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SCIENTIFIC PRINCIPLES

IN

PSYCHODYNAMIC INTERPRETATION

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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.

CONTENTS

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 de impersonalisation 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 sciencyes 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 these 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: salus extra ecclesiam non est.

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 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 <u>caused</u> 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 <u>their</u> 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 <u>alpha</u> and <u>omega</u> 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 other-
wise) 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 rigorous 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 so 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 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

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. Their 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 /guardians repositories 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 they 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) <u>What are we talking about?</u> 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 <u>not</u> 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* ~~means certain~~
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 _function_, 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 ex-
planation-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 _link_, and it is a problem to know what form
such vindication would take. For that depends on what _sort_ 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 <u>example</u> ? 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 l 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 t alking 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 <u>learned</u> 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 <u>definiendum</u>
'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 <u>absence</u> 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)   <u>Some aspects identified</u>. 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 <u>are</u> 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, L^evy 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 <u>some</u>
such proferred restructurings of my view of ^{an} action or habit of
mine are <u>interpretations</u> 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 <u>kind</u> 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 <u>any</u> sort of fuller understanding or greater
knowledge of <u>X</u> shall count as a psychodynamic interpretation of
<u>X</u>. 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 <u>what</u> 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 <u>Interpretation</u>
<u>of Dreams</u> suggest that he at least was prepared to think that

it might be the latter: <u>Flectere si nequeo superos,</u>
<u>Acheronta movebo</u> (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" (<u>latere</u>) 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 <u>ego</u> and the <u>superego</u>,
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 primi-
-tive 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, in terms of what *with respect to*
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 state-
ments 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 these
things that we do regard as works of art from those that we do not,
Wollheim found himself forced into a similar corner (1968, pp.??-??).
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 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 <u>resemblance</u> 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 <u>representation</u> of
Napoleon nor prevents it from being one of Wittgenstein.
True, somebody could <u>use</u> it <u>as</u> a representation of Napoleon
in a history class; but this only emphasises the role of
intention, and I could still insist that it was <u>really</u>
(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 <u>what</u> I was
drawing was Wittgenstein.

Since there are many ways of 'representing' any one
thing, (the first point above), some 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 <u>Tractatus</u>). 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 <u>The Creation</u>; 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 <u>The Creation</u>?

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; $\overset{a}{\phi}$n assumption from which it obviously
follows that it would be idle to expect a one-to-one
correspondence,$_\wedge$between observed behaviour and interpreted
over a variety of instances,
psychodynamic oontent. 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 execûtant performer gives
an interpretation of a literary or musical text. But the latter
analogy will recur in the context of the therapeutic use
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
as, or in the spirit of, art-work has to be allowed to count.
Indeed, some would extend the notion to include anything
perceived 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 is 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

critéria is abandoned (pp.119ℱ120), what is this spirit
which is put in its place? Another Wittgensteinian concept,
even more nebulous than 'family-resemblance': a "form of
life" (<u>Lebensform</u>). 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 cliches 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 of 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 connters 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) here.
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 _for_ the feelings, and so on, as expressions _of_ them:
and there are those, of course, who would even seem to _identify_
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 _inferring_ a concern with
conventionality; it was an _example_ 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 unlike 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 it 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 explanandum 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 inter-
pretations really are that sort of thing in disguise, or that their
function is different but nevertheless repectable in its own fashion.
Reluctance to be restricted in this way is reflected in the rather
more informal program of observing that the raison d'etre 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 one
logical way in which it is dispelled, and if there were thus only
one 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 expression of
something, rather than the effect 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 <u>hidden</u> to the agent, "over and above"what he
can see (in Rycroft's phrase), still leaves two possibilities:
what the latent meaning <u>is</u> may be hidden, or the <u>connection</u>
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 <u>him</u>. And it is
very different from my simply <u>telling</u> 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 <u>I</u> thought the damp palms
meant.   Does my ignorance of a connection between emotional
state and physiological correlate turn someone else's judgement
about the former into an interpretation? What seems to be important
is the <u>way</u> the judgement is derived from the observational data,
and what <u>sort</u> 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
     GSR

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 'Q' with P. The therapist has not said that Q is
another case of P, which might have amounted to an interpretation
along the lines of 'Q means the same as P, did you but know'.

