysis generally, includes many hypotheses, with all their limitations of language and imagery, both about such 'deep structure', and about the generative and transformational principles which determine the form taken by thoughts and actions in expressing particular feelings, attitudes or conflicts. The former would comprise basic formulations about the motivational and developmental character of libido and about the regulatory roles of ego and superego systems; the latter would be to do, for instance, with the 'primary' and 'secondary' processes of thought, and with defence-mechanisms.

Nevertheless, the first move in itself (the redescription, re-alignment, the 'seeing as') seems both to invite and to defy questions about the deployment of evidence. For it seems reasonable on the one hand, to ask what is the evidential basis for claiming that this behaviour-sequence represents an attempt to balance the 'psychic economy'. We are inclined to think that there must be some facts or observations which would be otherwise if the claim were not true: to think, that is, that it is susceptible of evidential support or refutation. And yet, on the other hand, it seems clear that there are no particular observations which the claim implies or 'predicts'; and the same goes for claims about 'significance' which, as we have just seen, play an important part in PAN. All the same, we must avoid saying that the reasons (rather than the evidence) for looking at the behaviour this way rather than that are quite independent of empirical considerations. For if this 'way-of-looking' is to lead to any empirical understanding, as we intend, then there must be some facts (we feel) to which it does more justice than do other 'ways-of-looking', or some possible observations which support it rather than such other ways. If a PAN aims to make sense of the facts, by reference to explanatory principles of any generality, then its contributory techniques (that is, the various kinds of argument and assumption which go to make it up) must have some factual implications outside the specific *explananda*. But such implications, and their corollaries in evidential support, are not necessarily as immediate and explicit as predictivist critics of PAN tend to suggest.

(c) Criteria for 'evidence'

A glib demand for "the evidence", coupled with an excessive admiration for certain forms of it, is characteristic of a naively experimentalist attitude to the scientific understanding of behaviour: an attitude which owes much to the prevalence of the "myth of deductivism". It is characteristic also of a belief-system based on dogma rather than reality that various taboos and totems replace rational consideration. Observations are accepted or rejected as valid 'evidence' according to whether they boast a certain property, provenance or pedigree, and not according to the role they play in a particular argument. Thus psychology students have been taught, within living memory, to despise evidence which could be categoresed (and therefore condemned) as 'anacdotal','introspective', 'subjective', 'clinical', or 'qualitative'; the totem properties, on the other hand, have been 'objectivity' (unspecified, but played off against the taboo of 'subjectivity'), 'quantifiability', 'predictability', 'repeatability', 'publicity' and 'experimental control'.

Many of the latter criteria are not, of course, necessarily met by the paradigm experimental situations in the "hard" sciences whose methodology the psychologists were trying to ape. But even if they were, it would be absurdly doctrinaire to transfer them en bloc: for it must be apparent that, depending upon the nature and form of an empirical argument (and we have noticed the various kinds of argument which a PAN draws on), evidence of different sorts and from different sources becomes relevant. I mean simply that, if a patient says "I know your desk is not really untidy, but if it were mine, I should have to straighten this book, that paper etc...."; and if he also gets angry when anyone so much as refers to the tag "le coeur a ses raisons ..."; and if he writes an artificially precise and orderly hand, these are different kinds of evidential manifestation of a need for intellectual impulse-control. And there is virtually no limit to the aspects of the patient's behaviour and experience which might provide further evidence in particular cases. But some experimentalists, as we shall see shortly. seem to suppose that all 'good' data must share some set of properties, regardless of their context and intended use. A second aspect of the use of evidence which varies with the pattern of argument to which it contributes is the logical relation which it bears to the point (premise, inference) which it supports. To go back to Farrell's "new neighbour": our observation that he has recently lost his wife supports the hypothesis that he really is a keen gardener (if only in the sense of discouraging us from rejecting that belief) in a logically much more indirect way than does the observation that his household effects contain much gardening equipment.

Consequently, since there is more than one pattern of argument involved in a typical PAN, we must expect both that various kinds of observation and factual consideration will have evidential weight, and that there are several variants of the logical support-relation between them and what they are evidence <u>for</u>. They may carry weight, for example, not through the medium (as it were) of 'tight' generalisations like 'Kite-type memories are produced only by people of homosexual disposition', but through that of a looser one like 'Kite-type fantasies tend to be expressions of homosexual disposition' (cp. ch. 7). Certainly we shall not be confined to the deductivist ritual of matching specific experimental observations against specific 'predictions'; and we shall even see that the logic of <u>that</u> procedure is not entirely free from the problem of relevance which <u>PAN</u> conspicuously faces (cp. section (d) below).

The would-be 'scientific' study of human (or indeed animal) learning, perception, memory, problem-solving and the rest was not very old before voices were heard stipulating what sort of 'evidence', or rather observation-report, was to be admissible. Leaving aside the sundry ideologies of behaviourism (Mace, 1946), we find Pavlov decreeing that the language of evidence in his laboratory is to exclude all terms attributing 'mental' states, processes etc. to the experimental animals. Hyman (1964, p.41) quotes a passage in which Pavlov gives the reason for his decree as the need for inter-observer reliability and explicitly says that "the use of such psychological expressions as the dog guessed, wanted, wished etc." was "prohibited"; indeed, Hyman represents the whole situation as Pavlov having previously been disconcerted by "the fact that two experimenters dealing with the same experimental situation could report different 'facts'".

But if certain ways of talking are banned, it follows that certain kinds of evidence will never see the light of day, and that we shall develop an incomplete and distorted view of the subject-matter. We shall mislead ourselves not only as to how it is to be understood, but even as to the nature of <u>what</u> is to be understood. Historically, much British and North American psychology has taken such pains to avoid an anthropomorphic view of animal behaviour that, in extrapolating from animals and turning blind eyes to the phenomenological tradition of the Continent, it has ended up with a zoomorphic view of man. So set has this attitude become, that considerable energy and ingenuity has to be expended in countering it, and in contending that, after all, "Man may be treated, for scientific purposes, as if he were human" (Harré and Secord, 197 , p.).

Some commentators, indeed, have been bold (rash or insightless) enough to set up criteria which observations must meet in order to qualify as 'scientific' data and therby aspire to the status of evidence. Thus Sidman assures us (1960, p.3) that "good data are always separable, with respect to their scientific importance, from the purposes for which they were obtained", without foreseeing the obvious objection that if you do not entertain certain purposes you will not get certain kinds of data at all, let alone "good" samples of their kind. Indeed the defects of Sidman's subsequent discussion are so conspicuous that its continued influence in some quarters, as an authoritative source for experimental method in psychology, is remarkable. For he soon goes on to tell us, for example, by way of refuting the charge that behavioural data drawn from laboratory experiments are artificially selective, that "the laws of behaviour may be expected to hold true inside the laboratory" (p.26), without divulging the grounds on which this "expectation" is based. From a methodological point of view, this assumption is steeped in (unspecified) theory; for why <u>should</u> the laws of non-laboratory behaviour hold for a laboratory situation? And, in point of <u>fact</u>, we happen to know, from follow-ups of Milgram's notorious work, that very significant meta-laws (specifically, those governing a kind of 'schizoid denial') can operate in the psychological laboratory to override the corresponding laws of 'real-life' behaviour (Argyle 1969, pp.19-20). Finally, we can gauge the general depth and quality of Sidman's study of evidence-assessment from his approach to the treatment of 'induction'. "Afew words, therefore, about induction", he writes (p.59), "which I have adapted from Polya's fascinating little book ... ". This leads, on the same page, to the stupefying dictum: "Induction is a behavioural process, not a logical one, which is the reason logical analysis has failed to account for it". Now the contrast here is flagrantly confused; for the fact that adding up a bill is a "behavioural" process does not prevent it also being a 'mathematical' one. What makes a process 'logical' is not its having some non-behavioural property, but its relation to other processes which (onSidman's terms) are equally "behavioural".

In the history of psychology, such doctrinaire exclusivism, with its attendant catechism of 'standardised' procedures and 'controlled' conditions, was soon, like the medieval theological dogmatism which it resembles, to be embarrassed by the facts. For it eventually became inescapable that at least <u>some</u> behavioural observations, which systematically failed to wear the right tie or come from the right school, nevertheless provided rich <u>evidence</u> about all sorts of things. These sources of embarrassment were, on the one hand whole <u>genera</u> of investigations like the 'naturalistic' studies of ethologists and anthropologists; and, on the other, the specific method (or lack of method, as he himself regarded it) of one Jean Piaget. Now, the catechism says "no 'good data' without 'control' ": so, if this sort of data that is, naturalistic and Piagetian is useful and can attain evidential status, there must be 'control' somewhere. Well, the conditions are not controlled, since the whole point is that they should flow and develop naturally; so it must be the observer who is 'controlling' <u>himself</u> by <u>not</u> influencing them! (Hyman 1964, pp. 42-46). Even so, in Piaget's so-called 'clinical method', the experimenter's next question or move is 'controlled' (at least in the confused Skinnerian sense) most significantly by the child's previous answer and only trivially by the experimenter himself.

This is an instructive paralles to the fallacy, noted above, of setting out academic prerequisites for what is to count as an 'explanation', rather than looking to see (naturalistically, if you like) what sort of account would, if true, have explanatory force for a particular kind of behavioural data, and then trying to schematise the properties of that account. For what we should learn to do in the case of 'evidance', is to consider what observations etc. actually <u>do</u>, or <u>would</u>, carry evidential weight in the particular investigation we are concerned with, and then to ask, if necessary, in virtue of what they carry that weight. From this it would be clear that data, of various sorts, acquire evidential status not from having certain fixed categorical properties but from bearing upon a question, in a variety of ways. Indeed, psychology is sometimes said to suffer from having too many facts and too little evidence. Clearly a fact which bears upon no relevant hypothesis has no evidential value; however, as Deutsch has pointed out (1960, p.169), the fewer hypotheses the fact is consistent with, the greater is its evidential value. But we can go further, and say that this value is greatest when it bears upon many but is consistent with only a few (at best, with only one): because in that case it is also helping to refute alternative contenders instead of merely being irrelevant to them. But do we not need to be able to characterise this relation of 'relevance' in some logically systematic way?

(d) The metaphysics of relevance. We have seen that it is not their having certain categorical properties or being derived from a certain source which converts observations into "good data" in respect of evidential status. They are "good" in so far as they impinge reliably upon a question or problem; and the ways of doing this seem to be many and diverse. Nor does it seem possible to specify in advance, with this sort of behavioural material, the range of possible observations that would count as relevant evidence. And yet we feel that there must be some principles, if not logical then at least tactical, governing a judgement that _this_ is relevant and _that_ is not; and that such principles should be specifiable. But perhaps this expectation is symptomatic of a pre-Wittgensteinian formalism, in that, if the concept of 'relevance' has the well-known characteristics of that of a 'game', we outht to stop expecting the way that _this_ observation is relevant to this hypothesis to have any particular feature in common with the way that _that_ fact or consideration is relevant to that problem. let us compare a borderline case, where the question of relevance is at best problematical, and where it is not clear what we do have a logical right to say and why, with some others where we seem able to be more definite.

When Gustav Mahler was consulting Freud in 1910, he (Mahler) proposed a _DI_ about why his music tended to lapse into banality immediately after, or indeed instead of, bringing a passage of emotional intensity to a climax. The _DI_ was to the effect that his musical habit reflected a childhood experience when Mahler had been distressed by his parents quarreling violently in the house, had run out-of-doors for relief, and had come across a barrel-organ playing the tune of a Viennese popular song in the street. Thus do the banal "barrel-organ" episodes in his symphonic compositions come to provide, on classical 'secondary reinforcement' principles, a form of escapist tension-reduction when the musical passion is riding uncomfortably high. (Never be it said that Freud did not accept, when appropriate, explanations which are expressible in terms of straightforward 'conditioning' theory.)

Now, there is an apparently quite independent biographical observation, which has been advanced as relevant to this D.I. (Mitchell 1973, pp.xv-xvi). It is Mahler's wife's report that, when they were in New York some two or three years earlier, a barrel-organ had started up in the street below their flat and she had had it moved on so as not to disturb the composer at work in another room. "The noise stopped at once. Then Mahler burst in: 'Such a lovely barrel-organ - took me straight back to my childhood - and now it's stopped!'" This separate corroboration that Mahler had (or thought he had) a childhood barrel-organ memory does not, of course, substantiate the D.I. that it was causally related to a stylistic habit of composition. But it is hard not to think it relevant to it. On the other hand, by virtue of what general principle of relevance is it so?

Suppose we try to say that, since the D.I. is about the relation between a childhood memory involving certain elements (parental quarrel, barrel-organ, the tune '*Ach, du lieber Augustin*') and a quirk of musical composition, any observation on his feelings towards those elements will be relevant. This, however, encompasses at once too much and too little: too much, because not all such remarks (e.g. that he thought the time was sentimental or jolly) would bear upon the question; too little, because many observations not specifically to do with those "elements" (e.g., perhaps, his erstwhile scorn for the *bel canto* style of singing, or his attitude to other people's quarrels) will also carry weight. Thus if we try to limit the scope of potentially relevant observations to the range of informal implications, or referential repercussions, of the D.I., we soon find that that is embarrassingly nebulous and elusive. This is the difficulty which 'operational definition', that deus ex machina of experimentalism, attempts to forestall by prescribing artificial boundaries of reference for behavioural terms (see ch.8 ( )). But we must notice in passing that philosophers have found it no straightforward matter to specify even what clear-cut empirical propositions of a non-'intentional' kind, such as this experimental reductionism aspires to employ, are 'about'; and consequently it is not easy to infer what observations are or would be relevant to testing their truth.

Without going into all, or even many of, the ramifications of the notorious "paradox of the ravens" and its associates, it is salutory to reflect that, if an observation which is consistent with p cannot well be said to irrelevant to p, it follows that the class of observations which are relevant to the truth of 'All ravens are black' is not limited to observations of ravens (Ayer 1972, pp.54-88; Cohen 1970, pp.95-105). For if I look around the world to check up on this proposition, anything that I see, so long as it is not a non-black raven, is consistent with the hypothesis that 'if a thing is a raven then it is black', and thus helps it to resist refutation. Indeed, if I have to scrutinise every non-black thing in order to establish that it is not a raven, this, along with Russell's hypothetical reformulation which we have just introduced, suggests that the implicit reference-range of the proposition is not the class of ravens but the class of things. It is thus a statement not merely 'about' ravens, but 'about' the contents of the world. Conversely put, in Hempel's example, if it is appropriate to test whether an unidentified substance is 'sodium salt' by seeing if it burns with a yellow flame, this arguably shows that the generalisation 'sodium salts burn yellow' is in some sense 'about' substances and not just about sodium salts. If such questions of relevance are raised even by standard universal generalisations, it would be foolish to expect a short and clear-cut answer to those raised by the use of our much more problematical 'A's tend to be B's' and 'Y's are an expression of X'.

However, if the logical principles are not easy to specify in theory, yet we have seen in practice, in the Leonardo study, how some observations which were once thought relevant and explantorily constructive, can be shown difinitively to be neither (e.g. the vulture-symbolism); and we have also noticed how, as a kind of halfway-house between relevance and irrelevance, the concept of 'overdetermination' retains some evidence as relevant to understanding da Vinci's personality, even though, on the art-historian's separate and self-sufficient account of the artist's actions, it alcks that relevance. It is possible, of course, to be even more explicit about what actions or observations impinge upon a problem, if we consider a very minutely defined question and set artificial limits to the range of actions open to the agent in respect of it. In investigations of the practice of deductive reasoning, such as those carried out by Wason et al (1968, 1969) in which the subject is faced with four cards, each showing either a letter or a shape, and with a hypothesis like 'All circles have a vowel on the back', we can say precisely which cards in a given display it is necessary and relevant to turn over in order to check the hypothesis. ~~(Wason, ed. 197 , pp.)~~ Here we can sharply distinguish what might be called "psychological relevance" (by analogy with Sherwood's 'psychological puzzlement') from logical relevance. For it is notorious that many subjects mistakenly feel that they need to turn the card exposing a vowel, to see if it has a circle on the back. But it is only because we have put artificial constraints on how the hypothesis is tested that we can be so specific about relevance: if we had not, the range of relevant actions would include such moves as bribing the technician who made the cards. And, even given this limitation and accepting that the question is 'Which cards do you need to turn to check the hypothesis?', it may still be relevant to turn an 'irrelevant' card if I have arranged with the technician to have the answer written on the table under the extreme right-hand card.

But it is for just this sort of reason that the experimentalist encourages us to convert the law-like statements on which our D.I.'s rest, from terms to do with the 'expression' of feelings and motives into terms to do with particular actions, objectively characterised, in particular circumstances. Even supposing that this could be done without loss of content, without distortion and without an absurdly arrogant prescriptivism (cp. Skinner 1954,; Taylor 1964, pp.88-90, 95-97), it would still be naive to think of it as transferring these poor floundering statements to a realm free from problems of evidential relevance. In any case people do not live in a laboratory world; they live, act, think and feel in the real world, of which the laboratory world is a highly unrepresentative part. And, however useful it may be to try to test our general hypotheses, about the behavioural expression of mental states and processes, by working out their implications for particular artificial situations, the fact is that we want to be able to _use_ them to account for non-artificially generated behaviour.

