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THE LOGIC OF MOTIVATION

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I - Motives as "Intervening Variables."

There is a kind of fallacy to which psychology, especially the psychology of motivation, seems particularly prone (although it is not by any means peculiar to it) and which has been pointed out many times under different names — e.g. "faculty-naming," "hypostatization," "the postulation of imaginary forces," "verbal magic," and so on. But its logical structure has rarely been made explicit, and one finds that even those writers who attack it frequently commit it themselves.

For example, it seemed on first reading that MacCorquodale and Mead, in their provocative and thoughtful paper, (36), had gone a good way towards clarifying this problem, but the fact that their authority has been invoked in support of widely diverse views, including methodologies that appear particularly disposed towards hypostatization, might be taken as a sign that that paper had failed of its effect. This is due to what seem to me to be inconsistencies in their train of thought, but when these are cleared away it can be seen that they have provided materials for a clearer understanding of the process of hypostatization.

The main burden of that paper was to draw a distinction between two kinds of theoretical construct — "Intervening variables," whose meaning and truth were completely reducible to those of the "empirical relationships" with which they dealt, or which they described, and "hypothetical constructs," which they said had "surplus meaning" so
that the truth of statements involving them was not completely
reducible to the truth of statements about the empirical relationships
in connection with which they were hypothesized. MacCorquodale and Meehl do not contend that either of these kinds is illeg-
itimate. What they do object to is the surreptitious use of
intervening variables as if they were hypothetical constructs - as
if they could sustain the functions of the latter. So far the
argument seems perfectly sound, and the fallacy to which they are
pointing seems to be the "verbal magic" one - i.e., giving a
name to a certain kind of event and then using that name as if
it accounted for the occurrence of that kind of event. One
example of this, I suggest, might be to use the phrase "having a
valence" as meaning "being the object of our striving", and then
seeming to account for our striving for something by saying that
it has a valence for us.

But when one considers their criticism of "libido" one feels
that they do not quite make the point that the situation one cannot
use the intervening variable to account for is just the situation
whose name it is. They simply say (11, p.105) that "certain
puzzling phenomena are deduced ('explained') by means of the
various properties of libido..." And in their examples of "pure"
intervening variables we find some that could hardly avoid being
used in the illegitimate way - e.g., "valence" itself - and
some that seem quite distinct from the sort of "calculational
device" (to use Spence's phrase) which, following Tolman's scheme for "breaking down into more manageable form the original complete $f_1$ function" between behaviour and the independent experimental variables (17), they originally offer as intervening variables.

This confusion, I contend, comes about mainly because they are not clear on what the "empirical relationships" they are discussing are between, or, in general, what sort of thing can have relations. Thus by manipulating the four variables which enter into Hull's equation for habit strength (9) - number of reinforcements, delay in reinforcement, amount of reward, and stimulus-response asynchronism ($N$, $t$, $w$, and $t'$) - they show clearly that intervening variables in the "calculational device" sense depend on quite arbitrary groupings of the empirical variables concerned, and contend that from these four they "could define 15 alternative and equivalent sets of intervening variables" (11, p.98). But concerning one of these, a new intervening variable involving only $t'$ and which they suggest might be called "cumulative reinforcement", they say: "Suppose now that a critic asks us whether our 'cumulative reinforcement' really exists. This amounts to asking whether we have formulated a 'correct statement' concerning the relation of this intervening variable to the anchoring (empirical) variables". Now, to ask
whether its relations to the empirical variables is correctly
stated is already to treat it as "existing", although this question
of existence, which is brought in at many points throughout the
essay, is not relevant, since, as Bergmann (3) points out, ratios
between the quantities of different variables exist even though
the number by which a ratio may be expressed may not itself stand
for a quantity of anything. More importantly, to ask about "its"
relation to anything is to treat it as being the sort of thing
that can have term of a relation - that is, as being qualit-
avive, as being some state or condition or "stuff" that there can
be quantities of. Such a notion must always have "surplus meaning":
that is, a term of a relation must have some nature, some col-
lection of properties, other than its having that relation, other-
wise it would be unintelligible to say that it had that relation.

What must "it" refer to?

Now, such a conclusion is precisely what MacCorquodale and
Mehil want to avoid, but it is entailed by their speaking of "its
relation to the empirical variables". What should really be
considered is whether a "correct statement" of the relationship of
the number of reinforcements to response strength has been formulated.
Concerning this in general, the point is that its "existence" is a
question of whether the functional relationship between environmental

variables and response strength has been correctly stated, which is precisely why Hull retained the $s$ and the $x$ in his notation: that is, in order to stress the point that it is a mathematical relationship between something done to the organism and something done by the organism, and is not in itself in any precise sense a description of the organism.

It is interesting however to note that Hull himself makes precisely the same error in giving his developed account (10) of what he means by the apocalyptic phrase "anchoring variables at both ends". He says (10, p.281): "... my own system ... requires that the habit strength ($SH^a$), afferent impulse ($a$), and drive intensity ($D$) must each be calculable from their antecedent conditions, that the nature and magnitude of the reaction potential ($SR^a$) must be calculable from the values of $SH^a$, $a$, and $D$ taken jointly, and that the nature and magnitude of the several reaction functions ... must each be calculable from $SR^a$." But the point is that the formulae for calculating "habit strength", "drive", etc., from "their" antecedent conditions were derived in the first place from the series of experiments in which variations in each of the environmental variables listed (the others being held constant) were correlated with variations in the different measures of response strength, not in any sense with variations in "intervening variables". Thus this process of calculation and verification that Hull is
describing it just a matter of verifying those empirical correlations with response strength for different values of the antecedent conditions - i.e., of checking the curve-fitting at different points of the curve.

We have to be clear, then, that the empirically found mathematical relations are not between (for example) sHr and its antecedents on the one hand, and between sHr and its consequents on the other, but just between the antecedent and consequent events - in fact, that that relationship is just what sHr is. The other notion of what it is - i.e., some qualitative condition of the organism, a given amount of it being produced by specified environmental events, and in its turn producing a given strength or frequency of response - is what MacCorquodale and Meshal refer to as a hypothetical construct, but, as I suggested, their description of the meaning of intervening variables seems in some places to imply that sort of notion.

Now, that might seem hardly more than a slip of the pen on their part, especially since they go on to say (11, p.39) that "when habit strength means the product of the four functions of \( w, t, t', \) and \( H \), then if the response strength is related to these empirical variables in the way described, habit strength 'exists' in the trivial sense that the law holds" - although one cannot see why this should be thought trivial if nothing else was ever expected of sHr. But if that really is what MacCorquodale and
Meehl mean by "intervening variable" then why do they not press home their sketched-in criticism of any rigid hierarchy of intervening variables? They suggest that Tolman's argument (17) that it is easier to determine the complicated function by parts than "as a whole" is "not very cogent". They do not elaborate this point, but I take it that what they are suggesting is that one can only do it step by step, so that setting up a doctrine which advocates the use of such intermediate steps cannot be any contribution (if that is really all it offers). One might say that if we want to discover the product of 3, 4 and 5 then it is not really possible to multiply them all together at once: we have to find the product of, say, 3 and 4 and then multiply that product by 5. But that procedure has no advantage whatever over first taking 3 and 5 together, or 4 and 5, so long as we really are concerned only with convenience of calculation, and in their manipulation of \( w, t, t', \) and \( H \) MacCorquodale and Meehl provide all the materials necessary for showing that the same applies to the grouping of all the environmental variables studied by Hull, including as well as those four the maintenance and stimulus variables effective at the time of any given trial. If convenience of manipulation is the only concern, it must be very difficult to show why that particular four out of all these factors should be taken together, and why their product should be given
a special name.

Hull, too, feels sensitive on this point, since he puts a similar objection into the mouth of "his friend Woodrow" (10, p. 284). His replies to it are quite unconvincing: he says in the first place that even if we did put all the subordinate equations together to form one, it would be found still to contain "in some form or other the mathematical equivalents of the various equations linking the observable and the hypothetical unobservable elements of the situation". But to say that the groupings are merely for convenience is to deny that one is thinking of any "hypothetical unobservable elements", and as for the rest, to say that the mathematical representatives of the observable elements (and the relations between them) would still be found in the master equation is to admit the point that "Woodrow" is making.

Hull's second point in reply to the critic gives up the attempt to justify intervening variables on a formal basis and introduces the material consideration that the action of the four training variables might be temporally remote from that of the critical stimulus and the state of need, and "while it is perfectly possible to put into a single equation the values of events which occur at very different times", still those past events cannot be causally active now, and "S-H is merely a quantitative representation of the perseverative after-effects" of those four
training variables (10, p. 265). Hull, then, groups the variables in this regular way not merely as a matter of convenience in calculation (if indeed one way could be more convenient than another), but also because he regards the variables in any group as acting together to build up some specific condition in the animal, and the intervening variable based on that group would then be thought of, if not actually as a "measure" of that condition, at least as varying quantitatively in direct relation with it.

One might contend, then, that some such speculation about the accompanying qualitative states could be the only reason for clinging to a set order of groups and giving names to their products when these products have still to be combined to discover the probability of a given response. But as "calculational devices" the only solid content for the notion of "intervening variables" is just the collection of correlation coefficients between response strength and each of the empirical variables found relevant to it. Without some such mathematical form we could not give even this slight meaning to the term "intervening", since we would be left only with the assertion that a given factor has some causal connection with a given response, and causality does not in any sense intervene between antecedents and consequents; it is just the antecedent event that produces the consequent, not
"causality".

The finding of such relationships, by the way, indicates the only sound meaning for the phrases "empirical variables" and "empirical relationships" — that is, what is really meant is more like "empirically-found relationships", and even here, for the empiricist, the phrase is redundant, since there is no other way of arriving at knowledge than by finding it empirically. By using the word in this way MacCorquodale and Mead convey a vague suggestion that empirical relationships are to be compared, unfavourably, with more certain, more fundamental, more intelligible "laws", and it is strange that this hint of the rational is to be found amongst the positivists at large (e.g., 7).

One point that helps to preserve this distinction in thought is that it is very difficult to discover perfect regularity in the sense of coextension when we are looking for causal connections. Frequently one can find only conditions that are sufficient but not necessary, or necessary but not sufficient. This has also led to the current suspicion that there must be something wrong with causality, has led to the not-talking about it, and to the being content with (and compensatory exaltation of) correlation coefficients, and statements of probability in general. Without denying the usefulness of such mathematical procedures as a first approach to a confused field, one can still say that the rejection of indeterminism will involve denying the adequacy of probabilities
in science, and will involve affirming the presence of a criterion for every case in which a necessary condition is not also sufficient, and vice versa. That is to say, it is always theoretically possible to extend our knowledge of the effective conditions until we arrive at a set that is necessary and sufficient for any given event. In seeking causes, such criteria can only be found by considering both the nature of the thing in which the change is produced and the nature of the thing acting on it (1). A recognition of this principle seems characteristic of the work of those scientists who increase the number of general propositions known in their subjects: to take only two examples in psychology, there is the recent work of Tinbergen (16) with his emphasis on the need for both a specific type of stimulus situation and a specific bodily condition for the production of an instinctive response, and the explicit formulations of Freud (8), which might have prevented a good deal of the dreary heredity-environment controversy, if they had been better known. Watsonian behaviourism, then, was based on a false premise - "... given the response the stimuli can be predicted; given the stimuli the response can be predicted" (18, p.167) - since the same stimulus situation will produce different responses in the same animal according to changing conditions in the animal - a point recognized in part by Hull in his emphasis on P.
If psychology is ever to make predictions, then, rather than mere statements of probability, it is, in MacCorquodale and Meehl's terms, committed to "hypothetical constructs", or more precisely to making hypotheses (and trying to verify those hypotheses) about what processes in the animal mediate a given change in its behaviour. As Bergmann points out (4), these hypotheses will be assertions that processes of a kind which we have known in other places are going on (so far unobserved) in this place - the point being that we can arrive at the notion of any term or kind only by confronting such a kind, only from experience. Where else could we get the material for our fantasies? Wherever we seem to "construct" the notion of a kind, it is always by conjoining properties that we have encountered (separately) in actual things - and if that is so, then it is sufficient to dispose of the doctrine that there can be "convenient fictions" in science, when these are said to be neither true nor false. MacCorquodale and Meehl themselves reject this latter doctrine, but their retention of the phrase "hypothetical constructs" renders them liable to be misunderstood as supporting it (cf. Bergmann, 3), as does their question whether constructs are "existential" - as though it were a real possibility that we could talk about some thing which was not existential.
Now, "intervening variables", in the sense described above - as
correlations between events impinging on the organism and responses
produced by the organism - cannot be states or properties or
qualities of the organism itself, even though they presuppose
the existence of such qualities. The environmental variables
may have been grouped in a specific way (as Hull's are) because
of some tentative speculation about a specific change produced in
the animal by each group, but even so, and even if these speculations
are put forward, the mathematical relations remain distinct
from the hypothesised processes which are held to account for
them. This is a point overlooked by MacCorquodale and Koch.
when they say (11, p.101) that "there are various places in
Hull's Principles where the verbal accompaniment of a concept,
which in its mathematical form is an intervening variable in
the strict (Tolman) sense, makes it a hypothetical construct".
That makes it seem that they always have half thought of inter-
vening variables as being in some very vague sense "in" or "of"
the organism (and have accepted them thus as legitimate), the
only point being that they are not/be ascribed properties,
not to be characterised in any way (cf. O'Neill, 12), else they
become hypothetical constructs. This suggestion is borne out
in their discussion of Skinner's treatment of emotion as "a
'state of the organism' which alters the proportionality between
reserve and strength. They say (p.102): "The 'state' of emotion is not to be described in any way except by specifying (a) the class of stimuli which are able to produce it and (b) the effects upon response strength. Hence emotion for Skinner is a true intervening variable in Tolman's original sense."

Now, previously they had described "state" as a "wholly non-committal word" which specifies "nothing except that the conditions are internal" (p.97). But this point about internality is precisely the one at issue in discussing the fallacy of hypostatization. We may agree that there always will be some condition of the organism in virtue of which, in specified circumstances, a given response will be produced, but, in direct contradiction to MacCorquodale and Meehl, it must be described in ways other than its relations to its antecedents and its consequences (or in general its relations to anything), otherwise (v.s.) statements about what produced it and what it produced (or about any of its relationships) will not be intelligible. In saying that it is not to be described except by specifying these relations, and in saying (by calling it a "true intervening variable") that it is identical with the "empirical relations" and yet is a state of the organism, MacCorquodale and Meehl are setting up the notion of something whose whole nature it is to stand in a given relationship.
Even if it is objected that it is the organism that has the relationships, and that they are not its whole nature (since it has many other properties) but only a part of it, still this modified doctrine faces the same difficulty - namely, that it is strictly "unspeakable" (2), since we can only grasp a relationship if we can distinguish the terms that have it to each other. That is, seeing them as distinct, as having distinct natures, is a part of seeing them as related. If we say that its relationship to a certain stimulus situation is part of the organism's nature, then the whole relation (including its other term, the stimulus) seems to be brought within the organism, so that we cannot really understand the assertion that there is this relationship between distinct terms (this being the insuperable problem for Lewin's life-space).

This unworkable view that a thing can be in whole or in part made up of its relationships is the crux of hypostatization - of all doctrines of unseen forces or magical entities. This may be seen more clearly if we consider the organism's relations to its own responses. For the most part a response really stands logically as a new property of the organism - i.e. as a change in its nature - and so of course it is a part of the organism. But we are concerned with the relationship of this new property to the state of the organism immediately prior to its appearance, and if we make that relationship a part of the preceding state
of the organism then we have the characteristic form of the fallacious doctrines we are discussing. That is, the organism produces that response simply because it is in its nature to act in that way: it has a propensity to do so. At one stroke we are absolved from seeking for those actual states of the animal which determined that, under some specific stimulation, it would produce that response, and from discovering precisely what the stimulus is, since it would seem merely an "inclining" cause at most, being in fact a necessary but not sufficient condition.

This is the very fallacy MacCorquodale and Meehl are criticizing in their attack on the use of libido to explain features of behaviour, but they mistakenly think it appears when libido is ascribed properties of its own, and that the way to avoid the fallacy is carefully to keep its nature devoid of any surplus meaning - i.e., surplus to "its" functions. The same prescription seems to be the one central to operationism, and though in the long run it goes astray, it is possible to see some force in it in one specific connection - namely, in the definition of dispositional concepts, with which in one place MacCorquodale and Meehl identify intervening variables as they conceive them. We must sympathise with at least the policy implied in Stevens's cry (15, p.330): "Only then shall we never think of energy or consciousness as a substance..."
Taking "solubility in water" as a dispositional concept, then following Bergmann's argument (3, p. 99) a proper (positivistic) definition of it would be to say, e.g., that "x is soluble in water" means "if x is put in water, it dissolves." The verification of the first sentence is held to be completely reducible to that of the second, which implies (and Bergmann makes this quite explicit) that the words in the first sentence mean precisely the same as the words in the second sentence. (Some positivists hold that this complete reducibility applies to all defined concepts, though that extreme view seems to have been modified by Carnap, (5, p. 464, ff.) The general proposition that all definitions are nominal comes down to saying that there is no such thing as coextension, which as I suggested above is tantamount to a rejection of determinism). Now, it seems to me that the suggested definition is deficient in that it does not take into account objects which in fact would dissolve in water but which never are placed in water. (Modern symbolic logic would hold that the defining sentence could be converted to "either x is not put in water or x dissolves", and that if the first of these disjuncts is satisfied by x never being put in water then the whole sentence is true of x - i.e., the conditions for x being soluble are satisfied, and the definition is held in this way to be adequate even for the negative instances. But if we admit that meaning of "either ... or..." then
as Carnap points out (5, p. 440) the definition would include not only lumps of sugar that are not put in water but also such things as a wooden match that is not put in water - anything, in fact, that does not meet that fate would "satisfy the conditions" for being soluble in water).

The difficulty can be met, however, by amending the definition to read "x is of such a nature that if it is put in water, it dissolves", even though we do not know what that "nature", that common quality or character of soluble things, may be. Now, in principle the verification of the presence of this character is not completely reducible to the observation that when x is put in water it dissolves, since any quality is theoretically observable, and its presence then could be established (if we knew what it was) by direct observation, without the necessity for observing its effects. But one might say roughly that the verification of solubility is thus reducible because (and this in my view is the point the positivists are really getting at here) it is not the character in question. To call a thing "soluble" is just to say that it has some unspecified character in virtue of which specified events produce a specified change in it, and that it retains that character even when those events do not materialize. In itself it merely poses the question, what this character is, but it is very frequently used as if it were the answer to that question - as
if it were that character itself — and that in fact is the typical way in which hypostatization occurs.

Ironically, however, by its very insistence that dispositional concepts are not "substances" and do not have "surplus meanings," positivism sometimes leads to precisely the sort of mysticism it is trying to make impossible. That is, it is taken up wrongly (even by many positivists) as suggesting that there is no characteristic there in virtue of which the events in question take place, that the mysticism lies in going on to look for it, and that the scientific procedure is to be content with "solubility," and that we cannot fall into confusion as long as we rigidly exclude from our thinking any suggestion of quality or "substance" in the matter at all (failing to see that it is only from the notion of "solubility" itself that it must be excluded). But such a course of thought (which seems to be MacCorquodale and Meehl's) makes the fallacy inevitable: if there is no relevant quality there, nothing which produces the dissolving but is describable in terms which make no reference to producing that effect, then that relationship (to dissolving) must be thought of as just "being in the nature of the thing." Not only is such a notion not itself a solution, but while it is retained it specifically makes a solution impossible.

The reply might be made in defence of MacCorquodale and Meehl
that they plainly do recognize the possibility of finding the qualitative processes mediating any response since they recognize "hypothetical constructs" as scientifically legitimate. But in my opinion the error remains: for them it is not that the hypothetical construct is found alongside of, or underlying (not in an ontological sense) the intervening variable - not that the qualities which determine that given events produce given consequences still remain distinct from that relation between antecedents and consequences. Rather it is that "the existence propositions... automatically make the construct 'hypothetical' rather than 'abstractive'" (11, p. 99) - i.e., the intervening variable becomes a hypothetical construct in the ascription to it of qualitative content. This is a further indication of what is made plain in their discussion of Skinner's "emotion": that the true intervening variable is thought of as a 'state' internal to the organism, in some sense part of its constitution yet stripped of all qualitative content - and in that case it can only be the relativistic, mystical sort of notion that they are confusedly setting out to attack.

Although I said that there is one real set of facts indicated by some of MacCorquodale and Nechi's uses of the term "intervening variable" - namely, the mathematical relations between types of event impinging on the organism and types of response produced by it...
it does not seem to me that the term "intervening variable" is necessary or suitable for referring to it, partly because of other explicit meanings that have accrued to it, and partly because the words themselves inevitably suggest some state-like thing that intervenes between stimulus and response. A relation is not "between" its terms even in the most neutral way: one should say rather that they have, or stand in, that relation.

The mere giving of a name or symbol to those relations strengthens the ever-present temptation to slip into the "imaginary force" way of thinking, especially when the correlations are less than unity. In this case we cannot say that the stimulus in question produces the specific response (because there are some cases in which it does not), but if we remain convinced that there is some connection then we are likely to say that the stimulus results in a tendency to produce that response. But this tendency regularly finds its way inside the organism (because of a confused recognition that the state of the organism has something to do with it), and appears as a "demand" or "propensity" (or any of the multifarious species of "tendency") to make that response. (As "valence" it has found its way into the stimulus-object - i.e., the stimulus has a tendency to produce the response).

Now, the "intervening variable" approach is sometimes defended by insisting that they are "imaginary" forces and are never intended as anything else. Their sole function is to help us grasp the observed facts and to organise our thoughts about them. But
it is possible for our thoughts to be "organised" in a way that is mistaken or even meaningless. It may for the most part be true that the actual verbal forms are nothing but ways of expressing the observed connections, but the mere fact of offering "intervening variable" statements makes it vaguely seem that our knowledge is being extended, and that we are "getting to understand the facts better" in the sense of seeing how they are produced. The attribution of events to occult forces is rarely explicit because then it is so blatantly unscientific; it creeps in unacknowledged and gains its influence by default, as it were - by our failing to look for the actual causes. (The Appendix, which is actually a separate paper, has been included because it contains detailed examples of this kind of thinking and of some related fallacies in connection with "homeostasis." Such a detailed treatment of actual texts is necessary to establish that these fallacies do occur, but it seemed that the thesis would become overloaded with digressions unless it were simply assumed in the main body that there is such a thing as hypostatization.)

To discover causal relations (which are always coextensive relations) we must take into account not only the nature of the forces acting on the thing in which the changes are produced, but also the properties, especially the fluctuating ones, of that thing itself (which, in psychology, will for the most part be, of course, the organism.) When we discover which of its actual properties are involved in any given reaction, then the need to fall back on imaginary forces whose
sole function is to produce these effects (i.e., on "intervening variables") will have disappeared.

If it be true that the "intervening variable" school of thought covertly explores relativistic notions of the propensy kind, then it, or the kind of behaviourism which adopts that method, has an unprecedented concurrence with that purism which seems to be its greatest opponent. Of course there are points of opposition between the two kinds of doctrine, the main ones being that behaviourism concentrates on the properties of the external situation to the virtual exclusion of those of the organism, whereas purism claims to be examining the properties of the organism, and claims that "in the last analysis" behaviour flows from the nature of the organism irrespective of the external situation. But if any event arises from an interaction of distinct entities, then the attempt to account for it by reference to the properties of only one of these, whichever it may be, must evaporate, in my opinion, either in relativism, with its obscurities of discourse, or in a notion of activity which requires foreseeable restrictions on the possibility of explanation, and of science in general.

There are two main kinds of purerivist doctrine — the teleological, which, when some evasive qualifications are cleared away,
II - Activity.

If it is true that the "intervening variable" school of thought covertly employs relativistic notions of the propensity kind, then it, or the kind of behaviourism which adopts that method, has an unsuspected concordance with that purposivism which seems to be its greatest opponent. Of course there are points of opposition between the two kinds of doctrine, the main one being that behaviourism concentrates on the properties of the external situation to the virtual exclusion of those of the organism, whereas purposivism claims to be examining the properties of the organism, and claims that "in the last analysis" behaviour flows from the nature of the organism irrespective of the external situation. But if any event arises from an interaction of distinct entities, then the attempt to account for it by reference to the properties of only one of these, whichever it may be, must eventuate, in my opinion, either in relativism, with its obfuscation of discourse, or in a notion of activity which requires formidable restrictions on the possibility of explanation, and of science in general.