But suppose that P had already been interpreted as meaning
X. Then to suggest, hint or convey the impression, that Q ~~xixa~~
is another case of P, or is analogous to it (in some way which is
often unspecified), is tantamount to interpreting Q also as meaning
X. What is the observer to say that the therapist has done in this
case?; was his intervention an 'interpretation' or not?; if so,
has the therapist given 'the same' interpretation (as given for P)
again? And what is the interpretation, then: that Q means the
same as P; or that Q, like P, means X? (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 inter-pretation; 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 further, and introduce some of the difficulties to which it leads.

    (a) Elucidatory features. It seems obvious that we can separate these two possible functions 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. Communieation 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 spirit 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 interppretations
(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, (portentous, they will do so
inconsistent or
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 that about
the child, some with how a man sees himself vis à vis 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 indep-
endent 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 just-
ifications 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 th 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 _passim_.

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 psycho-
dynamics; 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 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 food, 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 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 "X" is a d-symbol" is not sufficiently clear: does it or does it not, for instance, entail that Shakespeare intended X to represent d (and does it make sense to ask whether, if so, he succeeded?)?

Still less shall we be able always, or even usually, to predict 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 support 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 assertion, 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 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 dennotes), 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 conern 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 tr-
ansform.

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 really was 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 different
time or in a different way, and so on. 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:
-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 interpret 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 effective 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,
section5 *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 cerⁱtain 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 adjudgement 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 nôt 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 former sort of interpreation of specific material
without being able to make the latter. That is, you may be abâe
to identify the dynamic significance of for instance, a delusional
idea or obsessional thought (by seeing in what emotional conditions
it recurd to the patient and perhaps what sort of purpose it
serves in his psychic economy) without knowing how it opmes 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 trans-
ference-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
id-interpretation from ego-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
(op. 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 actirity is nominally based. We must
return, however to the logical relationship between the para-
digmatic 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 (1950), 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 Freud'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 more
'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 unreal-
istic, 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 _feels_ 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 them-
selves). 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 _Fantasy_ 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 Leitmotif 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 CI (and we have seen Weitz, in another context, insisting on
their variety), the propositional judgement on which the EI 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 CI (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 DIqattributes 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 there 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 or explicitly,
there is consequently a great variety of observations or considereations
which bear, in different ways, upon their vlidity.   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
give1the 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 'p' 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 'Vive la France' and 'Long live France' to "express"
the same thing in different languages. And the form in which a judgement
is expressed will depend partly on the purpose for 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. expresses some D.I. them in a
therapeutic way, for a particular person of 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 Parsifal 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 role/raison d'etre 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 _says_ to a patient, in order to get him to grasp a particular DI 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 DI (the 'message' which is being conveyed) but also by what the therapist believes about the insight- fulness, 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 _from_ which it is drawn, - as opposed to the context _into_ 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 under-
> stand 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 Crucifixion 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, rather than 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 some-
thing odd, of course, but not necessarily useless, in the idea of
someone painting like 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
feelings? 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 Weltanschaung, 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 of 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 given the interpretation, as surely as
Shakespeare has made 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 be 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 6f 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
RUBBISH!
"control", in some alarming 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 contrast 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
represented as producing "operant responses" (of a sort), in the
context of "discriminative stimuli" (of a sort) and meeting with
"reinforcement contingencies" (of a sort) does not 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 merely 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 _feel_ better (for a while, perhaps, or even for ever) but
actually _made_ 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.00). 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 interpreations 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)   The irrelevance of truth

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
never 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 sequelae) 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 _can_ arise as a result of _mis_-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
prescriptive definition that is done when setting up a symbol-
system _de novo_. If I decide, for some purpose like the Hanfmann-
Kasanin test of visuospatial concept-formation (Semeonoff & Trist
1958, Ch.2), to _assign_ 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 _really are_ "nevs" in the same sense that they
really are made of wood (nor even in the sense in which whales
are 'really' _mammals_). But, once you have decided to administer
my version of the test, it would be a _mistake_ 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 _use_ 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 _coding_ 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, _passim_).
I mean: even if ticket-hoarding is never, in point of fact, so
caused, it would still be true that such hoarding _is_ (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 _naming_ (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 X and Y as 'A-type' behaviour
(and not so classifying Z), when not postulating common properties,
is the assumption that X, Y 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 con-
firmation 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
chaice 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
can 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 any 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 meta-
physics (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
choosing one's facts ("la choix des faits";)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 knows 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
is a system, which is really a system, 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 sort 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 geometric 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 do, 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 <u>itself</u> intended to have transformatory force.   And
insofar as he is predicting anything, it is not that the tablet per as
<u>se</u> will cure the ailment, but that <u>taking</u> that tablet <u>in the belief</u>
<u>that</u> 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 manipulating 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 former
does not; but its real force, of course, is to <u>alter</u> 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 <u>TI</u> becomes merely a special form of benevolent
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
some 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/ive to quicker
and/or better masƀery (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/
the required ways of 'looking-at'. These statements may take the
form of categorical/ 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 pull the trigger: you squeeze 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 feel it as 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 _really_ a squeeze or a pull is both ontologically
odd and beside the point.