We therefore need to be able to face problems of relevance posed by actual behaviour in the actual world, that is, in its natural habitat; and by the fact that our background theory deals in tendency-generalisations, and in statements about the expression or transformation of psychic processes in overt bhaviour. For it must be evident that _DI_ typically appeals togeneral hypotheses whose logic is a good deal more problematical than attributing a particular colour-predicate to a quantified subject (as in the ravens' paradox). Such generalities take the forms: '_x_ tends to produce _y_'; '_y_ reflects _x_'; '_y_ is a form of _x_'; '_y_ is an allusion to _x_'; '_y_ is a denial of _x_'; '_y_ is an expression of _x_';

'<u>x</u> leads to <u>y</u>' and so on. How then do we, in practice, set about tackling the question of what evidence is relevant to checking the claim that certain behaviour is an 'expression' of some particular mental process, or a 'transform' of some emotional state, or an instance of some 'tendency'?

Chapter VII

Tactics of Linguistic Understanding

(a) Tendency and 'arguing back'
(b) The analogy of translation
(c) Dimensions of context
(d) Value-added, and the reduction of unlikelihood

"Another of his early words was 'Down'. If he was being carried, 'Down' meant 'Put me down'. If he wasn't being carried, 'Down' meant 'Pick me up'." (Holt 1969, p.63)

It has become clear, in the course of the inquiry so far, that the rationale on which much DI implicitly rests fails to meet some of the demands which are traditionally (though uncritically, I want to say) made of the logical procedures and background hypotheses which should underpin acceptable empirical explanations. In particular this rationale both depends upon unquantified tendency-generalisations and often consists in identifying _a posteriori_ (and without the possibility of specific predictive check) various allegedly significant patterns within the data to be explained. It often trades, furthermore, on propositions asserting the relation of _expression_ between observed _y_ and observed, or postulated, _x_, rather than the more familiar relations of 'cause' or demonstrable 'correlation'. We are asked to believe, that is to say, not so much that _y_ is caused by, or known to be correlated with, _x_; but that _y_ can be in some extended behavioural metaphor, an expression of _x_. And we have found ourselves plunged into the problem of what sort of observations etc. have evidential status _vis a vis_ the latter sort of claim as opposed to the former, - which, although more familiar, we have seen to be by no means problem-free.

Piecemeal examples have already been given of how some questions involving these limitations are dealt with in other types of empirical inquiry. We must now look more closely and systematically at some characteristics of the reasoning which is involved in such transactions, bearing in mind that the affirmative aspect of the argument here is still largely double-negative in principle. That is, the logical skeleton of the thesis is, for the most part: If certain objections to _P_ (_D.I._ and cognate psychodynamic procedures), on the ground that they have features _x_, _y_, _z_, were valid, then we ought also to dismiss _Q_ (certain analogous empirical enterprises), which also depend on x, y, z; but Q evidently can be both rationally and empirically sound; therefore we need not dismiss P for the given reason.

(a) Tendency and 'arguing back'. The background hypotheses and generalisations on which D.I. rests often do not readily lend themselves, as we have seen, to the kind of direct and specific check which some critics require of scientifically useful propositions (but cp. Cheshire 1973a, p.98); consequently, any particular D.I. is, on this view, more-or-less insecurely based. The simplest, and negative, course is to discount them as a source of empirical understanding on this ground, and to risk throwing some epistemological baby away with the methodological bath-water. But the positive alternative, the question of what can be done instead in order to assess the validity of psychodynamic hypotheses and use them coherently, is by no means so well understood. Indeed the contention that there is no intellectually respectable alternative open to the psychodynamist has already been met (pp.00-00 above). The contention, that is, that he is bound to ape the tactics of the experimentalist, only to be found logically or empirically wanting, because his induced law-like generalisations are either not universal or not specific enough to sustain the sorts of inference that he is supposed to want to make from them.

It is a mistake, however, to assume that the psychodynamist, when framing and justifying his D.I. in terms of generalisations which are admittedly loose, necessarily relies on such patterns of inference as one appropriate only to more rigorous premises, and that he is doomed to fallacy on this account. In a phrase which Eysenck turned against Bernard Shaw in another context, let us look at "a good rousing summary of these misconceptions" as made by Eysenck himself (19 , pp. 227-239). In this passage he is concerned to persuade us that to "argue back" from observed behaviour, and specifically from the data of 'projective' personality-tests such as Rorschach and TAT, to "the cause of factors which are responsible for our action is an exercise "based on a logical fallacy"; and this accusation of logical fallacy is made three times in the course of the three-page argument. Thus does Eysenck echo the sentiment of Pope, who had written (<u>Moral Essays</u> <u>i</u>, 99-103):

> "In vain the sage, with retrospective eye,
> Would from the apparent <u>what</u> conclude the <u>why</u>,
> Infer the motive from the deed, and show
> That what we chanced was what we meant to do."

Now, there is no dispute that psychodynamists do want to be able to argue back in this kind of way; what I am disputing here is that such argument <u>necessarily</u> involves the illicit conversion of the major premiss of a simple syllogism (All A's are B: therefore this B is an A). Eysenck can, of course, produce examples of such 'arguing back' which, <u>as he represents them</u>, do exhibit such a fallacy. But the questions at once arise whether the implied scheme of those particular arguments could be represented otherwise; and whether, even if they cannot, this is the sort of arguing back on which D.I. and PAN necessarily depend. Obviously the answer to the second query (the one that matters) is 'no'; because, while false conversion is a form of arguing back, it is not the only form of it. Nor is the psychodynamist's range of logical schemes restricted to isolated syllogisms whose major premises are insufficiently rigorous to do their job (cp. Sherwood 1969, pp. 231-244).

Specifically, Eysenck complains that "all the projective techniques", and, by implication, all D.I. which might be based on them and on analogous obervational procedures, take the fallacious form of his well-known illustration about sports-car buyers. The paradigm syllogism attributed to the psychodynamist in this illustration is:

(1) Sporting young men buy Jaguar sports cars
(2) This man has bought a Jaguar sports car
(3) Therefore, this man is a sporting young man.

This is clearly invalid; and it is only a tightening-up of the loose major premiss that will enable a valid deductive inference to be made from knowledge of what car a man has bought. Thus, if we tighten it to '<u>All</u> SYM buy JSC', then the observation that he has <u>not</u> bought a JSC warrants the conclusion that he is <u>not</u> a SYM; while the observation that he <u>has</u> bought one still tells us nothing deductively. It is only if we tighten premiss (1) to 'All SYM, only SYM, buy JSC' (that is, to

The bi-conditional 'If and only if A, then B'), that the affirmative observation of premiss (2) acquires implicatory force.

But it is plain that the generalisations of psycho-dynamic theory have not yet been refined to a corresponding degree of precision and exclusiveness, and it is probably in the nature of the behavioural material that they never could be. For the variables, which determine whether someone with motive, anxiety or conflict A actually produces behaviour B as a consequence or expression of it, are many, subtle and diverse; quite apart from the complication that what is to count as "behaviour B" for the purpose of psychodynamic theory would have to be conceptualised 'intentionally' in terms, such as 'greeting', 'threatening' or 'avoiding' etc., rather than in pseudo-laboratory specifications.

Indeed, in order to make the car-buying illustration at all realistic, we should have to be content with 'SYM tend to buy JSC'; because it is obvious that quite a number of SYM cannot afford to buy JSC, even if it were broadly true that SYM want to buy JSC. Here, at least, we might apply Braithwaite's correction for reducing tendency-generalisations to universals (p.00 above), and read 'All SYM, provided that they are wealthy enough, buy JSC.' And the psychologist is sometimes able to take crudely corresponding steps, as when Bowlby eventually tightens up 'Infants who experience maternal deprivation tend to develop affectless psychopathy' into something nearer 'All infants who experience MD, given

given that it occurs within the critical period and in the absence of adequate substitute mothering (Braithwaite's '... given C $_{\underline{a}, \underline{b}}$'), do become AP's".

But, granted that the psychodynamists's "laws" are for the most part still at the looser 'tendency' stage, and destined to remain so for a long time in practice if not perhaps, in the nature of the case, for ever, nevertheless it does not follow that their use in 'arguing back' operations necessarily involves "logical fallacy". The reason is they do not have to function as the major premise of a single unsupported syllogism which commits the fallacy of 'asserting the consequent'. For that is not the only way they can be used, not is it in fact the way they typically function in the arguments which underpin a DI or contribute to a PAN. In short, the imputation of fallacy is valid if, and only if, the logical schme on which such formulations depend has been represented accurately. Insofar as it has not been, if only because the part played by appeals to context, coherence and subsidiary generalisations is not considered, to that extent Eysenck's attack achieves no more than a hollow victory over a particularly slender straw man.

A further small point, which shows perhaps that he is not particularly concerned with the accuracy of his analogy between the sports-car syllogism and the reasoning of the psychodynamic interpreter or projective-test user is this. Eysenck writes (op.cit., p.228), "In other words, buying a Jaguar sports car is regarded as a kind of projection test ...". But if there is to be the remotest parallelism here, then the situation that corresponds to the projective test is not buying the Jaguar but rather the presentation, or existence, of a range of different makes and models from which to choose. The selection of a Jaguar from such a range corresponds, if anything, to the projective test record or protocol, which consists of the perceptual and apperceptual selections made by the patient from the range of material open to him. But the analogy is still precarious, even in this revision. For, what has our SYM "projected", in his case, on to the stimulus-material (that is, on to either the car of his choice or the range of possible cars)? He has in no sense perceived, re-structures, re-organised it in his own more-or-less idiosyncratic way: what he has done is to associate himself with some properties of it rather than others. This may be partly what Klein means by 'projective identification' (cp. Segal 1964, pp.42-53); but on the face of it the whole business looks more introjective than projective, and puts one in mind of Ferenczi's analysis, in terms of introjection, of Jung's word-association technique. For Ferenczi (1909, p.51) argued that "it is not that the stimulus-words evoke the complicated reaction, but that the stimulus-hungry affects ... come to meet them: ... the neurotic introjects the stimulus-word of the experiment".

Be that as it may, our purpose at the moment is to show that there may be 'arguing back' which is based on relatively (or indeed, very) loose premises, but is not necessarily fallacious. To this end let us examine the functioning of a semantic procedure which (a) is non-scientific, (b) employs premises of the loose forms that we are concerned with, and yet (c) is capable of generating conclusions that can be regarded with considerable (sometimes even complete) confidence as correct. Such a procedure is translation from one language to another; and especially, for the sake of the comparison, translation from a dead language. For there are specific features of the translation situation which resemble rather closely those very aspects of psychodynamic propositions and logical tactics which are sometimes supposed to vitiate the arguments in which they figure. In so doing we take up some implications of Freud's injunction to learn "the language of the Unconscious", and try to be rather more specific in working them out than Rycroft (1968) is when he contrasts this kind of approach with "causal" arguments.

(b) The analogy of language. When Jackson renders the first three words of the *Aeneid* as "Arms I sing and the man ... ", this can be treated as an empirical claim of the form: 'When Virgil wrote *arma virumque cano* he meant (or, intended to convey the idea) such-and-such'. It is in fact an instance of arguing back from a presented datum, in this case the Latin text, to the ideas and intentions that gave rise to it. But although it is thus a species of empirical explanation, connecting present observations with postulated antecedent determinants, there is evidently no question of deducing particular consequences from the 'hypothesis that by '*arma ...*'

Virgil meant 'Arms ...', and then running an empirical study to see whether in the stipulated conditions such consequences are observed to occur. We cannot control such conditions retrospectively, any more than we can wind back a patient's psychopathological development to see 'what would have happened if ...'. But the very idea of so doing raises a conceptual question whose behavioural corollary we have already met. What would the consequences of such a hypothesis look like? I mean; what are the differential implications of postulating, on the one hand, that Virgil meant 'Arms and the man ...' and, on the other, that he meant 'Ships and the man ...'? Everything that he goes on to say, and the subsequent history of the world in general, is consistent with either. (Other translations of Virgil, and the title of a Shaw play, do not count; because it could be _they_ that are based on a mistake, like Milton's "blind fury" who, in _Lycidas_, usurps the role of Atropos and unclassically "slits the thin-spun life".) Even the implications for the meaning of the word _arma_ do not entail that it will ever again be used to mean 'ships'. The behavioural corollary is that, when a _DI_ postualtes some anxiety or motive, the nature of the theory is such that there may be no differential implications for the way that feature will be expressed in particular actions outside the data on which the _DI_ is based.

As regards the translation, then, there are any number of consequences which, consistently with the meanings of the words, could be derived with equal cogency; and sometimes this range of consequences would include, as in psychodynamics also, mutually conflicting ones. And if we look for watertight generalisations about habits of word-usage, we shall find that it is the exception for a word to be used in a uniform way, or in such uniform circumstances that its otherwise variable meaning can always be fixed by reference to them. The meaning behind a particular use of a Latin word is not deducible from a rigorous generalisation about all instances of that word. The translator's conclusion, therefore, is not inferred from premises as logically tight as 'Nobody but a SYM buys a JSC', which Eysenck apparently thinks essential to rational arguing-back. So, assuming that translation is, or can be, a rational and empirical enterprise, it follows that at least one form of arguing back does not draw its epistemological respectability from exclusive, covering-law generalisations. Let us inquire a little further into how it is done.

The generalisations which a translator uses, in reaching his conclusions about what a passage of Latin means, are not of the form: 'Every instance of the word *mensa* reflects the idea of "table".' For although a minority of technical terms may possibly admit of an approximation to this degree of rigidity, it is obvious that most words can convey more than one idea. It may be the case that *curculio* always means 'boll-weevil'; but it is not even true that *et* always means 'and'. For the most part then, the translator cannot rely on arguments which follow the scheme:

(1) Instances of the word 'X' always convey idea 'Y'
(2) This is an instance of word 'X'
(3) Therefore this means 'Y'

Indeed, from the point of view of syllogistic reasoning, his raw-material looks hopelessly unpromising. He has to be content, for the most part with generalisations like, 'X usually means Y, but sometimes means Z'; or like, 'X can mean almost anything to do with Y: such as Ya, Yb, Yc, etc.'. Looser still, some of them even resemble, 'X sometimes means Y, sometimes Z, and sometimes P with about equal frequency'. So that, if the translator's implied scheme of argumentation were restricted to what Eysenck attributes to the psychodynamist framing his DI, he would obviously never be able to arrive rationally at conclusions such as, 'When Tacitus wrote arcana imperii he meant "secrets of government"'. But since we do regard translating from Latin as a rational and fact-stating enterprise, and the translation of this particular phrase as rational, fact-stating and true, the translator must have other logical cards up his sleeve. And if he does, why should not the psychodynamist also play a similar hand? Clearly the nature of these supplementary cards is worth investigating.

In order to do so instructively, we notice some further respects in which the looseness or flexibility of the translator's premises resembles that of the empirical generalisations with which the psychodynamic interpreter has to work. For the linguist has to make do not only (i) with propositions that are non-specific (so that X may tend to mean such-and-such, but may sometimes mean this, that or the other instead), but even (ii), in some cases, with propositions that are almost self-contradictory in the disjunctions which they encompass. Thus his premises may take the quixotic form, 'The word X can mean either Y or the opposite of Y'; where "the opposite" covers different sorts of antithesis. We may therefore counter one of Cioffi's objections by observing that the psychodynamic 'Unconscious' is not alone in failing to observe the principle of non-contradiction (cp. ch.5 (a) above). Any linguist will be able, of course, to multiply his own examples of (i) and (ii); and also, no doubt, to add to the dimensions of flexibility along which such definitional laws can very: by which I mean to suggest that variation of a word's meaning according, for example, to syntactic context (such as word-order within a clause) would be a different kind of variation from that which depends upon semantic context.

Examples of type (i) are two-a-penny. But it is worth noticing how examples of (ii) function in practice, and how this illuminates the use of their behavioural corollaries in supporting DI's and PAN's. A convenient Latin verb of type (ii) is subire, which can mean either 'to rise up' or 'to sink down', - thus exemplifying opposition in terms of spatial movement. Opposition in terms of temporal movement, as it were, and determined by syntactic as opposed to semantic context, would be exemplified by, for instance, the French adjective prochain meaning either 'gone before' or 'coming after' depending upon its own position after or before the noun. Again, the Latin adjective caecus can mean both (actively) 'unable to see' or (passively) 'unable to be seen'. Moreover, this active-versus-passive variation on a basic theme reflects a systematic transformational principle according to which the 'transferred' meanings of a word are generated out of its 'root' meaning; and it must be evident by now that a behavioural analogue of precisely this etymological situation is an important ingredient in much psychodynamic argumentation.

The psychodynamic generalisation corresponding to (i) will take the form: 'Such behaviour tends to derive from motive x, but can have different determinants'. Farrell (1961) has discussed an example of "the boy D", where the parallel proposition would run: 'In such a situation, aggressive anti-social behaviour tends to reflect the need to test out a new environment; but it may be produced by a brain-tumour among other things'. In a case like this, a DI which appeals to the tendency without explicitly ruling out the alternatives (which may not be possible in practice or in retrospect) is apt to be dismissed as arbitrarily selective.