There are two main kinds of purposivist doctrine - the teleological, which, when some evasive qualifications are cleared away,
may be seen to treat certain kinds of event as caused by some of their own consequences, and the activistic, which hold that creatures of a certain kind (including animals, and possibly some plants, and possibly some machines) are able to produce effects in themselves, i.e., independently of external action. That however would not be an adequate classification, because, although it seems to be exhaustive, there are theories which show both these features, and, more importantly, others which might be regarded as transitional between the two, but which are subject to similar disabilities.

Amongst these latter would be the doctrines that attribute behaviour to "states of need". Now, there are no states of objective or intrinsic"incompleteness". We may see that an object now lacks some property (e.g., a certain concentration of sugar in its blood) which it used to have, or that it lacks again some property possessed by other objects which it resembles in many ways (members of "its own species"), but still, whatever it is, it is that completely. But theories of motivations which seek to account for the initiation of action by reference to "states of need" rely, however indirectly or obscurely, on the view that there is some natural state of fulfillment, so to speak, for any animal, and that any departure from this state is, perforce, unnatural. These theories, however, derive what persuasiveness they have from
the notion that there cannot be anything unnatural or incomplete, that there is something illogical about such a notion, so that, whenever such a state "occurs," it must cancel itself out. Without some circularity of this kind one cannot argue that a state of need is a sufficient condition of behaviour that will get rid of the need, or in fact of any behaviour or event at all.

If there are no objective states of need then we must always say what the "needed" thing is needed for, e.g., that a man needs to eat a given food to maintain life, or that believing that someone has affection for him is a necessary condition of avoiding exhibitionism; in fact, we might say in general that statements about needs reduce to assertions of the form "X is necessary for Y," but that of course does not mean that X will be done or will occur, but only that if it does not occur, then nor will Y.

Now, the absence of X (supposing X once again to be a certain concentration of blood sugar) though not a sufficient, may be a necessary condition of the occurrence of some piece of behaviour (which may result in the acquiring of X though that is always a quite separate question), and in fact it may be always theoretically possible to specify a set of conditions necessary for the behaviour in question such that, given situations of this kind (an organism having specific neural mechanisms and various other physiological states, having certain pieces of cognition and being in an environment containing some of a specified range of foods)
the absence of X is a sufficient condition for the occurrence of
the behaviour. But this can still hardly be said to account for
behaviour by reference to a state of need - it is essential
to specify some set of actual properties or parts that the organism
has, and how they are acted on by external events and by each other,
in order to understand how the behaviour comes about.

Purposivist doctrines which rely on states of need are "trans-
itional" between the teleological and the activistic in the sense
that while the need-removal to which the behaviour is said to be
directed is a future event, one which (in "favourable circum-
stances") follows from the behaviour, the sufficient condition forward to the present; that is, it is held to be,
just prior to and at the time the behaviour starts, a part or
property of the organism, and one in virtue of which it acts —
i.e., produces a change in itself. (Upholders of the doctrine
would claim never to mean anything but this second interpretation,
but since the need for some state is really the need for attaining
that state, then the attempt to account for behaviour by reference
to a state of need always carries the suggestion that the behaviour
is "drawn along by" that potential future attaining, whether it
actually eventuates or not.)
In brief, and without concerning ourselves to pursue any further this question of classification, we might suggest that this kind of relativistic doctrine is not so much "transitional" as the genus of which the other two are species, that in speaking of the actions of a future state we are treating it as already having some kind of existence, that it will commonly be supposed to have "potential existence" within the organism whose behaviour it affects, since the organism is thought of as being in part constituted by its relation to that goal. Thus to describe the organism's behaviour as the effects of its own present state of need is to put forward the same (teleological) doctrine in terms of a difference in "kinds of existence" rather than of a difference in the temporal direction of action.

"State of need" is not the only notion which seeks to specify present states of the organism as conditions of its activity without giving up the field to a doctrine of efficient causes. There are such individual theories as that of "horns" in McDougall — something which is held to be the substance of mind yet which can be described only as goal-seeking — and the doctrine that is universal in everyday speech and thought and has been given more systematic working-out in psychological theories: namely, that amongst the most important conditions of our behaviour in general, and the sole condition of some of our acts, are purposes in the sense of desires, intentions, wishes,
strivings — that is, as some qualitative process actually going on in the person's mind. I say "sole condition" because (despite McDougall's notion of "inclining causes") there can be no degrees of sufficiency — if a thing is not quite sufficient for a given event then it is just not sufficient — and if we say that any other condition is necessary for an intention to produce a behavioural event then that is to say that the intention is not in any sense "able" to produce that event, and has not intrinsically, or in its own nature, even any "tendency" towards it. All we could say would be that there is some qualitative process in the person's mind (one which we are calling an intention or desire) of such a nature that if it is acted on by, or comes into a critical relationship with, some other process, then the effect in question will ensue. But this nature of the mental process which has this relationship to the desired situation must be describable in terms which make no reference to that relationship, or, more importantly, to the events that follow (in part) from it (i.e., from that desire); since otherwise, as I said before, we could not understand what it was that they followed from — what "it" meant. It.

Now, that account would differ so greatly from what it ordinarily meant by "intention" or "desire" that it could not be merely a detailed analysis of a desire, not just a making clear how a desire operates, but would be rather a rejection of the whole doctrine as a fallacious
or strictly meaningless notion, deriving from a false view of logic. On the one hand desires never actually are described without the relational reference — they are always desires for, or intentions to do (without however necessarily conveying that the person knows that the object is desired, or the action intended) — but the more important point is that if it is admitted that interaction with other processes is necessary for any act (and by "any act" I mean not only overt acts but any change, external or internal, at all) to follow from the desire, then it also seems necessary to abandon the whole notion of directedness and the special sense of "being active" that goes with the idea of desire or maximal intention — in fact with "motivation" in general. For these notions require that the active thing must be able to produce changes in itself, without any need for being acted on by something else. When we think of activity as meaning the producing of changes in something else, e.g., if we think of the qualitative process Desire No. 27 (or whichever it may be) as producing some change in an effector system, then we can say that it produces that change just by virtue of its own nature, when it has entered the critical relationship. (To take an example from physics, one such "critical relationship" might be simply physical juxtaposition — e.g., a rolling billiard ball may have all the properties required to move another but it will not actually do so until it hits it, and a fire will
not boil a pot of water unless the distance between them is less than a given distance dependent on the heat of the fire.) But changing something else would not give us "activity" in the required sense, in fact it rather emphasises the point that the effect was produced by not by the desire's activity, but just by the fact that, being (qualitatively)what it is, it came into conjunction with the thing in which the effect was produced. We would only get the positive sense for activity in thinking of that as desire as bringing itself into the required critical relationship — not at all if it were impelled into it by some still earlier event.

Just the same considerations apply to the notion of an activity being directed towards a given result, if that is to mean any more than that that result does follow from the action; that is, it must suggest that the entity which performs the action — the desire, say — directs itself to act on specific things, or directs what changes it will suffer on the occasion of something else acting on it.

In general, we might say that the notion of something producing changes in itself is central to any positive sense of "directedness" or "activity" because the recognition that any action is an interaction seems to make plain the externality to each other of the nature of a thing and the production of any change in it (or its production of changes in anything else.) To put it crudely, effects are just jolted into it or out of it will-he, nil-he,
so to speak, so that if interaction is necessary for any event 
("event" meaning the occurrence of a change in something), and if we really grasp the meaning of interaction, then we will not understand how we can in any strict sense account for an event by reference to its effects, or by reference to a tendency towards its effects or to a tendency towards the event itself, rather than by reference to the actual impinging of entities which did in fact 
mechanistically 
produce that event.

Supposing for the moment that this is so, that all non-teleological doctrines of purposivism are activistic in this sense, then are there any insuperable difficulties for activism? I suggested above (pp. 25-7) that such doctrines might always be shown to be relativistic, that they would always have to hold that it is just 
in the nature of the active thing to unfold its own predicators, but such an argument might be regarded as forcing an unnecessary formulation on the activistic doctrine simply in order to oppose it with criticisms of relativism as "unspeakable". Presumably it would always be possible to specify the qualities the active thing actually had, and simply to assert as a matter of fact that things of that kind did exhibit changes without the need for external action - that "activity" for them simply meant these changes occurring.

Now, it seems to me that one might not be able to show that such
a position involves any self-contradiction, and that one could only assert in opposition to it that it entails a rejection of science, entails an absolute limitation of understanding. For one could not show why such an active entity exhibited a change at one time rather than by another, since by definition there would be no conditions of such changes, or why it did not run through all its changes in an instant (which is just an elaboration of the first point.) (Attempts have been made to show that such doctrines are self-contradictory, that they are always formulated as "something producing changes in itself" so that there is an implicit differentiation between the part that acts and the part that is acted on, so that the action once again is external. But as I said, I do not think that such a formulation is necessary, even though it is undoubtedly found in many individual writers, and even though many writers (v. i.) do not understand the point which this objection of Anderson's is making, and speak as if external action between the parts of a thing were still internal to the whole - still a self-activity. Again, Anderson has contended that it cannot consistently be held that some changes are determined and others undetermined, since there would always be the possibility of one of the undetermined processes interfering in the determined ones. But this objection relies on the assumption that causality entails the coextension of the antecedents and the consequents, whereas a
view which held that changes simply happen might easily contend that on some occasions the action of P on Q causes Q to be X, while on other occasions it simply does not, because some other indeterminate factor, present all the time in Q, indeterminately interferes. This is just to deny the assumption that causality entails coextension, and the facility with which such an amendment might be adopted gives a further hint of the obscurantism that a doctrine of that kind would entail; decisive argument in certain fields at least would become impossible because of the acceptance of situations in which it is impossible by definition to find propositions which imply the presence or absence of a given property in a specific place.

Of course the demand for explanations involving the specifying of propositions which imply the fact to be explained, as against mere brute acceptance, does not mean that such propositions must always exist, but, to argue ad hominem, one feels that many psychologists who believe in activity would feel impelled to forego that belief if it were made plain that it entailed such a final mystification.

But do any psychologists argue for activity in this sense? Certainly one does not encounter this view in so many words, not at least in everyday secular psychology. It seems to be required, then, to show (i) that it is concealed in various theories of motivation and that it is central to them. Furthermore, having protested against
that theory without being able to show that it is untenable, it seems necessary (ii) to dispute the arguments which lead up to it, which in this case means to show that the various empirical phenomena said to require such a notion (or simply by inspection to be instances of such activity) do not require it, but can be referred to the antecedent action of one thing on another. Since the experience of "desiring something" or "striving for something" is a universal "psychological fact", then anyone who within psychology asserts that there is no such thing as a desire or a striving might be expected to indicate what the psychological phenomena are which give rise to this illusion, and this will turn out to be an important part of (ii), which cannot be a purely formal exercise but demands an empirical isolation of the antecedent conditions, or at least some empirical suggestions as to where they might be found.
III - Activity in McDougall's Purposivism.

When McDougall says, "When the movements of a human being exhibit the first five marks of behaviour, we do not hesitate to infer that they are purposive; by which we mean that they are made for the sake of attaining their natural end, and that this end is more or less clearly anticipated or foreseen..." (p.47), we find activity at the kernel of it. Even if we overlook the vague suggestion of rationality in his requirement that purposive action should be "governed or directed in some degree by prevision of its effects, by prevision of that which lies in the future," and reinterpret that by saying that it should be "governed and directed in some degree" by the person's beliefs (or some of them) or speculations about its effects; and even if we regard as unnecessary what seems to be McDougall's further intention (though it is not expressly stated) that the person should be aware that he is striving for that situation - which is additional to as well as distinct from the point about his (possibly unconscious) beliefs that the action will lead to that situation - we are still left with the assertion that "having certain beliefs and desires, he acted." McDougall does not mean that the occurrence of a certain belief made the man, being in a certain condition, act so and so; what he means, and what is always conveyed by the common notion (and usually by the psychological notion)
of doing something "deliberately" or "intentionally," is that
a given piece of knowledge or a perception comes to us not as
an agent, not as a cause, but as a message which we ponder over for
a greater or smaller time and then make use of, or act on.

When McDougall says that we can infer purpose from "the first
five marks of behaviour," he is usually taken to mean that these
marks are the outer and visible signs of an inner, unobservable
purpose - a particular mental event - i.e., that he is conceding
to the behaviourists the point that we can only observe in other
people their overt bodily behaviour, yet trying to show that we
can still retain purposes in such an "objective" psychology. The
objection is then brought that we could only make such an inference
if we had previously been able directly to observe the occurrence
of a purpose and the fact that those phenomena followed from it.

Having admitted the behaviourist view of observation, McDougall could
only say that one observes one's own purposes and their connection
with certain overtly visible features of one's behaviour, so that
when those features appear in the behaviour of another person
or animal the inference can be drawn that the behaviour in question
followed from a purpose.

Now, it is not correct to criticise this argument on the grounds
that it is only "by analogy," as is sometimes done, if cause and
effect are coextensive. But McDougall's "marks," taken in this
sense, can be effectively criticised by showing that they are question-begging: that they are not properties of the actions but interpretations imposed on them, and that when these interpretations are cleared away then for the most part the "marks" that remain are quite categorical; they are features common to simply every phase of action — such as that it has a beginning and an end — or at the very most they are found in the behaviour of kinds of machine which we do not believe to have "purposes."

Thus we can find mechanical behaviour which might roughly be described as having "a certain spontaneity of movement" (the first mark) even though we might not believe it to be literally spontaneous. There are many machines which can be observed now to be at rest and now in motion without any intervening impulsion or interference from without (this being the only empirical meaning of spontaneity as distinct from the question-begging connotation of self-activity) — for example, a steam locomotive that has been left in gear, freshly stoked up, so that when the fire reaches a certain heat and generates a sufficient pressure of steam, the locomotive moves. Again, even the rolling of a billiard ball does persist "independently of the continuance of the impression which may have initiated it" (the second mark), and a stream, flowing around obstacles, does exhibit "variation of direction of persistent movements" from higher to lower — and so
on through the list. Does the fact that a stone stops falling when its falling has brought it to the earth indicates that its falling is purposive (mark four), or the fact that it falls faster and faster mean that it is "preparing itself for the new situation towards the production of which" the falling contributes — i.e., preparing itself to make an indentation in the ground of that particular depth which it does make?

Now, such criticisms have force, but a rather different interpretation of McDougall's "marks" is possible, and one, I feel, rather nearer to his intention, despite his use of the terms "mark" and "infer". If we read "mark" as "property" or "characteristic" of purposiveness, rather than as an external sign of it, then we can see that the first five marks are just bald assertions of the fact of activity, the second, third, fourth, and fifth, being largely redundant except that they point to various kinds of change that an animal can produce in itself, and being included only in order to bring us to see the sort of thing he claims to be pointing to. With these four he is not really saying that the only way we can account for these changes is by assuming self-activity (since the logical possibility of unobserved antecedent conditions for any change is always present); rather is he contending that in these cases we are actually observing activity. The first mark ("spontaneity") is just the unvarnished assertion of activity, and the
interpretation I offered above that he meant only such things as the movement without external impact, or the breaking out of fire in a pile of manure, is really unjustified, and the consequent objection that such things can be accounted for by reference to the external action of the parts of the thing on one another is beside the point.

McDougall could agree that undoubtedly in some cases such antecedent interaction did occur, but could also assert that there are cases in which there is no interaction but just action — just literal spontaneity.

Similarly, the "persistence of activity" of the second mark, and the "variation of direction" of the third (which can hardly be properly distinguished from each other), are on this view just further assertions that an animal is not dependent on external action for what it does. Any change in the behaviour of an object is a "variation of direction" as far as the preceding behaviour is concerned, and for McDougall's purpose the important thing is that animals should change their own behaviour. He points out himself that an inanimate object exhibits "persistence" in that, when set in motion, it "continues to move in the same direction, if not acted on by any forces which deflect or arrest it." However, its movements and changes are in principle strictly predictable according to physical laws, which is the case even "if the object contains, like a rocket, or a torpedo, or an automobile, some supply of energy"
which maintains the movement in spite of friction or other resist-
ances." The critical distinction for him is that "when an animal
persists in the movements intimated by a sense-impression, its
movements are not predictable in detail," and this unpredictability,
I suggest, arises from the fact that the movements are changes
that the animal makes in itself, which means, as I said above, that
there can be no general connections between them and any other
event, from the knowledge of which they might be predicted.

Concerning the fourth mark, the "coming to an end of the animals
movements as soon as they have brought about a particular kind of
change in its situations" McDougall seems perfectly aware of the
possible non-purposivist interpretation of such external, overt
phenomena, since in distinguishing purposive from reflex behaviour
he says (p. 53) that "the cessation of the stimulus which provokes
the reflex might be claimed as the analogue of the goal of purposive
behaviour, as when the foot is withdrawn from the prick..." He
goes on: "But the natural goal of behaviour (i.e., of a purposive
movement) is more than the cessation of a stimulus; its attainment,
which brings the movement to a close, involves some positive novelty
in the total situation," although he quite fails to show in these
terms how reflex and behaviour differ. He says: "Thus, if we saw
a dog lying in the sunlight and then saw him get up and wander about," we might suppose that the heat of the sun's rays had stimulated
him to reflex walking; but if we saw him walk to a patch of shade and there lie down and resume his slumber, we should confidently infer that this was behaviour, a purposive movement attaining its natural goal." If we get rid of the question-begging distinction between wandering about and walking to, and say that the dog walks until it comes to a patch of shade (allowing for some points to be developed later about the influence of the dog's perceptions on its behaviour), then there is nothing in McDougall's contention that this "involves some positive novelty in the total situation" over and above the "mere cessation of a stimulus." His example relies on the assumption that the dog stops of its own accord when it gets to the shade, but that is a main part of what he is trying to demonstrate.

This notion is to be found also in J.M. Baldwin and G.F. Stout's definition of activity in Baldwin's "Dictionary of Psychology and Philosophy": "If and so far as the intrinsic nature of conscious process involves tendency towards a terminus, it is active process, and is said to have activity." Now, a terminus or end "consists in that relative cessation of conative activity which depends on the fact that the activity has completed itself and has nothing further to accomplish". They go on: "If a conative process is allowed to develop freely without interruption or repression, it tends to go on until a certain result ensues, and when the result is attained it ceases of itself." Apparently the suggestion is that if the
conative process ceases of itself then we must conclude that it occurred only in order to achieve the situation in which it ceased, but that is only to support the assertion that a thing can produce changes in itself (starting to do something) by pointing to another instance of its producing a change in itself (stepping it). One might ask, supposing a stream to be blocked up by some accumulation of rubble, whether the water rises until its pressure sweeps the obstacle away, and then decreases of itself. But such a question is misleading: while it shows that a particular instance has features similar to those mentioned in the definition and yet does not involve activity, it does not dispute the bald assertion of activity — and neither does the criticism that the alleged argument is question-begging.

"Preparation for the new situation;" McDougall's fifth mark, again entails activity. In a sense any event which makes possible (i.e., is a necessary condition of) another might be said to be a "preparation" for it, but McDougall of course means more than that; he means that the earlier event is undertaken for the later, and in fact that is true of all notions of purposive behaviour, and it enables us to sum up the connection between purposivism and activity. The two notions are not identical (and this is the reason McDougall said we infer purposivism from activity) — purposivism includes the requirement that the activity should be for what follows from
it (which in my opinion can only be dealt with, short of outright teleology, by some treatment of the effect of the person's beliefs about what will follow) - but purposivism (unless it recognises causation from the future) must hold that purposive behaviour is active behaviour, for the belief that the event is for what follow a (or is believed to follow) from it is incompatible with the belief that it was produced by necessary and sufficient antecedent conditions.

The minor point about "sub-goals" needs to be cleared up - i.e., the point that the "purposive" act may, with the performer's knowledge, initiate a series of events which do follow from it and attendant circumstances in the necessary and sufficient way, and that the one it is for does not occur until later in the chain. Each of the intermediate events then is sometimes said to be "for" the "goal", and yet each is recognised to follow from its antecedent necessary and sufficient conditions. But that is an inexact usage; if we ask whether the intermediate acts are done for their contribution towards arriving at the "goal", then it cannot sensibly be said that in any positive sense they are so "done" and yet that they follow mechanically from their antecedents. It is only the whole sequence of which they are parts, or perhaps only the first "uncaused" act which set it in motion, that could be described in that purposivistic way.
If there is not at some point an initiation, if there is no discontinuity in the infinitely-backwards-extending transitivity of necessary and sufficient antecedent conditions, then there is no sense in which any event could be said to have occurred for its consequences. One might occur because (in part) of someone's beliefs about what its consequences would be, but those beliefs must have been antecedent to it, and their entering into the prevailing complex of conditions entirely on a par, logically or causally, with that of any other of those conditions. One could not meaningfully say both that an event occurred for what came after it and as a result of what went before it. To take an example from biology, if one said that the adrenal glands secreted adrenalin in order to release red blood cells from the spleen, then one could not also say that the spleen released red blood cells in order to provide for the better distribution of oxygen. This latter statement attributing activity to the spleen would mean that the activity of the adrenal glands was a matter of indifference to it, and the belief that it is the adrenalin that procures the release of red blood cells would have to be given up.

In brief, then, my contention is that any form of purposivism involves a belief in activity, and is therefore incompatible with determinism.
IV - The "Descriptive" Use of the Term "Purposivism."

An incompatibility similar to that between determinism and
purposivism holds between the view that the actions of the parts
of an organism are deterministic, or exhibit efficient causation,
and the view that their modes of interaction, or the kinds of
changes that they produce in each other, are determined by the
nature of the organism of which they are parts. This view is regularly
extended to hold that the organism produces changes in its own
parts (this being readily recognised in the biological formula
that a species of organism was forced to develop new mechanisms
in order to accommodate to some environmental change) - sometimes
explicitly as in the classical philosophic notion of the organism
or organic whole (e.g., in Kant's "Kritik of Judgment") and in various forms of holism, and sometimes less clearly (and quite para-
doxically) in certain forms of behaviourism, especially some of those
which insist on the molarity of behaviour - which insist that the
organism as a whole has ways of working (kinds of behaviour) not redu-
able to the activities of its parts. That of course is not a neces-
sary consequence of rejecting reductionism; it is a misuse of molarity;
but, masquerading as the doctrine that wholes have properties of
their own, and produce effects in other things that their parts in
isolation could not produce, we find the treatment of them as having
activities (in the sense I have been criticizing) of their own,
Purpose in Cybernetics. This masquerade sometimes takes the form of simply reserving the right to select one's own subject-matter, as, for example, in the paper "Behavior, purpose, and teleology," by Rosenbleuth, Wiener, and Bigelow. Here we find an expression, clearer than in many more psychological works, of some central theoretical principles of molar behaviourism, and their formulation of "the relation of input to output," and their notion of "feedback," have in turn had a good deal of influence on certain schools of psychology.

They begin with a very clear statement of the behaviouristic approach. "Given any object, relatively abstracted from its surroundings for study, the behavioristic approach consists in the examination of the output of the object and of the relation of the output to the input. By output is meant any change produced in the surroundings by the object. By input, conversely, is meant any event external to the object which modifies this object in any manner" (p. 18). They point out that this approach "omits the specific structure and the intrinsic organization of the object," and that "this omission is fundamental because on it is based the distinction between the behavioristic and the alternative functional method of study. In a functional analysis, as opposed to a behavioristic approach, the main goal is the intrinsic organization of the object studied, its structure and its properties; the relations between the object and its surroundings are relatively incidental." This would be the point
on which I criticised (Section I) psychological behaviourism, claiming that the general relations between stimuli and responses cannot be discovered without recourse to "the alternative functional method," i.e., the study of the nature of the organism at the time the stimuli in question impinged on it.

Now, that should be qualified by the admission that there will be some perfectly regular connections between a given kind of stimulus and a given kind of response to it; namely, those which are mediated only by permanent properties of the organism - those to which its fluctuations in other respects are indifferent. These relationships, then, could be discovered without considering the structure of the organism - by taking it simply as a unit acted on by things external to it and in turn producing changes in things external to it - but where its fluctuations of state do make a difference to the effects produced in the organism by external action and consequently to the effects it consequently produces in other things (and such fluctuations will be the especial concern of the psychology of motivation), then the behaviouristic approach as outlined by these writers can achieve only statements of probability.