Now for 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' _up_ the keyboard and then _down_ again;
but the point of the remark is to get him to _think_ of it as
a series of one-way leaps, _all_ taken from base-camp (that is
to say, you don't _jump_ 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 remakk 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 trouble 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 the 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 ff 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 hypo-
theses 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 as 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. pp.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 perspect-
ivist is not the difference in symbol-system, like calling the
inhabitants of a field vaches and moutons 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 they 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 language,
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 <u>has</u>
found a given perspective helpful?; (2) how long do you wait
before deciding that it will <u>not</u> 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 <u>avoid</u> situations that
trigger neutotic reaction-patterns rather than by having
improved in ego-strength and adjustment so that he can now
deal with them maturely (Malan <u>et</u> <u>al</u>. 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, provided that 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 out 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 <u>not</u> 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 super-ego 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 per-
spective, 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
otherpeople'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</u> <u>as</u> preferable to pathological
dependence and passive manipulation; the fact is that the
'working-though' of resistances has been since early days
a central task in interpretive psychotherapy. Certainly;
but this depends upon the <u>theory</u> that 'help' consistsiin
moving the patient away from the 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 interpret-
ations, 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 any propositions or recommendations (however implicit)
about it.

Because he asserts nothing about 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 some 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 whiah direction by
a therapist has contributed. This claim intturn 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
add 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-organis-
ation 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 notmmerely 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 warn 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 recoverywwould 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 been 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 under-lying 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 character-
ised 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 content-
ment 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 xxxxxxx 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, is to accept herself 'realistically',
... This realism seems to be accompanied by a sense of
freedom and contentment" (ibid). 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-perseptions 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 some 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 mores of the group's
subculture determine what is to be accepted as the "true"
personality of the participant (Cheshire 1973).

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 an 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 inteeviews.

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 whih 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 _other_ 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 _do_, 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 anti-
thesis 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 method
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 over-
come, 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 <u>tertium quid</u> 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 problem arises of saying <u>what</u> 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 <u>tertium quid</u>
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 <u>imprimatur</u> for saying, as it often
seems reasonable to say, that psychotherapy can make
discoveries <u>of a sort</u>; and that a therapist's interpretive
discovery-claim may be valid <u>in a way</u>, 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 ⌐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 hhat 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
eridential 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 decontaminatdd 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.OO). 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 are and 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 alone. 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 both in the situations of
therapy and projective testing and also in day-to-day non-clinical
interactions, such as those on which Freud based his Psychopathology
of Everyday Life (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 trans-
ported 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 aee true only "internally" to the
industrialists' or the gambler's Weltanschauung. On the
contrary, they could both be true de facto, 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, wemmay 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 an 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 respective
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 <u>both</u> be system-dependent <u>and</u> 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 some 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 explicit 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 response 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 depreivation, 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 nôt
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 what 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 Tractatus was not
an atomic language but "atomic facts".

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 is
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 less 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 incon-
sistent 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 <u>ad hoc</u>
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
'<u>ad hoc</u>', 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, i n a
particular case.