It is notoriously difficult, however, to pin down the logical grammar of this sort of tendency-statement. As Braithwaite's conversion indicates (p.00 above), they are sometimes supposed to be just rather messy statistical generalisations which have not been properly quantified: it is assumed that 'in principle' (another deus ex machina of unregenerate scientism) they are reducible, if not to a universal linking X and Y in specifiable but hitherto unidentified conditions, nevertheless to the bare frequency-observation that, overall and regardless of conditions, '*n* percent of X turn out to be Y', from which observation the statistical probability of *this* X turning out Y can be inferred.

Obviously neither the psychodynamist nor the linguist is usually in a position to operate with *quantified* propositions of this kind. Nor are their tendency-statements necessarily making the unquantified majority-claim that 'most X's turn out Y', as we have seen in the case of word-meanings. For although they certainly convey something stronger than merely 'some X's turn out Y', they may do no more than postulate a systematic connection between X and Y that is firmer than any systematic connection between X and any class of phenomena that are not Y. If it were the case, therefore, that 40% of X's turned out to be Y, 20% turned out A, 20% B, 10% C and 10% D, then there would still be some sense, howbeit limited, in which 'X's tend to turn out Y' (understand: ' ... rather than anything else in particular').

But the tendencies of which both the linguist and the behavioural theorist speak are not all or necessarily of this kind which rests on the inadequate or idealised observation of the frequency of instances. They are often, in an important but inconveniently nebulous way, 'implicative', in the sense of implying of some structure which at once generates the tendency and is the ground for asserting it: cp. Sherwood's discussion of cows couchant (1969, pp. 211-214), and Harre's of the grounds for expecting a "flush of blue jackets" (1970, p.27). Thus, if I say "This die tends to show odd numbers", my claim may be valid (by which I mean 'rationally and empirically secure', not just 'going to be seen to be true') even before that die has ever been thrown, provided that I have weighted it, or know it to have been weighted, accordingly. For these reasons it would be a complex business indeed to systematise the rationale, implications and means of verification of tendency propositions in general. This is underlined by Ayer's recent remarks on the topic (1972, pp.61-63) which suggest that philosophers are especially puzzled by the logic of the support-relation between a "generalisation of tendency" and any particular warrant-statement for it, and that they do not go much further than articulating or formalising the intuitions of the perceptive layman. One such intuition which Ayer mentions, however, does bear closely upon the argument of this chapter: namely that, in deciding whether to expect this particular X to be a Y (on the strength of 'X's tend to be Y') we try to establish the absence of "countervailing factors"; that is, evidence suggesting that this is one of those X's which turn out not-Y inspite of the broad trend (cp. section (d) below).

As regards (ii), it is the precisely analogous versatility of some psychodynamic hypotheses, when they involve such transformational principles as repression, over-compensation and reaction-formation, that leads to the grievance that they can be used to account for anything in retrospect but to predict nothing in particular (cp. ch. 5(a), above). For a psychodynamic theory may well generate the hypothesis that a given emotional conflict can issue in either an excess or a lack of a certain sort of behaviour; or, in retrospect, that a given sort of behaviour may result from either an excess or a lack of a certain crucial sort of experience. Examples might be, of the latter, to link attention-seeking with either over-protection or emotional deprivation; and, of the former, to account for both anti-social aggressiveness and undue passivity by reference to an alleged conflict over authority-relations. This gives rise to the feeling that if both the presence and the absence of the behaviour (or experience) concerned will serve to support the <u>DI</u>, then there are no observations that could conceivably count against it, no particular truth conditions can be specified and the alleged interpretation therefore becomes vacuous. How is it then that the translator, apparently forced to rely on similar sorts of material, manages to escape similar charges of arbitrary selectiveness and vacuity?

We noticed above, however, that the parallel paradox of using <u>both</u> 'Well, the engine is hot' <u>and</u> 'Well, the engine is cold' to explain my car's failure to start is resolved by showing that in both propositions there is an implied 'too' qualifying the adjectives; and that a consistent explanation resides in that consistent 'too'. We may take some encouragement from this, and reflect that apparently flexible theories may be necessary to cope with the evident flexibility of human nature. People just <u>do</u> react in opposite ways to 'the same' predicament or stress, as Pope commented with his customary elegance and over-compression:

> "Behold! if fortune or a mistress frowns,
> Some plunge in business, others share their crowns:
> To ease the soul of one oppressive weight,
> This quits an empire, that embroils a state:
> The same adust complexion has impelled
> Charles to the convent, Philip to the field."
>
> (<u>Moral Essays</u> <u>i</u>, 103-8)

But, instead of concluding with him that reason is powerless in face of such behavioural caprice, let us look for a lead at how the linguist deals rationally with that capricious Latin verb <u>subire</u>.

Suppose that a pupil, after correct but incomplete use of his dictionary, mistakenly translates the form <u>subit</u> as 'rises up'; and that his teacher corrects him, pointing out that subire can also mean 'to sink down'. The pupil protests in exasperation, "Well, if it can mean opposite things, then you never know where you are with the word: truth-conditions for its meaning cannot be stipulated, and a particular claim about what it means in a particular instance therefore becomes vacuous." The teacher will answer this, of course, by showing that, when it is taken in this particular context to mean 'sink down', then the sentence concerned fits in better with what is being said before and after it. But these metaphors of 'fitting in', of 'coherence' and of 'goodness of fit', which we have already encountered in a behavioural context, have proved to be complex and to cover different kinds of coherence (cp. ch. 6(c)). For there is more than one way in which a thing may fit in, or fail to fit in, with other associated things: a book may fit in with the others on the shelf perfectly as regards size and colour, but be quite out of place with respect to subject-matter of alphabetical order of authors. Indeed, we have seen that one way in which a PAN makes the behavioural data 'fit' together is by telling a different kind of story, which deals in a different mode of coherence; as if one should discover that the books in someone's library were grouped, not according to biography, poetry, history and so on, but by publisher, colour or nationality of author. The question is raised, then, about what sort of coherence is assumed to be relevant to the particular situation in hand: with what, we have to ask ourselves, do we expect an interpretation, whether linguistic or behavioural, to cohere, - and why? We shall find, in the next section, that these questions also cannot readily be answered by simple and rigorous formulations.

If we refer back to translating subire, it is plain that this coherence is not to be identified with a priori deductiveness. That is to say, the teacher is not claiming that, if you assume that subit means at this point 'sinks down', then what comes in the next few sentences could have been strictly deduced from the combination of that assumption with some other given propositions (perhaps about the context or the writer's known intentions and verbal habits); because there are many things the author might go on to say, _any_ of which would be coherent and thus lend weight to the suggestion that _subit_ here means 'sink down'. Imagine that the subject of _subit_ is _sol_, - 'the sun'. Then the teacher's argument might run something like: 'Since this is a pastoral-cum-sociological bit of Virgil we are dealing with, then if he is talking about the sun setting, rather than rising, he is likely to go on to mention shadows lengthening, birds going to sleep, farmers coming home, lamps being lit, and that sort of thing'. It is important that subsequent reference to _anything_ of this (rather loosely circumscribed) _sort_ will do for the teacher's purpose.

We have already noticed the archaeologist relying on a parallel scheme of argument (p.00, above), and commented that it closely resembles the situation in which the psychodynamist might claim any of various different sorts of behaviour as confirmation of a _DI_ postulating a certain motive, conflict or whatever. In the case of the translation, and also _mutatis mutandis_ in the cases of the archaeologist and the _DI_, the scheme of argument is not so much: 'If and only if he means _x_ he will go on to say precisely _p_, _q_, _r_ ... and, lo and behold, he does. It is rather: 'He is _unlikely_ to have gone on to say _p_, _q_, _r_ ... _unless_ he had meant _x_'. This is partly because what would count, in the translation, as the 'consequences' of the 'hypothesis' are themselves part of the presented data; and this is another point which we have already met, when trying to withdraw the wedge driven between 'prediction' and 'coherence' (p.00 above). So that, faced with a word of equivocal significance, we do not in fact "frame hypotheses" of any precision and test them out: what we do say to ourselves is 'Let's see what he goes on to say', and in the light of that we decide what the writer must have meant by the problematical word. But, although not necessarily directed by specific hypotheses, this investigation of what he goes on to say is far from being open-minded or perspective-free; for it certainly needs to be structured by some idea of what to look for, which in turn must be based on some assumptions about what features of what he says will be relevant.

And so we again come up against the problem of how we recognise contextual relevance; but also against the analogy that, since we manage to use it efficiently and heuristically in understanding language even though its governing principles seem to defy precise articulation, we ought not to despair of being able to put the corresponding skill in the interpretation of behaviour on a rational and empirical footing. Let us try to clarify the problem, at least, by noticing some of the obstacles which <u>are</u> regularly overcome by successful appeals to contextual relevance, both linguistic and behavioural. It is obvious enough, in a particular case, what is being appealed to: the problem is to specify in a general and systematic way what defines the class of legitimate, and

and therefore potentially successful, appeals. Using the same broad analogy, Grice illustrates in a classical article (1957, p.388) the particular point but helps only vaguely with the general one:

> " ... in cases where there is doubt, say, about which of two or more things an utterer intends to convey, we tend to refer to the context (linguistic or otherwise) of the utterance and ask which of the alternatives would be relevant to other things he is saying or doing, or which intention in a situation would fit in with some purpose he obviously has (e.g. a man who calls for a 'pump' at a fire would not want a bicycle pump)."

But on what grounds are actions and intentions judged to "fit in with some purpose ..."?

(c) Dimensions of context.

(c) Dimensions of context. Questions about both linguistic and behavioural significance are complicated, then, not just by the need to appeal to context, but also by the fact that such appeals may be made to a wide, and amtecedently unspecifiable, variety of contextual considerations. We have looked at an example where a guide to establishing coherence can be derived from the assumption that a descriptive passage is likely to refer to events associated in time: sunset, home-going, lamp-lighting and so on. In so doing, we may be thought to be relying tacitly on that much misused 'principle of parsimony', by assuming it to be more likely that a passage contains a few general themes consistently worked than that it treats of several disparate ones. And as in literature, so perhaps in individual psychopathology. But there will also be cases where appeal is made, not so much to the content of what's being said, as to the purpose or motive for which it is being said.

Consider a speech in which some court-orator like Cicero is defending a dubious gang-leader. We come across a phrase containing a word that means, in a general sense, 'clever'; but it can take on either landatory shades and commotations or pejorative ones. So that sometimes it is to be rendered 'intelligent', say, and other times 'crafty' or 'scheming'. Our choice between these alternatives will be guided not merely by considerations about the speech hanging together internally from a semantic point of view, but by the assumption, external to what is actually being said, that Cicero wants his client to find favour with the judges and be acquitted. But this consideration does not allow us to show the simple conclusion that the orator will never use the word in a pejorative sense in the course of this speech, on the ground that he will not want to disparge his client in front of the judges. Because, on the one hand, Cicero will be willing, in the interests of appearing realistic and impartial, to admit that his man has a few pecadilloes and may have fallen from grace on some occasions; on the other, he will want to ask rhetorical questions (inviting the answer 'No') which emply derogatory predicates.

Thus there is no simple subsidiary context-generalisation which follows from the general strategy of trying to impress the judges (e.g. 'Cicero always refers to his client in favourable terms'), and which we implicitly invoke in deciding how to interpret the problematical work. There may be a broad tendency toward favourable references overall, but the tactic of a particular line of argument or stylistic device can override it. These tactics (such as rhetorical question, disarming candour double-bluff, flattery, tongue-in-cheek, sarcasm) must be recognised for what they are in the individual instance and allowed to supervene. Only in this way do we avoid the mistake of translating a question, consistently with the broad tendency, as 'Do you think my client is intelligent?' when the point is that Cicero is asking, consistently with the tactic of the moment, 'Do you take my client for a crafty schemer?'

The question of how people manage to make such recognitions raises complexities outside our scope; but the fact that they make them, and must necessarily make them if translation (and linguistic understanding generally) is to get off the ground, barks back to the need for a kind of conceptual empathy which we have discussed above in the context of behavioural understanding (ch. 5(b)). Furthermore, the fact that one can sometimes identify and understand such devices, even when coming across them for the first time, attests to the efficacy of some nebulous and unspecified context-considerations which must be at work. I have in mind, for example, the naive reader's capacity to discover for himself that, in fictional gangster-land at any rate, "I'm worried about your health" evidently means "I'll kill you if you don't play ball".

For this situation, then, of Cicero making a speech as defence-lawyer, the context-considerations, in the light of which we adjudicate the sense in which a particular instance of a word is to be taken, are rather different from those in the 'sunset' example. This fact, that different sorts of observation can legitimately be appealed to in the search for coherence, is another point of resemblance between the linguistic and the psychodynamic scene. For an interpretive 'argument back' to a patient's conflicts or anxieties may very well take account of data as diverse as the way he sits, the words he uses to describe a person, his fantasey to a Rorschach blot and the way he spends his money. But it is still a problem to know how the generalisations about such data, which are themselves loose and perhaps even mutually conflicting, can be used rationally and reliably. And yet, in the case of Cicero's speech also, the generalisations in which the context-generalisations are expressed, if they were to be formalised, would have this aspect, that some are more general than others; and even that these which obtain at the level of specific tactics may conflict, in respect of usage-prediction, with the broader tendencies of overall strategy.

These features may be contrasted with a few exceptional situations in which the linguist is armed, uncharacteristically with tighteand rigorous generalisations about the context. There are some purposes and occasions for which language is used in a restricted and stereotyped way; so that, if the context of a language-episode (or 'text') can be identified as such a purpose or occasion, then some very specific generalisations can be brought into play. A classical scholar would often be able to reconstruct a missing half time of Homer, if the page were torn, on the strength of his knowledge of the poet's verbal habits of prediction and time-ending, some of which are so invariant, as to be known as 'formulae'. Similarly the set-phrases used in inscribing monuments of all sorts (whether tombstones, votive altars or tribute-lists) were often so stereotyped that, given a few crucial syllables for the names of the people, gods or states concerned, the epigraphist can justifiably infer a great deal about how a damaged inscription must have read, and therefore about what it must have meant, even though it no longer says it (cf Burn 19.., p.   ). But all this requires being able to recognise the context as a line of Honour, or as a tribute-list rather than a tombstone.

For the most part, however, subsidiary generalisations are much looser than this, and can appeal only to the sort of thing to be expected in a given linguistic or behavioural context. We have seen that archaeologists, for example, obviously make only loose 'predictions' about the sort of thing that will coop up if it's this sort of site rather than that; and that they have the conceptual job of matching individual finds and observations against the species of things or features predicted. We have also seen, in the field of human behaviour at large that the question of what sort of behaviour is a appropriate, in a given context, for our angry man, a frightened man, a jealous man or an ambitious man, so as to be able to explain Smith's action as 'what you would expect', raises the further questions of specialist versus commonsense understanding of people, and of the need to conceptuaalise a particular action as aggressive, panicky, self-satisfied, defensive, ostentatious and so on.

In the caseeof language, any attempt to avoid these difficulties by trying to provide (or by arguing that we implicitly rely upon) an 'operational' definition of the meanings of words, analogouzly to such definition of terms like 'angry', 'frightened', 'jealous', 'ambitious', in behavioural experimentation, proves futile. There is no change of converting Cicero's tendency to use the word favourably into a matrix of tight generalisation to the effect that 'In contexts of type C, and with tactic T, operating word X will mean ; in C_2, with T_{1-4}, it will mean , etc.'. Because, on the one hand, this does not avoid the 'intentionality' problem of recognising contexts and tactics for what they are; and, on the other, there would be no end to the column of 'types of context' required by an exhaustive matrix. Further, some 'types' would have to be specified so minutely, in order to justify any generalising extrapolation, that we should risk ending up with merely an inventory of particular usages in particular contexts. I mean: from the observation that this frightened Persian slave in this dream-story uses the word to mean , do we infer that this usage is typical of dreams, slaves, Persians or frightened men?

There are indeed some higher-level rules-of-thumb which are apt to get invoked when there seem to be equally good grounds for interpreting the facts, whether textual or behavioural, in different ways: that is to say, when both p and q seem equally well supported by subsidiary context-generalisations. We have already encountered the textual critic's maxim that the "more difficult reading" should take preference in equivocal cases (ch.4 (a)). But "difficult" in what way? Not just hard to make sense of, or obscure in meaning; for the famous Virgilian crux provided by the last letter of the phrase facilis descensus Averno (Aeneid 6, 126: "it is easy to go down to Avernus") is only too readily understood by the schoolboy who has forgotten that this case-ending is not regularly used for 'motion towards' a place. And precisely because it is not, it is difficult for the expert to believe that this is what Virgil wrote; but the rule-of-thumb enjoins him to prefer this difficulty to the grammatically more orthodox variant found in some texts.

But the question of kinds of dimensions of difficulty is raised, and it can be seen to become more complicated if we apply the rubric to a larger textual question. An impromptu of Schubert (D899, no.3) is to be found published both in the key of G-flat and also in G-major. Which is the more "difficult" variant; or rather, what dimension of difficulty is relevant here? As regards reading the text, the G-flat version is harder; so it is, perhaps, as regards playing the piece: these dimensions therefore suggest G-flat as the original key. Matthews, however, specifically denies that the G-major is easier to play (1972, p.197), thus turning the rule-of-thumb decision towards that key. On the other hand, it is hard to understand, from another point of view, why such a rich and lyrical work should have been written in bright and innocent G-major rather than in a suptuous flat-key. (There is no doubt where Liszt would have written it; and you may say that Chopin did write it in flats, in the first of his op. 25 studies!) Consideration of this dimension of difficulty would lead us to prefer G-major, and to suppose that some editor made the obvious pianistic 'correction'. In fact it seems to be dear, in this case, that the original key (what Schubert 'really meant') was G-flat; and even if this were not independently demonstrable, a cogent 'structural' argument could be based on the close affinity between this key and that of the ending of the immediately preceding piece in the same set (Matthews, ibid; and cp. ch.8 below). This suggests that the relevant dimension of difficulty, for the application of our 'rule-of-thumb' to this problem, should have been ease of reading (even though it cuts across ease of playing!).

