

# ENVIRONMENTAL DETERMINANTS IN PSYCHOLOGICAL THEORIES

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## A. INTRODUCTION

Numerous attempts have been made to bring about discussions in which the basic concepts of different psychological theories can be brought into relation with each other. But such attempts have usually failed to yield fruitful results, and they have failed largely because the basic terms in which the data are organized by the different theories are not fully analyzed and discussion is often based on concepts that are not directly comparable. In order to bring together the different theoretical approaches to psychological problems and to discuss them, we must recognize that it is often possible, especially in sciences dealing with the organism, to organize data in several different ways, at least in a first approximation. The way in which data are organized and the method by which the identity of focal unities or focal variables is determined are significant characteristics of a theory. Until we understand clearly what a given theory considers essential for the description of phenomena and in what terms it describes a given process in order to connect it with what has preceded and to predict its future, we cannot try to bring that theory into relation with others.

The purpose of this paper is to consider some general problems of determination and of the derivation of subordinate systems. The distinction between proximal and distal determinants is treated in detail and the attempt is made to analyze several psychological theories from the point of view of this distinction.

## B. GENERAL REMARKS ABOUT DETERMINATION OF FOCAL VARIABLES

I. *Relevant determination.*—In order to discuss the determination of focal variables we can do no better than to begin with a quotation from Holt:

. . . it is inaccurate to say that a river flows toward the sea . . . while it is fairly accurate to describe it as always flowing towards the next lower level of the earth's surface, and this is a law describing flow as a constant function of the earth's crust and the position of the earth's center. The test is, of course, whether this or that could be removed *without changing* the river's course. . . . So in behavior, the flock of birds is not, with any accuracy, flying over the green field; it is, more essentially, flying southward; . . . the sole question which we need ever ask is, 'What is it doing?' (II, p. 166).

Or again:

. . . the man is walking past my window; no, I am wrong, it is not past my window that he is walking; it is *to* the theater; . . . the functional view . . . admonishes us to keep the man whole (if it is *behavior* that we are studying) and to study his movements until we have discovered *exactly what* he is doing, that is, until we have found that object, situation, process . . . of which his behavior is a *constant function* (II, p. 161 f.).

Koffka, in explaining the meaning of relevant and irrelevant description, uses almost the same example:

. . . a ball runs down an incline and finally falls into a hole. Now there may be water in the hole or not, and therefore I can say the ball falls into a hole with water or without water. But this difference does not affect the motions of the ball until it has reached that position in space where the water begins in the one case and not in the other. For the rest of the motion the presence or absence of water is wholly irrelevant; similarly, the statement that the rat does not run towards food when the experimenter has just removed it, is quite irrelevant to the run of the rat

until it is near enough to notice the absence of food (16, p. 37).

Of course the inquiry, "What are the objects doing?" is not so simple as Holt makes it out. Each theory uses a different set of concepts as the only valid and legitimate one. But we learn from these quotations first, that there exist a great number—as a matter of fact an infinite number—of possibilities of determining an event. We can determine the motion of the water in a river as towards the sea or towards the next lower level of the earth's surface; we can describe the motion of a flock of birds as one "over the green field," or as a "southward" one. Besides the determinants mentioned in the quotations, a great number of other determinants are possible.

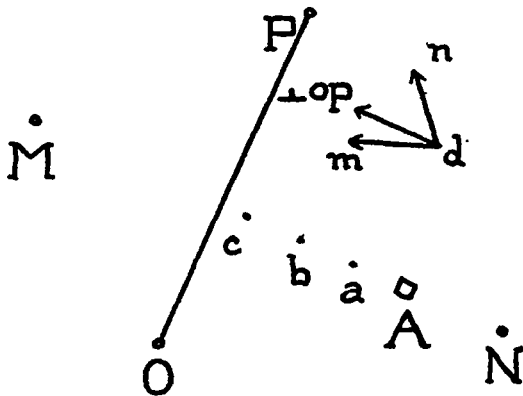


FIG. 1

Secondly we learn that only a few determinants are relevant; *i.e.*, there are only a few determinants that can be used to describe the events in a simple way. The test for this relevancy consists in finding constant functions. We may add another example which demonstrates the same facts in a more abstract way. Let us assume that one can observe that particle  $A$  in Fig. 1 moves through points  $a, b, c$ . We are free to determine this motion in many different ways. We can say  $A$  is moving toward  $M$ , or away from  $N$ , or that it is moving toward the line  $OP$  in a direction perpendicular to it.

Which determination is relevant? That is to say, what is *A* 'really' doing? We find the answer if we remove *A* to point *d*. If *A* then moves in direction *m* it is moving toward *M*. If it moves in direction *n* it is moving away from *N*, and so on.<sup>1</sup>

II. *Rival determinants*.—However, the task of determining relevantly is not always so simple. It is often made difficult by the possibility of rival determinants. Usually only one determinant will be successful; that is to say, will make a simple description of events possible and will yield constant coordinations for different situations. However, it often happens that determinants which belong to different regions seem to yield constant functions. Wherever we find two or more contending theories there exists such a situation. Examples are: description in terms of the whole or in terms of the parts (gas *vs.* molecule, group *vs.* individual, body *vs.* cell); in terms of 'higher' or 'lower' levels ('explaining' and 'understanding' psychology, physics-physiology-biology-psychology). In all of these cases one and the same phenomenon can be described more or less relevantly by using concepts belonging to different focal regions.