We can keep a certain grip, to be sure, in the archae-
ological 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 orient-
ation (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 post-

ulate 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 part-icular 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 un-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 inter alia, 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 only 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 with-
out 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 back-
ground 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 <u>afraid</u>
of <u>X</u>; let us suppose that (in a sense) you <u>want</u> <u>X</u>, and see
if that makes things fall into place'. 'You say that you are
trying to <u>achieve</u> <u>Y</u>, but let's suppose you are "trying"
to <u>avoid</u> it ..."; 'you say you <u>love</u> J, but let's suppose you
are <u>ambivalent</u> (and consequently guilty) about him ...'; and
even, as in our original example, 'you describe <u>my</u> behaviour
as conventional, but let's suppose you are concerned about how
conventional <u>your</u> <u>own</u> 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 <u>not</u> 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
<u>syllables</u> 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 sub-
sequent 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 <u>impossible</u> for someone

operating _without_ 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 back-
ground 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 mechanics
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 correotive 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 reconstruct-
ion 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 non-
sense.   (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 counter-
act 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 repetit-
iousness, 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 demon-
strated, 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 observ-
ations, 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 mis-
leading. Although the patient actually says or does 'p',
he should (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 (op. 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. Ahd 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 con-

structing 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 circum-

stance 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 part-

icular 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 general-isation 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 con-
form 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' ($\underline{P}$) or consequence such that, if it were false
that all A's are B, you would not observe $\underline{P}$ in specified
conditions $\underline{x, y, z}$; this $\underline{P}$ 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 $\underline{P}$ 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 observ-
ations 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 psychoanalys~is~ of tight generalisations and
testable predictions renders it simply "unscientific", -
which leaves it as no more than some sort of religion o~r~f
myth (1953, esp. p. 241). More recently he has gone so
far as to identify explanation, "in a scientific sense",
with hypothetico-~d~eductive 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 e~m~pirical explanation because it is intrin~s~dically
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 ps~y~choanalytic concepts", which, in the interests of
consistency, he ought to ~r~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 electro-

magnetism 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 different 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 generalisation 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 story
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~~ general-
isations, 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 <u>both</u> when the engine is
cold <u>and</u> when the engine is hot.   It is not that '(some)
cars start badly all the time" (i.e. at <u>any</u> engine temperature):
for in <u>that</u> 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 <u>extremes</u> of engine
temperature'; and that is why <u>both</u> "Well, the engine is cold
now" <u>and</u> "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 <u>D.I.</u> when it purports to show
that behaviour <u>B</u> arises from conflict (etc.) <u>C</u>, is that
in eliciting the intermediate stages and linking them
together, the implied argument is something like: <u>C</u> gives
rise to <u>x</u>, <u>x</u> is connected <u>y</u>, <u>y</u> is expressed in <u>z</u> and <u>B</u> is a
form of <u>z</u>; so there you are. The trouble here is that the
linking-relations, between the elements in the storyoor the
stages on the route (<u>x</u>, <u>y</u> and <u>z</u>), are of different <u>sorts</u>,
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 relationahips. 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 out-
come'. 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 evid-
ential 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. OO) that the doctrine that all
scientific explanation has a hypothetico-deductive frame-
work 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" Geisteswissenschaft
are being given more space, in contrast to those who favour
a tough-minded Naturwissenschaft (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 byaa 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 topoutline 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 theppast, the accounts they produce may often be true, evidentially-
based inferences about the physical world.   And yet, if we were torreduce
these successful explanatory accounts to syllogistic form, theywwould
rarely if ever include the universal law-like generatlisation ('In conditions
C, people always do X'; or 'when Z occurs, B alwaysffollows') 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, statistically-speaking, 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 (<u>Verstehen</u>), as opposed to adequate statistical records, to be able to see that a given action is likely (or would be <u>thought</u> likely) to produce the required results in given circumstances. Dray's paradigm, somewhat elaborated, for explaining action $\underline{X}$ of person $\underline{P}$, accordingly runs:

1.   In a situation óf type $\underline{C}$, the appropriate thing, for someone with motive $\underline{M}$, to do is $\underline{X}$

2.   (a) $\underline{P}$ was in situation type $\underline{C}$
      (b) $\underline{P}$ had motive $\underline{M}$

3.   Therefore $\underline{P}$ did $\underline{X}$

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 $\underline{P}$ with "the appropriate" or reasonable thing-to-do. For, in the absence of '$\underline{P}$ is a reasonable man', as 2 (c), we have no ground for expecting <u>him</u> 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 $\underline{C}_{b....e}$, rahher than $\underline{C}_{a...d}$ as previously assumed, or towards showing that the protagonist's main motives were $\underline{M}_{a,d}$, rather than $\underline{M}_{b,c}$; 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 <u>actions</u>,

as opposed to the movements or noises of bodies. The
issue is given away in Dray's phrase, "When in a situation
of type C_1, C_2...C_n"; and in Hempel's, "the empirical
circumstances, as seen by the agent" (op. cit., p. 73;
italics added).