Another such rubric, which we have also met, enjoins us to prefer the more economical of any two accounts of the data which are equally consistent with all relevant considerations, whether primary or contextual. Again there is scope for disputing what dimension, this time of 'economy', is relevant. For although Occam is sometimes said to have had economy of novel postualtes in mind, the sense in which Lloyd Morgan's "principle of parsimony" saves us from unnecessarily supposing that his dog knows how the gate-latch works, as opposed to merely being able to work it, is different (cp. Murphy and Kovach 1972, p.137). Some animals, of only human ones, do know such things. But the idea obviously is that, if an explanation involving n-familiar factors, etc. will do just as good a job as one involving n-plus-x such factors, we should assume the less complex system to be operating. Applied uncritically to human behaviour, and perhaps also to the physical sciences (cp. Harré 1972, p.45), it easily leads to falsehood. When Mozart was giving the first performance of one of his piano concertos, he had not had time to write out the piano-part and therefore played it out of his head. But since convention dictated at the time that it was bad form to 'show off' in this way, he set up the score of another work on the piano. Now, for someone who saw Mozart playing K.450 with a score in front of him, the economical interpretation would be that he was playing from the score; and it would be highly unparsimonious, indeed methodologically profligate, for the observer to suggest (truly) that Mozart was ignoring the score and playing from memory, even though we know that people can play from memory. However, if you fill in a bit more detail, or "tell more of the story", namely that it was the score of (say) K.414 that he had on the piano, then the simpler story no longer even covers the data. This should remind us that the outcome of

Appeals to parsimony, of which psychologists are ingenuously fond, depends upon how the behavioural data are represented in the first place. Thus if the ways in which behaviour is to be described are artificially and dogmatically restricted (as we saw Pavlov doing, p.6-18. above), then this inevitably means selecting what behaviour gets reported. Because of this selectivity, and especially when it is allied, for example, to the stultifying elasticity and circularity of Skinner's conception of 'stimulus', 'operant' and 'reinforcement', people can be encouraged to remain content with a speciously parsimonious (even a "three-term") theory of human action (cp. Skinner 1969, pp.10, 123, 138 and passim). That which defends ethology against anthropomorphism may impose zoomorphism upon psychology: if 'peak experiences' or 'identity diffusion' never get reported, there will seem to be no need for a theoretical account of them.

These meta-contextual rules-of-thumb (about difficult readings or parsimony) are presumably supposed to be not merely heuristic but also 'implicative' of some law-like principle of human functioning: indeed they are heuristic no doubt because, and insofar as, they are implicative. We have seen above that what they imply are generalisations about the differential likelihood of "trivialisation" as opposed to other errors, in the one case, and of certain evolutionary achievements, in the other. What has just been argued is the need to be able to identify the relevant dimension of context, whether linguistic-symbolic or behavioural, before they can be reliably applied.

One final way in which contextual considerations can determine the significance to be attributed to a particular sumbolic or behavioural element claims attention as a witness both to the power of contextual cues and to our ability to recognise that power at work in individual instances; even when these instances defy the lawlike system which regularly obtains. For situations do occur in which the rules defining the significance of a symbol are broken and yet we can recognise the fact, and understand the situation on internal evidence alone. This testifies to "the operation called Verstehen" (Abel 1948) which we have discussed before. That is to say: even though our understanding is based on the systematic assumption that 'all X's mean ', nevertheless we can identify a counter-systematic exception, from the context, and recognise that the rule has been broken. Thus, so far from our arguing-back necessarily depending upon tights and inflexible generalisations, it can succeed (thanks to contextual subsidiaries) even when the usual rules do not hold.

The linguist is used to leeting the context override the stipulated meaning of a particular word, when he recognises and translates a metaphorical usage. Thus, although the French word <u>dent</u> regularly means 'tooth', the English for it in the phrase "il a une dent contre moi" is not 'tooth' but 'grudge'. And the general question of how we manage to use background ones to identify and understand metaphorical expressions is a matter of some philosophical interest (Black 1962, pp. 24-47; cp. Hesse 1966, pp. 157-177). What concerns us here is simply that we <u>can</u> doso, in the absence of antecedent generalisations about how a particular context will reflect such rules of verbal usage as there are.

Linguists also know, of course, that, in some languages (e.g. classical Greek) a given word of negation will sometimes cancel and sometimes intensify another negative in the same sentence depending upon word-order. But this is straight forwardly to allow the outcome of 'X sometimes means and sometimes not ' to be determined by a specific and unambiguous subsidiary feature of syntactic context. When, however, the general rule is that multiple negatives cancel, as in English, we can still recognise Shakespeare's rare, rule-breaking intensive negative for what it is: "O horror, horror, horror! Tongue nor heart cannot conceive nor name thee" (Macbeth II, 3). We are able to say, on the strength of understanding a unique context, which can be referred to no rule, that Shakespeare must have broken the rule. We we correct newspaper-misprints for ourselves, again on the strength of very nebulous generalisations about what sort of thing the writer is likely to have meant, we are concluding that a rule (of correspondence, between writer's intention and newsprint) has been broken. The text of the music-critic's column says that Purcell's Ode to St. Cecilia "tickled by nerve of visibility"; but we can divine, on entirely rational and factual grounds, that what Cardus must have written was "tickled my nerve of risibility".

Some behavioural parallels to this contextual modification and even contradiction, of semantic stipulations will redeem this linguistic analogy. In the general field of personality assessment, for instance, let alone that of medical diagnostic practice, it has always been standard procedure not to attach fixed significance to a particular test-response but to judge its significance in the light of the rest of the record. Thus an explosive colour-response say, given to partoof the second Rorschach blot, 'means' something different when followed by tense rejection of the card than when followed by a realistic and sensitive W-response. Hence the whole rationale of "sequence analysis" in this (and any) projective technique (cp. Klopfer and Davidson 1962, p.128; Alcock 1963, pp.80-84).

Again, when dealing with a supposedly more objective and reliable questionnaire procedure, the MMPI, McKinnon found that what distinguished the test-profiles of his creative architects from those of psychiatric patients was not the relative absence of 'abnormal' scores on the clinical scales (for there were almost as many), but the fact that they occurred consistently in the context of much higher ratings on the sub-scale of 'ego-strength' (cp. Barron 1965, pp.57-67). Lastly, in the field of personality assessments based on physical or neurolophysiological 'constitution' (such as those of Kretschmer, Pavlov, Sheldon or Eysenck), there is scope for a corresponding interplay of factors in which one's constitutional predisposition to behave in one sort of way may have been overlaid, and become dominated, by the acquired learning of discrepant behaviour-patterns. Thus there may well be constitutional cerebrotones who do not typically behave cerebrotonically, a point which Pavlov thought Kretschner had failed to grasp (cp. Gray 1964; p.42). Whether the 'deep-structure' of one's temperament in this sense is expressed, or is the dominant expression, in a particular situation depends on that situation's capacity to activate acquired rather than built-in reaction tendencies. Consequently the theoretical postulation of a given temperamental deep-structure, P, is consistent with both p-like and q-like behaviour; and the same goes for psychodynamic deep-structure too.

(d) Value-added, and the reduction of unlikelihood. This general kind of argument which we have been advancing, in making the causal significance of an event or property dependent upon its longitudinal and cross-sectional context, is familiar to economists, and has been applied in sociology to the analysis of "collective behaviour" (Smelser 1962, pp.12-21). In order to create a product of some economic value, such as a motorcar, certain raw materials are needed and they have to be subjected to various kinds of processing. We shall require such substances as iron ore, glass, rubber, plastics and paint; and such processes as smelting, painting, cutting, tempering, moulding, and welding will be necessary. And it is of crucial importance, of course, to the production of a car that these processes be applied to the materials at the right time in the right sequence: it is no good painting the iron ore, or welding the tyres to the windscreen. But to the extent that each process is indeed carried out in the appropriate order (for a car), to that extent it cumulatively adds 'value' to the product; and the further the appropriate sequence is carried through, the less possibility there is of the end-product being anything other than a car. Up to a certain point, the production program could be switched to refrigerators, but there comes a stage when the only choice left is between a "coupé" and an "estate".

Smelser argues that attempts to analyse the determinants of some sociological phenomena, such as messianic movements or panic, have typically been content to spell out a list of necessary or perhaps sufficient ingredients, without being aware that, if you do not also specify a unique method of combining these ingredients, your recipes will not be exclusively for messiahs or for panic but for other eventualities as well. Thus your analysis will not have accounted specifically for panic (as opposed, say, to the persecution of aliens). Each individual determinant of panic adds its causal value according as it occurs in the right relation (temporal, spatial, quantitative etc.) to other co-determinants, so that this structuring of the determinants, is as necessary to the particular outcome as is the occurrence or existence of the determinants themselves.

If we transfer this argument to the generative antecedents of individual people's psychopathology and its behavioural expression, we see that it is not necessarily absurd to account for both behaviour X and behaviour Y by reference to the same group of determinants, as long as we realise (what is not always made clear) that such references are 'implicative' of differential structurings of the determinants in the two cases. Indeed it is a comparable property of structural organisation which distinguishes the familiar concept of a 'syndrome' from a mere list of symptoms (cp. Foulds 1965, pp. 67-68). What is unsatisfactory about psychodynamic elements p, q, r, is that this implicative metaphor of 'expression' smuggles in the most problematic part of the story, that to do with forms or mechanisms of expression, in an inarticulate way. But precisely what a coherently articulated DI does is to try to depict this differential structure or organisation among determinants by means of the various analogical concepts of psychodynamic theory. Just as pointing has a different value in the economics of car-production if it is done after, rather than before, panel-beating, so a given experience, such as loss of father, will affect a boy's psychic economy differently if it occurs before the resolution of the Oedipus complex rather than after it (cp. Grinder 1973, pp.351-353.). And, to revert to the general analogy of linguistic understanding, there is no greater paradox in holding that both over-dependency and over-aggressiveness may reflect 'oedipal guilt' than there is in believing (let alone being able to recognise de novo) that 'Ceasar was not ambitious' and, in context, "Brutus says he was ambitious" mean the same thing.

The other side of this progressive narrowing-down of outcome-possibilities as determinants occur according to a certain configuration, and of the economic value of an individual determinant depending upon its relative position in such a configuration, plays an important part in the retrospective arguments of D1. If, when asked to explain a feature of someone's behaviour, the interpreter says 'A conflict of type Q tends to produce this sort of behaviour' (as with "the boy D", p. 00 above), that leaves open the possibility that some other determinant was in fact responsible. And, according to the strength of the tendency for the postulated systematic connection to exist, there would be greater or less likelihood that the patient has not got a Q-conflict. So that, insofar as he is relying only on a likelihood generalisation, there exists a certain un-likelihood that the conclusion 'This person has a Q-conflict' is correct. Correspondingly, although it was time (in Smelsen's case) that 'these materials could produce a car', there is initially a certain unlikelihood that a car will materialise.

Investigation of the interpreter's claim does not necessarily take the form, 'If and only if he has a Q-conflict, then this and that will be observed under these and those conditions'. The argument is not that certain other things must be the case if such a conflict exists: it is rather that, so far as such other things as could arise from, or be expressions of, the postulated conflict are the case to that extent it becomes less un-likely that such a condition does exist. The more that the person's other behaviour can be shown to conform to the sort that could be produced by (among other things) that particular conflict, the likelihood that such a conflict is not present is reduced. But the temporal-causal situation is now, of course, different from that of car-production, in that we are now trying to identify the producer from the properties and structure of the produce; and the use of evidence consists not just in showing that certain behavioural elements are present but that they are inter-related, patterned or structured in significant ways (cp. part III, below).

This negative formulation, the cumulative reduction of unlikelihood, establishes a parallel with the traditional procedure of reputing th 'null hypothesis', and with the obligation, laid upon the innovator by Occam's razor, to defeat parsimonious scepticism by persuading us that we cannot get on without his postulate. To the extent that his postulate makes the data coherent, by showing through subsidiary tendency-laws and other contextual considerations, that they could have arisen from the postulated source (or be an expression of the postulated state), so far the un-likelihood is reduced that such a factor is at work. Nor is it merely the number of data accounted for in this way that tends support to the original contention; it is also, and perhaps especially, the oddness, the inconsistency, or the 'improbability' (from the point of view of information-theory) of the observations thus covered which matters. Miles' discussion (1966, esp. ch. 9) of greater and lesser behavioural "exemplaries" for the concept of, say, 'projection' makes the same kind of distinction.

This tacit appeal to the principle of cummulatively diminishing unlikelihood used to be made, in the days before blood-chemistry analysis and finger ridge counts, when deciding whether a pain of twins was identical by considering the degree of similarity they displayed inaa number of macroscopic physical features. The more specific points of subjectivity assessed 'identify' there are, the less likely it becomes that there is not a general genetic identity (cp. Millter 1971, pp. 161-165); although for any particular toughpoint taken separately it would be considerably un-likely that it was due to a universal genetic consequence. Indeed we regularly use this same principle in a much less structured social situation of everyday life, and in so doing we also recognise the differential weight of the evidential exemplaries involved.

Suppose a friend tells yous that he met a man at a party the other evening who said he was a physicist and came from Wiltshire. You think this sounds like someone you know, so you ask whether his name was Bill Jones. Your friend says that he did not catch the name; so you say 'was he a tall chap with glasses?' Your friend says yes, tallish and glasses certainly. You say, 'Was he married with three children?', your friend says he got the impression the chap was married, but cannot say about children. And so it goes on. But there comes a point, or there may come a point, when you feel that you can safely say "It must have been Bill Jones'. By establishing a range of properties which the man at the party and Bill Jones have in common, you reduce the likelihood that the first correspondence of properties noticed (being a physicist and coming from Wiltshire) was fortuitous. And although the contributory propositions in the argument are never of the rigorous form 'If and only if the man at the party was Bill Jones, could he have had big ears etc.', yet such loose hypotheticals can nevertheless culminate in a rationally and empirically 'tight' conclusion: because it eventually becomes more likely that he <u>was</u> Bill Jones, than that two different men should resemble each other so closely. But, as we have noticed above, it is not merely the <u>number</u> of such common properties that counts, but rather their information-loading. Thus, in the absence of definite points of discrepancy, the conginence of a coupld of highly 'improbable' features (Miles' ("greater exemplaries"), such as having both a wooden leg and an uncle who played football for Chelsea, might well be sufficient to convince us.

The corresponding psychodynamic argument, in the case of an interpretation postulating a particular conflict etc., would rely on some such suppressed claim as, "It is unlikely that <u>P</u>'s behaviour should show so many features that <u>could</u> derive from a <u>Q</u>-conflict, <u>without</u> being so derived?; a claim which (leaving 'overdetermination' aside) defies the construction of an alternative account of the same features. That is why <u>D1</u> is most plausible when drawing a thread of coherence through behavioural phenomena that on any other account remain odd, pointless, fantastic, inconsistent, disjointed etc.: for in this respect they have a high information-content, and appear to be major exemplaries of something.

We have already looked at an example of this kind of exercise (Freud's study of Leonardo, ch.6 (a)) from the point of view of its general rationale and problems of evidence. But, by way of illustrating the cumulative reduction of unlikelihood in clinical practice, and in the service of DI, let us consider some case-material which involves just such interpretation of projective-test responses as Eysenck criticises.

A young adult male presents among other symptoms a very severe stammer. A clinician of some psychodynamic persuasion might, when he heard of the case, rashly say to himself something like: 'Stammers tend to be associated with (even, produced by) conflict over inter-personal aggression'. He would call to mind, perhaps, the self-descriptions, written after 'fluency-construing' therapy, by ex-stammerers who felt that their aggressiveness had been liberated as well as their speech improved (Bannister & Fransella 1971, pp. 136-138). Nevertheless, it would be a rash generalisation because there may in fact be a stronger tendency for adult stammers to be determined by some other factor; but when he learns that in this instance the impediment has resisted physical treatments, speech-therapy etc. he may think that the case for such an emotional aetiology is strengthened. There still remains, however, the considerable un-likelihood (depending on the strength of the alleged "tendency") that this particular patient has such conflicts over interpersonal aggression. And this is only a relatively cautious psychodynamist at work: he may be well aware that some of his more indoctrinated colleagues would be prepared to make a further claim, that conflicts over inter-personal aggression tend to arise from the Oedipus situation, and therefore to be linked with more-or-less overtly sexualised conflicts over potency, castration fear and goodness-knows-what. He would regard these latter extravagances as even more unlikely to exist in this case than the non-specific aggression-conflict. However, if the patient systematically produced behaviour which, taken literally or symbolically, reflected (i.e. had certain features in common with the content of) such alleged further anxieties, he would consider the un-likelihood of their presence reduced. So that in the end there might come a point where he would say that the patient's behaviour was so loaded with certain themes, namely with the possible effects or 'expressions' of the alleged conflicts, that it was more economical - it made more sense - to suppose that such conflicts were at work than that they were not. What sort of data, then, would he consider, and how would he use it?