Such a conclusion, however, would be not at all disconcerting to our authors, since they make it plain in a later, more detailed paper, that they regard such statements of probability as the
only possible kind of true assertion about causality. Without adding to my previous remarks (Section I) on the consequences of that view, I would be concerned to dispute their criticism of the opposing view, which I have been supporting, that causality does not admit of degrees. Their opinion (one held also by Bertrand Russell) is that "in a rigid system, without degrees, if we introduce causation it pervades the entire system, and the only conceivable cause of any future status is the entire past" (p. 320). If that were true then their conclusion that "in a world in which the whole past causes the whole future in an integral inseparable way, the category of cause has no operational significance" would be quite justified. But to say that the network of events which produced any given event branches out in an infinite number of directions and extends infinitely far into the past, and that there is an infinite number of present factors which may be and in fact are influencing any given event (i.e., that any event has an infinite number of necessary and sufficient conditions), is not to say that the maximal "entire past" or the "entire universe" is involved. That is simply a non sequitur, and one need only assert that alongside the infinitude of relevant processes there is a further infinitude whose occurrence or non-occurrence is indifferent to that of the event in question. To say otherwise is to deny that there is a logical relation of indifferences; but to admit indifference is not to deny determinism.
"Input," then, is quite parallel to the broadened notion of "stimulus" in behaviourism, with the exception that it excludes proprioceptive stimuli and in fact all processes internal to the organism (and this exception would apply in the main only to Hull's usage), while "output" is parallel to the meaning of "response" with the exception of those responses which are changes in the organism itself — although, as I suggested above, this latter might be what is properly meant by "response", being obscured by the frequent confusion (or amalgamation) of the changes produced in the organism and its consequent production of changes in other things. Actually we find this (illogical) amalgamation coming in in their next paragraph to blur over their original clear definition of output as "any change produced in the surroundings by the object" (italics supplied). They say (p. 18): "By maximization behavior is meant any change of an object with respect to its surroundings. This change may be largely an output from the object, the input being then minimal, remote or irrelevant; or else the change may be immediately traceable to a certain input. Accordingly, any modification of an object, detectable externally, may be denoted as behaviour."

What could be meant by "a change of an object with respect to its surroundings"? If a thing passes from being X to being non-X then that change is not "with respect to" anything but X — not even "with respect to" what produced it or what follows from it. They
might mean a change in position with respect to its surroundings, but that, being purely relative, is not in any sense a "modification of" the object. At least they would have to mean an acceleration or change in direction of motion, and this, being mainly interested in physics, is what they commonly do mean — qualitative changes being either just alien to their thought or regarded as not susceptible of scientific treatment.

Now, to speak about a change of speed or direction "with respect to" the surroundings could, I suggest, only be interpreted as meaning the approaching or avoiding of something, and in fact when they go on to discuss "active, purposive" behaviour their examples are all forms of "hunting." They introduce a distracting technical definition of activity in terms of whether the object itself produces some or all of the "energy" used in the output or whether it is all supplied by the input, but activity in the sense in which I have been using it is already creeping into the notion of approaching or avoiding something when that is thought of as part of the behaviour of the organism — to use a fashionable phrase, as one of its ways of working — instead of as something that follows from its behaviour (changes in it). In brief, although their meaning for "purpose" is sometimes hard to discover, it seems to me that Rosenbleuth, Wiener, and Bigelow commence with this notion of "ways of working", take it as their subject-matter for classification,
and offer the term "purposive" as a description of behaviour, as
the name of a kind of behaviour, claiming to reject any notion of
causation from the future, and in fact any reference to causality
at all. But the notion of a special way of being a cause is implicit
in that starting-point. Ironically, however, they do provide, in
the greater part of their treatment of "feed-back", materials with
which to dispose of purposivism, and because of that I will return
to their argument, after making some observations on "ways of working."

Activism in some notions of "ways of working." "Ways of working" claim
to be "ways of being" - i.e., qualities - and if we consider that
psychology has a subject-matter of its own then our agreement with the
contention that animals have characteristic ways of working peculiar
to them as a class is likely to prevent our seeing that "ways of working"
(or kinds of "behaviour") sometimes turn out to be ways of causing.

There is a large class of behaviour-names within each of which this
fallacy is (or may be) encapsulated - the class of all names of
operations: digging, building, chopping, knowing, eating, intimidat-
ing, and so on. Chopping, for example, includes in its meaning
not only a way in which a man, let us say, "operates on" a block
of wood, but also a certain complicated property of the man (being a
chopper) and the occurrence of a certain condition in the wood
(the state of being "chopped"). We find, then, merging into each
other, the state of the man, the effects it produces in the wood,
and the fact of production — that is, we find the suggestion of
something whose nature it is to be able to produce a certain effect,
as if the effect were contained potentially in the agent and were
carried over and forcibly imposed on the patient. This meaning
seems to me to be especially clear in the case of intimidation,
where we find a number of supplementary words and phrases which
convey that meaning — "to instil fear," "to strike the fear of
God into their hearts," "awesome" and "awful," meaning something
whose very nature nature it is to instil fear, "fearful" itself,
meaning both terrifying and its correlative terrified. But there
produce
is simply nothing whose nature it is to instil fear, even when we
specify the kind of person or the individual person in whom fear
is to be produced. There are, certainly, things whose nature is
such that they produce fear in particular persons, some of them
perhaps in everyone, but, having said that, that nature is still
to be isolated, and when it is found it will be something that can
be recognised without paying any attention to its producing fear,
in fact without knowing that it does so. (Similarly, we could
describe the disposition and movements of the limbs of the man and
the movements of the axe without knowing mentioning or knowing their
connection with wood-chopping, and could describe the consequent
state of the wood without reference to how it was produced.)

The very fact that there will always be a limit to the kinds
of object in which any given thing can produce fear establishes the point that some specific property in the patient is just as necessary for the production of fear as is the presence of the agent. My clenched teeth and black frown impress some people but move others to ribald laughter. One should not write "just as necessary" for the production of the effect since, as I said, there are no degrees of necessity, but some such emphasis is required to counter the view that even though a certain property in the patient is necessary, still "in some sense" the agent is more important, is the producer of the effect, since the patient could possess those properties without the effect occurring until the agent comes on the scene to procure it. But the agent, too, possesses its special properties without being able to produce the effect in question until it comes into conjunction with a patient of that particular sort. Furthermore, when the conjunction does occur the agent is not in any sense active in a way that makes the patient is not: we could just as plausibly picture the "patient," like a succubus, seizing on the "agent" and forcing it to produce that effect in it. Of course, both these pictures are misleading, as are the terms "agent" and "patient". The distinction is not at all in terms of activity or passivity, but the question as to which of the two interacting things we call the agent and which the patient is decided only by which effects we are interested in: each of them produces effects in the other, and when we are attending to the effects in one then we
specify that direction, as it were, of the interaction by calling that one the patient.

Now, there is nothing in itself objectionable in having a name for the production of a certain kind of effect, provided that it is recognized that the differentia is in the kind of effect and not in the sense of "kinds" of production, since production is simply causality and is quite undifferentiated, and provided further that the production is seen to be quite external to the thing producing, and so that it must always be misleading to say that it is one of its ways of working, since anything has strictly only one way of working, although it has many different properties and characteristic sequences of properties, and although it produces many different kinds of effect in other things. If we think of the producing of a given effect as one of a thing's "operations" and so as one of its "ways of working," then (in addition to the problem of showing how all these different kinds of causality would fit in with each other) we would find that, since nothing exhibits such an operation just all the time, on whatever things it is in contact with, then this kind of view involves once again the notion of self-changing, because it would have to hold that the operative thing released its causality or initiated its operation "at will," as it were, on the occasion of (but not as a result of) coming into contact with a certain kind of patient.

In short, the appeal to "ways of working" is very frequently a
question-begging procedure, and is always so when it is used as an answer to such questions as "why do babies instinctively select certain foods?", "why do people care for their young?" and so on. If these are just said to be ways of working characteristic of the species, then we get the obvious point that this reply only indicates that there is something in their hereditary makeup that eventuates in these "activities," but the point that seems often to be missed, and that I am trying to make here, is that when we get an adequate answer to the question what these mechanisms may be, it will show that there are no such "characteristic ways of working" as seeking food or protecting the young. These are not something we can find by examining the behaviour of the organism (i.e., by examining the sequences of properties the organism exhibits) but the getting of food and the preservation of the young are things that follow from its behaviour, and we prevent ourselves from discovering what the organism's behaviour is by "describing" it in such directional terms.

Activism disguised as "molarity". The contention that the organism and its own ways of working is sometimes found in another connection: namely, when it is insisted that it has ways of working not reducible to the ways of working, or more accurately, the interactions, of its parts. Once again we notice that there are senses in which this assertion is correct; that is, it is true that the organism has properties not possessed by any of its parts, and moreover (by virtue of those properties) produces
effects in other things which none of its parts, as such, can produce. But we find, also, the irreducible "ways of working" are sometimes thought of in the way that has just been discussed, and they are of course still open to the same objections.

Sometimes however the purposivism is not merely smuggled in, as it was in that connection, but is explicitly asserted of the organism (or whatever kind of system is being discussed) as a whole even while it is admitted that the interaction of the parts of the system are mechanistic — or at least while it is admitted that the parts do affect each other in some independent way. Here agent and patient (the parts) are being taken as one system, and the system is taken as developing itself. As S. Alexander puts it, "a case of transzuent causality between two independent substances like the cricket-ball and the bat is immanent causality, if the ball and the bat and the intervening space are taken to be a single substance, as they may with perfect legitimacy be taken to be" (p. 233). But he also says that "the only self-contained reality in which all causality is immanent is the universe itself, and its immanent causality is but the transzuent causality of the existents it contains," and that "when the universe is spoken of as self-causing, this is either an illegitimate phrase, used metaphorically of the whole; or, when it is used with a clear apprehension of its meaning, it signifies only that the various movements within the world are the outcome of other movements in a different distribution"
Passing over his use of the notion of the universe, we can still say that in the particular substance composed of agent and patient it is perfectly misleading to say that a given phase of the substance as a whole produces the next phase. Certainly such an alteration could not be attributed to any actual quality or set of qualities of the system as a whole, but only to its possessing certain parts in a certain conjunction - so that really it is this "transient causality" or interaction to which we return.

Usually such holistic arguments the case is made to seem more complicated than just "an agent and a patient". This occurs in elaborating the notion of "patient" to include "the whole complex" of necessary conditions on which the agent impinges as the final sufficient condition. For example, if we only want to speak of some influx of hormone lowering the excitatory threshold of a specific system of nerve-cells, then it will be pointed out that the flowing of impulses along the path in question is determined also by the presence or absence of facilitating or competing impulses intruding from other paths, which depends in turn on the selective effect of chemical agents on those systems, on the concurrent patterns set up by foregoing perceptions and motor actions, and so on.

Now, the mere fact that we may, and commonly do, have to specify a conjunction of many properties in specifying the patient does not affect the logic of the situation at all. But speculations such
as this frequently go astray in arguing that if the course of process A is affected by neighbouring process B, then presumably B will also be affected by A, and in its modified state will immediately produce further modifications in A, and so on indefinitely, so that (it is said) the situation cannot be properly conveyed by statements of the form "A produces a given change X in B"; the relation between A and B is much more intimate than that, because A's nature is in part "dependent on" B, and B's on A, and we have to see them as a continuously self-modifying system.

One assumption underlying such an argument is that if two things are in any interdependent relation then every feature and thus every change in either one affects the other, or, what amounts to the same thing, that there are no relations of indifference in such a field. This point appears in a rather primitive way in Hull's doctrine of afferent neural interaction. He says that "all afferent neural impulses (s) which are active within the nervous system at any given time change one another into a state represented by the symbol \( \beta \)" (p. 136). Now, the use to which he puts this principle makes it fairly plain that \( s \) and \( \beta \) are simple, that the difference between them cannot be expressed by saying what they have in common and in what they differ, but rather that when \( s \) is altered then in a sense it is altered throughout its being, though \( s \) and \( \beta \) might still be located on the same "continuum."

Here then we have a lack of indifference within the interacting things, and it is lacking again in the system within which they interact, since
he says that at any given time all the afferent impulses change one another. But why, then, when $s_1$ and $s_2$ have changed one another into $x$ and $y$, do these latter not suffer a further change into something quite else, and so on? Everything within such a system would be in a perpetual state of flux, so that no true statement could be made about its parts—and (what Hull might feel more keenly) nor could any conditioning occur in it.

A similar view, based upon a weight of acute physiological observations but not justified by them, is advanced by Goldstein\(^1\). They are mostly to the effect that any reaction, even the "simplest" reflex, is affected by and affects processes in other parts of the organism, but it is a fallacy of composition to conclude that "we really must always speak of a reaction-Gestalt which comprises the entire organism" (p. 225). He reaches this conclusion by arguing that "the condition of the rest of the organism [treating this as a single, inclusive state] is not an indifferent factor for the course in the part, but that changes in the rest of the organism influence the latter in a definite way. Consequently, we must regard the process in the rest of the organism as belonging to that in the part; moreover both constitute a unit" (p. 225).

Now, even if it were true that the parts of the organism are so intimately related that, for some part-processes at least, the field on which the part acts is "the rest of the organism"—if there were no features of it indifferent to the production

\(^1\) Goldstein, E. *The Organism*. American Psychol., 1926
of the effect — still the agent itself never becomes part of
the field, the field on which it acts. (Conversely, we can say
that the organism as a whole never acts on one of its own parts.
It is true that the whole and the part are in an important sense
different things, but it is also true that the whole is a whole
of its parts, it is made up of them, and it cannot then itself
contend with any part of itself. As Alexander says, though
dealing once again with "the universe," "the whole system of things
does not descend into the arena and contend with one of its creatures"
(p. 233). To say that the process in the rest of the organism
belongs to that in the part, to treat them as one unit in this sense,
is once again to treat the whole organism as producing changes in
itself. The attempt is to render this intelligible by admitting
the deterministic action of the parts of the organism on each other,
and by arguing at the same time that when we consider these
interactions as occurring "within a system," when we see
their interlocking, mutual affection as a system, then we see that
the system as such is self-regulating. But the two views of caus-
ation, the efficient causation of the parts and the final causation
of the whole, are incompatible: if we recognise that the impact of
X on Y is sufficient to give Y the property Q, and go on to say that
this occurs only under the aegis, as it were, of the special kind
of organisation of which X and Y are parts, then this additional
statement is either redundant, as Alexander argued (that is, it simply says that both X and Y, in a specific conjunction, are necessary), or it is a negation of the first statement - it is a denial that it is just the brute impact of X, having the nature that it has, that makes Y to be Q regardless of what whole they are said to be parts of (always allowing that Y is a sufficient specification of the field, which is only to say that the statement "X makes Y to be Q" is true).

Thus it may be strictly true but quite misleading to say that when we cut off the arm of a starfish, it grows a new arm. What actually happens (save the mark) is something of this kind, that the cells at the cut surface, affected by air and presumably water, are affected and affect each other in various ways, divide and multiply, and this goes on until, as a matter of fact, the air and water can no longer reach a certain class of cells, the "inner" cells (since it happens that a relatively impermeable layer has formed outside them), and so the "growth" processes that were caused by the air and water stop, and so finally we have the new arm with its horny outer layer. The new arm is to be seen logically as simply an effect, a fortunate by-product, of what happens in the exposed cells as a result of the exposure.

But, nevertheless, is it not true that the starfish grows a new arm? In E. B. Holt's phrase, does not the starfish do anything? A new arm does at least appear, and it appears on the starfish, and so it
seems at least a permissible description of the starfish, a specification of part of its behaviour, to say that in certain circumstances it grows a new arm. Such at least is the defence that many theorists offer; they expressly withdraw any reference to causality in their use of such descriptions (although in that case it is difficult to see why one would not ordinarily say that a man coagulates his own blood.) However, as I have tried to show above, many terms which claim to be simply names of behaviour contain within themselves the doctrine of activity (for example, purposivism could hardly be kept out as "growing a new arm" since "growth" processes are going on for some time before one could say that a new arm had appeared, and the suggestion is that they are directed towards that achievement.)

"Mutual determination" as purposivistic. Further points concerning apparently self-regulating systems in psychology are raised in Dewey's famous reflex-arc paper ( ), in which he offers rather a different treatment of the continuing mutual modification of each other of two interrelated processes. His main contention is that "the older dualism between sensation and idea is repeated in the current dualism of peripheral and central structures and functions" (p. 357), and that "what is needed is that the principle underlying the idea of the reflex arc as the fundamental psychical unity shall react into and determine the value of its constituent factors. More specifically, what is wanted is that sensory stimulus, central connections and motor responses shall be
viewed, not as separate and complete entities in themselves, but as divisions of labor, functioning factors, within the single concrete whole, now designated the reflex arc."

Now, we must agree that the set of neurones which constitutes any "sensory-motor arc" (recognising that such circuits are never isolated from other parts of the nervous system) can be considered as, and in fact is, one structure, and that the effect produced in it by the stimulus is produced in it as a whole. But that is not to agree that it "reacts into and determines the value of" its parts.

The virtue of Dewey's position is its insistence that there are no mystical "gaps" or indeterminacies in the chain. Thus, although the course taken by any afferent impulse is determined not simply by anatomical connections but also by nervous processes already going on in the nervous system, that is, although an impulse starting along one afferent channel (to use the over-simple terms to which we are still restricted) has anatomically, alternative paths open to it, and may follow different ones on different occasions, still for any particular impulses there are no actual alternatives, the path that it takes is entirely determined. If there are no points at which hypothetical non-motor-neural processes could direct the current (e.g., the "ideas" he attacks) nor is there any freedom for the CNS to select which motor path an afferent impulse will be directed into.

However, within this single, determined arc we can still distinguish
phases that come earlier and others that come later, and recognize
also the irreversibility of causal relations. But Dewey goes on:
"... we find that we begin not with a sensory stimulus, but with a
sensori-motor coordination, the optical-ocular, and that in a certain
sense it is the movement which is primary, and the sensation which
is secondary, the movement of body, head and eye muscles determining
the quality of what is experienced. In other words, the real begin-
ning is with the act of seeing; it is looking, and not a sensation
of light." Now, it is true that the organism is not purely passive
in perception, but nor is it purely active; what it sees depends on
what properties it has (including "what it is doing") as well as
on what there is to be seen. Certainly the nature of the behaviour
already in progress when a given pattern of stimulation falls on the
retina (the light of a burning candle, in the example which Dewey
takes up, following on James's discussion) will in part determine
the course taken by the resulting afferent impulses - will in part
determine both "what is experienced" and what motor channels will
be activated as a result of the afferent impulses. But the behav-
ior already going on will be the result of some other afferent
stimulation, even if that only be some earlier phase of what we
quite justifiably call "the same" stimulation - i.e., an earlier
phase of the stimulation arising from the lighted candle. It will
never be the result of the pattern of stimulation which at any given
moment is entering the neural field, even though it may be immediately
modified by, and in the same interaction modify, that stimulation. And though the changes produced in it may in their turn affect the course of development of the next incoming "charge" of the stimulus, and may in fact affect in a later phase of its course that very charge which originally produced them, still this is a succession of nearly distinct interactions; even though the time elapsing between any two beats of this reverberation may be shorter than any specified time, still cause and effect never do coalesce. It is not the agent in the moment of interaction, it is the patient that does so. The effect may later on affect the thing that was the agent; but the agent is really the impinging of the thing or the thing-in-its-impinging, and the effect is the coming-into-being of the quality produced, rather than its continued existence.

Accordingly, even a system which exhibits this continuing reciprocity of control of two coordinated processes is not a self-developing system; the development comes from the external and successive actions of the parts on each other. The suggestion of activity is conveyed only when we mistakenly think of the effect springing out to make something its cause, as it is conveyed when Dewey says that "in a certain sense it is the movement which is primary, and the sensation which is secondary, the movement of body, head and eye muscles determining the quality of what is experienced." But although the movements of focussing on the candle are necessary for our"fuller
perception" of the candle, for our discovering more facts about what we originally saw, still it is necessary for them that we previously saw something of it, something bright, let us say, off to the left there.

Now, Dewey does not say in so many words that it is the movement of focussing on the candle that is primary to the sensation arising from that candle, in fact he later points out that "our experience of a noise" will differ according to what we were engaged in at the time the noise arrived, but why otherwise say "primary" and "secondary"? In my opinion that is the meaning that is smuggled in, and in fact constitutes the main tenor of the paper, for example when he goes on to say: "Now if this act, the seeing, stimulates another act, the reaching, it is because both of these acts fall within a larger coordination... If the sight did not inhibit as well as excite the reaching, the latter would be purely indeterminate, it would be for anything or nothing... The reaching, in turn, must both stimulate and control the seeing." But why this? It is true, more or less, as he goes on to say, that "the eye must be kept upon the candle if the arm is to do its work," but that is not to say that it will be kept there by the reaching for which it is necessary. When he says further that "we now have an enlarged and transformed coordination; the act is seeing no less than before, but it is now seeing-for-reaching-purposes," his functionalism has gone over into teleology.
One admits that there is a great deal of sound and important
psychological theory in the paper, at least by implication.
For example, he says, concerning the hand's being burned: "Only
because the heat-pain quale enters into the same circuit of experi-
ence with the optical-ocular and muscular quales, does the child
learn from the experience and get the ability to avoid the experi-
ence in the future." If he means that the child came to know
that it is the candle which he saw and reached for that burned him,
then that helps us to understand that kind of change in animal
behaviour which creates such intricate problems for non-cognitonal
stimulus-response theories — and concerning which, furthermore,
they are simply wrong. That is, there is such a thing as cognition,
as discovering that something is so, and it does affect behaviour,
even though we might make up sets of hypotheses which imply the
occurrence of that overt behaviour without admitting cognition. But
would Dewey be content then with cognition just in that sense? Why
would he go on to say that "the burn is the original seeing... en-
larged and transformed in its value," as if the burn "reacted into"
that identical original seeing? This is the central notion of the
"circuit" which, while retaining its example, develops as a whole.
He objects to the common reflex arc doctrine because "in its failure
to see that the arc of which it talks is virtually a circuit, a
continual reconstitution, it breaks continuity and leaves us with
nothing but a series of jerks." The stimulus and response "have
their significance purely from the part played in maintaining or reconstituting the coordination."

But it is not really that the arc is reconstituted (taking "arc" properly to indicate some inherited neural structure such that the perception of a certain kind of situation results in a certain kind of behaviour); if there was an arc which determined that bright things should be grasped then it remains despite the burning, but for lighted candles it is now opposed by another (already existing) arc which determines that burning things shall not be grasped. One consequence of Dewey's "reconstitution" is that there could not be opposing tendencies in the one situation; for him it is not that the old tendency has been suppressed but that it has just been changed into the new.

Now, it may seem facile and superficial to postulate an \textit{innate} "arc which determines that burning things shall not be grasped," but at least it is not question-begging; it is a hypothesis which could readily be tested, and in fact is one which casual observation suggests is true. Certainly one would not postulate innate "arcs" which determine that when any form of money, say, is perceived, it will be pursued by the motor responses which \textit{make} a skilled trade, or in fact that as money it will evoke any response at all, but the case Dewey discusses does not involve such elaborations either on the cognitive or the motor side, and at least the organism must be innately endowed with some such response mechanisms, else there
would be nothing to be elaborated (however that may occur) into those sophisticated forms. The overall effect of Dewey's paper is question-begging. He claims that his modification of the stimulus-response view strengthens the stand against teleology by rejecting any "superior force in the stimulus or an agency ad hoc in the center of the soul" (p. 364), and we must agree that a minimalist theory of behaviour as following directly from the nature of the stimulus and the nature of the organism is precisely opposed to teleology. But the problem he is addressing himself to might then be expressed by asking why (given that behaviour just follows on perception) the organism attends to those features of its surrounding which are relevant to the completion of what it is doing. (This is just a species of the problem that an event seems to occur for that for which it is necessary.) Just to call this a "purely self-created problem," and to account for that utmost attention by saying that those future phases of the motion are just parts of the present motion, and that the motion as a whole and the perceptual processes necessary for it are just heuristic divisions within a self-constituting coordination, does not advance but rather hinders our understanding of it. Even if we can agree that a movement and the perceptions which modify its various phases are just one movement, one piece of behaviour (and it seems to me that that might be a very important principle for psychology), still that response as a whole (or any subsection of it considered in turn as a whole) is
the result of some process outside itself; the perception which
initiated it cannot itself be a part of the response, and that
property of the organism in virtue of which the response occurs,
or in virtue of which any changes in the response (from one
appearance of it to another) appear is not the response itself,
even though the response itself is a property of the organism.
It is not "self-developing."