Two problems arise at once.   First, it is notoriously
difficult to specify individually and categorically the
properties of a type 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 Sinn.   And one-and
-the-same action, externally specified, may of course have
a quite different Sinn, in one context of perceptions
and intentions, from what it has in another.   Even within
the artificially restricted milieu of a chess-game, the
very same move may represent, or rather actually be, 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 <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
qualifies 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, except 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 is 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 <u>Sinn</u>.

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 <u>are</u> 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 <u>that</u> supposition is arguably one of the contributory superstitions in the 'myth of deductivism'.

(c)   <u>Analogies and structural relations</u>.   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 <u>various</u> ways, a plausible 'structure' which can be related to the phenomena of observed events and states (the subject-matter of traditional contingency propositions) in <u>various</u> 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 <u>inter</u> <u>alia</u> three main contentions.   One is that the part played by explanatory discovery in <u>D.I.</u> 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 how such structures
illuminate the behaviour to whcih 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 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 the 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 natura rerum, though not necessarily confined
to it, and the end-product of the postulated system will
often be only analogous to, not identical with, the explananda.
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 logical relation between identifying the
underlying structures and processes, on one hand, and
'understanding' the surface phenomena, on the other.   It is
entirely possible to mis-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
envisaged 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 proerties 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 anthro-
pomorphic 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.

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 the 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 may 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 'modal transform' in contrast to its
disjunctive type known as the 'causal transform', is
what obtains between descriptions of model-states and des-
criptions 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 causal transforms.   It
is a modal 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 ante-

cedents in common.   This amounts in practice to implying either

that behaviour-items are phenomenally alike (as going back to check

the windows is _like_ 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 _prelude_ 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 _general_ 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

(b) The totem of prediction

(c) Criteria for evidence

(d) The metaphysics of relevance

(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 fitted 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: <u>Narrative of a Child-
Analysis</u>. What we need is a <u>story</u>, not a syllogism. But what
<u>kind</u> 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: <u>how</u> it is despelled is
what counts. Sherwood's point seems to be that there should no
longer be a <u>logical</u> 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 (obses-
sionality, 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 'enigmatic 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 sus-
pended 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 _more_ of the story, as a different _kind_ 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 _Sinn_ (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 implic-ations 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 insuf-ficiently passimonious in its multiplication of explanatory _entia_, 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 _Leonards_ study is concerned precisely with the question whether Freud's overall picture (even if time) should be preferred to the piece-meal explanations of particular points of perplexity which the art hist-orian 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 psycho-pathology 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 spebial-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 puzzlempicture, 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) The totem of prediction

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 PAN's to this hypothetico-deductive procedure is
sufficient to persuade those critics for whom prediction is a sine
qua non, 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 PAN operates; in particular with respect
to how it tells a different kind 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 Theaetetus that 'true
opinion' does not presuppose understanding, and that, when bereft of
such logos, 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 libido 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
nece ssarily 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

many things which we know are consistent with a particular hypothesis about some 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 DI. 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 b, d, f, rather than a, c, e. Since, however, we usually do not know whether the relevant conditions were 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 PAN, 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 PAN about Lorenz's behaviour involves four or five different kinds 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-sufficient 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 difficulty, 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 ($\underline{C}_{f, g, r}$), act in a certain way (b).   This
presupposes that we can identify conditions f, g, 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, g, 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 different 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 under-
lying 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 'trans-
formational' principles, if we may now allude to Chomsky, which
are necessary 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 genrrative
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 _for_. 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 _that_ procedure is not entirely free from the problem of relevance which PAN 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 de-
velop 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 anthropo-
morphic 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 counter-
ing 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 should the laws of non-laboratory behaviour hold for a laboratory situation?   And, in point of fact, 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 (on Sidman'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 _some_ behavioural observations, which systematically failed to wear the right tie or come from the right school, nevertheless provided rich _evidence_ about all sorts of things. These sources of embarrassment were, on the one hand whole _genera_ 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' _himself_ by _not_ 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 'evidence', is to consider what observations etc. actually _do_, or _would_, 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 evi-
dential 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 hhis 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