A wide range of different sorts of behaviour would, of course be relevant, as has been said above; nor is there any way of devising a general formula to circumscribe that range. He would have to study the patient's biography, his descriptions of his feelings and conscious concerns, the memories he chooses to bring up in psychotherapy, the structure that he imposes (or fails to impose) on the ambiguous visual stimuli of projective material, etc. Sor that when, in the course of making imaginative stories to vague pictures (Phillipson 1965), he seems to be more than usually disturbedbby the idea of conflict and hostility, repeatedly denying that the figures in one such picture are in conflict (where most people in fact feel that such a relationship is appropriate) and completely misperceiving another picture when the theme of hostility to an authority figure is obvious to most people's way of thinking; when, in another, he speaks disparagingly of the figure whom he sees as dominating the scene; when he later writes an unnecessarily aggressive and critical letter to a senior doctor who is treating a friend of his; and when it emerges that he has been an active member of a proscribed left-wing minority political party - then the psychodynamist may be forgiven for thinking that the unlikelihood of there being an aggression/authority conflict has been reduced to negligible proportions. Especially when it turns out that the patient despises his father in many ways and has almost deliverately avoided identifying with him.

Similarly, with the alleged possible ramifications of the authority/potency theme. On the non-sexual aspect first: the patient, when invited to make up a picture for an entirely blank card, chooses the theme of a respected teacher or religious leader with a group of disciples hanging on his lips; he recalls in therapy that at school he developed a great admiration for his teachers' academic gowns and used to act out gown-wearing fantasies at home right up to late adolescence, and he reports that, at times of depression, academic success seems all-important to him.

But the additional, and more 'unlikely' supposition that there is a specifically sexual overtone to this concern about power and achievement will give coherence to a further range of observations that would otherwise remain odd or unaccountable. Why, in the first card of the picture-test, does he have doubts about whether the central figure, which everybody else can clearly see as a man, is male or female; why does he insist that nothing 'positive' will happen between the man and the woman that he perceives in the next card: why does he see rather sinister damaged and injured animals in the Rorschach blots, giving one response ('a cow's udder rotting away', to card D) where the confused castration symbolism is almost inescapable: why does he block, at first, to another blot (card D) that has superficial phallic features and then produce, when pressed, a fantasy in which the potency/destruction symbolism is laid on with a trowel ('a swordfish: it has a prominent spine that is extended in an extra sword on the end'): and why does he revert to the same themes of super-potency and destruction in a different guise in another response ('an aeroplane shooting out of a volcano with a real force behing it: it had to make rather a mess of the volcano')? Why does he display all this behaviour which could arise from the sort of oedipal concern about sexual achievement and so on unless he has such a concern?

Furthermore, when the behavioural material is looked at in this light, additional data-groupings that could relate to hitherto unsuspected corollaries of the main conflicts may emerge. Thus, in this case, a perceptive and adventurous interpreter would have picked up the hint of masochism in the patient's very first Rorschach response ('a moth which has been hitting itself against some glass...') and opined that the patient was likely to have some masochistic attitudes. A tenuous thread indeed: but he would have noted also the half-conscious satisfaction in the patient's manner of reporting how he had defeated all previous types of treatment, thus keeping himself symptom-crippled. An unlikely story? But some of the unlikelihood would have been diminished by the patient's subsequent description in therapy of how as a child he used in daydreams to imagine agonizing situations from which his mother in fantasy rescued him. And when he turned up for one therapy session carrying a copy of Rousseau's 'Confessions' (the first time he had brought a book with him) the psychodynamist might well consider most of the unlikelihood dispelled.

Of course, in the previous case, of the man at the party, the strength of the 'deriving' links is greater, in that it is at least true that if he was to be Bill Jones he must have had certain clearly delineated properties; and in that there is little room for doubt about what counts as having, or not having, many of those properties! In the clinical argument, however, the main proposition is weakened in two respects. Because (a) it is of the 'tend' not 'must' variety, and (b) it attributes only a vague range of expected effects, or manifestations, to be postulated cause. This makes it possible to fit a variety of different behaviour into the predicted classes of effects, etc.: particularly when (a) such behaviour can be taken not at its face value (difficult enough in itself to categorise) but rather as 'symbolising' something else, or (b) when the question may trun sometimes on whether the patient shows a significant lack or excess of a ce tain sort of response.

Now the rationale of symbolism is a study in itself (cp. Royce, ed. 1965); but we have already noticed, in pursuing the linguistic analogy, that the translator has a somewhat similar problem of deciding the metaphorical or figurative meaning of an expression that cannot be taken literally. The meaning he attributes to the total phrase in such a case may therefore very well be in defiance of the literal meaning of the constituent words. But this lack of specificity, however regrettable, is a feature of the lawlike propositions themselves at the present state of knowledge, which may well importantly reduce the weight that can in practice be attributed to a particular psychodynamic speculation. And it may be that the relevant knowlege is not amenable in practice, or even in the nature of the case, to being improved beyond a certain degree of vagueness. That does not, however, undermine the essential rationality of the operation. It is an old methodological muddle to confuse, as many of psychology's experimentalists still do, precision of description with rigour of rationale, - even though it is now more than a decade since Deutsch, for example, argued the distinction (1960, pp.163-167; and cp. Miles 1966, pp.19-20).

We have seen, indeed, from our excursion into various kinds of linguistic understanding and discovery, that lack of precision, in both basic generalisations and contextual subsidiaries, need not necessarily prevent rational procedure, assessment of evidence and valid empirical conclusions. If the obvious seems to have been laboured, I would take refuge in the converse of Grice's apology (1957, p. 388), "All this is very obvious; but surely to show that the criteria for judging linguistic intentions are very like the criteria for judging non-linguistic intentions is to show that linguistic intentions are very like non-linguistic intentions".

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PART III. The Structural Basis of Transformation

Introduction;

Introduction.

We tried to separate, in Part 1, the explanatory and transformative functions of psychodynamic interpretation; and in Part 11 it was shown that there are a variety of ways in which accounts of behaviour may carry explanatory force, and a variety of ways in which empirical observations may be relevant to assessing the validity of such accounts. I want to argue now that both the explanatory and the transformative efficacy of interpretations stem from their concern with what we may call the 'structure' of the behaviour to which they refer. That is to say, it is because they either consist of, or depend upon, propositions about such structure, that interpretations both resolve puzzlement about behaviour in an explanatory way, and also serve to transform people's perceptions of it, whether these perceptions are those of the subject himself or those of his family and social contacts. But it is also a feature of interpretations (not to say a defining characteristic) that these puzzle-resolving propositions, whatever they are immediately 'about', are derived in a characteristic way from the observations to be explained; namely, by treating these data as 'expressive' and appealing to their 'meaning'. Consequently, they oblige us to pursue a little further this dangerously figurative question of the relation between the discovery of structure and the attribution of significance.

We have already discussed the inclination to say that, when an interpretation identifies, explicitly or obliquely, the structure of some problematical behaviour, it resolves puzzlement by enabling us to see that behaviour as after all a case of 'y', rather than 'x' as we had seen it hitherto (for example, as a reaction-formation to feat of rejection rather than as straightforward boorishness); and that, having 'seen' this, we can then make sense of it and understand it by fitting it into generalisations and conceptual schemes where it would not fit before. The transformative force of this structure - identification, provided by the interpretation, may be either direct or indirect. When the subject comes to see his behaviour as structured in some way other than he had previously seen it, this may have the direct effect of relieving anxiety, assuaging guilt, or beneficially modifying his view of himself or of his world, and so on; on the other hand, it may have only the indirect consequence of showing him the relationships between certain features of his behaviour or feelings, in such a way that it becomes evidently reasonable or healthy (in the light of separate, accepted principles of realistic conduct or mental health) to change his self-assessment, life-style, demands on other people or whatever, if he can. It is as if some 'categoricals' (those babout structure) do imply imperatives; or, if not imperatively, at any rate jussives and hortatives, when seen against a certain background. This is not like arguing that 'is' can of itself simply 'ought'. It is like arguing that sometimes an 'ought' can be derived from an 'is' in combination with a suppressed recommendation-carrying premiss: e.g. (1) this action is a form of unconscious self-destruction; (2) (suppressed) unconscious self-destruction is unhealthy, and health is an agreed good; (3) this action ought to be discouraged. But if the therapist does not make the assumptions of stage (2) explicit, then there is a risk that they may run counter to those of the patient, or to those that he feels he has tacitly accepted by entering therapy. This is one source of the rather theatrical charge of psychotherapeutic 'violence', which has been noticed above (ch. 4(a)).

This identification and characterisation of structure, however, is, or at least may be, relatively independent of assertions about the casual antecedents of the behaviour concerned. For it is entirely possible to have good grounds for saying that the structure of a system is such-and-such without making, relying upon, or being committed to any causal claims about how the structure came to be as it is. The structure of Paley's watch could well be described by its finder without his saying or knowing anything about who made it, or how, or about which parts came first. Propositions about the relationships between parts, and in our case about those between componentselements of behaviour, can be made and supported, then, independently of propositions about how those elements and relationships came to be as they are. Nevertheless, I suspect that in practice many of the psycho-dynamic propositions about structure (or non-propositional representation of it), which logically underpin interpretations and cognate therapeutic transactions, would be seen, if winkled out and coherently formulated, to be an awkward analgam of structural and aetiogical claims. For this reason I have tried elsewhere to articulate some of the unformulated assumptions which permeate the activities of so-called 'encounter groups' run on Rogerion lines (Cheshire 1973 c). But the general argument in this Part is that propositions which are essentially about structure, rather than causes, are in themselves capable of generating both explanatory force and transformative consequences.

An important crollary of this potential independence, however, is that the sort of evidential considerations which support propositions about structure (e.g. that this resembles that; that this is a form of that; that this is the opposite of that) is different from the sort which supports causal claims (e.g. that this is the result of that, or that this sort of thing happens only when that sort of thing has occurre previously in such-and-such circumstances). Our earlier discussion (ch. 6) of the logic of evidential support for empirical explanations will be brought to bear on some practical examples of this point. And the dual thesis (that structure - claims may be relatively independent of cause claims, and that structure - claims may generate both explanation and transformation) will be illustrated by pressing further two analogies which we have already introduced; that of establishing the structural properties of a piece of music, and that of the relation between such structural analysis and executive performance.

Chapter 8

The Discovery and Significance of Pattern

"'If there's no meaning in it', said the King, 'that saves a world of trouble, you know, as we needn't try to find any. And yet I don't know', he went on, ...; 'I seem to see some meaning in them after all'". (Carroll 1865, p.155)

(a) When is a pattern not a pattern?
(b) Is there a message here?
(c) Evidence for particular structure.
(d) Status of propositions about structure.

(a) When is a pattern not a pattern? It seemed above (ch.6(a)) that a principal way in which DI and PAN render their problematical behavioural data more understandable is by recommending a construct-system through which various connections, interelations and patterns can be perceived in the material. But we had to admit the difficulty that this can sometimes lead to the illusory perception of bogus 'patterns', and that there appear to be no general criteria for distinguishing genuine from bogus patterns on internal evidence. For instance, if da Vinci's bird-fantasy had really been about a vulture (and not a kite), should we then have accepted the relevant part of Freud's PAN as wearing a genuine pattern? By way of answer we concentrated on the positive task of showing that, although the method and the data are such that many issues of this kind have to remain open (but liable to misguided declarations of closure), yet other disciplines which depend on similar principles and run similar rists of indeterminacy in some circumstances, nevertheless manage to establish clear-cut conclusions much of the time.

Another reservation about the method, which I have raised before (1964, pp.219-221) and which Cioffi has underlined in reply to Farrell (Cioffi 1970, pp.   ), concerns the relation which these 'patterns' perceived in the behavioural data bear to causal generalisations about behaviour. For it may seem that the logical point of constructing such patterns can only be to identify them as being of a certain sort and then to refer them to some kind of generalisation about what sort of determinants produce what sort of picture; and, further, that pseudo-patterns are to be avoided only by insisting that these 'covering-laws', about how such behavioural patterns are generated, be definitively checkable in the manner of the great Deductivist Myth.

One objection to these assumptions, which we have already encountered (ch.5 (b)), derives from the fact that, on the one hand, such 'laws' (about the sort of way people of a certain make-up and with certain motives react in certain circumstances) would have to be hopelessly vague if they were to be realistic; and, on the other, the procedure would run up against the problem of how to describe particular actions and circumstances in a sufficiently reliable, objective and non-'intentional' way for there to be no doubt as to which covering-law should be invoked. Since there are no general laws about the particular complex of images and actions which comprise da Vinci's kite-memory and the stammerer's gown-wearing play (in anything like such an immediate way as there <u>are</u> general laws 'about', or applicable to the complex of physical properties which comprise this pen or that table), and since laws about bird-fantasies in general or dressing-up activities in general would buy applicability at the expense of psychological relevance, we return to the problem of theory-laden categorisation preceding the invocation of empirical generalisations (ch. 3 (b, c)). Add to this the expectation that a given conflict etc. may be 'expressed' in a variety of behavioural patterns, just as a 'kernel' sentence may be expressed through a variety of grammatical transformations, and the idea of coupling rigorous generality with knock-down verification is left far behind.

A second way of dealing with this feeling, that psychodynamic pattern-wearing ought to rest ultimately on some causal substate, is to bear in mind some aspects of the epistemology of structure-depiction at which we have already glanced (ch.5 (c)). Let us explore the idea that to interpret B (a behaviour-episode, such as my client saying "Ah, your conventional opening") as x (i.e. a projection of his concern about his own conventionality) is to characterise it as having a certain structure or to say that it has x-type structural characteristics, rather than to say that it is a consequence of x-type causes. The structure is characterised by analogy; the sources of these analogies are familiar actions, and processes and states, whether mechanical (projection, equilibrium), linguistic (denial, expression) or emotional (wishes, guilt) and so forth. Such analogies may or may not carry implications about the causal origins of the structure. A group which, on the surface, would seem to do so rather clearly is that of the 'parapraxes', or symbolically bungled actions.

For we seem to want to say of Freud's slip of the pen, which we have discussed above (p.00), that it was an expression of his wish that the patient had been going to come sooner (1924, p.116); as if the wish existed independently of this particular action, and might have been expressed in other ways. The objection that this is to misunderstand the logical grammar of 'expression', on the ground that, in simplistic Ryleanism, wishes just consist in expressions-of-wishes and dispositions-to-express, will not suffice; if only because serious attempts have been made by others to characterise the objects of such expressions (Geach 1957, pp. 1-17; Tormey 1971, pp.5-60). Perhaps it is now safe for psychology to be less demure about its psychic unmentionables, and to face up to the facts of mental life. Indeed, in the famous case of Freud forgetting the name 'Signorelli' (1924, pp.2-5), the perceptions, thoughts and feelings which seem to have caused the slip (namely the stories and ruminations about death and sexuality) were quite clearly separate, in time and space, from their seeming effect. However much we may wish to dismiss this crude way of talking, or else try to cash it into terms of neurophysiological science-fiction (cp. Cheshire 1966, ch.9), the essence of the interpretation is to postulate a significant relationship between those antecedent phenomena (or their causal sequelae) and the memory-lapse. If we concentrate on the sequelae, we can mobilise the analogy of 'expression' and say that the memory-lapse expresses a concurrent state of mind or state-of-functioning (what Tormey calls an "intentional state") which in its turn can be illuminated by certain ontogenetic observations. But whatever tactic we adopt, we can hardly avoid drawing attention, for some purposes, to the "internal structure of the parapraxis" (Wollheim 1971, p.80), and looking for some way of representing it.

One such way of looking at things, which emphasises in the first instance the contemporary and heuristic consequences of structure-depiction rather than its implications for inferences about causal antecedents, seems to be Harre's conception of the 'modal transform', at which we have already glanced (p.00 above). In this case, "the state of the model is existentially identical with the phenomena"; and "there is no separate question as to the existence of the hypothetical medravism and its states ... , for they are the same states of the world looked at from a different point of view" (1970, pp.53, 54). Thus to construct a cubical lattice of sodium and chloride ions just is to make a common-salt crystal, and vice versa; or to cause something to reflect light of wavelength n just is to colour it pink. This view invites us to conceive of a slip of the tongue as a 'modal transform' of admission of guilt, or whatever (p.55): the slip is not a causal product of guilt-admission, but a way of admitting guilt. And yet, since the guilt might have been admitted, or Freud's wish about his patient expressed, in other ways, we are still inclined to think of an independent 'intentional state' which can be subjected to various transformations. Indeed, what Freud gives us in the 'Signorelli' example seems to be precisely the cogs and pulleys of the generative and transformational mechanisms (association, repression, substitution etc.) which determine the form in which the relevant wish is expressed; and such mechanisms sound more like the media of Harré's causal transforms. And certainly the latter's reference to the need to avoid mere re-description suggests that model-schemes which are dominantly modal may nevertheless have some causal features.