The fact of multiple focus provides many of the classical problems of philosophy. Multiple focus is distasteful to thinking; it blocks the tendency to unified determination. It is as if it were possible to read one and the same book in two ways; once by organizing the letter configurations into English sentences, and then again by organizing them into German sentences.

III. *Types of derivation of subordinate systems of determinants*.—In order to escape the dilemma of rival determinants different devices are used. Thus, many theories say: "Yes, it is true that one can find more or less constant functions if one chooses terms of region *B*. However, using these determinants, one will never attain a complete description of the cases in question. The seemingly constant functions one finds in this way are the product of a combination of functions which can only be determined adequately if one

<sup>1</sup> Cf., in regard to methods for finding relevant determinations, the important studies of Heinrich Klüver (14, 15).

applies terms of region *A*." The derivation of subordinate systems of functions becomes, therefore, a main problem for many theories.

This derivation follows certain patterns. A typical kind of derivation is, for instance, derivation by selection. Let us assume that a given manifold of processes or objects can be determined more or less relevantly in terms of two different regions, *A* and *B*. This is a case of multiple focus. Let us now assume that one theory, in order to reduce this multiple focus to single focus, declares *A* to be the focus, the independent system, and *B* to be the derived system.

The theory contends, then, that originally there were an infinite number of processes or objects, and they were all relevantly determinable in system *A*. The fact that we find a few processes which show constancy when determined in system *B* is due to chance. If there is an infinite number of items available we can arbitrarily choose any determinants and we shall always find a certain number of items which show constant functions in regard to those determinants. Furthermore, the theory contends that there is a selection going on which works in such a way that in the end only items conforming to system *B* are left over, and all items not conforming to *B* are eradicated. Since all items conform to *A*, the items which are left over conform to both *A* and *B*. It is assumed that the selection itself can also be explained in terms of system *A* alone. Thus it can be shown that only determination in system *A* is inherently relevant. Determination in *B* is not independently relevant; it is an outgrowth of determination in *A*.

Darwin's theory of selection uses this method in establishing the system of teleological determination as a dependent system. An infinite number of possibilities is realized, all conforming to system *A*, which is in this case the system of causal explanation. Some of them possess characteristics which 'make sense' in system *B* also; that is to say, they can be determined as having some use for the organism. Natural selection then works in such a way that only these possibilities are left over.

Other cases of derivation by selection are offered by the theories which explain the teleological properties of perception or thinking, namely the fact that in some way they show a correspondence to the 'real' world, in terms of experience. The problem is very similar: Double determination, that is to say, determination in causal and in teleological terms, has to be reduced to single determination in causal terms. An infinite number of possible bonds is realized, all conforming to causal system *A*. Some of them also fit the teleological system *B*, and a law of effect, for instance, takes care of the selection. Theories of association, of conditioned reflex, positivistic and pragmatistic theories of the derivation of logical forms, all make use of this type of derivation.

Often the defenders of system *B* use counter-arguments, which again follow typical patterns. They can say, first, that the assumption of an originally infinite number of possibilities is untenable. In observation we find only a small number of items  $A\bar{B}$  (conforming to *A* but not to *B*); most of the items are *AB*. It cannot be due to chance, therefore, that *B*'s appear.

Or they can say that the *B*'s are of such great complexity that the probability of *B*'s coming into existence among the *A*'s through chance is infinitely small.

Or they can say that there exist *B*'s in regions where the process by which *B*'s were selected from the infinite number of *A*'s never occurred.

To this last argument the defenders of *A* can retort by using the device of extrapolation. They assume that selection worked so often on items conforming to system *A* that these items, in time, got used to conforming to *B* and so exhibit *B*-characteristics even in regions where the selection was not directly active.

IV. *Tendencies affecting the selection of determinants.*—One might assume that the question as to which region shall be used as the primary system of determination is always answered on the basis of empirical findings. However, this assumption does not always seem to hold. General *a priori* tendencies are often responsible for the choice of a certain

region as a focus. A few of these tendencies may be enumerated.

*a.* A tendency to use regions close to the observer as the focal region. This tendency is the result of a reluctance to make theories and of an inclination to premature measurement. Surface processes or surface characteristics are taken as relevant determinants and correlations are computed. Both the psychology of personality and of social functions provide many examples. This tendency is supported by positivism. The important principle which states that every concept should be anchored in observations or 'operations,' is often misunderstood. It is thought that one should take the directly observed entities themselves as focus and that one should be satisfied with a language of data. Cruder forms of behaviorism thus find an ally in a misunderstood positivism.<sup>2</sup>

*b.* A tendency to use the same focus for different regions of a science. One does not want the science to be broken up into regions with different determinants. If that is done there will always be attempts to find constancies of a higher order and to put the focus into a deeper layer so as to combine the different regions.

*c.* A tendency to use proximal determinants and to avoid distant determinants. No change should be determined by something which is distant in time or space. More will be said about this point later.