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the range of actions open to the agent in respect of it.   In investiga-
tions of the practice of deductive reasoning, such as those carried out
by Wason ~~et al~~ (1968, ~~et al~~ 1969) in which the subject is faced with
four cards, each showing either a letter or a shape, and with a hypo-
thesis like 'All circles have a vowel on the back', we can say pre-
cisely 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 in-
clude 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 techni-
cian 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 to general 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';

'$\underline{x}$ leads to $\underline{y}$' 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 pro-

cedures and background hypotheses which should underpin acceptable
empirical explanations.   In particular this rationale both depends
upon unquantified tendency-generalisations and often consists in identi-
fying 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 postu-
lated, 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 <u>for</u> <u>the</u> <u>given</u> <u>reason</u>.

(a) <u>Tendency and 'arguing back'</u>.   The background hypotheses and generalisa-

tions on which <u>D.I.</u> 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 <u>D.I.</u> is, on this view, more-or-less insecure-

ly 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 <u>instead</u> 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 <u>no</u> 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 generalisa-

tions 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 <u>D.I.</u> in terms of generalisations which are ad-

mittedly 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 sen-
timent of Pope, who had written (Moral Essays i, 99-103):

> "In vain the sage, with retrospective eye,
> Would from the apparent what conclude the why,
> 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 necessarily 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, as he rep-
resents them, do exhibit such a fallacy.   But the questions at once arise
whether the implied scheme of those particular arguments could be repre-

sented 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'; be-

cause, 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 ana-

logous 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 'All SYM buy JSC', then the observation that he has not bought a

JSC warrants the conclusion that he is not a SYM; while the observation

that he has 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', 'threaten-ing' 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 psychodynamist'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 schmme 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 intro-
jective 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
can 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
(<u>a</u>) is non-scientific, (<u>b</u>) employs premises of the loose forms that
we are concerned with, and yet (<u>c</u>) 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)   <u>The analogy of language</u>. When Jackson renders the first three
words of the <u>Aeneid</u> as "Arms I sing and the man ... ", this can be
treated as an empirical claim of the form: 'When Virgil wrote <u>arma
virumque cano</u> 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 ante-
cedent determinants, there is evidently no question of deducing
particular consequences from the 'hypothesis that by '<u>arma ...</u>'

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 respec-
tability 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 con-
clusions 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 approxima-
tion 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 frequancy'.   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 trans-
formational 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/situation is an im-
portant 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 situa-
tion, 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 selec-
tive.

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 statis-
tical 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 condi-
tions, '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 any usually
in a position to operate with quantified propositions of this kind. Nor
are their tendency-statements necessarily making the unquantified majori-
ty-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 systema-
tic 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, 'implica-
tive', in the sense of implying of some structure which at once generates

the tendency and is the ground for asserting it: cp. Sherwood's dis-
cussion 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 tenden-
cy" and any particular warrant-statement for it, and that they do not go
much further than articulating or formalising the intuitions of the per-
ceptive 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 retro-
spect 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 ab-
sence of the behaviour (or experience) concerned will serve to support
the DI, 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 both
'Well, the engine is hot' and '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 en-
couragement 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."

<div align="right">(<u>Moral Essays</u> <u>i</u>, 103-8)</div>

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 dic-
tionary, 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 con-
cerned 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(o)). 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 situ-
ation 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 co-
herence 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 con-

textual 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 ob-

stacles which are 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) <u>Dimensions of context</u>. 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, harks 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 crop 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 conceptualise a particular action as aggressive, panicky, self-satisfied, defensive, ostentatious and so on.

In the case of 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, analogously 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 clear, 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
dent regularly2means '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 can 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 bolt, '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
r$_a$ther 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. OO 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 unlikeli-
hood, 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 odd-
ness, 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 pair 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 was Bill Jones, than that two
different men should resemble each other so closely. But, as we have
noticed above, it is not merely the number of such common properties
that counts, but rather their information-loading. Thus, in the absence
of definite points of discrepancy, the congruence of a couple 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 P's behaviour should
show so many features that could derive from a Q-conflict, without
being so derived?; a claim which (leaving 'overdetermination' aside)
defies the construction of an alternative account of the same features.
That is why D1 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 seems 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 p*r*oblem 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".