At all events, one main function of those structural models which operate as the basis of modal transforms is to illuminate their explicanda by aligning them with other phenomena which, by reason of various sorts of difference, are not regularly associated (or may even be actively contrasted) with them; cp. p.00 above. In our particular case, the transforms invite us to align memory-lapses and pen-slips with verbalised fears, optatives and confessions, rather than with it raining on the day of the picnic. In so doing, our conceptualisation of behaviour-episodes is restructured: because, if there are more <u>ways</u> of admitting guilt than we had thought, then there will be a higher incidence of guilt-signifying actions in a behaviour-sample and many fewer 'accidents' and 'coincidences'.

This discovery depends, of course, on having decided, at some point, to take seriously the assumption that there <u>are</u> fewer accidents, and to go looking for patterns where patterns are not usually expected. And many of the investigations into everyday psychodynamics which Freud reports (1924 <u>passim</u>, e.g. pp.15-20) are explicitly cast in the form of experiments directed at this hypothesis. However, the decision whether the macro-structural hypothesis, that this kind of incident is psychodynamically significant, turns on one's success in finding some organised (or organisable) micro-structure in the particular instances investigated.

Now, on one hand, statisticians warn us that the educated layman tends to think of randomness, or numerical randomness at least, as more pattern-free than it actually is. That is to say: he expects 'organised' sequences, such as 3-2-1 or 3-6-9 or 5-5-5-, to occur by chance much less often than they actually do (Macrae 1974). Consequently he is liable to attribute significant structure too readily to a run of numbers; and if to a run of numbers, perhaps also to a run of actions. On the other hand, what strikes us about <u>some</u> of the microstructures that come to light, once we take the general hypothesis seriously, is that their minuteness, articulation and relevance are so improbably cogent that all antecedent chance likelihoods are left standing; and this is after making all due allowance for the sceptical argument that, in such many-sided material, <u>some</u> sorts of relationship are bound to show up if you keep looking. The basic rationale, and the corresponding problem of eliminating pseudo-patterns, are to be found in other inquiries concerned with the exposure of significant structure.

(b) <u>Is there a message here?</u> It is well recongised that people from different cultures often misunderstand each other's expressive non-verbal behaviour; and that this is sometimes due to failure to recognise that a particular nuance of gesture, posture or facial aspect <u>is</u> expressive at all, never mind <u>what</u> it expresses. Acculturalisation consequently depends, in part, on learning where to look for such expression (cp. Fong 1965). The same holds, indeed, <u>within</u> cultures. Argyle, for example, has canvassed the view that some socially maladapted youngsters have simply failed to learn the stimulus-value, for the purpose of personal interactions, of certain basic cues of glance, vocal inflection and body-movement (Argyle and   ). This raises the more general question of how we discover, or on what grounds we maintain in the face of doubt, that some particular actions, episodes or products <u>are</u> expressive after all. The problem is epitomised in psychology by the controversy over attributing significance to (some) dreams; and the sources of continuing dubiety here may be contrasted with the relatively knock-down dissolution of disbelief in that comparable ethological cause célèbre, 'the bees' dance'. Ethology, and von Frisch in particular, were faced with the situation that 'information' about the distance, orientation and richness of a food-source is transmitted by a successful forager to its hive-mates, apparently by means of some schematic demonstration put on inside the hive. Thus von Frisch can say, of one such forager, "evidently this bee must have announced its discovery at home" (1950, p.63). Revision of details aside, what he established was, of course, precisely which aspects of the bees' movements "at home" carry which aspects of the necessary information; e.g. that, in the 'tail-wagging dance', the angle at which the bee's diagonal path leaves the upper vertical radian corresponds to that subtended at the hive by the food-location and the sun respectively (ibid., p.84).

Here we are persuaded of the general point, that there is meaning in the bees' dance† by the detailed explication of the specific code used. And this may suggest that there is no way of upholding the broad thesis that dreams, parapraxes and so on do have significant structure apart from exposing specific structures and translating their specific messages. But if we can, literally speaking, recognise as a language some signs or noises which we cannot translate, why should we not be able to identify the characteristics of other symbolic behaviour without necessarily knowing the meaning of the symbols? Attempts to do this depend upon showing up various regularities, patterns and correlations in the data; and objections often depend upon arguing that such features are coincidental or are artefacts of the way the data have been selected and represented. Perhaps we can get some help from another area in which there are a whole range of disputes about whether the presented material is significantly structured, and if so how.

In the analysis of music, as in that of behaviour, the ideas of structure and of representation may become entangled at some points. We may think that we come to understand a dream by seeing both what the various images represent and also how the relations between them, in terms of juxtaposition, sequence and change, reflect wishes, fears, guilts, defences and so forth. So in music, the fact that composers sometimes tell us quite explicitly that particular phrases, sequences, notes or instruments represent particular things suggest that the significance of the music consists in the deployment of these symbolic features; and it encourages the listener to wonder whether there is similar, but unannounced, symbolism in other places. If some music is avowedly 'program music', as some behaviour is consciously 'motivated', may not all music have a 'program', and all behaviour a 'motive', of sorts? Well, but the danger of 'program' losing meaning by dilution and lack of contrast is obvious; though less so, unless involuntary reflexes are included in "behaviour", in the case of 'motive'. Similarly, it seems less unreasonable to suggest that all novels are 'autobiographical' in a sense, than that all symphonies are a kind of Peter and the Wolf.

We know, however, that some composers expressed themselves in sonic cartoons, such as Strauss in Till Eulonspiegl; and that Tchaikovsky published the story of one symphony, and said of another that there was a story but he would not tell it. In the last kind of case, critics sometimes accept the challenge and try to reconstruct the latent story by 'interpreting' the manifest musical material. It is often said that Gilman, in a program-note of 1922, did this so successfully for Strauss's tone-poem Ein Heldenleben (then over twenty years old) that the composer commended the accuracy with which the critic identified what each of the six sections of the work represented. Gilman's interpretation went into this kind of detail, in section two:

> "Herein are pictured the Hero's opponents and detractors, ... There is a malignly ponderous phrase, intended to picture the malevolence of the dull-witted among the foe. The theme of the Hero, in sad and meditative guise, suggests his sorrowful surprise that his adversaries should so reveal the smallness of their souls."

We must, unfortunately, credit Del Mar's view (1962, p.166) that such constructions were not always derived, as many projective-test interpretations also are not, exclusively from internal evidence. For Strauss seems to have let slip to friends and commentators quite a number of hints about the message contained in the piece, without ever issuing an official screen-play.

One further, but contrasted, example of representational structure in music raises some familiar issues, about the validity of alleged patterns, in a fresh guise. The question has often been asked whether Schumann composed in code: or rather, since we know he sometimes did (because he tells us), to what extent he did so (Sams 1965). The fact that musical notes can be identified, with or without qualification, by letters of the alphabet opens up the possibility of writing times or chords which spelt out words. Several composers took advantage of this to pay homage to J.S. Bach, by writing music which incorporates the motif-spelled out by his surname in German notation, where 'H' stands for B-national and 'B' for B-flat. In a more intimate vein, the young Schumann, though not defaulting from conventional tribute to the master (in his op. 60), enciphered also the surname of one girl-friend in the theme of the eponymous Abegg Variations, op. 1, and the birth-place of another in the harmonic structure of parts of Carnaval, op. 9. There is no mystery about this: he tells us in the text what he is doing. The mystery begins, however, if we ask, with Sams (1965), where and how we are to find, musically translated, the beloved wife and soul-mate Clara, compared with whom these other ladies were of little emotional importance. If anyone merited musical encipherment, it was she.

It is too pointlessly easy, of course, to find five-note phrases with C, A and A at the appropriate points, especially if you allow indiscriminate sharps and flats, and even a modicum of transposition such that what appears as B-flat can be called A-sharp. We need to be able to identify a whole system of transliteration, such that it not only pins down L and R (which have no direct musical equivalents) but also can be used to generate other relevant messages when applied to other phrases. Since any system which accommodates twenty-six alphabetical letters with about eight note-names (depending upon notation) will have to be repetitive, in the sense that the note A comes to signify not only its own name but also, say, letters K and S, the door is opened to the objection, whose psychodynamic analogue is all too familiar, that there is so much flexibility in this system that almost any message can be 'discovered' anywhere. But, once again, this objection is overcome or at least weakened by showing that the system produces messages that are so precise, so contextually apposite and so improbable by change that it becomes more reasonable to believe in the system than in coincidence.

To this end, Sams multiplies a great variety of examples. He can demonstrate that the same scheme that fixes L and R for 'Clara' also spells out her surname, the highly unlikely 'WIECK', in a theme occurring in Carnaval and other piano-pieces. In the case of his songs, there is an added dimension of relevance; for the music can be shown to spell out phrases that are appropriate to the words being set. Sometimes, again, the encoded messages fit in very precisely with Schumann's own verbal comments on a piece. Thus Sams indicates (1965, p.587) how the last of the Davidsbundlertanze, which represents, according to the composer, the end of an evening's celebrations and the chimes of midnight, can be seen to yield the German for "there was utter stillness" (es gab lautes Schweigen), the name of Clara and the word for "bed" (Bett). As a final illustration, which at once depends upon and explains Schumann's puzzling and sometimes bizane use of accentuation-marks, we may notice the case of the overture Hermann und Doro-thea. If we decipher just the accented notes in the first-eight bars, using the first edition of the composer's own two-piano reduction, we spell out "Hermann"; and if we do exactly the same for the second subject we get "Dorothea". This single example helps to establish three things: first, that widespread encoding occurs more or less for its own sake, and that Sams' particular solution must be pretty near the mark; second, that the odd accentuation is odd because it denotes not musical but cryptographic structure; third, that the reason why such markings are less frequent in later works and editions is that demonstrative exuberance is giving place to caution and even secrecy.

This all serves to expose some of the assumptions, methods and evidential considerations which are involved in tackling the question whether there are 'messages' in a body of data or not. Sometimes the 'fit' is so good, so appropriate and so revealing that we are under pressure to concede that there are more messages around than we had thought. But since the considerations of coherence and likelihood on which the argument depends are essentially unquantifiable, there will be no objectively calculable point at which we should succumb to that pressure; and our readiness or recalcitrance in doing so will be partly a matter of temperament.

It will also reflect, however, the weight we attach to negative evidence, or rather perhaps 'false positive' evidence. For one way in which such message-detecting systems are discredited, as we have seen for psychodynamics in general and the *Leonardo* story in particular, is to show that in some circumstances they generate what are palpably *pseudo*-messages. This line of argument is sometimes allowed to be more demoralising than its cogency warrants, and if we can cut it down to size in the musical analogue, this may help in handling its psychodynamic counterpart.

Sams shies away from claiming to have found Schumann's own christian name encoded in a particular anpeggio-figure, because he realises that it is a commonplace motif, used by many composers *without* any cryptographic intent. The consequence that there are places where Mozart and Beethoven could be made, spuriously, to spell out "Robert" in their music has an unsettling incongruity about it. Similarly it is hard to assess the destructive weight of one critic's observation that the Sam-Schumann code enables you (almost) to discover "scram Yankees" in the first bars of 'God Save the Queen' (ibid., p.590). The implied argument seems to be that, since this pair of messages just is not there in these data, any system which generates them must be, or is indistinguishable from, an illusion-generating system. But two observations will reduce the impact of this.

First, and corresponding to one of our comments above about 'overdetermination', if you have a cryptographic outlook, you may well use as vehicles for your codes all sorts of material which other people do not so use. For a crossword addict, the word 'times' just is a double anagram of 'mites' and 'emits' (and much more besides depending on what languages one knows). Thus to suggest that a common arpeggio may be "Robert" for Schumann is to imply nothing about what it is for Mozart. Nor is there any reason why a message-carrying episode should be an intrinsically odd-looking episode. If randomness, or the absence of system, produces patterns' more often that we think, then one system may be expected to generate another system's patterns still more frequently: we do not find it strange, for instance, that a French word is also a word in English. Consequently, we do not allow ourselves to be demoralised by the fact that, if we found a page corner torn from a book and bearing the words "son", "chat" and "fin", we shouldnnot be able to tell whether the book had been in French or English. We do not, that is to say, feel bound as a result to abandon the practice of distinguishing between French and English. Sams recognises also (1970, p.258) that, from the code-user's point of view, the best hiding-place for a pebble may be on a beach; in which case the code-breaker must sometimes explain the congruous with the system developed primarily for the incongruous, and the psychodynamic corollary of this is not far to seek.

Second, an admittedly flexible system is bound to give rise to some 'messages' by chance. What marks off Schumann's messages from "scram Yankees" is their much more precise fit and contextual relevance.

There will be cases, of course, of which we cannot say whether they occur by chance or by design, because we do not know just how much looseness or 'play' to tolerate in the system. The balance of tight and appropriate fits over loose and pointless ones, or <u>vice versa</u>, will eventually vindicate or discredit the system; but the material is such, inbboth music and psychodynamics that this balance is to be assessed by educated judgement not statistical calculation.

(c) <u>Themes and Variations</u>. We have already come up against the general question of how this unspecificity or 'play' in the matching of model-generated expectations against particular observations, can be dealt with in practice in some explanatory enterprises. And there are some further aspects of musical structure which run closely parallel to this problem in the characterisation of the psychodynamic structure of behaviour. Many compositions, and not only musical ones, depend for their coherence andiidentity upon the concept of variation; that is to say, upon the fact that some note-sequences (or coincidences even) can be recognised as being 'derived from', or a 'form of', or 'related to' another note-pattern. Sometimes the relation between variant and parent theme is simple, direct and complete: obviously the same time, with a change in ornamentation, lay-out, harmony or tempo. Sometimes the association is altogether more free, so that the relation is complex, indirect and incomplete: only a part of the time being treated, and its character rather than its contour being the focus of creative reflection. Again some sets of variations remain politely subordinate to their themes; as in the first movement of Mozart K.311, perhaps, or Handel's <u>Harmonious Blacksmith</u>. Others outgrow and dominate theirs: Beethoven's <u>Diabelli</u> set, of course.

But with all this variation on the theme of 'variation', to which may be added the associated but different business of thermatic 'transformation' or 'metamorphosis' (as exemplified in the <u>Wanderer</u>

fantasy of Schubert and the B-minor sonata of Liszt), do we not lose touch with the possibility of distinguishing a real from a bogus variation? When, we are inclined to ask, is a variation not a variation but a new tune; and cannot any motif be seen as a variation of some other motif, if we allow this degree of licence and flexibility of relationship? The wee-known difficulty of drawing lines and setting out criteria, to provide clear answers to such questions, leads some commentators to take up extreme positions. The problem is to know how much variation , in terms of wrong notes and omissions, I can introduce into a performance of the third Liebestraum before it ceases to be a 'performance' of the third Liebestraum at all. If I go far enough, what I play becomes indistinguishable from, and thus identical with, a performance of Three Blind Mice: the difference between the two works is 'only' a matter of variation and omission carried to absurdity. But since we cannot say where precisely absurdity begins, in order to avoid it we must jib at the thin end of the wedge and make it a necessary condition of a 'performance' that it be note-perfect. (As Ovid had it, "principiis obsta ...".) Or so argues Goodman (1969, pp.185-187).

Well, but there must be something wrong with a view that leads to aesthetic nonsense. There can be not doubt that a pianist who plays very slowly all the notes, and only the notes, of the first of Chopin's Studies op. 10 destroys its musical identity more surely than one who splits a couple of top-notes at a suitable tempo. And yet Goodman specifically says that "no departure from the indicated tempo disqualified a performance as an instance ... of the work defined by the score" (p.185). On the other hand, metronome-markings of speed do count as parts of the hallowed score, and must therefore be observed. But this is to swallow a camel after straining at a gnat; because we happen to know that some of Beethoven's metronome marks, for instance, do not represent the speed that he intended, since he apparently could not be bothered to grasp fully the arithmetic of the notation-system and certainly regarded verbal tempo-indications as carrying more weight (Schindler 1860, pp.425-427). And more recently, even Bartok admits in retrospect that some of his own metronome- marks were mistakes

(Dememyi, ed. 197., p.00). Again there are some well-known places where the received scores of the manuscripts and early published editions almost certainly do not reflect perfectly Beethoven's intentions as to the notes the notes themselves. Thus in the 15th Diabelli variation he seems to have forgotten to change the clef-sign on the lower stave at bar 6; and there are a number of apparent slips-of-the-pen in the texts of some piano sonatas, (cp. Von Bulow and Lebert 1894, pp.566-593). Goodmanian fidelity to "the score" would in these cases result in infidelity to "the work".

The artificiality of Goodman's prescription for avoiding uncertainty, in the matter of legitimate 'variation', consequently emerges as more awkward than the uncertainty which it is designed to dispel. But the problem of uncertainty-tolerance is exactly parallel in psychodynamics, where manifestly discrimination actions may be interpreted as variations on a common latent theme of psychpathology, with the corresponding rish of spuriously unifying behavioural elements which in fact reflect separate themes. We have to be careful, however, that whatever provisions we make to obviate this sort of mistake do not lead to the other extreme of discounting valid interpretations for wrong reasons (cp. ch. 7(a) above).