"Purposive Behaviourism". As a final example of this doctrine
of the purposiveness of the whole despite the mechanism of its
parts, we might take the "purposive behaviourism" of E.C. Tolman.
While he contributes nothing to the defence of the logic of pur-
positivism, still his ingenious appeal to the "objective observation"
of behaviour gains some force by its very primitivism. He insists
(and we have already admitted the force of this contention) that
the organism as a whole has features not possessed by its parts.
"It will be contended by us (if not by Watson) that 'behavior-acts',
though no doubt in complete one-to-one correspondence with the
under-lying molecular facts of physics and physiology, have, as
'molar' wholes, certain emergent properties of their own" (p. 7) 1.
What are these molar properties? "The first item in answer to this
question is ... that behavior... always seems to have the character
of getting-to or getting-from a specific goal-object, or goal-sit-
uation..." (p. 10). Later he says: "Behavior as behavior, that is,
as molar, is purposive and is cognitive. These purposes and cognit-

ions are of its immediate descriptive warp and woof. It no doubt, is strictly and completely dependent upon an underlying manifold of physics and chemistry, but initially and as a matter of first identification, behavior as behavior reeks of purpose and cognition." (p. 13). In this last sentence we find quite clearly the conflict between the purposiveness of molar behavior, with its unavoidable connotation of initiation, autonomy, self-direction, and its alleged "strict and complete dependence" on mechanically-caused physical and chemical events. The former element derives from an undoubtedly genuine observation of some principle in animal behavior which distinguishes it from inanimate events, and the latter from the desire to pursue psychology as a science — or, putting it more directly, from the recognition that the set of logical categories remains identical for every event. It follows then that the distinguishing principle of animate behavior is an empirical and not a logical one, and so not purposivism, which does require a distinction in terms of causality.

Concerning the discovery of such empirical characters, Tolman's turning to observation is not only a permissible but the only possible procedure. Kinds of quality and kinds of relation can only be discovered, they cannot be "inferred", because inference can only lead us to the discovery of the conjunction of terms with which we are already acquainted (since we must have them in the premises.) And his recognition of cognition as distinct from mere reaction to stim-
uli, of cognition as being acquainted with what is the case, with "what leads to what", was, while hardly original, of great merit in the face of the behaviourist tradition, and does away with many of the problems connected with the modification of behaviour that have almost monopolised the attention of the behaviourists, and led to the piling up of ad hoc hypotheses such as those concerned with the "fractional anticipatory goal response" which Hull uses to account for "secondary reinforcement," the arbitrary slopes of "gradients of generalisation of reinforcement", and the like. But Tolman's admission of purposivism so debauched the behaviourist tradition that he can hardly lay claim to the title of behaviourist at all. The great virtue of behaviourism is its attempt to get the magic out of psychology, and if it erred in rejecting cognition completely just because most theories of it were magical (i.e., relativistic), still in insisting on mechanical causation it (although it was not quite alone in this) took that step prior to which there cannot be any psychology. If purposivism, as I have argued, is always magical, then to that extent "purposive behaviourism" is a contradiction in terms, even though certain treatments of the behaviourist approach, such as that of Rosenbleuth, Wiener, and Bigelow, finally collapse back into activism themselves.

The greater lucidity and informativeness of Hull's behaviourism over that of Tolman is exemplified in their treatment of the modification of behaviour according to whether or not it is, in the popular
sense, "rewarding." For Tolman this is, in the long run, just summed up in the notion of "docility," which is to be accepted as an intractable, irreducible character of behaviour; he says concerning Thorndike's kitten, that docility is "his tendency on successive occasions to select sooner and sooner the act which gets him out easily and quickly" (p. 14). The glossary says, in part: "Docility is the term used to designate that character of behaviour qua molecular which consists in the fact that, if a given behavior-act in a given environment proves relatively unsuccessful, i.e., does not get to the demanded type of goal-object at all or gets there only by a relatively long distance... it will on subsequent occasions, tend to give way to an act or acts which will tend to get the organism to this demanded type of goal-object and will tend to get him to it by a relatively short route." Now, nothing in any strict sense "selects" its own acts, nor, is it really sufficient to say that a new act will appear simply because it leads to a certain result. But is that what Tolman is saying in this glossary definition? Is he trying to account for the dropping out of certain acts and the entrance of "stamping in" of others, or is it simply that he is offering certain criteria of those acts which as a matter of fact do drop out, and leaving the questions concerning the mechanism which causes that dropping out quite open? It seems plain that he would claim that only the latter is his intention, that what he is offering is only "a matter of first
identification", simply a primitive description of some of the phenomena to be studied and to be accounted for. Now, to the best of my knowledge Tolman never does make any suggestions about the mechanisms which produce the dropping out of "unsuccessful" responses, but that of course still does not mean that his procedure is invalid or that the work he has done rests on an insecure basis. However, in my opinion Tolman does convey the impression, partly because of his failure to say anything else about this central problem and partly by his terminology, that the problem has been solved or that there is no real problem there. The criterion that he offers, that it is "unsuccessful" acts which tend to disappear, itself takes the problem for granted, since it can only be defined by reference to goal-striving. Once we take it for granted that the organism goes towards an object because it demands that object, then it seems perfectly "natural", almost an analytic truth, that, when certain other conditions are fulfilled, that it will give up acts which do not bring it to the goal and adopt others which it finds by experience do so. These other conditions will include its discriminatory and manipulatory abilities in the particular situation — in a word, whether it knows that the lack of success is due to a given act, and whether it can do anything about it — and the uncritical view will be that "goal-striving" simply will eventuate in the "appropriate" behaviour when conditions such as these are satisfied. The fact that Tolman directs his attention so much to these factors is the nearest
verification of our suspicion that he regards goal-striving as an
"explanatory concept"—that is, as an actual state of the organism
which produces modifications of behaviour—but whether or not we
can establish that that was his view, at least his failure to explain
the "molar determinism property of docility" is a serious lacuna in
his work.

In trying to discover just what Tolman means by "purposivism" we are
quickly bemused by that assertion about the "immanent determinants"
which caused Lewin such justifiable concern, that "they are objective
and it is we, the outside observers, who discover—or, if you will,
infer or invent them—then as immanent in, and determining, behav-

iuor" (p. 19). There is a plain incompatibility between discovering
and inventing, and another between immanent and determinant. If
"immanent" means just "being a property of", then the purposes
and cognitions which determine (i.e., cause) any particular act are
not immanent in it (even though it may have, or be said to have,
"purposes" and cognitions inside it), but in something else. (It is
hard to see how a cognition, at least, can be a property of behaviour.
It is the organism that cognises, not its behaviour, and cognitions
are parts of its behaviour, and affect other parts of it). The
notion of a process having "immanent determinants" suggests the
"self-developing" process which we criticised above, and whether or
not that is Tolman's intention, at least in saying that purposes do
determine the course of behaviour he is blatantly departing from that
merely "descriptive" use of purposiveness that behaviour does exhibit certain kinds of change. Again, when he speaks of the our noting in behaviour its "character as a getting-to such and such by means of such and such/selected patterns of commences-with" (p. 13), then this notion of the getting-to being a part of the character of the behaviour, rather than as something which happens to follow in this particular situation from its characters (which will be something quite independent of that) does suggest that the behaviour contains its effects within itself, or directs itself to produce those effects.

On the other hand, Hull's account of those changes in behaviour following on "reward" (that is, of what is fashionably called "learning"), while it may actually be mistaken, is at least logically sound in so far as it is couched in terms of minim efficient causation. His "law of primary reinforcement" is that "whenever an effector activity occurs in temporal contiguity with the afferent impulse, or the perseverative trace of such an impulse, resulting from the impact of a stimulus energy upon a receptor, and this conjunction is closely associated in time with the diminution in the receptor discharge characteristic of a need, there will result an increment to the tendency for that stimulus on subsequent occasions to evoke that response." (p. 80)¹.

The point has sometimes been made that even here the diminution of is subsequent to the effector activity which produced it, and so Hull too is faced with the problem of how that diminution could act

"backwards" in time to deepen the "neural channel" between the receptor and effector processes. Now, one might say that even though those processes have ceased, still the actual neural structures remain, and it is some change in these that Hull is referring to. However, the question remains why the causal event, the diminution of \( S_D \) (or some state resulting from it) acts on those structures which have just been active, rather than any other. This is a quite empirical rather than a logical question; and Hull's suggested answer to it makes use of the notion of "reverberating circuits" - i.e., the suggestion is that there is still effect, some "after-discharge", of the receptor-effector conjunction (that which was followed by the diminution of \( S_D \)) still going on, and the \( S_D \) diminution would strengthen the connection between any minim receptor and effector elements which were still in that state of after-discharge. The complications become formidable when the \( S_D \) diminution is delayed, so that other effector activities have occurred meanwhile, but at least Hull's attempts to solve these problems (cf. e.g. his doctrine of secondary reinforcement) rely always on efficient causation. (We find, for example, that the "fractional anticipatory goal response" comes forward to the beginning of the chain of responses, so as to exercise its invaluable functions of short-circuiting on the one hand and linking-together on the other, not because the organism is striving to make that goal-response, and so "tries" to make it as soon as possible, but simply because the \( E_G \) has been bonded to the \( S_D \), which is also present at
the beginning of the chain, by primary reinforcement.) Certainly
his suggestions about the neural changes occurring are almost entirely
speculative, and are continually patched up and modified by to fit in
with new facts of observation (and for the most part, as I have said,
necessitated maxims only by his rejection of cognition), but they are
never "that by which" solutions, they are testable in principle — that
is, capable of being false and so of being true.

(It is true that in another formulation of the law of primary rein-
forcement of p. 71 he refers to "the diminution in a need (and the
associated diminution in the drive, \( e_D \), and in the drive receptor
discharge, \( e_D \))" and in his note on the chapter (p. 81) poses it as
an empirical problem, whether the \( e_D \) or the "state of need" is the
determining factor. But Hull, as I have suggested before, is no
logician; the soundness of his methodological approach rests rather
on an empirical habit of mind, and so his theoretical formulations
often conflict with the main burden of his position and his practice.

At least his concern with the physiological bases of "needs" in
Chapter V does suggest that he thinks of them rather as qualitative
states of the organism than as "states of incompleteness". And in
his actual proffered solutions of problems, e.g. in the Psychological
Review 1935, though they may be disputed on other grounds, he
uses the \( e_D \) notion rather than any relativistic state of need.)
Draithwaite's defence of purposivism as a description of "plasticity."

Despite Tolman’s individual inconsistencies, some real issues are raised by the claim to use the term "purposive" just to indicate certain kinds of behavioural sequence. One example of this kind of phenomenon, and perhaps the most difficult to account for, is found in an elaboration of Tolman’s notion of "docility." If an animal has been making a "successful" response in a certain kind of situation, and then the circumstances are changed so that that response is no longer "successful," then as Tolman pointed out what frequently happens is that the same initial situation now comes to produce a different sort of reaction, but one which arrives at the same "goal" situation as the first, and if a further change is made then the behaviour changes once again, and so on, and the point is that in each case (provided as above that certain other conditions are met) the new behaviour, provoked it seems by the same sort of initial stimulus situation produces by different course, and, it seems again, despite the prevailing circumstances, the same sort of effect as the original behaviour (prior to the change in the situation) produced. I said that this phenomenon might be the most difficult to account for because it seems to be the only one of the examples commonly quoted (e.g., by McDougall) which can be defined objectively, without the question-begging use of such terms as "preparation" and "efficiency", which already contain within themselves the notion of purpose. With this example it is not necessary to say that they arrive at the samé
"goal" situation but simply at something which is recognisably the
same sort of situation.

While in my opinion it is only a misleading metaphor to say that
the effect is achieved "despite" the prevailing circumstances, and
it would usually be much closer to the mark to say that it occurs
because of them, still the facts which give rise to that appearance
require accounting for; if they are to be fitted into a scheme of
efficient causation then some fairly detailed factual account of their
actual antecedent conditions, not merely a declaration of the logi-
cal possibility of such conditions, is required, and that will lead
us to the next main section of our enquiry. But first I want to
consider a philosophic defence of this "descriptive" theory of
purposivism - one advanced, with much greater understanding and
clarity than Tolman's, by R. B. Braithwaite, in a presidential address
to the Aristotelian Society.

Braithwaite specifically rejects final causes, and says (p. vi):
"It seems to me that the orthodox biologists are right in rejecting
the postulation of a non-physical comatus mind drive to explain...
goal-directed behaviour... but wrong in minimising the intellectual
satisfaction to be derived from teleological explanations. I believe
that we can go on the orthodox assumption that every biological event
is physico-chemically determined, and yet find an important place in
biology for such explanations." Teleological laws are "in principle
reducible to causal laws" (p. vi), and to call a series of events
"goal-directed" with respect to a situation X means just that it is
one of that class of "causal chains" in a given system which od as a matter of fact result in X. How then can teleological laws give "intellectual satisfaction, or, one might ask more pertinently, what meaning remains for "teleological"? Towards the end of the paper he says that both mechanists and teleologists will have their own objections to his view, and "will join forces in criticising my treatment as being unduly epistemological; the controversy, they will make both say, is not as to how we derive our knowledge of general propositions about goal-directed activity, but is about the content of these general propositions: it is a question of the exist ultimate elements in the biological facts, not of the organisation of our present biological knowledge" (p. xvii). But Brathwaite replies that the function of a scientific law "is just exactly that of organising our empirical knowledge so as to give both intellectual satisfaction and power to predict the unknown."

Now, we said before that "laws" might give a misguided "intellectual satisfaction," and furthermore they cannot really give power to predict the unknown unless they are both meaningful and true. But in general Brathwaite's reply does not meet the criticism at all; it remains true that his so-called teleological laws show only how we can predict (i.e., infer) a prior from a causally related subsequent event, and not at all that the occurrence of the prior event is "determined by" (i.e., caused by) the subsequent one. They do little more than point out that if cause and effect are coextensive, and if
we know a causal law, and know that the effect has occurred, then we can infer that (in its other sense, "determine") that the cause occurred. They are, then, not teleological at all.

(This cross identification of "determined by" and "caused by" is found also in Rosenbleuth and Wiener1, thus: "The prediction of the future from the past belongs to the theory of causality; the determination of the past from the present belongs to the theory of purpose", p. 321).

One of Braithwaite's example is that "by knowledge of the conditions under which a swallow will migrate is derived from knowledge about past migrations of swallows and of other migrants, fortified perhaps by general teleological principles which I accept about the external conditions for self-preservation or the survival of the species, themselves derived inductively from past experience." But the fact that there are necessary conditions for survival is not a teleological principle unless we go on to say that these conditions occur, or are sought, simply because they are necessary. Again, "the conditions under which a swallow will migrate" seems at first glance to mean the conditions which cause a swallow to migrate, and this too would have nothing to do with teleology. But it transpires that that is not what Braithwaite meant when he goes on immediately to say, "It is when our knowledge of the relevant variability has been obtained independently of any knowledge of causal laws [as in this case of the swallow] that a teleological explanation is valuable. For in this case..."
we are unable, through ignorance of the causal laws concerned, to infer the future behaviour of the system from causal laws of the ordinary sort; but we are able to make a probable inference as to its behaviour from knowledge of how similar systems have operated in the past" (p. xiii).

But the variancy referred to is the class of those sets of field-conditions which are such that every causal chain in the system in question (e.g., in the swallow), starting from a given initial state of the system, will produce a given maximum situation X, and the "field-conditions" are all those "causally relevant factors in the system's environment or field" which "affect the system with respect to the events in question during the period". Further, we "make the ordinary deterministic assumption that every event in the system is causally determined by the whole previous state of the system together with... the 'field-conditions'..."

Now, in the sort of case he is discussing, in which teleological explanations are useful, we have only a "reasonable belief" about what the variancy is, based on past observation of the conditions under which similar behaviour has taken place. But the question is whether we ever have indisputably more than that. Kraitnwaite's demand for "ultimate causal laws" seems to be based on some notion of the laws concerning the "ultimate parts" of the system, or some notion that we can finally fix "fill up the gaps" between "field-conditions" and...
and effect is quite transparent and rational and indisputable — as if when we found an ultimate causal law we must be able to see, in the nature of the cause, that it must produce the effect in question. But of course there is no such stage to be reached; the effect is always external to the cause and their relationship of the kind that is called "brute" fact, so that we never have more than a "reasonable belief" that one observed event was the cause of another. Furthermore, causal relations, like all others, are general relations, they hold between kinds of event, so that logically there is no particular problem about Braithwaite's "variancy" — about the fact that there is a range of field-conditions under which causal chains in a given system arrive at the same kind of "goal" situations (the point being that the varying sets of relevant conditions are members of the same class, which in our general law is the cause). One might make the same point about the variations in the "causal chains" resulting from the variations in field-conditions, that they are all members of the same class, but while that is true it does not really deal properly with the same phenomena that lead us to say that some systems compensate for the variations in field-conditions — a point that I hope to treat shortly in connection with cybernetics.

What Braithwaite really seems to mean by the example of the swallows is this: we believe that swallows could not survive in temperatures below a certain point, we know that the temperature in winter in a place where swallows have often been found in summer regularly falls below that minimum. We further find that they seem not to die
there, since their bodies do not litter the ground, and we infer that either they do something (or something is done to them) to protect them from the cold, or they go away to a warmer place. As a matter of fact, let us say, we have actually seen them flying away towards warmer latitudes. Now, unfortunately, at the beginning of the article Braithwaitez said that the answer "To circulate the blood round the body" to the question "Why does the heart beat?" was a genuine explanation, that is, an answer "which in any way answers the question, and thereby gives some degree of intellectual satisfaction to the questioner" (p. ii). Presumably then he would say that we could explain the birds' flying away by saying that it was to preserve them from the cold. But of course that does not tell us why the birds fly away, and certainly does not follow from the information listed above. What might be said to follow is this, that the swallows have some unknown mechanism which, under stimulation by some factor concomitant with the falling temperature (or perhaps just by the fall in temperature itself) works in conjunction with other parts of their physiological structure in such a way that the swallows fly, and their flying is so affected by some further unknown (but quite concrete and here-and-now) factor that it takes them to a warmer zone. (Furthermore, we can predict that, if the temperature falls low enough/to make them fly, then they will fly. One rather suspects that Braithwaite would prefer to say that, if we believe that the temperature will fall low enough this year to
kill them, we can predict that they will fly away.) All this gains a suggestion of teleology if we express it by saying that because the cold would kill the swallows (and they are not killed) and since they are not saved in any other way, they "must" fly away. But that is to say little more than that an event which we know occurs, namely, their not being killed, must have a cause, and not to say that the cause was for it. (If we just know "otherwise they will be killed", it certainly does not follow that they will not be killed.)

Now, for Braithwaite the feature that makes this a case in which "teleological explanations" are useful is this: admittedly the event - namely, their flying away - which causes the swallows not to be killed must in its turn have a cause, but in this case we are ignorant of its cause; that is, we have only an "inductively inferred variancy" of conditions "under which" it occurs. (Evidently then "conditions under which" does not mean "conditions as a result of which", and so again we have a suggestion - though nothing more -- of "a degree of freedom", of causes that only "incline", such as we find also when he speaks of the "plasticity" of a system with respect to a goal.) Now, one might admit that if we did not know how the swallows were preserved from the cold, if we had never seen them migrating or had never realised that this flying was taking them away from the cold, then there would be a flash of enlightenment when we saw that connection, when we discovered the immediate cause of their being preserved - it might even be enlightening to say in
that "descriptive" way that this flying was a \textit{going away from} the cold — but if we regard this as being in any sense an explanation of the migration (instead of an explanation of their \textit{preservation}), then the "intellectual satisfaction" in that connection is unjustified.

In many places I have used the term "explanation" as if I assumed that the only way of explaining an event is by specifying its cause, but I have only given it that meaning when criticizing alleged teleological explanations in which there was a reference to causality. One might offer as an acceptable explanation of the swallows' flying this syllogism:

\begin{quote}
\textbf{All swallows' cold-avoiding activities are flyings.}
\textbf{This is a cold-avoiding activity.}
\textbf{Therefore this is a flying.}
\end{quote}

But there is still a sense in which we \textit{cannot} say "This is a flying (or flying occurs here) \textit{because} it is a cold-avoiding activity", and that sense is ruled out when we see that we can draw the conclusion "therefore flying occurs here" only if we have the matter-of-fact, empirically-discovered maximum major premise, and not from the minor premise alone.

Braithwaite's "inductively inferred variancy" must of course be a specification of some of the sufficient conditions, when we do not know what is \textit{necessary} and sufficient. It is in this connection that the "plasticity" of causal chains arises (p.x): "It is because the
size of the variance may be greater than the number of possible causal chains if the variance included only necessary conditions that the notion of the variance has been introduced. For it may be the case that there are various sets of field-conditions each of which, together with \( o \), determines exactly the same causal chain." However, he says, there is usually "more than one causally possible chain in the system in question which attains the required goal," yet he believes that the "essential feature... about plasticity of behaviour is that the goal can be attained under a variety of circumstances, not that it can be attained by a variety of means." What Braithwaite does not see is that, if we reject indeterminism, these two conditions congeals; if there is a variety of relevant circumstances, then there must be a variation in the activities leading to the same event, and conversely if there is a variation of activities then this must have arisen (given that the initial state \( o \) of the system remains the same) from a variation in circumstances.

Thus, he gives us a case in which "exactly the same causal chain" is determined by a variety of sets of field-conditions, the path of a pilotless plane \( \text{K\textsc{e}} \) which is fitted with ingenious devices so designed that the plane will maintain a straight course at the correct height to the desired goal irrespective of the weather conditions it might encounter" (p. x). "Now, if even if the path were straight it is hard to see how this is a "causal chain";
In part depend on the path it was following just previously, but it is not that that previous motion causes the present motion; it is rather that external forces relative to that motion, and the changes in parts of the plane, that cause it, and these will differ under differing circumstances, as Braithwaite himself admits. But the more important point is that if the plane encounters any change in relevant weather conditions during its flight then it will inevitably deviate from its straight course, because the controls of the plane at the time the known change in weather occurred were set to contend with the preceding weather conditions, and it is only the deviation consequent on the changed weather that brings into play the mechanisms which alter the plane’s course so as to compensate for the displacement. To say that the plane maintains an straight course is to suggest, as Dewey did, that the effect (the change in the setting of the controls) springs up in anticipation of its cause (the change in weather conditions.)

There are, of course, machines which may be said to “anticipate” certain events and to “prepare themselves” in advance for them, and so it is theoretically possible for that a pilotless plane might be equipped with perfect weather predictors which would actuate time mechanisms which would in turn so alter the controls at the precise moment of the change in weather that the plane did in fact maintain a straight course. But there are no such pilotless planes.
or weather predictors, and Braithwaite is not thinking in terms of them. Even with so-called "anticipation" the crucial point is this, that the causal sequence is not an anticipation of future events but a reaction to present events. Thus, with anti-aircraft predictors, for example, no matter how sophisticated, work on the principle that if the target keeps on describing the kind of curve it is now describing, then after a specified interval of time it will be at such-and-such a place, which is of course not in any sense an "anticipation" but just an assertion about the shape of the curve and the plane's position and speed. These data arrive at the predictor not as "signals" which it interprets but simply as causes which are quite mechanistically produce their effects according to the structural and the momentary properties of the machine.

(Having made that sort of point one often feels forced to admit that if this arrangement of mechanisms "just happened" then it was an incredible unlikely coincidence that it should just happen in such a way as to produce this effect. It seems that it must have been arranged with the purpose of producing this effect, and since we have an actual human being, the designer of the plane, ready to hand, we feel that the purposiveness is in him, if it is not in the plane. But this is just a shifting of ground; it was supposed to be the plane which was purposive, and is now seen to be mechanistic, that might lead us to expect, and, as I argued above and hope to elaborate shortly, that the
appearance of purposivism in the man will also turn out to be illusory. Furthermore one might point out (cf. Appendix) that there are "compensatory mechanisms" not designed by men, and again that anything at all, artifact or not, once we understand how it works, will appear to be "eminently suited" to produce those effects which it does produce.

In general, we might say that the appearance of "plasticity" which Braithwaite mentions, the appearance of achieving something "despite" the appearances, is a function of our ignorance of the conditions that are necessary as well as sufficient to produce the behaviour in question. He has not shown how teleology can be compatible with determinism, nor how a teleological explanation can be scientifically useful.

Activity in feedback mechanisms. The cybernetics people also claim that their "methodological approach does not imply the philosophical belief in final causes" (p. 326). They say that "the instantaneous interactions between an object and its environment are in no way different in non-purposeful and in purposeful behavior. But the adoption of a teleological approach simplifies the analysis of goal-directed behavior and enlarges the scope of this analysis." But the criteria they offer of purposive behaviour are, like those of McDougall, either question-begging or simply assert that there is self-activity, which does make a difference in terms of "instantaneous interactions."