V. *The psychological fallacies.*—In determining the focus of a psychological theory one has always to keep in mind the possibility of a disguised focus. The relevant variables may be assumed to be in region *A*; however, since there often exists a confusion between regions, or an unwarranted assumption of equality of regions, the relevant variables are really described in terms of another region *B*. By means of a disguised focus sham solutions of many problems are possible. This fact has often been noticed, and it has been discussed in a more general way under the name of 'fallacy' or 'error.'

<sup>2</sup> Cf. Koffka's concepts of achievement and performance (16, p. 530); Lewin's confrontation of historic-geographic and systematic concepts, phenotypic and genotypic language (18).

Some of these confusions (*cf.* Table I) are:

a. James' 'psychologist's fallacy': The 'mental state' is determined in terms of the coordinated object. 'Equal to' is substituted for 'referring to.'

The *great* snare of the psychologist is the *confusion of his own standpoint with that of the mental fact* about which he is making his report. I shall hereafter call this the 'psychologist's fallacy' *par excellence*. . . . Now when it is a *cognitive* state (percept, thought, concept, etc.), he ordinarily has no other way of naming it than as the thought, percept, etc., *of that object*. He himself, meanwhile, knowing the self-same object in *his* way, gets easily led to suppose that the thought, which is *of* it, knows it in the same way in which he knows it, although this is often very far from being the case (13, Vol. I, p. 196).

TABLE I  
SOME OF THE PSYCHOLOGICAL 'FALLACIES'

The arrows start in the region, the terms of which are supposedly used; they end in the region, the terms of which are actually used.

	images	sensations	stimuli	objects
James' fallacy	—————→			
Stimulus error (Titchener)		—————→		
Constancy hypothesis		—————→		
Experience error			—————→	

b. Titchener's stimulus error: The psychological 'elements' (sensations, etc.) are described in terms of the physical environment. "The psychologist commits the stimulus-error when he lapses from the psychological point of view into some other, like the physical" (Boring, 2, p. 410).

c. Köhler's constancy hypothesis: 'Sensations' are hypothesized and described in terms of the stimuli (17, p. 96).

d. Köhler's experience error: Stimuli are described in terms of the experiential or distant object.

In psychology much has been said about the stimulus-error which consists in our confusing our knowledge about the physical conditions of sensory experience with experience as such. But another mistake, which I propose to call the *experience-error*, is not less unfortunate. It occurs when we unintentionally attribute certain properties of



sensory experience to the actual constellation of stimuli, properties which are so very common that we tend to apply them to whatever we are thinking about. This is the case primarily, wherever we have not yet learned to see the *problem* contained in those common properties of experience. No wonder, then, that neurologists and some psychologists still talk about 'the retinal stimuli' corresponding to an object, as though there were something like detached functional units on the retina (17, p. 176).

Because the distant object is a thing by itself, the assumption is tacitly made that the retinal image corresponding to it is also (16, p. 97).

### C. DISTAL AND PROXIMAL DETERMINANTS IN SEVERAL PSYCHOLOGICAL THEORIES

A consideration of relevant description and its relation to a manifold of regions is so important for psychology because, as the discussion of the fallacies has shown, there are so many regions involved in each psychological process. And not only are there many regions involved, but determination in terms of each region seems to give sense; that is to say, leads to relatively constant functions. Therefore the multiplicity of psychological theories, each of which places the focal concepts in a different region.

In the following an attempt is made to describe a few psychological theories in regard to the place of focus. We shall, for the most part, consider only the outer field, that is to say, environmental determinants, and we shall only occasionally refer to internal, inner-organismic determination.

Of prime importance for all theories is the question whether distal or proximal data are used as the focus in the determination. One can treat perception and action either in terms of the distant object (perception functions in such a way that the distant object is 'attained'; the organism moves towards the food, etc.); or one can treat it in terms of proximal influences and effects, that is to say, in terms of processes close to the skin, stimuli, muscle contractions or movements of the limbs.<sup>3</sup>

<sup>3</sup> Cf. Koffka's use of the terms proximal and distant stimuli (16, p. 80).

I. *Theories in terms of proximal influences and effects.*—These theories fall into two groups. One group stresses perception; to it belong the older theories of perception which emphasized the stimulus-oriented sensations. The other group treats the psychological processes more from the point of view of action and motor phenomena; to it belong the stimulus-response theories.<sup>4</sup>

It is easy to see that these theories get their vitality from the general tendency to use proximal determinants and not from observation; observation favors distal determinants much more. The exponents of these theories want to relate psychological processes to the actual concrete influences which organism and environment exert on each other.

The most important arguments against these theories can be reduced to a single point: Observation shows that often distal determination is possible where proximal determination is impossible. Von Kries, Becher, Ehrenfels, and the Gestalt theory used this argument against the older theories of perception; different teleological systems (McDougall, etc.) used this argument against stimulus-response theories.

Indeed, the most important problem for all theories using proximal determinants is to show that it is possible to establish that system of determination as the independent one, and further that it is possible to derive from that system the existence of relevant distal determinants, which are found in observation, and to treat them as only apparently relevant determinants. The device which is almost exclusively used for this derivation is selection. There is the infinite number of possibilities of bonds of association or conditioned reflexes between any stimuli and any responses. Contact with the environment establishes or strengthens only a limited, selected number of these bonds. Selection works in such a way that distal determination, that is to say, correspondence to the objects of the environment, is brought about.