PART III. The Structural Basis of Transformation

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 - identifi-
cation, 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 thera-peutic 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 accep-
ted 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, al-
though 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 determin-
ants 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 _are_ 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 inter-

pret B (a behaviour-episode, such as my client saying "Ah, your conven-

tional 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 sym-

bolically 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 exis-

ted 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 for-
getting 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 relation-
ship 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 con-
current state of mind or state-of-functioning (what Tormey calls an
"intentional state") which in its turn can be illuminated by certain onto-
genetic 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 in-
stance 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 ex-**
pressed, 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 ex-
plicanda 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 ways
of admitting guilt than we had thought, then there will be a higher in-
cidence 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 are 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 passim, 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 organis-
able) 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 some 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,

some 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) Is there a message here?   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 is expressive at

all, never mind what it expresses.   Acculturalisation consequently depends,

in part, on learning where to look for such expression (cp. Fong 1965).

The same holds, indeed, within cultures.   Argyle, for example, has can-

vassed 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 are 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 <u>tell</u> us quite explicitly that particular phrases, sequences,

notes or instruments represent particular things suggest that the sig-

nificance 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,

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it seems less unreasonable to suggest that all novels are 'autobiographi-
cal' 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 inter-
pretation went into this kind of detail, in section two:

> "Herein are pictured the Hero's opponents and de-
> tractors, ...   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 inter-

pretations 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 psycho-dynamic 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 inconguity 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 she 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 makes 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 _vice versa_, 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 thematic '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 indist-
inguishable 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 apposition, between implied actions and observed actions, is allowed to count as behavioural variation on a psychodynamic theme, then any 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 identi-
fies (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, varia-
tions 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 (<u>meta</u> <u>logou</u>) to qualify as know-
ledge.   Sams' study provides just such a <u>logos</u>, which meets all require-
ments 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 <u>DI</u>
of behavioural structure and the <u>TI</u> 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 accom-
paniment, 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 con-
ventional diatonic frame of reference.   Thus, at the end of <u>Three Blind</u>
<u>Mice</u> 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 struc-
ture 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 <u>TI</u> re-
commends a revised view of the internal structure of the agent's behaviour
and thereby provides the opportunity of reorganising the structural ele-
ments (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 argu-
mentation, 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 <u>Tractatus</u>
(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 about. 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 con-
textual 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. Neverthe-
less it is possible to reconstruct and elaborate a view of this parti-
cular context such that these observations do arguably support one inter-
pretation 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 considera-
tions 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 philo-
sophical 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 be-
tween 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 struc-
ture and the positive contention that specific structural properties are

indeed deomonstrable.   Furthermore, the example should perhaps
rely as much as possible on <u>internal</u> evidence.

Orthodox formal criticism was for a long time embarrassed
by Chopin's B-flat minor piano sonata, op.35.   There is the dis-
concerting 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 <u>be</u> 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 contour 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, inter-
vals, 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 struc-
tural 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 per-
formance.   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 <u>T.I.</u> 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 under-

pinning 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 <u>DI</u> 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 <u>merely</u> 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 <u>not</u> seeing it. So the tables of arbitrariness are turned. Given what you <u>can</u> 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 <u>X</u> is <u>S</u>' can vary. For when Bach tells us that this section of the <u>Art of Fugue</u> is a canon at the sixth, does not this suggest that its structure <u>is</u> 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 psycho-

logical 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) <u>Executive implications</u>. 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' (<u>TI</u>) 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 <u>TI</u> consequently derives not just from appropriate <u>timing</u> (cp. ch. 2) but, even more fundamentally, from its <u>placing</u> 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 <u>TI</u>'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 <u>desiderata</u> 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 per-
locutionary 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 perform-
ance 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 inter-
pretational 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' and 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 con-structive, 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 inter-ventions 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 child-hood 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 use 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 use 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 TI. It also underlines a reservation, which Dalbiez made many years ago in a classic review (1941), to which any discussion of the relation between DI and TI 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 de-conditioning programme, 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 investi-

gations, 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 mis-conceived.

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 'intention-ality' seriously, and will avoid a phoney atomism and experimentalism; and second that we can expect to draw some help, in conceptualising and under-standing 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 <u>DI</u> is characterised in practice by forging structural links between surprising elements or aspects of behaviour, in such a way that the resulting <u>gestalt</u> 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 <u>DI</u> which interprets the self-destructive Rorschach moth, the bringing of the Rousseau book and the daydreams of maternal rescue from gratuitous danger, all as 'model 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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