Criticisms on these lines were made very cogently by Richard Taylor ( ).
The only empirical differentiation that Rosenbleuth, Wiener, and Bigelow offered was that of control by feedback, which characterized the "highest degree" of purposefulness, namely "teleological purposeful behavior." The term "feedback" indicates "that the behavior of an object is controlled by the margin of error at which the object stands at a given time with reference to a relatively specific goal," or as Taylor puts it more clearly, getting rid of the question-begging terms "error" and "goal," "an object is controlled by negative feedback when the effects of its behavior in turn act indirectly upon the object itself to oppose whatever it is doing." Taylor points out very forcefully that as far as the logic of the situation is concerned, a torpedo that is guided to a ship by the sound waves it emits does not differ from one that runs along a cable attached to the target. If this second torpedo is diverted from its path, or the target moves, then as a result the torpedo pulls sideways on the cable, but so does the cable pull sideways on it, and the torpedo is turned back once again more or less on course. In the first case, if the torpedo is diverted, the sound waves, coming in a different direction and phase, act on the sonic mechanism, which in turn acts on the rudders and vanes, in such a way as to bring it back on course. Rosenbleuth and Wiener ( ) say that the analogy is quite erroneous, since "the design, mechanics, and trajectory of a pulled missile are quite different from those of a missile controlled by negative feed-back" (to which Taylor would immediately assent, since he was speaking only of logical
categories), and "the difference is obvious if we realise that the pulled torpedo does not adjust its rudders and vanes to minimize an error, whereas the sound-guided one does" (p.321). But this distinction is precisely in terms of that internal organization of the object which they claimed their approach enabled them to avoid, and this verifies my original contention that by clinging to the behaviouristic approach as they define it (and ignoring the internal organization) they cannot deal with the phenomena ("spontaneity" so-called) which give rise to the ordinary notion of purposefulness without implying some special doctrine of causality - without departing from determinism either by just accepting "probability" as ultimate or by regarding the object studied as having activity. It is this latter notion which is becoming clearer here when they speak of the sound as a "signal" to the sound-guided torpedo to adjust its own rudders and vanes. Where a thing seems to produce changes in itself then we must regard this as following immediately from the interaction of its parts.

It is quite true that we need not concern ourselves with the origin of such changes, and one might agree for the moment that the method they advocate here, the "open use of the notion that the torpedo seeks the target" might "simplify and clarify the description" of the gross event under consideration and so be useful if we were concerned say with naval tactics rather than with armament techniques. But such
fields of enquiry, though admittedly independent in a logical sense, will be found to interpenetrate at many points when we are actually pursuing them. If, for example, the defending fleet discovered some way of altering the sounds so that they were outside the range of the torpedoes' sonic mechanisms, or caused it to overcompensate, it would be impossible to understand how that occurred without considering the detailed structure of those mechanisms. Now, if one took the phrase "the torpedo seeks the target" very literally, then it would be an obstacle to thinking of enquiring into its parts, because it suggests that it is just something that the torpedo as a whole does in the positive sense. In point of fact, it would not deter a naval technician for a moment, but that is only because mechanical principles and the construction of the specific torpedo are so well understood by them, but the parallel notion of seeking and all the various "dynamic" terms used in motivational psychology constitute a very effective hindrance to discovering even the general field within which the solutions to which many similar problems of behaviour lie.
V - Knowledge as a Critical Relationship.

The virtue that I have once or twice hinted at in "feedback", its unnecessary purposivism being cleared away, is no more than this: it points out that a "goal-seeking" organism does not seek the goal, it is joined to it. We have to interpret "goal" here as meaning not the situation "striven for" but the object with which the organism enters into some specified relation - this being the "goal" in the more common usage, in the sense of "target". This difference in meaning seems as if it were only an inexact usage in Rosenbleuth, Wiener, and Bigelow, who use the word quite indiscriminately to mean both the situation finally arrived at by the "seeker" and the other term of the maximization situation. But although they never make the point clear, the notion is there that indicates the lines along which we might solve such problems as the "nice adjustment" of the movement of the hunting torpedo or the hunting dog or the catching hand to the movements of the object of "its pursuit." Once again to oppose one fallacious exaggeration with another, we might say that the hunter is directed towards the hunted by the "hunted," not (as we have asserted on the basis of our logic) by itself. It reaches the prey "despite" its twists and turns because it is joined to the prey whether by a cable or by sound-waves or by light-rays.

One objection that immediately presents itself is that in the case of organisms we frequently find that the "goal" in this new sense is not present, yet the organism's behaviour shows that kind of sequence
of phases which gives rise to the notion that it is seeking the goal. Not only does the target not stimulate any of the organism's receptors but we might say that as regards the early "casting-about" behaviour there is as yet no particular object standing as the "goal" — any member of the species would do. But at least the organism (if this is the sort of behaviour that would be called "goal-directed" by some-one who, believed the term had meaning and was careful in using it) must cognize that there is such a kind of object, and have some hypothesis, however vague, about how it is to be reached. Even here, of course, it is still not true that it is joined (by cognition) to that particular object with which, in the as yet unknown future, it will enter the required relation, since it is still not acquainted with that individual. But it is "joined" by cognition to many features of its actual present environment, and any hypothesis (about how to get to the "goal") which can be effective in starting behaviour must be some belief about a path leading from here to a place in which some member of the species in question is likely to be (in general, some belief about the relationship of the "goal" class to something that is here and now.)

Now, an argument which in some ways has a good deal of force, is that the assertion "X leads to Y" has no implications for action: this piece of knowledge might be used either to pursue Y or to avoid it, so that it seems itself to be quite neutral with reference to behaviours, which depends on "driving force." That conclusion (with some reservations
concerning "force") is literally true; the assertion "X leads to Y" does not by itself imply any policy or course of action, but for any particular man for whom the occurrence of Y is not a matter of indifference the arrival of this piece of knowledge concerning X's connection with Y will necessarily produce some response to X.

(Obviously some opposing impulse might prevent this impulse from leading to an overt action, but some internal rearrangement would still take place.) That argument concerning theory not implying policy is sometimes used to preserve a belief in "freedom" or some notion of activity; but what really happens, I suggest, is that when that piece of knowledge occurs, or better, when the belief is produced, that X leads to Y, then X (and later on anything that is believed to lead to X, and so on) immediately produces an effect similar (allowing for certain qualifications to be developed later) to that originally produced by Y. Taking Y to be the beginning of the chain, the kind of object which produces the original instinctive response (v.i.), then the mechanistic nature of the reaction to X and to the many other things which are later, on the basis of experience, believed to lead to Y, seems to me to become plain.

(Perhaps some apology should be offered for baldly introducing and using the notion of cognition here, without any discussion of the "evidence" concerning its existence or the possibility of accounting for behaviour without making any use of that notion. But that evidence and those arguments can never be conclusive. It is impossible to
prove that a given behaviour \( P \) cannot be accounted for without reference to cognition - i.e., that "all \( P \)-behaviours are cognitive" - unless we already have cognition in our premises. It is only possible to prove, that is, that cognition occurs in a particular place, and not that there is such a kind of relation as cognition, and if the behaviourists are not prepared to admit that there is such a relation, and cannot be convinced that they tacitly admit it in, e.g., advancing their theory, then they can regard arguments that experimental results establish cognition as not only inconclusive but strictly meaningless. Certainly one can offer definitions of cognition and then, if the terms are admitted to be real, prove that there is such a conjunction of terms, but that cannot establish the existence of the cognitive relation which, as with the definienda of all non-verbal definitions, is something over and above the defining terms - that is, is not identical with them but rather a subject of which they are predicates.}

The occurrence of unlearned activities even in humans should no longer be held in serious doubt. It is basically a logical assertion that anything at all has "unlearned activities", that because of its own qualitative nature specific effects will be produced in it by the impact of other things, and instinctive activity is to be seen then as the effect produced quite deterministically in the organism by the perception of some specific kind of situation. But it seems that this provoking situation or object can never be specified just by one word, or as "just one
things" it will of course have various properties and as a matter of fact it is regularly the case that any selection of these (within a certain range) may produce the instinctive response, depending sometimes on the intensity of the relevant motivational condition — say, the concentration of a hormone or the intensity of a feeling, and so on. A great deal of evidence has been adduced, e.g., by Lashley, (21), U. P. Stone (20), and Tinbergen (23), concerning the "complexity" of the stimuli for instinctive acts, and (especially with the latter two) the differential effect of changes in the intensity of motivation on the reaction threshold for these various properties. The general pattern is that there is (in some animals and with some instincts at least) a hierarchy of such thresholds, so that up to moderate intensities of motivation the reaction can be elicited only by some conjunction of properties; this conjunction approaching more or less to the "ideal" of being peculiar to the situation to which the reaction is "appropriate," where "appropriate" means that an animal which exhibits this reaction in this situation will be more likely to survive than one which does not. If that compound stimulus pattern with the lowest threshold were peculiar to the "appropriate" situation, then this too might have a degree of survival value in the above sense; e.g., if it were not peculiar, then the reaction would sometimes occur in an "inappropriate" place leaving the organism temporarily unable to give the reaction in the "appropriate" one.
But as the degree of motivation rises, the reaction comes to be evoked by individual components of that original compound stimulus despite the absence of other components which may be universally characteristic of (necessary for) the "appropriate" stimulus situation; e.g., Tinbergen says, "A female [stickleback] will respond to a dummy that displays only one of the sign-stimuli, e.g. the zigzag dance and not the red colour" — that is, the red-coloured stomach which all male sticklebacks develop when their "reproductive drive" is high. This increasing sensitivity extends, then, at least in "higher" species, to objects or events which are believed by the organism on the basis of experience to be signs of the presence or approach of the original stimulus object.

This might offer a partial insight into some kinds of error, with reference to the problem of "what is before the mind when we are in error?" That is, on an earlier occasion the organism notes that Y is accompanied by X (though in fact some Y are not X), without necessarily asserting then or later that all Y are X, and then on a later occasion, when the motivational system in question is in a state of high tension, perceiving X, acts as if Y were present. Is that sufficient, however, to say that error is present? Error, it seems, can only occur if there is an assertion, such as "Y is here." Now, it seems to me that in the experienced organism one very common, and for human psychology very important, kind of response by the highly motivated organism is just to believe in the (perhaps concealed)
presence or imminence of the situation which in the past has evoked the instinctive response and has been cognized as doing so. This tendency would become stronger and the belief more isolated from the effect of conflicting evidence the higher the intensity of motivation. This of course is the phenomenon often referred to as "wishful thinking," a description which on my view is not strictly meaningful. Furthermore it would occur even with those expectations which we think of as "painful." It is often remarked that when a person's fear is strongly aroused he "starts at shadows," and even quite unconvincing indications of the presence of the feared object can make him give way to despair. (The obvious objection, and one which in particular cases is sound, is that in such a case the person "really wanted" the dénouement which he apparently feared, but this can be dealt with only by considering the conflict of motives, a subject which I hope to treat later on.) It seems to me that at present we must accept such believing just as part of the repertoire of unlearned activities, and so as one which, when motivation is extreme, can be touched off by stimuli only remotely associated with the original situation, yet which were in fact associated with it, and were consequent perceived as being so. The materials for the assertion, then, "Y is here," were provided by experience, though many problems remain in connection with other kinds of erroneous belief.

The point I am making here is that even though in some "goal-directed" behaviour the "goal" is not yet some concrete object to which the
organism is joined, nevertheless it is "joined to," or provoked or directed by, those objects in its immediate surroundings with which it deals, believing them to be connected in a specific way (geographically or causally) with a certain other kind of object (the goal.) We might put it a little more strongly by saying that some parts of the immediate environment are seen as parts of that kind of situation which automatically produces the response in question, and so act as a situation of that kind. Actually it is quite plain that the behaviour they produce is greatly altered from the kind of effect (the behaviour) which the original instinct-provoking situation produces. If the same behaviour were produced it would inevitably be "inappropriate," that is it would not produce in the objects in the immediate situation the effects which it produces in the original provoking situation. What happens is that that response is delayed, and locomotion or other "preparatory" actions occur. But before I go on to discuss that extremely difficult problem I would like to refer to an objection that is sometimes made to the sort of view I have been expressing (the view that all behaviour is a response to perceptual stimulation), namely, that purposefulness has only been driven back to another place, and that we still have to account for the selectiveness of perception. At any time there is an infinite number of situations available for the organism's cognition, and yet it never cognizes more than a limited selection of them, and it is a
matter of common observation that those that are cognized, or, as it is said, "attended to," are for the most part facts relevant to the performance of the instinctive action in question.

The question arises, what is meant by saying that these facts are "available?" There cannot, we believe, be any differentiation in this connection between the facts of themselves; it seems that we must just take it as a postulate that there are no facts which are of their nature unknowable. For the individual some limitations might be set up by his education; that is, there may be facts which we cannot know until we know certain others, and any particular individual may not have been "exposed to" these prerequisite facts. But it can also be observed that a man cognizes fact \( p \) on one occasion and does not cognize it on another when it is again present, or at least does not at the time act as if he cognized it, and cannot report that he cognized it, though that might mean simply that he did not know that he cognized it. It is hardly necessary to add that on this subsequent occasion when the man seemed not to know \( p \), \( p \) to the best of our knowledge stimulated his distance receptors in much the same way as it did on the earlier occasion when he knew \( p \).

There are two possibilities here, which raise rather different problems. Either on the second occasion his central nervous system was in such a condition that (depending on humoral factors and the pattern and direction of the electrical forces already operating, etc.) that the nervous processes resulting from the stimulation of the receptors
did not ever result in the kind of neural processes which cognized
in such a condition that despite the physical presence of $p$, $p$ was
not able to produce in that mind the awareness of $p$, or $p$ was
cognized and the kind of event occurred which we refer to as $p$
being "ignored", or "the attention being withdrawn from $p$", though a
variety of mental events might be included under this general head.
There is a good deal of evidence to the effect that this latter
possibility is a real one, that even though at the time the person acts
as if he did not know $p$, and cannot report that he knows $p$, still
(on some such occasions) he did know it, because he can be brought to
recall it, or his subsequent behaviour can be affected in a way which
seems to depend on his having known $p$ to be there.

Now, can it be said that a person (or some part of a person) knows
a particular fact at one time but at a later time does not know it?
This is, of course, not a logical question but an empirical one,
depending on the nature of the relation, cognition. (We cannot say
that a man at one time was a descendant of $X$ and at another time not
a descendant of him.) What seems to me to be crucial in deciding it
is the point that after this apparent lapse of knowledge the knowledge
may appear again without external persuasion or further evidence,
which seems to make it plain that in some sense it was present throughout.
If that is so then we must distinguish between some persisting (unknown)
change in the mental-neural constitution by virtue of which we know the fact, and the occurrence of that actual relation, the fact of actual contemplation or attending to, which is clearly intermittent. But the occurrence of this contemplation, like any other event, must arise from some external impact, and presumably, putting it in neurological terms, it would arise either from the occurrence of a certain chemical state in the fluids surrounding the tracts in question or the irruption from other areas of some particular pattern of nervous impulses, or both, or in psychological terms from the occurrence of a certain state of motivation or of perceptions or other acts of contemplation of facts associated with the one in question historically or geographically or logically, or a conjunction of those two events, and it is maintained, presumably, only while such facilitating processes are going on.

But whichever of those two possibilities occurs in a particular case - the failure to cognize or the failure of contemplation to be maintained - the logical point remains the same: the organism does not select those objects to which it will pay attention except in the sense in which any object at all "selects" those effects which will be produced in it by a given kind of event. The occurrence of the relation of cognition depends both on the nature of the situation cognized and on that of the organism cognizing. It simply is not the case that "unpleasant" or irrelevant facts must be cognized in order to be ignored,
(although it seems true that sometimes within the one mind the same fact is both asserted and denied) and yet their failure to be cognized may be due in part to their logical relations to the facts which are at the time being cognized. That is, it may be possible to specify that class of facts whose nature is such that they will not produce the effect of cognition (of them) in this man in his present state, by indicating their logical relations (e.g., indifference, incompatibility) with the beliefs which he is now having. It is this which gives the impression that he "discriminates" against certain perceptions in an active sense, and that perception in general is an active grasping.

But the contamination of the notion of cognition by that of purpose is even more pervasive. Not only do people refuse to think of cognitions as being effects produced in them, rather than one of their own positive operations, they refuse to think that the cognitions in turn produce actions in them, rather than that, having gathered together the information, they decide what to do. Thus, to do something "knowingly" or "deliberately" now means to do it "on purpose," but that is simply a confusion. To say that an action is guided by cognition of features of the environment is not to say that it is purposefully, but for the organism knowledge is one of those "critical relationships" which I referred to above in connection with causality, into which one object must enter with another if it is to produce any effect in it. It is not then, strictly, cognition that
guides our actions but the objects cognized; it is only by entering into that relation with the organism (being cognized by it) that they are able to produce certain kinds of effect in it, but when they do enter that relationship then those effects are produced immediately and automatically. (It is by treating knowledge as a critical relationship in this way that we can give a workable account of the phenomena which caused Lewin to set up his notion of "life-space."

Rather, then, than saying that we find our way around obstacles we should say that certain obstacles deflect us from our path and others turn us back again; they do this without the necessity for physical impact but just as determinately: this is the only true "action at a distance."

Bringing the discussion back, then, to the point from which this section started, is it the case that behaviour must always be initiated (given that some specific internal condition is also necessary) by the perception of objects in the immediate environment as related to the "goal-object" (specifically, in those cases where the "goal-object" is not present)? Could it not be that the mere raising of the level of motivation was sufficient to reinstate the notion of the goal-object (which would then automatically initiate the behaviour in question) without the necessity for our "being reminded of it" (as it were) by something in the immediate environment? (The point of these questions is this: how can we give a deterministic account of those phenomena,
which give rise to the notion that "the organism does not wait for
the stimuli to produce a response, it goes out to look for the so-
called stimulus".)

It was suggested above that such an increase in motivation might be
sufficient to bring about the re-contemplation of some previously
known fact, but the question now under consideration does not concern
only cognition. It arose in connection with that sort of behaviour
characterised by Rosenbluth, Wiener, and Bigelow as being guided
by and "nicely adjusted to" (without actually being physically moulded
by) the behaviour of the object with which it deals. (If one squeezes,
say, a hand dynamometer, then the behaviour of the dynamometer will
be "nicely adjusted to" the behaviour of the squeezing hand, but while
in point of fact the logic of the situation is the same, the cyber-
neticists claim to be talking about something more.) But, even allowing
that the "idea of the goal-object" might arise independently of any
present perception, if there were not then some cognition of features of the present situation, and if behaviour were not affected
by them, then whatever behaviour occurred would be purely "random",
and this (if it ever occurs) could not be cited as "objective evidence"
for purposivism.

The point arises that our acts of knowledge or belief do not refer to
just one term, as is suggested by the contention that an increase in
motivation activates the "idea of the goal-object." (These same consid-
erations apply also to Freud's early doctrine of "ideas becoming charged
with affect." Whatever we know or believe or assert is propositional in form. Now, what sort of belief about a goal-object is it that initiates activity in connection with that object? I suggest that it is not (or not alone) "X is red" (or round, or even sweet), or the "the last time I saw X, I did so-and-so, and such-and-such an effect was produced in me," unless we accept also the notion that the organism strives towards or away from that effect. (This is the routine way of accounting for the "learning of motives"; i.e., "the organism associates a given response to a given stimulus with the satisfaction of some demand, and so repeats that action when the situation occurs again" — in fact, the consequent in that argument is often taken to be so self-evident that it is left unstated.) Either of those two types of belief might evoke action if and only if they were accompanied by the belief "X is here" (or "in a specific direction from here"), and in that case the feature of the situation to which the action would basically be a reaction would be not X but in the first case red, and in the second "the ability to produce the effect such-and-such in me" — otherwise those beliefs would be irrelevant.

When we are talking about reactions to X, actions in which X stands as the goal-object in the sense adopted above, then "X is here" is the typical evoking belief, because basically, or in historically the first instance, it is X which produces the reaction (by virtue of being cognized), even though later it can be produced by a mistaken belief in the presence of X (or, as I suggested, as a reaction to
something more or less remotely or occasionally associated with X.)

(When I use the phrase "X is in such-and-such a direction from here”,
I intend it to be taken not merely literally, although in some cases
it would be correct in the literal sense, but also as a possible
metaphor — e.g., a "path" from here to X might be a series of actions
not involving locomotion at all.)

In fine, then, all the truly objective characters of this "hunting"
type of behaviour which distinguish it from that of the dynamometer
are entirely accounted for by the type of "critical relationship"
in virtue of which the surrounding objects determine it — specifi-
cally, for animals, by cognition, which mediates that "action at a
distance"on the part of environmental factors which is the reality
underlying the illusion of "anticipation" and "adaption" on the part
of the organism.
VI - The Nature of an Instinct.

Something must be made of the notion of persistence of activity and variation of direction "towards the same goal" - some treatment must be given, that is, of whatever phenomena gave rise to this notion. The problem might be approached by considering the notion of "frustration." This too, as it is commonly used, i.e. as the frustration of a response-tendency, involves a logical anomaly. That is, it suggests that some situation has arisen (e.g., in the Barker, Dembo, and Lewin experiment, the sight of the new toys) which would evoke a certain response (plying with them) if it were not for some factor which prevents that response being made (the intervening screen.) But this is only to say that the conditions necessary and sufficient for producing that behaviour are not fulfilled. To say that the irruption of X into the field Y would produce effect Q if X had the property P is to say that "X" is not a sufficient specification of the kind of agent whose impact on Y gives it the property Q. X simply does not produce that effect in Y and that is all there is to it. But the notion of frustration suggests that is not all there is to it, but that the effect was produced "potentially"; it is as if the response were penned up inside the person and were struggling always to get out - and the difficulty is that that seems quite a proper description of "what it feels like" to be frustrated. Nor does the point that the impact of the "incomplete" stimulus situation produces other effects (e.g., anger, cognition of what would
have happened if the other conditions had been fulfilled, and so on) seem to cover the situation adequately. However, the notion of the penned-up response is illogical, and it seems to me that what we have to recognize to account for "frustration" (and, in part, for the "variations in direction" that follow it) is just the quantitative aspect of motivation— that fact that what is penned up inside us is an increasing quantity of that kind of "stuff" which would have been, in part at least, exhausted by the frustrated activity, which would still eventuate in or perform that activity if the barrier were removed, and is building up until it bursts out in other activities which are allied to the frustrated one in the sense, simply, that they are done by the same body of "stuff."

It is an empirical and linguistic question how precisely one should interpret the word "stuff", but at least it emphasises the qualitative nature of whatever it is that varies quantitatively. It must be something that we could discover, say, in a physiological investigation, and yet be ignorant of any of its functions in determining behaviour. A fairly adequate schematic way to describe the matter, it seems to me, is to liken each motivational "stuff" to a liquid continually pouring into a receptacle (though probably not at a constant rate), this receptacle having outlets of various sizes and at various heights, closed of by seals of a resistance differing from one outlet to another, and changing from time to time for each particular outlet.
These outlets represent various kinds of action, their nature being innately determined, which exhaust in their varying degree the supply of motivational substance, and the seals are simply the competing pressures of other motives, determined largely by beliefs about probable consequences of the action represented by that outlet. This simile, it seems to me, at least has the virtue of giving a meaning to "frustration" and "substitute activity" without the need for any notion of directiveness. It suffers the disability, however, of not being able to account for the fact that on many occasions a variety of activities is possible as far as the state of the organism is concerned, and which of these actually occurs depends on the nature of the environmental situation, or at least on the organism's beliefs about it. That is, the simile so far ignores the stimulus which, on many occasions at least, sets off the action. We must regard the perception of the external situation as tugging at the barrier to the outlet in question, but unable actually to open it unless the level of the fluid inside has reached a certain height and so the pressure on the barrier a sufficient magnitude to burst the barrier in cooperation with the stimulus. Is it possible for the barrier to be burst without the relevant stimulus? Apparently it is possible but only at extremely high degrees of motivation. The behaviour so resulting would be by definition "inappropriate" - it would include such things as eating movements where there is no food, and in fact would be of the kind which would lead us popularly to say that the person had been "driven mad"
by his privations, and had fallen prey to hallucinations.