However, very often the derivation of distal from proximal determination is effected by the surreptitious substitution of

<sup>4</sup> That these two groups of theories belong together has been shown, for instance, by Köhler (17).

distal for proximal terms.<sup>5</sup> The following cases are examples of disguised focus. We quote here from one study, where distal determinants are quite openly introduced into a 'stimulus-response' theory.

Suppose that we are studying the behavior of such an organism as a rat in pressing a lever. The number of distinguishable acts on the part of the rat that will give the required movement of the lever is indefinite and very large. Except for certain rare cases they constitute a class, which is sufficiently well-defined by the phrase 'pressing the lever.' Now it may be shown that under various circumstances the rate of responding is significant—that is to say, it maintains itself or changes in lawful ways (Skinner, 19, p. 44).

We note that the author starts out to study "the behavior of such an organism as a rat in pressing a lever," that is to say, a phenomenon which is from the beginning defined by distal determinants. It is conceded that it is impossible to find coördinated proximal events: "The number of distinguishable acts on the part of the rat that will give the required movement of the lever is indefinite and very large." Therefore, instead of proximal events, there is substituted a "class which is sufficiently well defined by the phrase 'pressing a lever,'" a phrase which of course denotes distal determination in its purest form. And, placing the focus in the distant environment, one can find that "the rate of responding is significant—that is to say, it maintains itself or changes in lawful ways," namely, distal determination yields constant functions.

That distal determination is forced upon us by the observed facts is also stressed in the following quotation:

The uniformity of the change in rate excludes any supposition that we are dealing with a group of separate reflexes and forces the conclusion that 'pressing the lever' behaves experimentally as a unitary thing (19, p. 45).

We should like to add one more quotation from the same

<sup>5</sup> Cf. especially the keen analysis of stimulus-response theories by Arthur F. Bentley (1).

paper, in which the principle of finding the relevant determination is clearly expressed:

It is then possible to test the irrelevance of a non-defining property by showing that two responses, one of which possesses the property, the other not, contribute equally well to a total number [of elicitations of a reflex] (19, p. 44).

Not always is the change in focus as patent as in this paper. It is much more difficult, for instance, to find in Hull's theory of family-hierarchy the place where distal determinants are substituted for proximal determinants. The problem that this theory attempts to solve comes from the fact that 'habits' which are determined distally, by the achievement, represent a confusing variety in proximal terms. "Instead of presenting a single unvarying and undistinguishable sameness, as is too often assumed, habits . . . present a remarkably varied series of patterns" (12, p. 33).

According to Hull, some mechanism has to be found which gives identity to this 'family' of patterns in terms of action sequences. The problem is essentially the same as that which von Kries stated for perception: Is there anything identical in the different stimulus patterns that correspond to an identical object? If the analogous problem were solved for action, it would be a decided advance toward an explanation of goal directed behavior. "It [the principle of habit-family hierarchy] is operative in all situations wherever there is more than one distinct action sequence which will lead to the attainment of a particular goal or subgoal. It is believed, for example, that the habit-family hierarchy constitutes the dominant physical mechanism which mediates such tests of truth and error as organisms employ—that it provides the basis for a purely physical theory of knowledge" (12, p. 40 f.).

We do not have to go into the details of this interesting and ingenious theory in order to show why we believe that it, too, achieves the solution by means of a disguised focus. The critical point is the definition of the identical element which makes the different action sequences belong to one family

and thereby interchangeable. This identical element is the 'same' goal reaction which is brought forward in the action sequence as "fractional anticipatory goal reaction." "It thus seems probable that the fractional anticipatory goal reaction is the major mechanism which brings about the integration of the habit-family hierarchy" (12, p. 43).

The whole theory depends on the terms in which the goal reaction is defined. If it is possible to define it in real stimulus response terms, that is to say, proximally, and without including environmental determinants, then the theory has solved its problem.

It seems, however, that the goal reaction is not defined in this way. This fact becomes especially clear if we examine the case of 'purposeful' locomotion and orientation in space which Hull discusses in terms of this theory. Which determinants do we have to use in order to be able to say that goal reactions belonging to different action sequences (action sequences which correspond to different paths to the same goal in the environmental space) are all one and the same goal reaction? According to Hull, these goal reactions seem to be distinguished from other goal reactions only in that they all occur at a certain place in the environment, which lies in a specified (again environmentally determined) direction from the starting point. But are these goal reactions identical from the point of view of proximal determination? That would be the case only if, for instance, the movements of the rat in eating the food (*i.e.*, the real response) varied when the rat approached the food from different points of the environment, and were the same when it approached from the same place. In other words we could only make this assumption if there were a differentiation in the movements corresponding to the differentiation of the environmental space, and in some way coördinated to the spatial relation between starting point and goal. Actually, however, the identity of goal reactions is not determined proximally: Two occurrences of goal reaction are called the same goal reaction when they occur at the same place in the environment. Thus, we find that in this theory also the identical element conceals distal determinants

behind apparent proximal determinants; it is a case of disguised focus.