Sometimes the inform thesis that a person is miserly or aggressively contemptuous of others is defended, against the apparently conflicting observation that he has just behaved in a conspicuously generous or subserviently polite may, by the contention that these manifestations are but "the other side of the coin". This triggers the reaction that, if such a discrepancy as opposition, between implied actions and observed actions, is allowed to count as behavioural variation on a psychodynamic theme, then <u>any</u> discrepant action may spuriously be pressed into confirmatory service. But opposition is not mere 'discrepancy': it is a systematic relationship. The mechanisms of 'denial' and 'reaction-formation' are not, as we have seen above (ch. 5(a)), licences to admit to the class of confirmatory actions any departure from the manifest 'acting-out' of a postulated dynamics. Correspondingly, the fact that turning a theme systematically upside-down or inside-out is an explicitly acknowledged mode of musical variation or transformation (as in Bach's Art of Fugue, for instance) does not mean that any less drastic, but less systematic, modification must also be countenanced as a 'variation'. Indeed, sceptics might begin to doubt whether Reti's principle of 'interversion', which reverses the order of only some of the notes of a series and allows interpolation as well, is sufficiently systematic to be a 'principle' at all; but they are likely to be persuaded by the remarkable example which Walker identifies (1966, p.245) in his study of the structure of Chopin's B-flat minor sonata.

To pursue this comparison one step further, it might be thought that what sets psychodynamic 'variations' apart from musical ones is that, in the former case the germinal motive usually has to be inferred from the given behavioural variations; that they are typically, as it were, variations on an unstated or unplayed theme. But precisely this occurs in music also. Part of the puzzle of Elgar's Enigma variations is, in his own words, that "through and over the whole set, another and larger theme 'goes' but is not played" (Sams 1970, p.258). And a solution must state not only what goes but how it goes; which is why he also said that the mere statement that the tune concerned is Auld Lang Syne (which it is) "will not do". True opinion, as in Plato's Theaetetus, again, must be supplemented by an explanatory account (meta logou) to qualify as knowledge. Sams' study provides just such a logos, which meets all requirements and shows that you cannot really understand how it goes without cracking an Elgarian cryptogram somewhat in the manner of Schumann.

Perhaps even more pertinent, however, to the relation between a DI of behavioural structure and the TI which it engenders, is the fundamental concept of 'implied harmony'. If a melody is picked out in single notes, as one might sing or whistle it, then someone who is providing an accompaniment, say on the piano or guitar, has a certain amount of choice as to what harmony he plays for some of the melody-notes even within a conventional diatonic frame of reference. Thus, at the end of Three Blind Mice in G-major, the most conservative accompanist may choose to harmonise the C (last note but three) in either A-minor or C-major, each of which 'goes' equally well, determining thereby the emotional colour taken on by the tune. At other points, most obviously in the final cadence, the shape, character and direction of the melody will strongly 'imply' one particular harmonic structure and sequence rather than any other; though even here, a minor-mode tune which ends on the tonic-note may always be rescued from melancholy at the last moment by a consolatory major third in the accompaniment.

Given, then, that the harmonic structure or undercurrents of a melodic line may be more or less ambiguous, and that the way in which harmony is conceived, or realised in practice, affects the character and significance of the line, it follows that a proper perception (or aesthetic intuition) of what the implied harmony _is_ is necessary to the musical understanding of that phrase, as regards, for example its relation to what comes before and after it. It follows also that, as in psychodynamics, there is the possibility both of diagnostic dispute and of executive implications. And musicians do, of course, disagree about the implied harmonic structure of passages in the unaccompanied violin and cello works of Bach; about whether, for instance, you 'see' a full close and a fresh start at this point, or an inconclusive transitional harmony which looks forward and welds two phrases into a larger _gestalt_. For the way you 'see' or hear it will influence the way you play it and your conception of the work as a whole. This is not confined to compositions for largely single-line instruments, but applies equally to sparsely harmonised passages in any medium. Thus one editor can write, of bar 25 in the second variation of the last movement of Beethoven's piano sonata op.109 (which consists of four unison D's plus a trill on D): "The 'latent' fundamental harmony is the B-minor chord of the sixth, and a correct performance of the bar depends upon the player feeling this harmony" (von Bülow and Lebert 1894, p.627).

Now, it can scarcely be necessary to spell out the psychodynamic parallel of this. Consider the melodic line or the bare text as a person's overt behaviour; statements about implied harmony correspond to _DI_'s depicting the structural processes or intentional states of which it is the expression, and these may be quite different from the agents' own conception of them; thus, in communicating this new way of looking at things, however metaphorically and indirectly it may be done, a TI recommends a revised view of the internal structure of the agent's behaviour and thereby provides the opportunity of reorganising the structural elements (the anxieties, wishes, defences, conflicts) or at least of checking their maladaptive influence. Thus does one 'come to see', as opposed to merely agreeing, that an action or attitude, which one had perceived and intended in one way, really serves some other emotional purpose (as well).

Even works of philosophical exposition, which rely on explicit argumentation, can sometimes illustrate the fact that the way in which we perceive the 'form' of a behavioural episode or product can affect quite literally our understanding of its purpose and nature. If, for instance, some features of the opening fantasy of Parmenides' treatise on what "is" have left commentators puzzling about how to relate them to the stark logic epistemology and metaphysics which follow (Taran 1965, pp.17-31), the general issue has been thrown into much sharper relief recently by renewed discussion of the problematical structure of Wittgenstein's Tractatus (Janik and Toulmin 1973, pp.23-32, 167-201). The specific difficulty here is how to conceive the relationship between the last five or so pages, which deal with broad ethical and theological topics, and the whole first part of the book, which treats of the logico-mathematical representation of the knowable world ("that which is the case"). The point is that what we make of this structure profoundly affects what we think the work 'means' or is 'saying'; or, even more radically, what we think it is <u>about</u>. For if we see it as main exposition plus curious epilogue, then it will seem to be "about" the application of a neo-Russellian logical calculus to the metaphysics of 'representation', with some implications for traditional problems of ethics sketched in as an afterthought. If, however, we take it to be a meditation on certain themes introduced by a long technical prelude, then it will appear to be about central questions of ethics and value, upon which a particular logical technique, once developed, is brought to bear (howbeit somewhat inarticulately) to indicate what might be achieved. We are, therefore, "confronted with two contrasting views about the very subject-matter of the book" (ibid., p.25).

But how can such a dispute, about which assessment of the structure is the more valid, be resolved? What considerations are relevant, and to what do they owe their evidential weight? Once again, their range and general type are unspecifiable antecedently; and what concerns us about Janik and Toulmin's discussion is the enormous variety of observations which are invoked as having some bearing on the question, and as tending to support the latter of the two interpretations sketched above. There is, for example, the fact that Wittgenstein was dissatisfied with Russell's logically orientated preface; there is the way the book is viewed in Austrian philosophical circles; there is the strong impression made upon Engelmann, who had corresponded with Wittgenstein about it; these are the ideas which are known to have been in the cultural air in end-of-century

Vienna, with its non-specialist attitude to philosophy; there is Wittgenstein's admiration for Kierkegaard; there is his asceticism, and his all-pervasively moral outlook (exemplified in his indignation at Russell being polite to "fools" in the Aristotelian Society). And so the list could go on. But such a list can be drawn up only post hoc and in a particular case. There is no general principle of contextual relevance which will tell you in advance that light is shed on the form of a philosophical work by the fact that its author gave away the family inheritance or taught in a village school. Nevertheless it is possible to reconstruct and elaborate a view of this particular context such that these observations do arguably support one interpretation of the work rather than another.

This, then, was a case of a decision between one way of looking at the structure of the 'medium' has implications for what we take the 'message' to be; and it reminds us that the quest for relevant considerations may take us far afield, and may comprise observations whose evidential support-relations to that which they support are heterogenious (in that they concern cultural history, individual biography and specialised philosophical comment, for example) and more-or-less indirect (in that some of them become relevant only in the light of, and in combination with, others). But at least there were two fairly clear-cut candidates between which to choose for our view of the structure. Our final example, however, should, for the sake of its psychodynamic parallel, concern deciding between the nihilistic claim that a work has no particular structure and the positive contention that specific structural properties are indeed deomonstrable. Furthermore, the example should perhaps rely as much as possible on internal evidence.

Orthodox formal criticism was for a long time embarrassed by Chopin's B-flat minor piano sonata, op.35. There is the disconcerting originality of the stark and perfunctory finale; there is the fact that the slow movement was written as a separate piece two years earlier; there is the absence of traditional 'recapitulation' in the first movement; there is the harmonically odd introduction, and no doubt much else besides. All this led Schumann, for example, to say that in this sonata Chopin had thrown together "four of his wildest children" into a makeshift family, and thereby to imply the absence of coherent structure. More recently, Huneker clearly found his own half-hearted apology an uphill struggle (1900, pp.166-169). Walker, on the other hand, has maintained that the "inspired utterance" of the first four bars "determines the thematic destiny of the entire work" (1966, p.239); and the validity of his elaborate analytical argument for this contention seems to be taken for granted by Matthews (1972, p.226). This argument consists largely, of course, in showing that many important thematic and harmonic features of the later movements can be seen, when looked at in certain ways, simply to be the same patterns as constitute the material of the first movement and its portentous introduction. These "certain ways", which define the enlightening perspective, depend implicitly on two assumptions: one, that certain characteristics of the music are relevant, in the sense of being the likely vehicles of its structural "destiny", such as a bass-figure here, a chord there, an interval-sequence there or a melodic contour here; the other, that certain principles of 'transformation' (such as compression, transposition, inversion, reversal, interpolation or superimposition), which are explicitly acknowledged in other music, may be operating implicitly here. That is to say, you have to concede that collapsing a countour into a chord, turning a tune inside out, and combining two motifs one on top of another, all *are* relevant structural relationships; but once this is conceded, there is often little room for doubt thereafter that the postulated relation does hold between the features which have been picked out. Specifically, the development section of the first movement just *does* contain (in bars 238-248) the first subject superimposed on the falling bass-interval of the introduction; and the opening tune of the slow movement just *does* consist of the same notes as the first movement 's first subject "in strict retrograde motion" (i.e. backwards) ignoring rhythm, tempo and repeated notes (ibid., p.246). The doubt is not whether such relationships as the latter exist, for they undoubtedly do: it is whether the transformations on which they depend are relevant and significant, and whether the elements subjected to transformation are artefacts of the particular way in which the material has been divided up. The answer to the first doubt is that we are appealing only to the implicit operation of transformation-principles which operante more-or-less explicitly in other music, such as the mutation of fugue-subjects in Bach's *Art of Fugue* or the thematic "metamorphosis" in Schubert's *Wanderer* fantasy and the Liszt B-minor *Sonata*. The second doubt is met by challenging the sceptic to show that, and why, the designated elements *are* factitious:

that is, that they do not represent genuine functional units or "cells" as Reti has called them (1951). The perception of such relationships is certainly perspective-dependent (cp. ch.4), in the sense that they will not emerge unless you can 'construe' the data for phrases, intervals, harmonies, contours, balance, compression and so on. But this is not a licence for arbitrary decimation, for we are only "cutting up reality" (in Bergson's phrase) along the lines on which we know it is usually put together.

Thus the musician who seeks functional structure in the Chopin sonata is assuming that internal relationships of familiar kinds can be brought to light, if you allow that certain ways of looking at, or categorising, the data are appropriate, and that certain principles of transformation are likely to be operative. To demonstrate these structural properties is not, of course, to have exposed the composer's plans, intentions or deliberations during its composition; but it is to suggest something about how it should be viewed here and now, and it does carry some implications as to what should be done about it in executive performance. Precisely the same goes for DI. The interpreter adopts the perspective of construing the behavioural data for intentions, fears, wishes, guilts etc., where they are not manifest in the 'usual' way (i.e. to the agent); and, by appeal to a range of transformation principles, he claims to show how one aspect of the presented behaviour relates to another or to some postulated "intentional state". Although a good deal of retrospection may be necessary before these interpretive claims can be formulated, their implications are largely prospective in that they recommend one view of the patient's behaviour rather than another. The job of the therapist's T.I. is to communicate this view to the patient, so that he comes to see and feel it in that way also. Similarly the pianist's job is to get the nearer to "see and feel" the relation between the development section and the introduction. But the way in which the respective executants do this will depend to some extent on their respective audiences (cp. ch.2(c)).

Chapter 9

The Communication of Structure

(a) The ontology of structural diagnosis

(b) Executive implications of DI

(c) The Story so Far

"A musical structure contains the answer to the problem of its own interpretation. A great interpretation is never 'applied' from without; it always emerges from within" (Walker 1966, p. 256).

In this concluding chapter we bring the comparison between musical and behavioural structure back to the question of what sort of claims these structural assertions are making. And we take up again, in the light of this, a question which was broached early on (ch. 2 above), namely that of the relation between such assertions and the TI's which communicate to the subject a view of his behaviour in such a way that he is supposedly enabled to change beneficially. Finally the main features of the arguments we have advanced are reviewed in summary.

(a) The ontology of structural diagnosis. Having reminded ourselves of the sort of considerations that are regularly adduced to support the claim that there is some structure in this work or this behavioural episode (or the claim that the structure is not P but Q), we have to face again the question whether such claims say anything more than that the subject of interest can be seen in this or that way if you like. On the one hand, we have to avoid too much causal-categorical implication. In the musical case, when Walker exposes and spells out certain structural relationships within the Chopin sonata, he is not of course claiming that the composer devised or conceived them in those terms. There seems, however, to be a sense in which they exist nevertheless; and it is tempting to think of the terms themselves as reflecting the formal underpinning of Chopin's essentially preconscious aesthetic judgement that the various ideas did indeed 'go together' (cp. Gombrich 1966, pp. 35-36). And in parallel behavioural cases, we need to disclaim the idea that the postulated relationships are envisaged as obtaining necessarily or immediately between hypostasised determinants and observed effects. This is to reiterate that DI is often, if not usually, 'about' the character rather than the causes of the action etc. to which it refers.

On the other hand, I am resisting the sceptical suggestion that the structural relations which can be 'seen' are merely a function of the point of view which is taken up; and that there is something perniciously arbitrary about adopting one point of view rather than another. Well, they are indeed a function of that view, but not merely a function thereof. It is not like standing on one's head in order to see some phenomena which cannot be experienced otherwise. If I "come to see" (as the therapists say), and hence to sense aesthetically, the relation Walker demonstrates between the first subjects of the first and third movements, there is no point thereafter in not seeing it. So the tables of arbitrariness are turned. Given what you can see if you adopt this conceptual viewpoint, it would be unreasonable and destructive to abandon it.

Within these limits there seems to be scope for a certain ontological gradation, in that the sense of "is" in which 'the structure of this X is S' can vary. For when Bach tells us that this section of the Art of Fugue is a canon at the sixth, does not this suggest that its structure is indeed that of "a canon..." in a somehow stronger sense than that in which, for instance, Keller contends (1966, pp. 349-350) that the last movement of Tchaikovsky's F-minor symphony is "in reality" not "a set of free variations upon a Russian folk-song", as many critics say, but "another of Tchaikovsky's intriguing sonata structures" (which Keller sometimes calls a "sonata-rondo"). The test is that we feel more inclined to substitute 'should be seen as' for "is" in the latter case than in the former. Equally, the sense in which Satie's Three Pieces of 1903 really are "in the shape of a pear" (because he says so in their title) seems more than that in which Bach's so-called "wedge" fugue, from BWV 548, actually is wedge-shaped. It is true enough that, if you draw a line along the top of the higher notes of the first subject and another under its lower notes, you will unequivocally produce something which could just as well be a drawing of a wedge. But then, why on earth should you start drawing lines round the notes of the score in the first place? The 'point of view' surely is questionable, and to a significant degree "arbitrary", here; as also is the fact that it is not called the "expanding" fugue.

We can begin to makr this distinction, and at the same time capitalise on the observation just made that it is aesthetically destructive to reject certain points of view, if we take the metaphor of 'structure' rather more literally. The walls of a house may all be very similar in respect of material, thickness, colour and so on; but some are 'structural' and others are not. What defines the structural ones is that they are doing a certain job, namely holding up the house. Consequently, one way of telling whether a particular wall is structural is to knock it down and see what happens. We might then say that these walls which look alike are descriptively related, insofar as they are all green or six-inches-thick or brick-built, but that only some are 'structurally' alike in the sense that they do the same structural job. Some relationships among musical and behavioural data will be merely 'descriptive' while others will be 'structural'; and the test will be what happens, to our aesthetic experience or our psychological understanding, if you take them away. Thus behaviour-episodes which are descriptively similar may be structurally different: we saw that the mother's tense coat-buttoning might resemble an obsessional practice but really express something else. This is not to say that the similarity is illusory, for it is not. It exists: but it is descriptive not functional. Thus coincidental similarities or relationships are not bogus, but merely descriptive. This allows us to concede, for instance, that the slow movement of Rachmaninov's G-minor piano concerto just does start with a variation on Three Blind Mice, but to deny this fact any significance; the same goes for the "wedginess" of Bach's fugue-subject. Take away the idea, and what do you lose? Nothing. In fact you gain by the loss of irritating associations which some of us wish we could get rid of. But take away Walker's ideas from the Chopin sonata, and you may well feel that it falls apart into Schumann's "arbitrary family", and that in Huneker's words "these four movements have no common life" (1900, p.167).