Furthermore, it is possible that a barrier be so strengthened by events in the organism's history that even the combined action of the relevant stimulus and a very high degree of motivation cannot burst through it. Except in those cases where by force of external circumstances no action is possible which consumes the accumulated motivation (e.g., where it is physically impossible to get any food) this strengthening of one barrier would not lead to such an unrelieved accumulation of motivation that the barrier would eventually be overcome; rather is it the case that there are other outlets either higher up the "reservoir walls" or if lower the having less rigid barriers which, under the accumulating motivation and appropriate stimulation, come into play to reduce, to some degree at least, the accumulating pressure and keep it for an indefinite time below "catastrophic" levels. (To say that the outlets are at different heights is only to say that any given activity can at its most effective reduce the motivation only to a certain level and no further.)

How is it that the barriers come to alter in rigidity? I said that the barriers are actually the competition of other motives - that is, competition for those structures (as a first approach, we might say the effector structures, or the "avenues to motility") which carry out the activity represented by the outlet in question. The force with which the competing motives bear on the one effector structure is determined to a large extent by beliefs possessed by the person (or rather,
as I shall argue later, by the individual motivational structures) concerning the consequences of the competing motive's gaining control of it. The imminence of such a gaining of control can become one of the class of stimulus-situations (be assimilated to or seen as a member of the class of original instinct-provoking stimulus-situations) which now result in learned reactions (the discussion of the nature of such learning still being postponed) which appropriate (if the motivation is strong enough) the effector structure in question.

This introduction of questions of cognition enables us to offer a definition of the subject-matter of the psychology of motivation. (\textsuperscript{155})

P. T. Young in one place wants to define it as the study of all the conditions affecting the behaviour of organisms; but that seems much too broad. (It would include almost all psychology, and fails to distinguish between psychological processes and their causes, and physiological ones and their causes; (always admitting that these two sometimes impinge on one another.) It is difficult to see that the study of the action of atmospheric gases on the lung is a part of the study of motivation, even though at certain points it might become relevant to it; and one might suggest further that the study of the processes of perception, of the stimulus properties and organ properties necessary for correct perception, is not a part of the study of motivation, even though some problems in each science even though some problems in each science require information from the other for their elucidation.

Now, even if we reject the notion of motives as active processes in the organism, at least we can say that the emphasis on internality, on
the importance of the organism's own nature for its behaviour, is the central thing in the study of motivation, and marks it off from that study of the external conditions which was characteristic, e.g., of the more rigid behaviourists, and regarded by them as the only necessary and perhaps only possible field of study for giving an account of behaviour. We argued above that such a programme could not be sufficient, but we recognize that it is an essential auxiliary to the study of the internal factors.

However, if we are still content to define motivational psychology as the science of all those conditions of behaviour which are internal to or properties of the organism we find ourselves still including all of physiology. I suggest, in the criterion, psychological behaviour is behaviour directed by knowledge or involving belief (whether these themselves be known to the organism or not), and the study of motivation is the study of the nature and functioning of those internal structures which have knowledge or hold beliefs. It is not enough to say that it is the study of the processes, even the fluctuating ones, mediating the response to a "stimulus", since this latter term is used so frequently to cover e.g. a blow on the patellar tendon. The distinction between "behaviour" and reflex is not purposefulness, as McDougall contended, since there is no such thing; but at least there is a distinction between kinds of action in terms of cognition, since it is not necessary for the blow to be known in order for the knee-jerk to occur, yet it is necessary for some stimulus-situations to be known before they evoke
a response. To suggest that the latter responses should be called the peculiarly psychological responses is not to legislate usage in too arbitrary a fashion, since cognition has long been recognized as one of the alleged three marks of mind—though usually mind was being conceived in the structuralist way. This too is a part of what G. F. Stout is pointing to when he distinguishes instinctive from reflex reactions by saying that instincts involve "intelligence," although a good deal of purposivism in the sense of effective adaptation to novel situations creeps into his use of this notion (22).

Parts of the above account of motivational structures as "reservoirs" find a parallel in quite literal physiological accounts of instincts in Tinbergen (33), and of the bases of motivation in Morgan (6).

Tinbergen regards an instinct as "a hierarchically organized nervous mechanism which is susceptible to certain priming, releasing and directing impulses of internal as well as of external origin, and which responds to these impulses by coordinated movements that contribute to the maintenance of the individual and the species" (p. 112).

He makes it clear that a large number of different activities is carried on by any one instinct. Thus he says: "In spring, the gradual increase in length of day brings the males [the male sticklebacks] into a condition of increased reproductive motivation, which drives them to immigrate into shallow fresh water. Here, as we have seen, a rise in temperature, together with a visual stimulus pattern received from a suitable territory, releases the reproductive pattern as a whole."
The male settles on the territory, its erythrophores expand, it reacts to strangers by fighting, and starts to build a nest..." but "...fighting, for instance, has to be released by a specific stimulus, viz., 'red male intruding into the territory.' Building is not released by this stimulus situation but depends on other stimuli. Thus these two activities, though both depend on activation of the reproductive drive as a whole, are also dependent on additional (external) factors... Thus the effect of a stimulus situation on an animal may be of different kinds. The visual stimulus 'suitable territory' activates both fighting and nest-building, the visual situation 'red male in territory' is specific in releasing fighting, but it merely causes a general readiness to fight and does not determine the type of fighting."

(p.103).

The general tendency in this view is to move away from the definition of instincts by reference to the goals the "seek" or even the actions they perform — e.g., "the instinct of flight," "the gregarious instinct," "the maternal instinct" — because when some detailed, concrete notion of the relevant physiological systems is entertained, it is readily realized that each such system (like anything else) performs different actions in different situations. McDougall, in his early accounts of instinct, also had this notion of a physiological structure, but owing perhaps in part to his slighter physiological information, and in greater part to his pervading vitalism, he defined his instincts by reference to the effects they produced, and so was driven to an indefinite multi-
plication of them. Tinbergen too, despite his more empirical bias,
tends to define instincts by reference to their results, e.g. the "
"reproductive instinct," and so is in some danger of the same excesses,
despite his controlled observations of the conjunctions or covariations
of different activities, and his equally important recognition that
any one activity might be carried out by a variety of instincts.
(As far as he discusses this issue in "the Study of Instinct," he
means that the one activity may at different times be performed by
different instincts, whereas in order to understand some behavioural
phenomena - e.g. those which C. Allport (1) cites as evidence for
"functional autonomy" - it is essential to see that on any one
occasion a given activity can be the result of the mutual interaction
of several different instincts, as has been pointed out e.g. by D.
McClelland and H. Maslow).

The reference to "covariation of activities" immediately suggests
the application of factor analysis to the task of classifying instincts,
but, although that has been specifically suggested by Durt, (1/1), and
is an extension of the observational methods (i.e., observation of
behaviour or overt acts) advocated by McDougall and Tinbergen, it
seems to me that it could be successful only in providing hints for further
investigations of a different type, or, as Eysenck suggests, for the
testing of hypotheses arrived at in another way. Despite the fact that
a number of users of factor analysis in psychology now recognize it as
of the proper scope, etc. etc. etc.
simply an effective way of classifying behaviour, the view persists that although factor analysis cannot give us any direct information concerning the components of mind (or whatever we think of as doing the behaviour), still, to carry out each of the major or "basic" kinds of activity there must be one unitary structure. That is simply a mistake, as I have been arguing. Another way of saying so is to point out that none of the features of behaviour are "major," or more "basic", than any other. The only distinction would be in terms of extension, of how many different kinds of activity had a given feature common to them. Any piece of behaviour has any number of properties, that is, is of any number of different kinds, and so there is an indefinitely large number of ways of classifying behaviour. That, of course, does not cast the smallest shadow on the validity of classification, as P. T. Young thinks it does, but the point is that no particular classification of behaviour is any more fundamental than any other, and the attempt to discover the structures of mind by this approach would provide us with an infinite number of alternative hypotheses — all of them, however, based on a false postulation concerning the "one-to-one" relationship between structure and activity. (The only virtue of factor analysis as a technique of classification would seem to be that it might bring us to discover properties of behaviour which we had overlooked. The factorisation of measures of variables of social groups might be especially valuable in this way, since we know so few of the properties of social entities, but there is a tendency in factor
analysis which also opposes the discovery of qualities; that is, the
search for greater extension, for a limited set of dimensions in terms
of which we can describe any activity, results in a loss of intension,
a disregard of qualitative differences. Description is always incomplete;
and even though we find a set of dimensions which is sufficient to
"account for the variance" there will always be an infinity of qualita-
tive differences which are not mentioned in these dimensions and
which cannot be reduced to quantitative differences.)

Concerning the discovery of instincts or motivational structures one
can make the same point as that concerning the question whether there
is such a thing as knowledge: the finding of kinds of thing can only
be done by observation, and the appeal to observation is the only way
of convincing others of their existence. What techniques of observation
could be of use here? Certainly one approach would be the combination
of physiological techniques with observations of behaviour; that is,
the investigation of chemical properties of the body fluids, the
conditions of their fluctuations and their differential effect on
parts of the CNS, an elaboration of knowledge of the effects produced
by stimulation under varying hormonal conditions of different parts
of the CNS, and so on. There is a large body of information on these
questions, advanced by such people as Stone, Beach, and Morgan. But
when we remember that these central nervous processes also cognize, and
may conceivably be differentiated on the basis of what they cognize,
and if we believe in general that they have the sort of mental properties which we find partially and confusedly recognized throughout the history of psychology under the heading of Affection, then we see that there are other approaches to the problem than the physiological, even though they and the physiological must mutually correct and assist one another. It seems to me, as I shall argue later, that one of the very few attempts at a mentalistic approach to the isolation of motivational structures is that of Freud in his notion of the libido — in fact, that he was one of the very few non-physiological psychologists who ever at all grasped the point that the motivational structure must, identifiable without reference to its effects, even though there are strains in his treatment of libido which are in palpable conflict with this interpretation.

Even psychologists who concentrate on the physiological structures frequently fail to keep traces of relativism out of their conclusions. C. I. Morgan, for example, in describing the Central Active State which his researches have made him postulate, says that it has a priming property which provides a "readiness to perceive and react to stimulus situations in particular ways" (ibid., p.461). This, one must admit, is the slightest of fallings away, and in a writer whose sound empiricism compels respect. Tinbergen, however, retains the notion of goal-seeking; quite explicitly, in his distinction between appetitive and consummatory acts. This distinction is connected with his notion of a hierarchy of nervous centres for each instinct. In the "reproduct-
ive instinct," for example, there is a centre which activates "the reproductive pattern as a whole," at the next level there is a centre for fighting and others for building, mating, and so on, and then governed by the centre for "fighting in general" is a set of five collateral centres for each of the five main types of fighting, while the other chains branch out in turn.

He says, then, that "the activation of a centre of the lowest level usually, perhaps always, results in a relatively simple motor response; biting, chasing threatening, etc., in the case of fighting in the stickleback; actual eating, actual escape, actual coition, etc., in other instincts..." (p.104). He goes on: "These relatively simple responses are, usually, the end of a bout of prolonged activity, and their performance seems to 'satisfy' the animal, that is to say, to bring about a sudden drop of motivation. This means that such an end-response consumes the specific impulses responsible for its activation.... Craig(1918) ...called them 'consummatory actions'..."

To rewrite "satisfaction" by saying that the end responses consume the "specific impulses," seems to me to be an extremely good way of putting the matter, but why then does Tinbergen call the earlier activities which make these consummatory actions possible "appetitive behaviour," and speak of the animal as purposefully casting about to find the appropriate situation? A further tendency to purposivism appears, I feel, in the nature of the hierarchy, or perhaps just in the order in which Tinbergen arranges the activities within it. The notion of "the
reproductive instinct" requires more literal examination than we have
given it: it may be more than a merely verbal appearance of purposivism.
What would be the action of a centre which "activated" the "reproductive
pattern as a whole," and yet did not (putting it roughly) dig or fight
or copulate or produce any action at all? There is of course an empirical
content to the notion of a centre being thrown into such a condition that
a certain perception will produce, say, fighting, but in Tinbergen's
scheme the fighting comes from a lower centre, not the central reproduc-
tive one. In point of fact, it does not come strictly from this next
lower centre, but from one of the five next lower down again — but
it is difficult to see why a centre which simply passes on impulses to
one of the actual fighting centres should itself be called the fighting
centre, or how in fact we could ever be forced to assume the existence
of such a separate, merely directive centre at all.
To approach the problem in another way, we might ask why Tinbergen
should be content to end his hierarchy at biting, chasing, threatening,
and so on. This is in fact precisely the problem of the infima species:
there are species of biting and of chasing, and species of those species,
and so on, despite the fact that Tinbergen describes them as "relatively
simple" and "relatively stereotyped." He could then never arrive at
centres which actually performed an action, rather than merely made
ready for it.
This is very like the problem faced by the "muscle-twitch" psychologies,
and even that of Hull though he denies that he is committed to muscle-
twitches. The problem for such psychologies is to avoid introducing as 
the R in their S-R formulae any of those popularly-admitted kinds of 
behaviour which seem to include purpose and cognition within themselves, 
and still to give a workable account of the bonding-together of the 
response units that they do recognize as real into chains long enough 
to accomplish the things that animals do in fact accomplish. Watson 
points to this problem in saying: "In psychology, too, our study is 
sometimes concerned with simple responses of the reflex types, but more 
often with several complex responses taking place simultaneously. In 
the latter case we sometimes use the popular term 'act' or adjustment, 
meaning by that that the whole group of responses is integrated in such 
a way (instinct or habit) that the individual does something which we 
have a name for, that is, 'takes food,' 'builds a house,' 'swims,' 
'writes a letter,' 'talks.'" (7, p. 10 f). Watson, then, apparently finds 
that such names of operations seem to involve purposivism, but for a 
different reason from that given above. The notion would be that "build-
ing a house" does seem to convey fairly directly the suggestion that we 
perform certain actions in order to produce a house, and even "swimming" 
is a matter moving our limbs thus and so in order to progress through 
the water. But if Watson does find them purposive then he should never 
use such "popular" terms in his psychology, without the inverted commas 
to indicate that there is no such thing, that there is nothing but 
groups of groups of unit responses of the non-purposive, non-cognitive 
kind going off in set sequences and combinations, and the central problem
is how they have come to be joined together in such groups. We know that he did in fact address himself to this problem, using the notion of responses being conditioned to internal as well as external stimuli; and that later Hull attacked it too, with the addition of the principle of reinforcement and his other elaborations.

Hull also finds himself faced with the question of what sort of "behaviour" can be a response, or rather he poses it for himself, because of his apparent belief (cf. 32, Ch. XIX, and 33) that admitting the possibility of differential responses to stimulus configurations, the reaction to relational situations as such (rather than a complication of the reactions to the separate terms of the relation), would be an acceptance of cognition. Thus, such a thing as "running to the end of corridor X" could not be a response worthy to be called "R" because (even if we can deny that it entails purpose) it involves a discrimination of the shape of the corridor and the rat's position relative to it. The unit of response in this instance would then have to be broken down to taking a step (and then another step) conditioned to the unitary doses of "simple" stimuli coming from the successive views of the walls of the maze, or starting with that and going on to proprioceptive stimuli.

If we take seriously the doctrine that the least, and most, that can occur is propositional in form, is complex, then we see that not only rats but machines and in fact anything at all reacts to complex situations "as complex." Such reaction, then, cannot entail cognition, unless cognition too is categorial. (P. E. Meehl points out very forcibly ( )
that Hull has confused the issue over cognition, that relations are not imposed on the stimuli by a cognizer but are a part of their physical existence, but he does not make complexity a general logical principle, and thinks it is a question still to be decided whether a theoretical system must admit "configurational postulates."

Leaving aside for the moment the question whether there are such "unitary doses" of stimuli, we might agree that there is some plausibility about "taking a step" as a non-cognitive, non-purposive reaction, because even though we might theoretically say that we flex a series of muscles in order to take a step, there is empirically a lot of doubt about how much we know of the muscles involved, and such knowledge as we have is a late development. As far as the infant animal is concerned, taking a step is as much a unitary response as taking a breath. But while perhaps an infima species has been found here, the empirical difficulties in showing how everyday behaviour is built up out of such movements are formidable. For example, in learning a maze, instead of the early trial-and-error having to be continued up to the last choice point, it would have to be continued up to the last step, and then the second-last, and so on, and if the rat should happen to arrive in the final path "on the other foot" then its previous training would be no good to it, and we should have to think of these separate response-series growing up side-by-side for the running of the same maze. In order to account for maze-learning as it actually occurs, Hull would have to contend not only that such a connection between successive steps would be
formed quite firmly on one reinforcement, but that one reinforcement would have to suffice to connect quite a long series of steps all at once. However, the premises which led to this fractionation of response (or would lead to it if Hull abided by them) are mistaken.

While this rejection of stimulus atomism does not immediately free us, on the response side, from restraint to such things as "taking a step," it does enable us to see that some kinds of object, especially some kinds of organism, may be coupled to specific objects by distance-receptors (though not necessarily by cognition) in such a way that their actions are continuously determined by the relations and changes of relation between that object (which may in itself be a many-termed relational situation) and themselves. It seems to me that this kind of coupling (which need not be merely spatial; that is, which may determine other kinds of change in behaviour than mere changes in direction of locomotion, dependent on what mechanisms the organism has) is overlooked by Tinbergen in his hierarchy of centres which merely "prepare for" actions until we come to a centre which carries out a stereotyped action.

What is the basis of this distinction between stereotyped and non-stereotyped actions? It cannot properly be that "stereotyped" means that that kind of action always occurs whenever the given situation occurs, because then there would be no non-stereotyped actions; nor can it properly be that different instances of "stereotyped" actions are all precisely alike, i.e., alike in every particular, since such repetition simply does not occur. The actual basis of the distinction is that
"stereotyped" actions are not continuously modified by the changing nature of the situation. Their slight changes from one to the other are determined for example by the physical influence of geographical factors, or by muscle tone, or the like. But while undoubtedly there are such actions, Tinbergen's use of the notion of a hierarchy of centres, the way in which he infers these e.g. from the existence of "species" of fighting behaviour, suggests that in his praiseworthy insistence on the mechanistic causation of behaviour (though as we saw he falls away from that view) he does not recognize this continual modification of behaviour by the changing situation (as against "in order to fit in with" the changing situation.) He inserts the higher centres in an endeavour to give a mechanistic account of the sort of behavioural phenomena we have been discussing, "preparatory" behaviour, but unless these centres actually produce, regularly and deterministically, some kind of behaviour (as against merely setting up a "state of preparedness") then either there is no non-stereotyped behaviour at all (all actual behaviour being accounted for by the lowest centres) or the non-stereotyped behaviour is the self-active "appetitive" sort to which (although he does use the term) Tinbergen's basic position is opposed.

Now, it seems to me that in order to account for the empirical facts of behaviour - e.g., for the fact that even biting, which is one of the "simple" activities at the bottom of Tinbergen's hierarchy, is modified and directed by the position and movements of the object which elicits it - we must realize that, for some kinds of behaviour at
least, one "centre" produces an infinite variety of species of the same act even before any modification by experience takes place. (It is a common assertion in neurophysiology today that there are no "centres" - like "raisins in a pudding". But while the word "centre" has been used with that kind of notion in mind, and we have to recognize that these structures usually involve interconnections of parts in different levels of the NS, each contributing some property to the behaviour, and that even then the functions of the anatomical parts are altered by chemical factors; it remains true that there are distinct, if not isolated, structures for different groups of activities - that there is not a complete "equipotentiality of function". [It is interesting to note here the evidence concerning the specificity of centres for fighting, eating, and sleeping that Tinbergen quotes (p.108) from Hess.] The objections to "a distinct centre for a given piece of behaviour" are based on the assumptions that when the person proposing them says "distinct" he always means functionally and anatomically isolated, when he says "a given piece of behaviour" he means something perfectly invariable, and in general that he means that the centre does nothing but that piece of behaviour, and does it with no cooperation. None of these assumptions is justified, and one cannot, on each occasion of using such a phrase, add those qualifications concerning interrelatedness and complexity - which are, after all, categorial and so should be unnecessary.)
It will always be difficult to decide whether there is a complex
centre for biting or whether it is subsumed under fighting, or
whether fighting is subsumed under mating, etc., but one factor in
deciding some such problems might be to ask whether biting occurs apart
from fighting, or fighting from mating. We might see biting as some sort
of subdivision of the fighting centre. In general it seems to me prefer-
able to say that biting is one of the things done by the centre which
does fighting and any number of other things, depending on the motiva-
tional circumstances and the perceptual circumstances. But the essential
point is that there must be something, some property of that centre,
in virtue of which it does each of these things (even though that
property will again be concerned in other activities too.) Though any
centre does an infinite variety of things it does not do just anything;
an organism may be "coupled to" a stimulating situation by its distance
receptors and associated mechanisms but that goal-object cannot lead
it just anywhere. The changes produced in the organism (its behaviour)
are dependent on its structure as well as on the nature of the stimulating
object and we cannot understand its behaviour just by considering its
relation to the goal.

Here we return to the point I stressed above, namely, that behaviour
is not "with respect to" anything. It would seem to fit in most easily
with that assertion that instinctive reactions, at least, should be
of a "compulsive, automatic" nature (as suggested in pp. 80-81)
- i.e., of what Tinbergen calls the relatively stereotyped kind,
which we saw means the non-coupled kind. But the difficulty is that
don first inspection instinctive acts, even on their initial appearance,
doe seem to be "with respect to" things. That is, babies do not just suck,
but suck such-and-such a kind of object, and even though two of Stone's
rats did run through the sequence of copulatory movements while mounted
at the head end of the female, nevertheless a larger number of them
copulated successfully at the first attempt, and presumably this demands
a considerable modification of their movements in accordance with the
female's movements. It is simply the mere recognition of feedback or
coupling to the object which enables us to see that even behaviour of
that kind is not "with respect to" the object in the sense to which
I objected. One reason why this recognition (of something which in a
way we have always known) has been difficult to achieve is that the
direction of action by knowledge has always been thought of as direction
by the knower rather than by the known. A further difficulty is that
of accepting the sheer "improbability" of an organism's happening to
possess so many mechanisms stimulable by the objects known and relevant
to preserving the organism's existence.

Several points can be made however to help us overcome this incredul-
ousness. For one thing, the scope and fluidity of the cognitive relation
itself makes possible a very great sensitivity to the imminence of the
instinct-evoking objects with little of the corresponding loss in
specificity of impression which is so often found in physical systems
that are made extremely sensitive to a stimulus — that is, they tend to react to stimuli other than the desired one. There have been many speculative treatments of the neurophysiological changes accompanying cognition, and attempts to show its parallel in the storing of "information" in electronic calculators (e.g., ), and while one can hardly deny that there is such a basis, it seems plain that the "cross-references" that are automatically set up when a new piece of cognitive information is acquired are much more elaborate and far-reaching than anything that has been accomplished as yet in artifacts. One might say that it is impossible to add only one new piece of cognitive information as soon as it occurs it is brought into manifold relations with an indefinitely large network of existing information both on the side of the subject and of the predicate, as it were, and additional pieces of information in the form of inferences from it and other knowledge are immediately available for use without conscious ratiocination. (It is also plain that mind is not just a logical machine, that there are rigid limitations to and hiatuses in these chains of inference, and that many of them will be invalid, but the positive contribution remains.) On the other hand (as far as one can discover) the storing of information in machines even electronically is of the card-index type, and where there are cross-each reference requires a separate act of recording. The search for information too in such machines, however rapidly it is done, is of the machine-card-sorting type. Instead of following more or less a continuous path through the branching-out of interconnected knowledge,
where whole sections of knowledge are immediately eliminated as patently not containing anything relevant, the machine scans every item it has at a particular level, collecting all those that have the first mark of the conjunction of characters it is seeking, then scans through all those looking for the second mark, and so on, as Wiener indicates (4) in Chapters V and VI, and in his final note on the unimaginative mechanical chess player. The nature of the cognitive relation, then, means that a considerable variety of the "same sort" of action, in such a wide range of situations that they seem at first to require the provision of many mechanisms, may be the work of only one.

A closely related consideration (one in which the application of factor analysis might be fruitful) is that we may overlook the common property of a group of actions because the remnants of purposivist thinking make it seem that what they had in common was a striving towards the same goal. For example, locomotion seemed to be only the goal of a variety of kinds of behaviour which have no corresponding common quality, and if there is no such thing as a goal then presumably we just have a variety of activities which as it happens produce the same kind of result, but have different mechanisms. But something must be made of locomotion, it is not a complete fabrication like striving in general; for example we have the point that when one of these activities is made impossible another immediately takes its place without the need for any intervening trial and error period. An even more striking phenomenon is seen in Lashley’s rats in which the cerebellum was damaged by operations the new techniques of locomotion were not just preestablished patterns of
movement (as e.g. with rats in the flooded maze, who used to run and now swim) but confused, opportunist writhings which, if anything could constitute evidence for purposefulness, would constitute such evidence.