Many critics have objected to the use of environmental (distal) determinants in stimulus-response theories. The following quotations are examples of such objections:

. . . the conditioned response formula seems to me inadequate in that the two stimuli and the two responses which are picked out in the above example by the conditioned response formula are not, it must be observed, stimuli and responses in any strict physiological sense. They are not physiologically, but environmentally defined affairs. Thus food and string, as visual, olfactory, and tactual stimuli patterns, may be quite different from occasion to occasion. They retain their respective identities from time to time only by virtue of their environmental 'meanings.' And eating and string-pulling, as responses, do not correspond to specific and invariant sets of muscle contractions, but are only identifiable through successive times in terms of environmentally nameable 'manipulations' . . . the conditioned response formula . . . must be loosened so as to allow both 'stimuli' and 'responses' to be identifiable in terms of relatively gross and meaningful characters and not in terms of any precise or necessarily constant sense-organ and muscle processes (Tolman, 20, p. 200).

These supplementations do more than call attention to the complexity of stimulus-response relationships in conditioning experiments; they introduce terms such as direction, organization, means-end relationships, which are foreign to the logic of stimulus-substitution. To confess that the bald statement of association by contiguity is unsatisfactory because we oversimplify the items which are contiguous is one thing; to supplement the concept with heterogeneous organizing principles not coherent with it is another (Hilgard, 9, p. 550).

Sometimes it is believed that the variations in the stimuli and responses, which are meaningful if they are determined environmentally, can be explained away by pointing out that there are random variations in every stimulus-response

experiment.<sup>6</sup> However, distal order cannot be derived from proximal disorder. A demonstration of the variations in the stimuli and responses shows only that the function between them is more complex than originally assumed; but it does not explain why it is possible to find distal constants in these proximal variations. Tolman has stated:

. . . there is a big difference between admitting that stimuli or responses probably vary from time to time and being able to give any account of (in truly stimulus-response terms) why they can nonetheless be called identical with their former selves. It is this latter requirement which I think both Guthrie and Skinner have failed to satisfy (20, note on p. 201; cf. also 17, p. 122, and 14, pp. 344 ff.).

“Why they can, nonetheless, be called identical with their former selves”; that, indeed, is the central problem for any theory using stimulus-response terms. It is the problem of reducing distal terms to proximal terms.

Thus we see that all the theories that employ proximal determinants, whether in terms of perception or of stimulus-response, are faced with the problem of explaining the existence of relevant distal determinants. The weak point of these theories seems to be that they end up by substituting distal for proximal determinants without realizing the change of focus.

II. *Theories in terms of distal determination.*—Distal determination of the entities to be coördinated to the processes in the organism seems at first sight to be entirely possible. Hobhouse, for instance, comes to the conclusion that the organization of observations in distal terms is as legitimate as the organization in terms of proximal determination.

If a philosopher from another planet, ignorant of all forms of life as they exist upon this world, were to watch a stone rolling downhill and a man running to catch his train, he would come to the conclusion that the stone and the man were actuated by very different principles. He would, for

<sup>6</sup> Cf. Hilgard (9, p. 373).

example, see the man go round the obstacle which caught up the stone, and if he proceeded to compare their behavior under many circumstances and in different relations, he would arrive at the result that the broad difference could be most easily formulated by conceiving the stone's action as determined always by the reaction of its inherent qualities upon the forces directly impressed upon it without regard to the ultimate issue, while the man's action would be, in the majority of cases, determined by its relation from moment to moment to some result more or less remote. . . . That is to say, proceeding purely by inferences based on comparison of behavior, he would discover two fundamentally distinct types of correlation, one in which each element of behavior is conditioned by its relation to its result, the other in which no such relation is operative although the result is in fact produced. Now he might ultimately decide that these two types are reducible to one. . . . But even in the latter alternative he would still acknowledge two clearly distinct types of correlated behavior, in one of which the bearing of act on result is operative, while in the other it is not. He would now hold that this relation is made operative by a mechanical arrangement. But operative it still would be, and this would generically distinguish the type of correlation from the type in which there is no such element operating (10, p. 15).

Hobhouse means that, even if we are able to reduce distal to proximal determination, we have to acknowledge that we find in observation cases which give constant coordinations in distal terms. And that is perfectly true, if one limits oneself to a 'majority of cases'; however, there is no distally determined process or movement of the organism which could not be disturbed or made impossible by processes coordinated with proximal events. Pure distal determination is an absurdity; it would mean perception without sense-stimulation and action without muscle-contraction. And worse still, it would be a logical impossibility, because distal determination without coordinated mediating processes could give, in the last analysis, no definite determination at all. If perception is entirely determined by the distant environment, what deter-



mines which object is perceived in a concrete situation? Distal determination seems to imply the impossibility of complete determination and that may be the real cause for the general distrust of distal determinants.<sup>7</sup>

Without doubt, the most imposing system using distal determination is Brunswik's psychology in terms of objects. Its importance lies not only in that it stresses distal determinants, but also in that it offers concepts and methods which allow systematic experimentation. It seems to give the ideal fulfillment to Holt's program, to find "that object, situation, process of which . . . behavior is a constant function" (II, p. 161). At first sight, this psychology seems to combine distal and proximal determination, since the 'real objects' of perception or action are determined both in terms of the environmental objects and in terms of stimuli.