The fact that mistakes can be made in attributing structural properties to relations which are really no more than descriptive, in this sense, does not itself show that the distinction in invalid. And yet critics still persist in trying to discredit DI in this confused way. Thus Bandura (1969, pp. 49-50) discusses, for this purpose, the case of a psychotic patient who was induced by selective reinforcement to display temporarily what might be called a 'broom-fixation'. A "psychotherapist" who was invited to interpret the phenomenon duly obliged with talk about child-substitutes and feelings of omnipotence. The point of all this is to imply (rather than to argue explicitly of course) that such psychodynamic hypotheses are as a class scientifically redundant twaddle. But what precisely is, or would be, the argument? Identical reasoning, or lack of it, would lead, given that stammering can regularly be induced in fluent speakers by electronically delayed auditory feedback, to the conclusion that all stammers are produced thus and that none are due to neurotic conflict or neuro-muscular deficiency. The psychotherapist's error was to mistake the descriptive relation between broom-cuddling and child-cuddling for a structural relation. We all know, however, that in many fields expertise can be defeated by forgery; and this was simply a piece of behavioural forgery. But we do not conclude, from the fact that art-experts mistook the descriptive association between Van Meegeren's forgeries and Vermeer's paintings for a structural one (namely that of having been produced by the same hand), that art-expertise *per se* is bogus.

This general distinction then between 'descriptive' and 'structural' enables us to escape the ontological embarrassment of having to suppose, *pace* Parmenides, that some of these relationships are more real than others. What makes 'wild etymology' wild, and distinguishes it from its controlled counterpart, is not that the superficial relationships between the words to be derived and their postulated sources are less real in the former case than in the latter. They are just as real; but the point is that they are not derived from an appropriate transformation -system. There is indeed a relation between the Italian *maestoso* and the Latin *maestus*: but it is only descriptive. The relation which is both descriptive and structural, because consistent with the transformations which happen to operate, holds, of course, between *maestus* and the Italian *mesto*. These descriptive correspondences between words of different origin and meaning, whether the correspondences are visual or acoustic, can be turned to 'transformative' account in certain rhetorical tropes and figures of speech, by using one word to evoke two sets of associations. Devices associated with the pun and the *double entendre* obviously function in this way. And it is part of our general contention that the way in which therapists represent the behavioural relationships to which they draw attention have transformative implications for the patient.

This has some affinity with Walker's feeling, expressed in the quotation above, that the diagnosis of musical structure implies or generates of itself certain consequences for executive interpretation. For both the pianist and the therapist are trying to get an audience to take a certain 'view' of their respective material.

(b) Executive implications. Not only interpretive psychotherapy of psychoanalytic orientation, but also the supposedly "non-directive" procedures typified by Rogers, aim to get the patient to 'see' his feelings and actions in a different way, to feel differently about them, and consequently to feel and act in some respects differently in the future. But whereas, so-called "client-centred therapy" seems to be characterised by the belief that the client can achieve such modifications as a result of what amounts to supportively facilitated introspection and catharsis, without needing to be moved in one direction rather than another, it is the job of 'transformative interpretations' (TI) to recommend to the client particular readjustments in his apperception of himself and to provide the psychological conditions for making them.

These conditions depend classically on the concept of the "transference", which is that process of stimulus - generalisation whereby the client comes to respond to the therapist as he had done, in significant respects, to the parent-figures of childhood. The efficacy of TI consequently derives not just from appropriate timing (cp. ch. 2) but, even more fundamentally, from its placing in such stimulus-conditions. It is, of course, precisely such features of the context in which they are used which give rise to the familiar objection that TI's trade for their effect upon the emotional authority of the therapist and upon the dependent suggestibility of the client. Alternatively, it may be, as we have also noticed above (ch. 3(a)), that client and therapist share a common background theory about the desiderata of mental health such that what look like diagnostic-descriptive statements about what the client _is_ doing in certain situations cannot help but acquire both evaluative connotations about what he _ought_ to be doing, and some perlocutionary force to nudge him in that direction. It is the same with Austin's example of the back-seat driver who says "Driving like this is a good way to break the springs" or words to that effect.

Perhaps it is misguided, therefore, to seek to derive the 'transformative' from the 'diagnostic', that is to recommend and precipitate psychological shifts in the patient, in the absence of some such over-riding theory. There may well be no implications strictly _sub specie aeternitatis_; but only for people living in the here-and-now with some conception of how they want (or ought) to function. This is what makes the claims of some theorists, who think that they have and need no such theory, pernicious; for it engenders needless guilt in those who have a theory, but also are aware of the fact.

Even in the musical case Walker's claim that a structural diagnosis of the Chopin sonata implies of itself certain aspects of executive performance is rather too ebullient. Firstly, there is obviously no one way of playing, say, the first three bars in order to get the hearer to hear them _as_ the "germ" of the whole work. Secondly, if he means rather that the pianist's 'executive interpretation' will be coloured in a general way by his wish to communicate certain internal relationships, then this interpretational motive depends on the aesthetic and evaluative assumption that grasping the structure of a work is a higher and fuller musical experience for the listener than merely enjoying the tunes or being dazzled by the player's technique. Now the "non-directive" therapy of Rogers is theory-dependent in the same kind of way. For it assumes _both_ that much emotional disturbance etc. is a consequence of inaccurate self-perception or inadequate 'self-actualisation' <u>and</u> that autonomously acquired insight is a sufficient condition for overcoming these deficiencies. There are indeed further assumptions, about how you can tell when the deficiencies have been overcome. Even if they are true for some clients, these assumptions nevertheless constitute just as definite a theoretical background to the process of therapy as the Freudian or Jungian has.

It is hard to avoid remarking, while on the subject, that, by comparison with this mild theoretical myopia of Rogers, the holier-than-thou protestations of freedom-from-theory which are made doctrinally on behalf of Skinnerian methodology are grotesquely impertinent. Fortunately for the cause of truth, however, they are often so clumsy as to contain their own two-line refutation, as witness the following vignette. "Suppose we are interested in the problem of human depression following the loss of a loved one, and we feel that the investigation could profitably be carried out in the animal laboratory" (Sidman 1960, p. 27). One does not move so smartly from the melancholic wards to the simian laboratory without some doctrinal propulsion (and cp. ch. 6(c) above).

By comparison with the purportedly non-directive 'play-back' of client-material by the Rogerian therapist to his client, the interpreting therapist is certainly trying, part of the time, to communicate to the patient a 'view' of the material, and to get him to entertain it empathetically rather than just intellectually. This will rarely be enough to effect the required change; but it can be a necessary condition for self-help, and for being able to use non-specialist support (cp. Winnicott 1971, pp. 2-6). It is one thing to recognise one's conflicts, anxieties, wishes and defence-patterns, and another to 'work through' them so as to bring them under ego-control or 'integrate' them into ego-function. The emotional atmosphere of the transference, however, being regressive, supportive and directive, is intended to facilitate the necessary un-learning, maturation and readjustment.

Seen from this standpoint, which may well be that of a relatively inefficient form of therapy, it will merely be a first step to identify the areas of poor or neurotic adjustment. In practice this may mean getting the patient to see the relation between his pathological, puzzling or unwanted behaviour and his other feelings, actions and attitudes. The idea is that, by coming to see one's 'accidental' missing of the train as related to his wishes, he may both get a clearer picture of his real feelings (which may be disconcerting) and thus be enabled to prevent them from confusing his future actions. Similarly, to see some misjudgement of a person, or some unrealistic fear, as a projective transformation of a feeling about oneself, opens up quite short-term possibilities of improving one's social perception and of reducing anxiety. Thus people can to some extent use insights of this kind, into the structure of their feelings and actions, in an immediately constructive, relief-giving and prophylactic way; and this will depend upon their having sufficient ego-strength to tolerate the diagnostic insight, and sufficient flexibility to put it into practice. These resources are sometimes tested in clinical interview, as we saw above (ch. 2(b)), by seeing how a patient reacts to being confronted with a DI. Two general consequences about the transformative mobilisation of DI follow from this: one concerns the prevention of pathological developments, and the other the specificity of transformative techniques.

We have seen that the transformative efficacy of therapeutic interventions which are concerned mainly to communicate a DI depends on the context in which it is given. This context includes both the nature of the aims and assumptions common to client and therapist, and the psychological make-up of the individual client. The new perspective, or insight, which is offered can be turned to constructive use most immediately when the client can appreciate what other situations and actions are analogous to the one interpreted, so that he can be on his guard against, or 'catch himself', doing the same kind of thing again; and when putting the insight to use in this way does not conflict with deeply established habits of emotional reaction. For when they do so conflict, a good deal of un-learning and re-training of such reactions will need to be done; and it is for this regressive and reconstructive work that the special climate of the transference-relationship is held to be necessary, because those reactions and attitudes have been laid down in childhood and 'stamped-in' ever since.

Suppose for instance, that we eventually give a student the interpretation that his avoidable examination-failures, or his 'crowding out' of necessary work by trivialities which assume momentary importance, are a way of preventing himself from succeeding. The extent to which he can <u>use</u> this for self-monitored readjustment would be expected to depend upon whether this emotional attitude to success is associated with the sub-cultural norms of some current prestige-group which holds that "egg-heads are freaks", or with more generalised, deeper-seated and longer-standing feelings about adult achievement, father-rivalry, potency and the rest of it. A splendid example of an under-achieving schoolboy patient being able to <u>use</u> transformatively a nakedly 'diagnostic' psychological report, which was intended for a clinical colleague but literally came into the boy's own hands, is given by Murray and Jacobson (1971, p. 737).

This kind of distinction has been expressed in various ways, which reflect different attitudes to the possibility and nature of therapeutic practices that should operate without plumbing the remote and long-term depths of classical psychoanalysis. For the example just given raises the general question whether there are aspects of the personality, or "areas of ego-functioning" (in the provocative but cashable spatial metaphor), which are relatively uninvolved in the particular conflict and can consequently mobilise their own therapeutic forces with temporary guidance and support from outside.

This objection, that traditional psychoanalysis underestimated the potentiality of collaborating with "conflict-free ego spheres", was notably expounded by Hartmann (1939, 1964). The consequent shift of emphasis has paved the way directly or indirectly toward a variety of more cognitively orientated approaches such as the "rational" psychotherapy of Ellis (1962) and the "assertive" techniques of Phillips (1956; Phillips and Weiner 1966); not to mention methods derived from Kelly, which seem to suggest that patients can revise their construct-systems by acting-out alternative ones, and can experience a transformative outcome from being taught to construe positively rather than negatively (Bannister and Fransella 1971, pp. 130-159). The apologist is always tempted to see innovations as but a selective and elaborated emphasis upon procedures which played some part, howbeit relatively minor, in primordial psychoanalytic activities. Did not Freud's treatment of "Elizabeth von R", whose throat paraesthesia was traced to the suppression of anger towards an uncle, include 'assertion therapy'? "I did my best", he writes (Breuer and Freud 1985, p. 171), "to get rid of this 'retention hysteria' by getting her to reproduce all her agitating experiences. I made her abuse her uncle, lecture him, tell him the unvarnished truth, and so on, and this treatment did her good".

Be that as it may, the development and reported utility of psychotherapies which appear to proceed by pressing healthy ego-functions into service in a variety of ways leads to the question, which has been taken up, for example, by Malan (1963), whether more orthodox 'interpretive' methods can be more efficient, or at least less time-consuming, by assessing and mobilising the insight, flexibility of defences and stress-tolerance which the patient can call upon in his constructive response to <u>TI</u>. It also underlines a reservation, which Dalbiez made many years ago in a classic review (1941), to which any discussion of the relation between <u>DI</u> and <u>TI</u> must bow:

namely that the links between the explanatory elements of psychoanlytic theory and the orthodox therapeutic techniques are relatively tenuous. That is to say, the diagnostic account that someone behaves in a certain way because of some unresolved psycho-developmental conflict may be entirely true without entailing, even on the theory's own terms, that the conflict needs to be dissolved and overcome by the same kind of interactional processes as those through which it arose. The converse assumption of some behaviour-therapists that, if you can abolish a neurotic habit by some deconditioning program~~me~~, it must have been learned from a corresponding paradigm is equally misguided. The fact that appendicitis may be cured by surgical excision does not entitle the conclusion that it was caused by surgical implantation. This complementing of psychodynamic DI with transformative procedures which are essentially non-psychodynamic has been discussed more recently by Kline (1972, pp. 353-359).

It seems to emerge from all this, then, that the transformative use of interpretation may be most effective in dealing with rather specific and 'acute' emotional reactions in basically healthy people; and perhaps especially in forestalling maladaptive emotional development by identifying latent reaction-tendencies and dynamic patterns at a stage when they may still be diverted from pathogenic trends by beneficient manipulation of events, experiences or contingencies of reinforcement (or just by plain discussion). In practice this would mean that one of the most fruitful applications of DI may be in identifying the incipiently pathogenic structuring of feelings, attitudes and defences, in their early behavioural expressions, while they are still relatively malleable by non-specialist means. Of course we all try to do this informally; and to that extent all parents and teachers play the part of preventive therapists, basing their situational therapy on more or less acute and sophisticated diagnosis of the dynamics which are operating.

The procedure of actually presenting a person with a new view of the structure of his behaviour cannot be expected to be constructively transformative until he has sufficient security, not to be unduly threatened by it; and until he has sufficient ego-skills, flexibility and insight to try out and monitor an unfamiliar way of 'looking at' himself, his feelings and his actions. Consequently the fresh 'perspective' and 'communication' of structure, contained in the well-worn phrases "It's a bit as if you were trying to....", "Perhaps part of you really wants to...." and "So you felt that he/she was....", will be most readily helpful when generally well-functioning people are thrown into anxiety, confusion or one of Levy's "binds" by some new or temporary situational stress. Thus the counselling of everyday-life reactions and the prevention of needless conflict, rather than the modification of profound pathology may be the most appropriate, and the ubiquitous, application of TI. But this will come as no surprise to those students of psychodynamics who find the most persuasive demonstration of its diagnostic counterpart not in the aetiology of the neuroses but in The Psychopathology of Everyday Life.

(c) The story so far. It remains only to delineate in retrospect what my main contentions have been; or rather, perhaps, what they should have been, in case my treatment of some particular episodes has distorted the story I have been trying to tell. On the surface it comprises some negative and some positive themes; though I have already remarked that the grammatically negative ones draw some constructiveness from being logically double-negative.

(i) Negative. The possibility of valid psychodynamic explanations, involving DI, is defended against certain criticisms based on assumptions about "scientific" procedure. The objection that it does not proceed by hypothetico-deductive steps is met by showing that many empirical investigations, both within and outside the paradigmatic hard sciences also do not. The objection that its data are necessarily or very largely contaminated by methods and perspectives of observation is met by showing how such limitations are overcome in other factual studies, with special reference to the uses of contextual evidence. A number of linguistic analogies are used to counter the objection that arguing back from admittedly loose generalisations about behaviour to the significance of particular instances is logically misconceived.

As far as the therapeutic use of interpretation is concerned, we try to rescue some categorical and propositional core, from the scepticism of Levy and Farrell, by arguing that it essentially communicates (in terms adaptable to people and situations) a 'view' of patient-behaviour which at best depicts relevant structural features no less objectively than do some theoretical 'models' in established sciences.

(ii) Positive. Since what we are concerned with is ex hypothesi "expressive" behaviour, it follows first that our description of data and of relations between data will have to take the implications of 'intentionality' seriously, and will avoid a phoney atomism and experimentalism; and second that we can expect to draw some help, in conceptualising and understanding our material, from other disciplines which deal with the idea of A expressing B, such as aesthetics and language-study. In particular the logic and practice of translation and the linguistic concepts of 'deep structure' and of 'generative-transformational' principles, are assumed to be potentially fruitful. If we apply these latter linguistic analogies to my patient's remark "Ah, your conventional opening" (ch. 1(a)), which occasioned the TI "Perhaps you feel that what you have to say... is not so conventional", we can see this as my postulating, by way of the "deep structure" of the patient's behaviour, an 'intentional state' of concern for conventionality or of wishing to be conventional, which is expressed via the transformational principle of (the defence-mechanism) 'projection'.

Thirdly, the fact that *DI* is characterised in practice by forging structural links between surprising elements or aspects of behaviour, in such a way that the resulting *gestalt* both confers coherence upon, and also suggests heuristic affinities with, other actions or feelings, invites comparison with some of the logically diverse model-schemes in the physical sciences. Thus in the case of the stammerer (ch. 7(d)), the *DI* which interprets the self-destructive Rorschach moth, the bringing of the Rousseau book and the daydreams of maternal rescue from gratuitous danger, all as 'mod~~e~~al transforms' (rather than causal products) of a masochistic trait encourages us to align them with other material reflecting guilt and punishment (such as his 'oedipal' attitudes to his father). And this is precisely the kind of purpose which the crystallographer's structural model of common salt as "a cubical lattice..." serves in a vastly different area of empirical understanding (ch. 5(c)). The closer investigation of such parallels as these in the philosophy of science may be expected to enable us to differentiate coherent features of psychodynamic interpretation from misguided ones, provided that such investigation is carried out with a conception of rationality which is adapted to the essential humanity of human behaviour.

> "Oft in the passions' wild rotation tossed
> Our spring of action to ourselves is lost:...
> As the last image of that troubled heap,
> When sense subsides and fancy sports in sleep
> (Though past the recollection of the thought),
> Becomes the stuff of which our dream is wrought;
> Something as dim to our internal view
> Is thus, perhaps, the cause of most we do".

(Pope: Moral Essays(i), 41-50)

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