To the extent that they were a "confused writhing" they must have been very similar to the movements a young infant makes, before it can crawl, when we say it is "trying to get something" just out of reach. It seems to me, then, that we might, without slipping back into purposivism, say that "locomotion" is the name of a kind of activity that organisms perform (once again, one directed by objects to which the organism becomes coupled), and that we might have to see it as one of a special class of activities which run through all the instincts without being explicitly tied together and do not, in the first instance at least, exhaust any of the tension of the instinctual structures, and do not do so unless the organism comes to "see them as" (believe that they are) parts or instances of the kind of activities which do exhaust the tension — e.g., if walking is seen as an aesthetic or thrifty activity.

It would be easy to slip into the comfortable procedure (once having recognized locomotion as a kind of activity with many different species) of treating all those acquired, so-called preparatory acts which make possible the motivation-consuming acts as being just instances of this constitutionally-provided piece of behaviour, locomotion, but at least some of these "instrumental acts" can be correctly treated in that way. Locomotion must be an integral part of some instinctive acts right from
the beginning, especially those evoked by visual or auditory perceptions; any reaction, that is, in which the stimulus-object (which on the first appearance of an instinctive act is always, observation suggests, the goal object) need not be in actual physical contact with the organism. To say that is really only to extend the principle of feedback slightly: if we recognize that the movements of a grasping hand or of biting jaws can mechanistically be directed by the object then it is no great strain on our credulity to think that as well as the supporting muscles of the head and neck, say, being brought into play together with the slight but widespread adjustments to skeletal muscles that are involved in any movement, the locomotor mechanisms should also be activated by the feedback mechanism. [Specific descriptions of neurological feedback mechanisms, quite parallel to those which for example aim an anti-aircraft gun, are offered by Wiener (62). I feel that I have not emphasised sufficiently the fact that "feedback" is more than just a logical principle; it refers also to a number of concrete discoveries of the mechanisms by which the effects of an action are in part turned back into the organism and alter its actions - of the mechanisms, in fact, which enable us to say positively what the causes are of some of those events which seem to be self-caused, rather than just declaring that there could be such deterministic causes.]

At this point one finds oneself faced with the recurring difficulty: "What about increases in skill? What about the acquiring of new acts? It is all very well to say that locomotion is innately given, and that
any instinctive act which includes motivation includes (potentially but still innately) all of the animal's kinds of locomotion, but you still have to account for learning."

Now, it seems to me that one must say quite boldly that there is no such thing as the acquiring of "new" acts in that sense, that there is only the rearranging of old ones. Such an assertion must immediately be qualified by admitting that the rearrangements are new, that they may have molar properties which their constituents do not have. But what I am contesting here is the notion of plasticity, the notion that one finds creeping in so consistently in e.g. treatments of the "cultural conditioning" of behaviour, of its "social origins," the notion that the individual is just moulded by "social pressures," as against the view that society can only exert "pressure" on him by, as Freud says, appealing to impulses that he already has. The point is that although an infinite variety of effects can be produced in the organism by the infinite variety of things that impinge on it, nevertheless there is still an absolute limitation to the kinds of effect that can be produced in any given thing, there are properties which it can never have no matter what happens to it, and this limitation by negation is imposed, by, or is a function of, the thing's own qualitative nature. And so there are limitations to the kinds of behaviour that a person can be made to perform; he can only do whatever is "provided for" in his constitution.

This assertion that "new" kinds of act are always rearrangements of old
ones is not new; it seems not to have been clearly stated very often
but it is implicit in most behaviourism, with its foundation in Pav-
lovian conditioning, which, with certain reservations, is an account of
how the same given act comes to be evoked by new stimuli.

At the present stage of our enquiry, it seems to me that the most
important, if still insufficient, thing we can say about the mechanism
of the rearrangement of pieces of behaviour is that it is brought about
by the accumulation of knowledge. Not only is it by beliefs about them
that new situations come to evoke preexisting responses, but also it
is because of beliefs about the necessary and sufficient conditions af
for those responses, and about the consequences of other actions, that
intermediary or "preparatory" actions come to be inserted into existing
instinctual responses. Because of our purposivistic habit of thought,
such a statement is inevitably unconvincing and appears question-begging.
Why should a mere belief about what other actions are necessary to make a
given action possible result in the performance of those preparatory
acts? Is it not being assumed that the animal is trying to perform the
contingent act? As I said above, one can only answer no, it is not
being so assumed, and the question "why should it be so?" is not a scient-
ifically proper one. One may ask "why should it be so?" meaning "what caused
this to become so?"; but what is being asked here is why should this
cause (these cognitions) produce this effect (the change in behaviour)?
Such causal connections can only be taken as brute facts.
Concerning the dropping out of "unreinforced" acts we should have to say again that this is a result of a change in beliefs. An "unreinforced" act is one which is produced by a stimulus-situation which is wrongly taken to as a sign of the approach of the instinct-evoking situation (the Unconditioned Stimulus). Not all acts based on mistaken beliefs drop out, and not all can in any sense be said to be unreinforced. Some illusions are protected almost indefinitely by the nature of the material dealt with; for example, to anticipate a little, if one of the activities carried out by a person's sexuality, say, is just to believe that someone loves him, then the most forceful expressions of ill-will from the person who arouses this belief may fail to change it, and in fact be taken as evidence of its truth - i.e., "she's only trying to cover up," or on a more sophisticated level, "this animosity must be just one side of her ambivalent feelings; it means that she also has a strong attachment to me." (I take it that this believing that somebody loves one is the only clear meaning we can give to the frequently-made, and most important, psychological observation that we "need affection." Affection is not a substance like food, for example, which can be transferred from one person to another and actually taken physically into the body to produce its effects there: it is one of those things which can only produce their effects in us by way of knowledge, by way of our believing in them. That is no reason, however, for reducing the "need for affection" to a need for material substances, food, and so on, as is sometimes done. It is not that we must believe in someone el-
I,

need affection p. 140. Belief of own loved.

affection because then we shall have to believe that our supply of food is assured, we know that people can have an assured and plentiful supply of the necessities of life and believe it to be assured and still require to believe they are loved. In my term, that belief numerically some body of motivation in a way maintain or to an extent not possible for any other of its acts.

But when the instinctual activity in question is e.g., copulation or eating, then while one may go through some of the actions in the absence of a partner or of food, plainly there will be difference were in the massages themselves (e.g., because of the absence of resistance to the actions of one's love) and in any case, having pointed out the facts of being coupled to objects by impulse, we can now say that the instinctive acts in question are to copulate with another organism or to eat food, without being assured of that action or special ways of operating that I criticized before. Since what a person believes is not determined solely by his motivational state but also to the nature of his surroundings, it will be increasingly difficult to maintain the belief that these instinctual acts are being carried out rather than some pale substitute for them, the absence of the object (the VE) will be recognized and it will be seen further that the alleged sign of it (the "VE") is not a true sign, and so it will no longer function as it will no longer be a part of the VE.

Still we feel we should like to know more about this matter, not so much how many more facts are in our understanding of the changes of
affection because then we shall be able to believe that our supplies of food are assured; we know that people can have an assured and plentiful supply of the necessities of life and believe it to be assured and still require to believe they are loved. In my terms, that belief exhausts some body of motivation in a way maxim or to an extent not possible for any other of its acts.)

But when the instinctual activity in question is e.g. copulation or eating, then while one may go through some of the motions in the absence of a partner or of food, plainly there will be differences even in the movements themselves (e.g. because of the absence of resistance to the motions of one's jaws) and in any case, having pointed out the facts of being coupled to objects by knowledge, we can now say that the instinctive acts in question are to copulate with another organism or to eat food, without being accused of that notion of special ways of operating that I criticised before. Since what a person believes is not determined solely by his motivational state but also by the nature of his surroundings, it will be increasingly difficult to maintain the belief that these instinctual acts are being carried out rather than some pale substitute for them; the absence of the object (the US) will be recognized and it will be seen further that the alleged sign of it (the "CS") is not a true sign, and so it will no longer function as (will no longer be a part of) the US.

Still we feel we should like to know more about the matter, but no matter how many gaps there are in our understanding of the changes of
behaviour, I feel convinced that basically the correct way of thinking of such development is by thinking of the original form of the behaviour as being always an automatic instinctual response to some perceived situation. As the organism grows in experience this is elaborated and extended on both sides: on the stimulus side in the way we have described, so that further situations believed to be related in a specific way to that original one thereby come to evoke the instinctive response; and on the response side by the formation of beliefs concerning "how to do things" - i.e., by beliefs concerning what actions are necessary in this kind of situation to make it possible eventually to perform the instinctive response. It is not as if the organism in any positive sense "orders the means to the end"; one would better describe what happens (though still in an animistic way) by saying that the response develops itself, adds to its phases (and we might remember that any response, even in its earliest form, already has or is a succession of various phases, the earlier being necessary for the later.) Thus we would in a sense read Tinbergen's hierarchy of responses in the opposite direction; instead of the centre for such an act say as copulation being the last and lowest in a chain it would rather be the highest or the central one, and the "preparatory" actions, instead of being carried out by higher centres which finally send on their stimulating impulses to it, would be carried out by that copulatory centre. One would prefer to say that those "preparatory" acts are not only not a preparation but are not even external to the copulatory act, they are
parts or early phases of it. Thus, to stress the point, we might say, with rather an Old Testament flavour, that the man who challenges a woman to play word-games is already copulating with her ("in his heart", as it were), or to take Tinbergen's example, that when the stickleback is only swimming towards an intruding red male, he is already fighting it. We might give some actuality to the suggestion of "acts in potency" here by saying that the word-games and the swimming-towards are the work of the copulatory and the fighting centres respectively (each recruiting other neurological structures as well), but there is no necessity for the physiological reference: we might assert that some word-games are copulations and some are not, and find differentiae for example in the tone and facial expression with which the cues in the word-game are delivered, in the choice of words, in the unwelcoming way other players are received into the game, but, embracingly, in the emotional tone of the man's activity. It is not that we need to have watched word-games with these other incidental characteristics develop into copulation, and other word-games without them run their course without such a denouement. We just recognize in the game the same feeling-tone that we have found (even though perhaps in a greater degree and perhaps differing in some other ways) in copulation, or in violent sexual activity generally.

The use of such criteria as this of feeling-tone would have to be permitted (as at present it would not be by most self-styled "objective" psychologists) before the general hypothesis given above concerning the nature of an instinct could receive experimental verification. One
consequence of this hypothesis would be that if there are two different activities characteristic of the one instinct, each requiring a fairly high level of motivation for its occurrence, and each when it occurs consuming a large proportion of the existing motivation, then when one has occurred the other will not be able to be elicited before sufficient time has elapsed for the motivation to be able to build up again. (Difficulties arise immediately concerning how long it does take for the motivation to be built up, because at present we can only tell this by seeing whether the actions occur. But at least with sex and hunger we know that when a certain amount of the relevant food has been eaten, or ejaculation has occurred; at least the motivation remains quite low for some minutes.) But what complicates this apparently straightforward relationship is that some (and possibly all) activities come, through experience, to be able (or perhaps are able from the very beginning, as I should say was the case with sucking) to be produced by, and to consume, a number of different bodies of motivation - that is, they are carried out by different instincts, in various combinations. If that is so then in the projected experiment, having putatively exhausted a given instinct, we should still be able to elicit that activity which was said to be characteristic of that instinct (but is now characteristic of others too.) This would be taken to falsify the original hypothesis but, if our qualifications of it are correct, would not do so. But the objection is very likely to be raised that the hypothesis has now become such that it cannot be
falsified. Now, in my opinion, if this objection is true, it is true only because of technical difficulties, and not because of the nature of the hypothesis (that is, the hypothesis is not a concealed tautology.)

It is not true that any instinct can be brought to carry out just any of the activities of the other instincts without exception, and so for any given activity we might be able to arrive at a hypothesis that only a certain group of instincts could carry it out, and if in various ways we exhausted all of these we could test that hypothesis by seeing whether the action in question could still be elicited. But such a procedure presents great technical difficulties, and one which does not require it (and is still not question-begging, though it would be held by many experimenters to be so) is just to rely on one's observations of what I have been roughly calling "feeling-tone" to see whether the experimental conditions had been fulfilled. Thus, to take copulation as an example again, though it may not be especially suitable for experimentation, casual observation suggests (and receives confirmation, for whatever it is worth, from novelists of stature, who must after all get their ideas from somewhere, and to mention one psychologist, from W. Reich) that copulation can be carried out up to and including ejaculation without dispelling to any great degree the sexual motivation. (Reich does not simply equate ejaculation with "orgastic potency"; he insists that certain emotional experiences are essential for the latter.) Now, I suggest that the difference between this and "sexually-satisfying" copulation (especially the difference in
feeling-tone) must not only be readily observable by the person himself but should be discoverable by an observer (and one might make the same point about observing whether eating is done from hunger or from something else — say, from frustrated sexuality), and that we could use these observations in our suggested experiment on substitute activity.

But, one might ask, if it is possible to observe whether this action is sexual and when it is not, and when that action is sexual and when it is not, why bother to carry out the experiment to see whether they are both sexual, or both consume the same body of energy? The point is that, if we observe the appearance of one in its sexual form, and then immediately elicit the other in its sexual form, then some one of our assumptions is false. Perhaps we were mistaken in believing that the first exhausts much of the sexual tension, and we might test this by attempts to elicit various other sexual acts; or we might have been mistaken in believing that the second required a high level of motivation for its occurrence, in which case we might set out to try it after various other and various combinations of allegedly sexual acts. If it seemed we had been right on both these points then we might have to conclude that there was more than one kind of sexuality, or that the character in one or other of the activities that we had identified as sexuality was really something quite else. But this profusion of possibilities is common to all fields when we have insufficient well-attested facts to take as premises along with our hypothesis; a falsification casts doubt on many
of our premises. Nor does the difficulty of recognizing the quality, sexuality, constitute a methodological objection, but only a technical one. (We might find that any given theorist was using the word "sexuality" in a way that confused qualitative and relational references, when it would face methodological difficulties as we have seen, but even that does not mean that he has not vaguely seen a true qualitative difference, so that the term might be purified of its relativism. (I should also say here that, although I have been speaking as if we find the property "sexuality" in the behaviour, it is more correct to say that we observe sexuality at work in the man. If sexuality is a motive in the sense I have been proposing, then strictly there is not "sexual behaviour" but only behaviour done by sexuality.)

We could then get a falsification; we could also get a verification, even if the second activity were elicited, provided it was in its non-sexual form. This presupposes the honesty of our observations — that is, our observation of sexuality or non-sexuality — but in varying degrees that is true of all experiments. Also it is quite pointless to say that something other than the exhaustion of a common body of motivation may have prevented the second activity appearing, since that is only to say that verification in general does not establish the truth of the hypothesis.

To return to the point about instinctive responses being elaborated simply by the building up mix of beliefs about cause and effect, it further tends to take away the appearance of implicit purposivism
in this if we stress that such elaboration only occurs if the relevant physiological mechanisms (concerning which, however, practically nothing is known) are present - it is not as if changes in behaviour follow on any change in beliefs, or any perception of relevant causal sequences. Tinbergen (25) indicates many "blind spots", many places in which given species cannot learn to modify their behaviour in the "appropriate" way, even though, as he puts it, their sense organs are undoubtedly capable of differentiating the relevant features of the situation.

What complicates the issue here for us is that cognition is not solely dependent on the adequacy of sense organs, and we may consider that an animal has cognized a certain set of facts when in fact it has not. That makes it difficult to evaluate the following piece of information, which is not reported fully enough, but which opens up many possibilities concerning these relevant mechanisms. Tinbergen says (p.150): "There is not only a localization of learning in relation to the antememuraa reaction concerned, but there is also a certain localization in time, giving rise to the phenomenon of critical periods.

"The Eskimo dogs of east Greenland live in packs of 5-10 individuals. The members of a pack defend their group territory against all other dogs. All dogs of an Eskimo settlement have an exact and detailed knowledge of the topography of the territories of other packs; they know where attacks from other packs must be feared. Immature dogs do not defend the territory. Moreover, they often roam through the whole settlement, very often trespassing into other territories, where they
are promptly chased. In spite of these frequent attacks, during which they may be severely treated, they do not learn the territories' topography, and for the observer their stupidity in this respect is amazing. While the young dogs are growing sexually mature, however, they begin to learn the other territories and within a week their trespassing adventures are over. In two male dogs the first copulation, the first defence of territory, and the first avoidance of strange territory all occurred within one week...

If we suppose that the young dogs did actually know the areas in which they would be subject to attack (assuming that when Tinbergen says they "do not learn" he means they do not not as if they know) then we might suggest that whatever instinct it is that in other circumstances makes them avoid situations in which they get physically hurt (and at least domestic dogs are able to learn such avoidance before sexual maturity) just does not incorporate avoidance of areas guarded in this particular fashion. (This does not follow from the information, since there might be a "tendency to approach" avoid" on the part of that instinct but a "tendency to approach" on the part of some conflicting instinct, but at least the information verifies our supposition.) But their sexual instinct, when it develops to a certain stage, does involve mechanisms such that that knowledge now produces a modification in behaviour.

Finally, in order to minimize the natural incredulity concerning the "tremendous good fortune" of an animal possessing so many life-preserving
mechanisms, we might go on from the assertion that for any organism the possible modifications of its behaviour are limited, and point out that there is a limitation too to the kinds of environment in which an organism can survive. Those members of a given species which find their way into an environment in which they cannot survive will not arouse amazement at their biological good fortune, but they of course are in a decided minority amongst those members of their species which we are likely to encounter. We might take any object at all and wonder at the incredibly intricate network of contingencies which determine that, at any given moment of its varied career, it possesses precisely those qualities requisite for the impinging events to be able to produce those effects in it which they actually do produce.
VII - Freud's "Libido" as an Instinct.

I suggested above that Freud's account of the libido was parallel to the account I gave of a motivational structure or an instinct, and that it was one of the very few attempts to identify an instinct by reference to its mental properties as well as to its physiological ones — by its mental qualities, that is, irrespective of the various relations they enter into. Now, that interpretation of Freud's doctrine would not be a commonly accepted one, since it is usually taken that "libido" simply means a set of sexual desires, or at least a body of energy of an essentially directed sort, defined by its tendency to carry out those kinds of actions usually designated as sexual together with maxim certain others which Freud asserted to have the same sexual property.

Certainly this more common view can find ample and accurate support right throughout Freud's writings, but in my opinion, though statements which explicitly set out the sort of doctrine which I attribute to him are in the minority, it is his basic and characteristic doctrine, at least until about 1920 — that is, until his increasing emphasis on the ego, and his formulations concerning the pleasure-principle, and then concerning Eros and Thanatos, and so on.

Nor can one accuse Freud of carrying out a self-conscious and systematic rejection of purposivism, but it seems to me that his recognition of unconscious impulses, in conjunction with his recognition of a plurality of mental institutions, bore strongly against any treatment of the behav-
-iour of the person as a whole being purposive. He very frequently speaks of the activity of each of the mental constituents as being purposive or goal-seeking, but the conflict between them is treated in such a way that the behaviour of the person as a whole is simply the resultant of these opposing forces.

The passages which most clearly support the qualitative interpretation of the libido occur in the "Three Contributions to the Theory of Sex" (p. 37). Thus in the first paper, "The Sexual Aberrations," he says that perversions "can be referred to a number of 'partial impulses', which are not, however, primary but are subject to further analysis. By an 'impulse' we can understand in the first place nothing but the psychic representative of a continually flowing internal somatic source of excitement, in contradistinction to the 'stimulus' which is produced by isolated excitations coming from without... The simplest and most obvious assumption concerning the nature of the impulses would be that in themselves they possess no quality but are only taken into account as a measure of the demand for effort in the psychic life. What distinguishes the impulses from one another and furnishes them with specific attributes is their relation to their somatic sources and to their aims. The source of the impulse is an exciting process in an organ, and the immediate aim of the impulse lies in the elimination of this organic stimulus" (p. 31).

Now, the reference to "the psychic representative of a continually flowing somatic source of excitement" seems to me to bear out the qualitative view of libido, but there is a difficulty when he goes on to
deny the qualitative difference between the impulses. What he means,
I contend, is that the partial (e.g., the oral and anal) impulses are
just different kinds of activity carried out by the one body of libido
as a result of different kinds of erogenous stimulation, so that he is
not denying quality to the libido, but only denying that each particular
kind of action or "aim" has its special qualitatively distinct body of
libido - which is just the sort of thing I have been arguing. But
there is a difficulty of interpretation here in that in a later edition
(27) Brill gives the translation "instinct" instead of "impulse" each
time it appears in the above passage except the first, in the phrase
"partial impulse." Thus Freud now seems to be denying any qualitative
basis to instincts, to be giving them a purely relational treatment:
that is, as tendencies to certain kinds of action. This interpretation
is favoured by F. Alexander, in accordance with his vector view, so that
all actions find a common denominator in the discharge of tension - keep-
ing tension-in-general at its lowest possible level. That convertibility
is the sort of view suggested by Freud's doctrine of the pleasure-principle,
but why then does he criticise Jung as undoing all the important
contributions of psychoanalysis by denying the qualitative distinction
between libido and the other mental forces?

This insistence on the qualitative difference appears in the third paper,
"The Transformations of Puberty," when he says (p. 77), "This libido we
distinguished from the energy which is to be generally adjudged to the
psychic processes with reference to its special origin and thus we
attribute to it also a qualitative character. In separating libidinous from other psychic energy we give expression to the assumption that the sexual processes of the organisms are differentiated from the nutritional processes by a special chemistry. We thus formulate for ourselves the concept of a libido-quantum whose psychic representative we designate as the ego-libido; the production, increase, distribution and displacement of this ego-libido will offer the possible explanation for the observed psycho-sexual phenomena." We find then a specific qualitative physiological basis for libido, a psychic representative of it (which I propose to take just as libido itself in so far as it is mental, or in so far as it is a certain kind of feeling), and the assertions that it varies quantitatively and carries out the varying sexual activities.

One might point out here that it does not seem correct to say, as J. Anderson is often interpreted as saying, that mind is composed of feelings and emotions. Emotions seem to me to be the kind of quality that colours are. Without drawing a distinction in terms of "levels", or in terms of functioning as subject or predicate, one can distinguish between qualitative things which are compound, i.e., have structure, and others which are not structured but are always found inhering in or being qualities of structures. Emotions are one of the latter kind, and so cannot really be constituents or parts of things. We might then rather say that mentality itself is of this kind— that it is a property of certain neural events, rather than a structured thing.
Concerning the physiological aspects of libido, Freud's view at that
time (1904) was just as sophisticated, and very little less-informed,
than present-day theories of the physiological bases of sexuality. He
recognized that it did not depend solely on the products of the testes
or ovaries, since castrated men and ovariectomised women still exhibited
sexual behaviour, though altered in kind. He indicated that there must
be some pervading chemical factor (Morgan's "HMF") which acted on the
organs of reproduction either directly (by bathing the tissues) or
indirectly by supplying a stimulus "to the spinal centre connected with
them." This spinal centre is activated in any case, and its state is
then perceived by the higher centres [Morgan's "CMS"] "which then produce
in consciousness the familiar feeling of tension." The arousal of this
central nervous mechanism is also contributed to by the stimulation of the
erogenous zones and by the perception of certain types of external sit-
uation.

The anti-purposivist strain in Freud's thinking appears clearly when
he says (p.33) that when the libido is unable to obtain normal sexual
"gratification" it "behaves like a stream the principal bed of which is
dammed; it fills the collateral beds which until now perhaps have been
empty." It must be plain that the schematic account of motivational
"reservoirs" which I gave above owes a good deal directly to this image
of Freud's. The point is that the libido is not pictured as actively
seeking another channel, but as simply banking up until it flows into
one.
His early view of the origin of anxiety (that is, that accumulated libido is transformed into anxiety when its more usual channels of discharge have been blocked) also has this mechanistic strain, and in my opinion is methodologically much sounder than the later view which has been approved by so many of his successors, that anxiety is felt by the ego at the approach of some "dangerous" situation, particularly such as might arise from the bursting out of a great accumulation of instinctual tension, and that it functions as or is consciously used as a signal to arouse competing instincts, to rally them into defensive formation, and the suggestion seems also that it "intimidates" the suppressed motive.