However, we find that the role of the stimuli in mediation and their place 'between' objects and the organism is more or less disregarded. Brunswik writes:

All of the above facts concerning the functioning of the organism in perception suggest a general way of consideration which would seem to be the one most profitable for psychology. Thus, both for reception and for action, it turns out that the special manner in which anything is mediated (or done) is not especially essential or significant. One and the same means-object may be represented at different times by very different stimulus configurations. And one and the same goal may be reached equally well by very different kinds of movements and means-object manipulations. The focal-points of life occurrences, *i.e.*, means-objects and final goal-effects, lie, respectively: relatively far away in time and space, backward (in cognition), or forward (in action). They are removed from the actual stimulus conditions and the actual body movements, so

<sup>7</sup> The impossibility of complete determination in distal terms is clearly expressed by Brunswik although his own theory is based on distal determinants: ". . . we are dealing with causally distant effects, for which all conditions are not yet ascertained. The relation is therefore not one which is univocally determined in advance but only a more or less probable one. For, it is always possible that unexpected 'marginal' causal chains interfere, or that conditions are absent which one can expect in a normal environment" (6, p. 18 f.).

that the really significant question always is: What are the kinds of such objects and final goal-effects which the organism is able to attain independently of all the varying circumstances with a relatively large degree of accuracy and probability; achieving them by perception, on the one hand, and by action, on the other. In short, questions of 'what' are much more important in psychology than questions of 'how.' And thus to seek to describe the abilities and performance of an organism by giving an inventory of the kinds of objects attained by it, may be called 'Psychology in Terms of Objects.' In principle, this viewpoint need not have any concern with the organism's actual sensory, nervous, or motor conditions—*i.e.*, with mere mediation problems, as studied in traditional behaviorism, psychophysics, and physiological psychology (4, p. 125).

As we can see, mediation problems, or problems of proximal determinants, have no place in a psychology in terms of objects. At best, mediating entities are considered in their role as good or bad cues for the distant objects (*cf.* especially Tolman and Brunswik, 21).

What kind of psychology can be built upon such foundations, what determination-tendencies can it satisfy? It is true that it is without fault from the point of view of a positivistic program, and that its statements are verifiable by experiment and carefully grounded in observation. It blocks, however, the tendency to unified determination and it fails to make possible complete determination. Unified determination is not attempted, since its goal is an 'inventory,' a multitude of coördinations which it cannot, and does not, strive to reduce to coördinations of a higher order. Because it only asks 'what' and not 'how,' it cannot achieve complete determination. If it were to ask 'how' and if it wanted to describe completely the processes involved in a single concrete case of behavior, it would have to consider proximal and interior determinants; it is, in the end, even questionable whether it is possible to give a complete answer to the question 'what' without doing so.<sup>8</sup>

<sup>8</sup> Brunswik himself recognizes that 'what' and 'how' problems are intimately connected: "Since the ways of mediation will always determine the achievement, the

These remarks are not intended to belittle the importance of the theory. Its contribution to the development of psychology is a substantial one, since theories emphasizing proximal determinants have neglected to study many distal coordinations, even when, in principle, they have taken account of the fact of relevant distal determinants. There are many fields of psychology in which we are still ignorant of the distal coordinations of behavior, and often the problem has not even been stated properly. The psychology of the mental development of the child, or of language, offers many examples. An experimental method of determining the 'attained objects' of behavior can certainly contribute much to the body of psychology.

III. *A theory in terms of orientation.*—The theory of tropism, as it is presented by Crozier and Hoagland (7), coordinates 'stimuli' with 'orientation,' that is to say, direction which is determined in relation to the environmental space. Not what is closest to the skin-muscle contractions or movements of the limbs—is taken as the focus, but an effect, an achievement of the movements of the limbs. Thus, this theory goes a step beyond pure proximal determination. Determination by orientation lies between determination by the movements of the organs and distal determination in terms of the objects of the environment.

From the following quotations it will be clear that the authors distinguish sharply between proximal determination and determination in terms of orientation, and that they do not think that the second can be reduced to the first.

Since the anatomical basis for such actions is quite different in diverse organisms, but the behavior element dynamically identical, it is clear that the quantitative formulations arrived at refer to the *behavior*, and not to specific accidents of structure . . . (7, p. 6).

The "anatomical basis" and "accidents of structure" are proximally determined entities; the "dynamically identical behavior element" refers to orientation.

highly abstracted type of object-critical analysis as outlined above would lead, ultimately, to a statement of all psychologically relevant types of 'how'-problems and -findings in terms of 'what,' *i.e.*, of objects attained" (5, p. 251, note).

IV. *Gestalt Theory*.—In order to describe the focal terms of Gestalt theory, we do best to present first the reasoning we find in Chapter III of Koffka's *Principles of Gestalt Psychology*. The question, in what terms one should describe perceptual processes, is put in the form: Why do things look as they do? (16, pp. 76 ff.).

1. Distal determinants: "A first answer would be: things look as they look because they are what they are." The method of finding out whether this answer is adequate is to "single out a few aspects of behavioral things and compare them with real ones." That is to say, one may determine how constant the coördinations between perceptual phenomena and distant objects are. Of course, it is easy to find cases in which there are no constant coördinations, such as the moon illusion where there is no constancy. Distal determinants are therefore discarded, because there are cases of disagreement.