This latter notion of anxiety, and the popular meaning that has so much in common with it, are riddled with purposivism. It is always "I", the self, the executive, which has anxiety, and it is not so much that the belief that a given event will occur produces fear in me, which has further effects, and so on, but rather that "If I can I will do something to avert the threatened situation." This latter consequence has the "near self-evident" status of so many purposive accounts of behaviour; that is, the sort of thing that causes one to worry is to be defined only as anything that one does not want to happen, one tries and so "naturally" to prevent it happening. This to-be-avoided nature is commonly transferred to anxiety itself; of its very nature it is something which we must get rid of.

Now, one must admit immediately that it is next to impossible to
convict the later psychoanalysts of such a simple-minded purposivism by much detailed examination of the texts. It is always possible to maintain that they are just treating anxiety as a state which affects the responses of the organism in a deterministic way; that it is not that "we" try to get away from anxiety but simply that the banked-up body of motivation has reached such a height that its barriers are on the point of bursting, and the new qualitative thing "anxiety" stands either as a stimulus evoking strong competitive responses from the other motives, or as an effect produced in them by the action of the banked-up tension and so affecting their responses to other stimuli.

But this would be basically just the same as Freud's early view, and we could not understand the insistence on the fundamental theoretical difference between the two positions. Certainly Freud restricted his formulation to the transformation of libido, but then almost all his views on motivation were worked out in connection with libido, even though he did not deny (as he has been so frequently accused of denying) the existence of other coordinate instincts - as we see if only from the assertion quoted above (p. 154) that the sexual processes are differentiated from the nutritional processes by a special chemism. There is no a priori reason why he could not also assert that any body of motivation would "become transformed into" or acquire the property of anxiety when it reached a certain pitch (though one cannot of course claim that that was his implicit view.) The mere contention that anxiety is not restricted to libido is no objection to his basic notion of how
it is produced, i.e., to his "toxicological" notion.

If anxiety is qualitative rather than relational then it too will be of the kind which I referred to above, the non-structured quality which inheres in structures. There is then no problem over the notion of "conversion" of libido into anxiety (though there would be over the notion of "desexualised libido", and transformations of that kind.) Anything at all can and does have many properties and many changes of property without losing its identity. This is a point overlooked by McDougall, e.g., in contending that instincts have only one emotion, (and that one peculiar to its instinct). In order to account for, e.g., the variety of feelings aroused by a loved object he was forced to speak of a number of instincts becoming organized about the one object in a "sentiment", but it is not as if (in the sort of phenomenon he was describing) both love and fear (say) are aroused by the same object in any strict sense. They are aroused by the same-object-in-different-situations - i.e., fear when the loved object is endangered. But all these feelings would be had by the one instinct ("libido" in Freud's terms) - they are all a part of having a love-object.

The question arises, of what can anxiety be a property? Of the man as a whole, or rather of every part of his mental structure? Or only of one or some few parts of it at a time? Melanie Klein and Karen Horney, despite large differences between their views, both treat it as being a property of the person as a whole (and the view that seems to go with that, namely, that not only is it in itself something-to-be-
avoided but perhaps the only thing to-be-avoided — i.e., that all activity is ultimately an attempt to be avoid anxiety.) Thus Klein reiterates (e.g., 24) that the young child is made extremely anxious by the recognition of its own stored-up aggression (the "Death Instinct"), and Horney (25, p. 57) makes a similar point about libido — anxiety, or one form of it at least, is "the individual's feelings of fear and helplessness towards such a pent-up libido tension." Using the same language for a moment we might say that his libido is in fact a part of the person (even though he may sometimes deny it), and it certainly does not feel anxious about what will happen if it bursts out. Like all the other bodies of tension its action is "utterly self-interested" — that is, it is simply pushing outwards all the time, as the water in a tank is pushing outwards. If it were "anxious about" anything it would be anxious about the possibility of its not being able to burst out — which translated into deterministic terms means that when it reaches a certain height it acquires the feeling-property of anxiety.

It seems necessary here to introduce a modification of that principle; namely that it is not so much the absolute height or quantity of motivation which determines the occurrence of anxiety, but rather the ratio between on the one hand its present quantity and rate of production and on the other the objective possibilities of discharge which that motivational structure believes to exist. This modification seems to be called for by the fact that the perception of some approaching change in the situation which would make permanently impossible (or even just
very difficult) the outflowing in action of any given body of motivation produces an immediate attack of anxiety (provided it is believed in as a real possibility) even though that motivation may be at quite a low level (has recently "been gratified"). The ease of conviction of danger, and the intensity of the anxiety, may be determined in some degree by the current level of motivation; but usually a person would be thought very foolish who, because he had just had a good meal, faced complacently the prospect of being permanently cut off from food.

Now, one part of the mind may undergo anxiety as a result of perceiving that some other part of the mind (i.e., some other instinct) is about to burst out in a certain kind of activity, or even if it believed that another instinct would not be able to carry out a certain kind of activity. But the appearance of purposefulness in the first case and of altruism in the second disappears when we say that because of past observations of cause and effect these happenings in the other instinct have been assimilated to those situations which, when perceived, result in a sudden increase in the relative intensity in the accumulated motivation in the first instinct; i.e., situations which make the tension-consuming instinctual actions impossible. To perceive such situations is in the experienced organism just a part of doing the instinctual actions.

This principle of the relativity of the abundance of stored-up motivation might help us to see through the difficulty Freud finds concerning the fore-pleasure — that is, that sexual fore-play results in a rapid
increase of sexual motivation and yet this is experienced as "pleasurable". Getting rid first of all of the notion that pleasure is the only object of pursuit (by asserting that there are no "objects of pursuit") one might suggest, in brief, that pleasure is a feeling or emotion resulting as a brute matter of fact from the subsidence or possibly just from the outflowing of any body of motivational tension. (The point that it arises from any body of tension is the empirical content of the doctrine that all "goals" are reducible to pleasure.) Now, the fore-play may in fact consume some of the sexual tension but the production of tension resulting all the time from it seems by inspection to be much more rapid (otherwise copulation could barely be attained), so that in this case at least we would have to say that pleasure resulted from the outflowing, since there was no subsidence. But the reason why the rapidly increasing body of libido does not (for the most part) reach the level of anxiety is that the very factor which gives rise to the increase, the fore-play, also gives rise to the belief that the eventual performance of instinctual act is assured. This hypothesis is at least verified by the fact that a chance interruption of the behaviour gives rise immediately to intense annoyance and aggression, and if the interruption arises from some factor which appears to be a permanent barrier to sexual activity — from some threatened punishment for this activity in general — then the result will be anxiety.

To admit that this introduction of the notion of the relativity of the tension of an instinct is ad hoc is not to admit that it is
meaningless or methodologically suspect. Certainly it might be difficult to conceive what physiological processes underlie it, and one might be forced to say simply that the perception of the restriction simply does so alter the neurological pattern that it now carries the property of anxiety. This once again seems to imply that the instinct is striving to reduce tension, but that inference is unjustified. In point of fact, as clinical psychology has demonstrated, one can do a great deal of deterministic psychology while still talking of the particular mental impulses as wishes or strivings, provided that the interaction of the strivings, and its resultant, is treated as mechanistic. This can however sometimes lead to errors, e.g., in Freud's account of transference, (25), in which at one point he suggests that the resistance is carried in part by the repressed impulses, in order to remain under cover, as it were, and carry on with their hidden phantasmic delights, but it seems much more plausible and less fundamentally purposivistic to say with e.g. Alexander (7) that the resistance proceeds solely from the repressing forces, which have been momentarily relaxed by the analytic technique, letting some of the repressed impulses emerge into action, and then becoming subject to anxiety because of a too great breaking through, or because of a revivification of the beliefs in severe punishment which the repressing force has not yet given up, with the consequence that the repression is reinstated.

Another place where the purposivistic treatment of impulses or instincts leads to a confusion is in the understanding of the dream-work — whether
the manifest content is a disguise adopted by the repressed forces to deceive the ego, or whether it is simply a distortion forced on the repressed impulses by the ego. These alternatives have intricate implications for, for example, the state of education of the repressed instincts, their ability to cognize the world, the knowledge possessed of their activities by the repressing instincts, and so on.

Reverting for the moment to the view found in Klein and Horney that it is the self that feels anxiety about instinctual urges, and the objection that the anxiety-provoking instinct is a part of the self, we might expect them to say that by "self" they really mean the "organized self", or "ego", as distinct from the unorganized instincts. But if that is so, if the ego is just another part of the mind alongside the id, then it would be difficult to see what it could be composed of except impulses or instincts of the same order as those in the id, (and to say that these have anxiety because they are opposed by other instincts seems, as I have been arguing, to come back to a view of anxiety very much like that early one of Freud's which they reject.) We said above that where anxiety is provoked by the activity or the threatened activity of an instinct it is provoked in another instinct, and summing up this discussion of anxiety, we might say that, like pleasure, it is a feeling that can be and is from time to time a property of any instinct, that it is a result of the possibility of consumption of that particular instinct's tension falling below its rate of production, and that it indicates that that tension is pressing with extraordinary strength against the barriers to its activities — that is, in a long-range way.
but very powerfully, it is **doing** these instinctive activities.

Horney herself points out, in connection with Freud's assertion in a very late paper (2) that neurotic conflicts are between the ego and the id, that "if that be so, it is difficult to see what concretely this ego consists of." Her solution, however, in line with her general "cultural orientation," is not to show that the ego is itself a group of instincts, but more or less to deny that there are any instincts at all. I propose to take up these views of hers fairly briefly, because they echo common objections to Freud, and in discussing them we should be able to bring out certain difficulties in orthodox motivation theories that are avoided by, or shown to be pseudo-problems by, the view I have been putting forward.

If we take libido in the way I have elsewhere been arguing for, as a variable body of tension in a structure determining different responses in different stimulus situations, then we will be able to understand something which Horney finds a difficulty in Freud, and which she suggests is not really comprehensible, namely the "extended meaning" of sexuality to include more than just erotic or just genital activities. If sexuality were simply kinds of action then that extension would be difficult to justify, and its limits would seem quite arbitrary, but on our view we see that by "sexual activities" Freud meant simply all the activities which are carried out by sexuality — by the libido. Specifically, it enables us to see a basis for the alleged continuity between oral, anal, and genital activities, and their alleged communication with each other.
Secondly, it enables us to see through a converse difficulty and a rather more important one, typified by Horney in saying (neglecting now the "extended meaning") that Freud's concept of the ego "means that theoretically there is no liking or disliking people, no sympathy, no generosity, no feeling of justice, no devotion to a cause, which is not in the last analysis determined by libidinal or destructive drives." (4, p. 187). She equates this with "the denial that mental faculties may exist in their own right".

In part this is like the quite wide-spread resentment against what Freud sometimes seems to be saying - namely, that plastic art is nothing but a (disguised) playing with faeces, that one cannot really accumulate or find difficulty in parting with, or give as a present, anything but faeces, that all enquiry is scopophilia, all delight in the spoken word oral eroticism, and so on. Now, of course there are such activities as painting, enquiring, making poetry, and so on; they are not mere comfortable illusions, and one cannot believe that Freud seriously denied this. On the other hand, many people who believe they are practising these arts are deceiving themselves; it might be more than a metaphor to say that they "really are" playing with faeces; at least we might say that their painting stands to some of their repressed activities as the manifest content of a dream stands to its latent content. Some repressed part of these people believes itself to be playing with faeces. But other artists are not self-deceived in that way, and I suggest that Freud recognizes that distinction in differentiating
between substitution and sublimation; specifically in saying that the libido invested in sublimatory activities is "desexualised", even though that notion may itself be ultimately incomprehensible.

Possibly Freud would contend that even genuine artists are driven to that activity because (in part at least) of their playing with faeces having been repressed in childhood (or perhaps indirectly reinforced as a regression from prohibited genital activities), but it seems not too great a refurbishing of his views to say, as a number of authors have said, that that infantile pastime is not simply coprophilic and in fact not originally a love of filth at all, since infants simply do not have the notion of "filth"; it may itself be an artistic activity.

Again, although Freud contends (22) that the child's first real efforts at enquiry are usually into sexual matters, and often include attempts to look on at other people's excretory activities, it is quite plain that he does not equate it with voyeurism, with the obtaining of sexual pleasure in such looking; in fact where voyeurism occurs it is a subsequent development and is more likely to arise, in his view, as an indirect consequence of the virtual giving up of enquiry when these first intense curiosities have been baffled by the (presumptive) ignorance of the vagina and by the parents' obscurantism.

In general (without suggesting that this view is to be found in any detail in Freud's works, though I believe it to be the general trend throughout, and certainly without wanting to defend the Life- and Death-Instinct theory which Horney regards as the essence of Freud's
instinctivism) one might say concerning this "reduction" of all behaviour to instinctual acts, which Horney finds so disconcerting, that any activity, no matter how sophisticated, of the organism is one or more of the constitutionally-provided activities of one or more of its instinctual structures. But to say that any a particular act is one of these instinctual activities does not mean that it is nothing but that, that every detail of it is unlearned; it is just to say that it is a species of, i.e., has the property of being, either eating or masturbation or copulation (masculine or feminine) or believing or loving or fearing, and so on and so on, even though many of its other properties (its "form"), and its relation to objects, are determined by features of the environment ("social pressures") as well as features of the person.

We might notice, however, that Horney does not here actually name concrete, qualitative activities, such as painting or enquiry, which are not to be reduced to something else, but uses terms which carry an atmosphere of approval of the activities concerned - "generosity," "devotion" - as if they were admirable or praiseworthy or "good" in themselves. In many other places she accepts as explanatory, and not requiring further psychological analysis, such motives for action as "she despised her husband," or not doing something "because it is cheap," "because it is undignified." Again, she regards as a special contribution of her own (and this is conceded by Clara Thompson) the
recognition that the most "valuable," "spontaneous" impulses of a person can be subject to repression as well as his reprehensible ones. One feels inclined to declare that in Freud's view only valuable impulses are repressed, until one remembers that he is amongst those people who have done most to free us from moralism - from thinking that any actions or feelings are praiseworthy or reprehensible in themselves - by showing us the underlying motives for which these conventional notions are cloaks. It is precisely by taking such notions uncritically that Horney shows how little she understands the most characteristic doctrines of psychoanalysis, how little she has grasped the discoveries on which in the main those doctrines are based. One can say of her as Freud said of Adler (1), that she does not understand even the notion of unconscious mental impulses, and this would be an error into which any theory of motivation based on "interpersonal relations" is likely to fall. For such theories seem to deal always with relations into which the person-as-a-whole enters and the actions which he as a whole carries out "with respect to" other people, whereas if we believe in unconscious - that is, functionally unconscious or repressed impulses, we have to recognize not only that there is a plurality of motives but that they can have incompatible impulses. To be content, as a psychologist, to speak of the actions of the person as a whole is to overlook these conflicts. Once again, the problem of accounting for what "he" does can only be solved by asking what his particular instincts are doing, since "he", the psychological self, is made up of them.
"Interpersonal relations" theories account for a person's actions by reference to his efforts at "personal aggrandizement." His relationships of "dependency," "competitiveness," "defiance," and so on, without ever asking what the person thinks of as aggrandizement - that is, aggrandizement in what connection? dependency for what? competition for what? defiance over what? These relations are never perfectly general, and while those questions are still formulated in the "goal-seeking" way, still the answers to them that would satisfy a good clinician, for example, are beginning to suggest a set of qualitative forces in the person, and a set moreover common in varying mixtures to people in general.

Thus the clinician keeps asking "what is the patient really trying to get by doing that?" - what is it that Mrs. A (to take Dollard and Miller's sample case history) really wants when she asks me to advise her whether or not to go to visit some people she has been obsessively afraid of visiting? He is not satisfied (as her clinician was not satisfied) even with the assertion that she is trying to avoid the anxiety and helplessness consequent on being asked to make up her own mind. He saw further that there was a special and apparently most important field within which this anxiety occurred, namely, in the field of sexual activities, and that she could not bear the anxiety arising from recognizing that her frequent seductions were very largely of her own initiation. But why does that make her anxious? Because of the early harsh prohibitions and threats of her foster-mother. But even then, what he should have gone
on to ask, in my opinion, though this is just not reported in Dollard
and Miller, is "Threats of what?" What specific punishments or depriva-
tions were threatened the girl? What was other motive in her was
turned against her libido?

That is what I take to be the sound core of the current emphasis
on the analysis of the ego: that is, when a qualitative view is being
taken of the components of the ego, and it is not being thought of
as "the person's" conscious beliefs, likes and dislikes, morals, and
so on - in a word, as the modern equivalent of Reason. Freud is suggest-
ing such a qualitative approach when he says (5) that "... the ego is
identical with the id, and is merely a specially differentiated part
of it"; and this approach is repeated in a number of places throughout
his writings. The notion is that certain parts of the id - that is,
certain instincts - become better able than others to cognize "the
world" and in fact "control the avenues to cognition and motility". What
determines which of the instincts gain this advantage? Freud in places
(e.g., in the New Introductory Lectures) speaks as if it were just a
topographical question - as if those parts of the brain-mind which
are at the surface exposed to the world simply become modified by this
contact, as it were like the membranes of a cell. But I suggest that
some instincts, thrown into opposition to others because of contingencies
laid down by the social environment, receive favourable treatment from
society - that is, instead of simply being offered a choice, the
person is encouraged to take one side of it by the offer of greater
"rewards" (to make a first approach to the problem) — and so in some way become able to "repress" the less favoured ones, a process which includes, as Freud points out, not only barring the repressed instincts' paths to motility, to action, and "refusing to recognize" (a difficult notion which seems to involve an infinite regress) their existence, but also interfering in some way with their ability to cognize the outside world — in particular, with their ability to discover "what leads to what" with regard to the instinct-provoking objects, and so to modify their responses in the "instrumental" and "goal-seeking" way. This tends to preserve and strengthen the original advantage, and so we find that the ego is a persistent, stable coterie of instincts (their identity largely depending on the particular society's moral code), rather than simply any instinct which happens at a given moment to have gained the upper hand. The ego, then, is very much in the position of the governing party of a society, which, because of its greater strength (though this would arise rather from its ability to command troops, for example, rather than, as in the case of the ego, its own strength), and its greater command of communications, information, and publicity, is able to set itself up as representing as representing at least the "essential part" of the existing society, as "speaking for" the society, but in fact is only a part of the society, and not in absolute control. Thus, when subversive elements do things opposed to the government's policy, say in international affairs, while the government disowns these actions and asserts that they are not its, not characteristic of the society as a whole, it knows that other societies will regard the subversive element
as part of its society (and there is a good deal to justify each of these attitudes), and that those other societies will almost always insist on dealing with it as a whole. So the ego-instincts disown the actions of the repressed impulses but have to recognize that at least they occur within the same individual, and that they (the ego-instincts) will be held responsible. (According to the method of dealing with this common responsibility, especially according to how much reliance is placed on simply disowning, we will get varying degrees of "disruption" and disassociation.)

Coming back to the point about certain instincts gaining the initial advantage by being offered "greater rewards", this would not really meet the case since (on a deterministic view) a motivational structure cannot be induced to exert greater pressure by giving it more outlets. The strength of motivation is increased rather by making discharge more difficult - in this case by threatening greater punishments. Here we can make an approach at least to the question of the nature of the "super-ego." It is not to be accepted in the Freudian sense as the a substantial structure, since it would be describable only by its function of law-giving, and we have to ask what those entities are which exert this pressure.

The empirical facts involved in Freud's doctrine are preserved by regarding the ego-instincts as having had moralistic beliefs forced on them in some way; that is, while sometimes they repress the other instincts just for conscious expediency's sake, in other connections the repressed activity is seen not just as involving a specific punishment but as evil, as being in itself simply not to be done. Punishment is still involved, but it is
not (or not recognized to be) some concrete happening which might conceivably be circumvented, but is felt as something nameless, permanent, and inescapable. Admittedly the fear of this "metaphysical" punishment must derive from and be a screen for some actual threatened punishment (if only because it is "metaphysical" and nameless - that is, not really comprehensible, as the notion of something being wrong-in-itself is not really comprehensible), but the reality of moralistic beliefs and the notion of morality itself cannot be denied, and this is the empirical content of what Freud meant by his notion of "introjected prohibitions."

It represents a crippling of educability, in some sense a partial renunciation of cognition, on the part of the repressing instincts as well as the repressed ones, and makes the repression more stable because the belief in the inevitability of these metaphysical punishments is not so subject to the attrition of the growing knowledge of the world as would be the hidden infantile fears - e.g., that of castration, supposing that that does occur. There are problems as to how these fears become hidden; we can see that parents frequently command their children to believe that it is not a matter of temporal punishments but of "metaphysical" ones (without of course using, or needing to use, such technical terms), but the problem remains as to how moralistic beliefs first arose. But at least the superego is the result of whatever caused those fears to become unconscious; it is just the holding of moralistic beliefs by the ego-instincts, and the actual force which maintains repression is exerted by those instincts, not by some separate institution.
The important question, then, especially as regards the understanding of conflicts, is not, what does the person fear?, what does the person want?, but, what parts of him are threatened by what other parts of him? Although the clinician, in investigating the meaning of his patient's symptoms, is ordinarily content with final answers attributing behaviour to the desire for certain states, nevertheless he has the notion of basic wants - desires that people have just by virtue of their constitution. He rejects certain statements of wanting which are offered as explanations, on the grounds that such wants are plainly secondary or derived; that is, he distinguishes between things that are "wanted for what follows from them" (contending that the antecedents are not wanted at all, but only the consequences) and things that are wanted for themselves, and in my opinion, when such an enquiry is carried out with "insight" (just to accept that notion uncritically) the "basic wants" will be found to fall into classes corresponding to the activities of a group of qualitatively different EXCERPT instincts in the sense I have been advancing.

But such a method of investigating the springs of behaviour depends very greatly on the ability of the individual observer, and depends moreover on his already having an instinctivist view, even if of a McDougallian character. There have been many attempts to arrive at a list of basic wants or goals of which all other goals are species, and the difficulty is in knowing where to stop the chain of dependence, or in finding criteria of those things that are wanted for themselves. Many theorists seem ultimately to find only one goal - life itself. Even
the "urge to reproduce" can plausibly be brought under this, if it interpreted as a grossly materialistic desire to have sons to support one, or as the primitive notion of "living on" in one's progeny.

Horney provides an example of this kind of reductionism. She says, "Man is ruled not by the pleasure principle alone but by two guiding principles: safety and satisfaction... People can renounce food, money, attention, affection as long as they are only renouncing satisfaction, but they cannot renounce these things if without them they would be or feel in danger of destitution or starvation or of being helplessly exposed to hostility, in other words, if they would lose their feeling of safety" (p. 73). That is, the only reason for not renouncing something is that it is necessary for the preservation of life, because if "safety" referred to anything less than life then it could only be the safety of the means of satisfying (in her terms) a particular desire - but apparently any of these can be renounced.

Now, it seems to me a psychological fact that no-one literally fears death or being "lost" in a perfectly complete way, because no-one has ever experienced that condition and so no-one can conceive it. We understand the distinction between living and non-living, but we cannot conceive what it "feels like" to be dead. Horney elaborates by saying that "he feels in danger of his individuality being obliterated, his happiness prevented."

Now, such feelings are conscious notions, intellectual constructions; for example, "happiness-in-general" is a euphemism for the carrying out of a particular, forbidden act, and the "fear of obliteration" is just a screen for the fear of not being able to do some specific thing, the impulse to it
Having been repressed, its existence not admitted.

In general, we might say that as no-one really fears death, so no-one desires simply to live; it is always just a striving to do particular things, to be in specific qualitative states. Getting rid of the references to striving, we might say that the difference between being alive and being dead is just the presence or absence of particular groups of these properties, especially those quantitatively changing, cognizing ones that we have called motives or instincts, and "being alive" is not something that the person as a whole does over and above being and doing those particular things. "Safety" is just the safety (possibility) of specific activities; "hostility" is an obstacle to a specific behaviour; "punishment" is not merely for a specific behaviour but a prevention of some other behaviour, and "anxiety", once again, is produced by a threat to specific behaviours, and is really just some particular instinctual tension pressing against its barriers with exceptionally great intensity.

Thus, while the question "what is it that the man really wants?" may lead on to some understanding of behaviour, as a method (even overlooking the difficulty of giving a non-activistic account of "wanting") it suffers this disability: it leads to a regress which cannot, I think be shown to be logically vicious - it might be brought to an end just by appealing to observation as to whether a given man wants a thing for itself - but one for whose termination it is very difficult to find objective criteria. But if instead of asking that question we try to discover, as I have suggested, firstly, which of the man's motivational structures contribute to this
activity, and secondly, of which of their constitutionally-provided activities it is a species, then we have clearly formulated questions which are capable of unambiguous answers.
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