2. Proximal determinants: We have to distinguish between (a) local proximal determinants and (b) non-local proximal determinants.

a. Local proximal determinants. A second possible answer is: "Things look as they do because the proximal stimuli are what they are" (16, p. 80 ff.). Again, we find many cases in which there is no correspondence between the local proximal stimulus and the perceptual phenomenon. For example: "The constancy of real things is to a great extent preserved in the constancy of the *phenomenal* things despite variations in their proximal stimuli" (16, p. 83).

b. The principle of non-local proximal determination has to be accepted. For example: "If, without a table and even without a light . . . , we could produce the same pattern of excitation . . . which is ordinarily produced on our retinae when we fixate a table, then the person on whose retinae these excitations were produced should and would see a table" (16, p. 79). That means, in such an experiment we would find the perceptual process coördinated only to proximal events, not to objects.

Thus, both distal and local proximal determinants are discarded; that distal determination is possible in many cases

is recognized but used only in the refutation of local proximal determination. Only non-local proximal determinants are accepted, and that means for Gestalt theory that external determination is made in terms of stimulus patterns, internal determination in terms of fields and Gestalt processes. Koffka states:

All we intend to do is to replace laws of local correspondence, laws of machine effects, by laws of a much more comprehensive correspondence between the total perceptual field and the total stimulation . . . (16, p. 97). Things look as they do because of the field organization to which the proximal stimulus distribution gives rise (16, p. 98).

Thus we find the program of Gestalt psychology to be: perceptual processes have to be defined in terms of stimulus pattern and field organizations; the question, "Why do things look as they do?" should be answered in these terms.

Gestalt theory has found a new device for the derivation of distal from proximal determination. As we have seen, several forms of conditioned reflex theory take into account both proximal and distal determinants. They make use of local proximal determination and from it derive distal determination by the device of selection. For Gestalt theory, too, it is possible to take into account both kinds of determinants. However, it does not derive the one from the other in a way which makes the derived focus a spurious one.

Let us consider, as an example, visual fixation and pursuit (16, pp. 311 ff.). As long as we determine the coördinations proximally and locally, in terms of peripheral movements and sensations (or local stimuli), we find a bewildering confusion without constancy. Distal determination is in many cases easy: The eye is tuned in on the object world, it follows moving objects, etc. The problem is: How can we anchor distal determination in proximal determination, how can we exclude teleology?

The solution proposed by Gestalt psychology is the following: It is wrong to start with an identification of muscle movement qua movement (local determinants), and then hook

on to these entities connections which are forced upon the organism by the contact with the environment. Muscle movements and stimuli are of course there and play their role in the process, but they can be determined only as parts of an organization. And this organization takes care of the distal determination at the same time. According to Koffka, ". . . a stimulus inhomogeneity [starts the movement] and the movement takes place in such a way that this stimulus inhomogeneity is brought into the center of the retina" (16, p. 313).

The organization in which the local stimulations and the local movements are embedded is of such a kind that it gains its equilibrium when the requirements of distal coördination are fulfilled; that is to say, when the eye is directed towards the object, which is coördinated to the stimulus inhomogeneity. In this way proximal and distal determinants, the local events and their 'achievement,' are brought into harmony. The model of this combination is taken from physics. In physics, too, an event can often be described in both proximal and distal terms.

We may best visualize the relationship between the responses that make up the so-called purposive behavior category by the rain-drop analogy. We may start with the assumption that every drop of rain in some way or other gets to the ocean. . . . Anthropomorphizing this condition, we may say that it is the *purpose* of every drop of rain to get to the ocean. Of course, this only means that virtually every drop *does* get there eventually. How it gets there depends upon where it falls. . . . Each stage, each fall from one leaf to the next, may be designated as a *means* toward the final end, the sea, and a number of the intermediate stages may be grouped together and the terminal stage designated as the purpose of the antecedent stages. . . . Human behavior is merely a complication of the same factors. Instead of only a few physical forces such as gravity, temperature, humidity, surface tension, friction, that act on the drop of rain, the stimuli which act on the sensori-motor system of man are much more numerous (22, pp. 346-347).

Gestalt psychologists can agree essentially with these statements of Weiss. They show that distal determination in physics is in principle identical with that of behavior, and that physics makes use of a device which resolves distal into proximal determination. Thus it does not leave teleology hanging in the air. However, the most important concept, by which physics achieves this end, is not given enough credit in the above quotation. It is the gravitational field and not leaves, ground, or arbitrarily arranged forces which make the movement of the drop of water one which can be determined distally. Field and equilibrium are the concepts by which in physics the distal determination is made congruent with the proximal. The field brings together the end of the movement and the forces which affect the moving body directly and makes them actually one and the same thing. The 'goal' is a unique place within a field at every point of which there are forces directed towards this place. Field determination is really neither distal nor proximal determination; both these determinants are merely aspects of field determination.

The total process of a psychological organization is, of course, much more complicated than the organization of these simple physical examples. Several regions often take part in the total process, regions which have more or less autonomous organizations.

In the actual work of Gestalt psychology, we find a discrepancy between the treatment of perceptual problems and that of behavioral problems. The coördination of the organism to the object world, that is to say, distal determination in terms of the objective environment, is considered and 'explained' in the treatment of the psychology of action. In the treatment of perception, however, the fact of correspondence to the object world is often neglected and the goal or final state, toward which a process is directed, is determined in terms of figural properties.

It is significant that Koffka introduces his discussion of action by a section which is headed: "The results of behavior" (16, p. 306). In this chapter we read:

. . . if we want to explain behaviour, behaviour which has been such a powerful agent in the world, can we ever hope to succeed if, right at the outset, we forget what behaviour has accomplished? That is to say, must we not, in order to explain behaviour, first gain some knowledge about those universal aspects of behaviour which have been responsible for its success? Will it do to introduce explanatory principles indifferent to the results of behaviour, principles which would explain as well, or better, utter chaos . . . ? (16, p. 307).

However, in the chapter on the constancies, we read:

. . . the connection between this uniqueness of one set of conditions and its cognitive value should not be used in any sense as explanatory of the uniqueness . . . the constancy problem should be re-formulated in this way: What shape, size, brightness, will correspond to a certain local stimulus pattern under various total external and internal conditions? Once we have answered this question we shall know when to expect constancy, when not. Indeed some effects of non-constancy are just as striking as the effects of constancy which have been so much emphasized . . . (16, p. 227).

Does that not mean, that we should "introduce explanatory principles indifferent to the results" of the perception?

We find thus an inconsistency in the attitude towards distal determinants. Especially in perceptual problems, Gestalt theory has limited itself to an investigation of figural organizations, and has more or less disregarded the original program of taking into account the 'achievements' in terms of equilibria. The original program was to make 'meaning' dynamically real and to give a solution for the problem of the coördination of the organism to the object world.

This is also the meaning of the claim of Gestalt psychology to make the 'order' in the psychological processes understandable. Order always refers to some particular kind of determination. Something can be orderly in regard to one determination and disorderly in regard to another. The order, about which Gestalt psychology talks in its general



program, is mainly the order we find when we determine the events distally. As Köhler has said: "All this order is as remarkable as it is necessary for our response to the objects which, in the form of bodily movement, must be adjusted properly to the physical world" (17, p. 115). That other systems of psychology failed to explain adequately the coördination of the organism to the environment, is again and again pointed out by Gestalt psychologists.

However, as we have seen, in the psychology of perception the environmental distal determination is disregarded. The focus is placed in the figural, geometrical properties of the stimulus pattern, and the distal determinants which are used are figural-distal determinants; *i.e.*, the fact is pointed out that we can often describe perceptual processes as tending towards a certain configuration.

We have seen that distal determination does not order the events in the same way as local proximal determination; we have to add it does not order them in the same way as figural determination. That means, when we determine in terms of figural properties we have not yet solved the problem why environmental distal determination is possible. This fact has been pointed out by several authors:

The same thing can 'express itself' in many different stimulus patterns, which are, also from a figural point of view, different; and we can recognize it through these different mediations (Heider, 8, p. 384).

One can call this multitude of possibilities a transposibility—perhaps 'sign transposibility.' This is different from the gestalt or sum-transposibility and plays, so far, only an unimportant role in gestalt psychology. For, the different equivalent possibilities do not show common form (figural) properties . . . but only common empirical significance (Bewährung) . . . they have in common their character as a sign of something that is causally more distant than they are (Brunswik, 3, p. 228).

Thus, we see that Gestalt psychology has developed a new device for the solution of the problem of the discrepancy between proximal and distal determination. However, it has

not made use of this device consistently, and in some cases it has lost sight of the environmental distal determinants. Connected with this is the fact that it has not yet developed a theory of experience in terms of organization; it does not account for the fact that contact with the environment makes the organism more coördinated with it. Of course, the limitation to an intense study of figural processes was probably very wise; one might only raise the question whether the psychology of perception in its present stage would not be advanced greatly by a more extensive consideration of distal environmental coördinations.

#### D. SUMMARY

In organizing any data one can apply an infinite number of determinants. Scientifically fruitful and relevant determinants are those that yield constant coördinations. This empirical test may lead to rival determinations: At least for a first approximation the same group of data can sometimes be determined relevantly in two or more different ways.

In order to escape the dilemma of multiple determination one kind of determinant is often selected as primary and the attempt is then made to derive the fact that other relevant determinants exist from this primary determination. These derivations follow certain patterns; derivation by selection is a common type. Empirical findings are not always responsible for the selection of a particular determination. We find *a priori* tendencies which favor certain determinants.

Questions of determination are especially important in psychology because there are several regions involved in every psychological process. The rivalry between proximal and distal determinants is one of the most significant for psychological theories. Theories using proximal determinants as the primary ones are faced with the problem of explaining the existence of relevant distal determinants. In the attempts to solve this problem use is often made of the device of derivation by selection. It can be shown, however, that the apparent success of these derivations is brought about by the unnoticed

introduction of distal determinants into the assumptions from which these determinants are supposedly derived.

Theories using distal determinants as the primary ones are faced with the problem of what to do with the fact of relevant proximal determinants. They often state explicitly that this fact has no place in psychological theory. Thus the way to complete and unified determination is cut off.

Gestalt theory offers a framework of concepts which makes it possible to take both distal and proximal determinants into account and which resolves the dilemma of deciding between them. Gestalt theory, however, has not yet made consistent use of these concepts.

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