Gestalt Psychology Today [1]

Wolfgang Köhler (1959)

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In 1949, the late Herbert Langfeld gave a lecture in Europe in which he described what appeared to him to be the major trends in American psychology. He also mentioned Gestalt psychology; but he added that the main observations, questions, and principles characteristic of this school had become part of every American psychologist's mental equipment. I was not so optimistic. And, in fact, the very next year attempts were made to explain the molar units in perception by processes which gradually connect neural elements. Soon afterwards, a theory of conditioning was developed, according to which more and more components of a stimulus object are gradually conditioned, and the course of the whole process can be explained in this fashion. Such theories may prove to be very useful, but one can hardly say that, at the time, their authors were greatly influenced by Gestalt psychology. It is for this and similar reasons that a new discussion of old questions seems to me indicated.

I should like to begin with a few remarks about the history of Gestalt psychology -- because not all chapters of this history are generally known. In the eighties of the past century, psychologists in Europe were greatly disturbed by von Ehrenfels' claim that thousands of percepts have characteristics which cannot be derived from the characteristics of their ultimate components, the so-called sensations. Chords and melodies in hearing, the shape characteristics of visual objects, the roughness or the smoothness of tactual impressions, and so forth were used as examples. All these "Gestalt qualities" have one thing in common. When the physical stimuli in question are considerably changed, while their relations are kept constant, the Gestalt qualities remain about the same. But, At the time, it was generally assumed that the sensations involved are individually determined by their individual stimuli and must therefore change when these are greatly changed. How, then, could any characteristics of the perceptual situation remain constant under these conditions? Where did the Gestalt qualities come from? Ehrenfels' qualities are not fancy ingredients of this or that particular situation which we might safely ignore. Both positive and negative esthetic characteristics of the world around us, not only of ornaments, paintings, sculptures, tunes, and so forth, but also of trees, landscapes, houses, cars -- and other persons -- belong to this class. That relations between the sexes largely depend on specimens of the same class need hardly be emphasized. It is, therefore, not safe to deal with problems of psychology as though there were no such qualities. And yet, beginning with Ehrenfels himself, psychologists have not been able to explain their nature.

This holds also for the men who were later called Gestalt psychologists, including the present speaker. Wertheimer's ideas and investigations developed in a different direction. His thinking was also more radical than that of Ehrenfels. He did not ask: How are Gestalt qualities possible

when, basically, the perceptual scene consists of separate elements? Rather, he objected to this premise, the thesis that the psychologist's thinking must begin with a consideration of such elements. From a subjective. point of view, he felt, it may be tempting to assume that all perceptual situations consist of independent, very small components. For, on this assumption, we obtain a maximally clear picture of what lies behind the observed facts. But, how do we know that a subjective clarity of this kind agrees with the nature of what we have before us? Perhaps we pay for the subjective clearness of the customary picture by ignoring all processes, all functional interrelations, which may have operated before there is a perceptual scene and which thus influence the characteristics of this scene. Are we allowed to impose on perception an extreme simplicity which, objectively, it may not possess?

Wertheimer, we remember, began to reason in this fashion when experimenting not with percep- [p. 728] tual situations which were stationary, and therefore comparatively silent, but with visual objects in motion when corresponding stimuli did not move. Such "apparent movements," we would now say, occur when several visual objects appear or disappear in certain temporal relations. Again in our present language, under these circumstances an interaction takes place which, for instance, makes a second object appear too near, or coincident with, a first object which is just disappearing, so that only when the first object, and therefore the interaction, really fades, the second object can move toward its normal position. If this is interaction, it does not, as such, occur on the perceptual scene. On this scene, we merely observe a movement. That movements of this kind do not correspond to real movements of the stimulus objects and must therefore be brought about by the sequence of the two objects, we can discover only by examining the physical situation. It follows that, if the seen movement is the perceptual result of an interaction, this interaction itself takes place outside the perceptual field. Thus, the apparent movement confirmed Wertheimer's more general suspicion: we cannot assume that the perceptual scene is an aggregate of unrelated elements because underlying processes are already functionally interrelated when that scene emerges, and now exhibits corresponding effects.

Wertheimer did not offer a more specific physiological explanation. At the time, this would have been impossible. He next turned to the problem of whether the characteristics of stationary perceptual fields are also influenced by interactions. I need not repeat how he investigated the formation of molar perceptual units, and more particularly of groups of such objects. Patterns which he used for this purpose are now reproduced in many textbooks. They clearly demonstrate that it is *relations* among visual objects which decide what objects become group members, and what others do not, and where, therefore, one group separates itself from another. This fact strongly suggests that perceptual groups are established by interactions; and, since a naive observer is merely aware of the result, the perceived groups, but not of their dependence upon particular relations, such interactions would again occur among the underlying processes rather than within the perceptual field.

Let me add a further remark about this early stage of the development. Surely, in those years, Gestalt psychologists were not satisfied with a quiet consideration of available facts. It seems that no major new trend in a science ever is. We were excited by what we found, and even more by the prospect of finding further revealing facts. Moreover, it was not only the stimulating newness of our enterprise which inspired us. There was also a great wave of relief -- as though we were escaping, from a prison. The prison was psychology as taught at the universities when we still were students. At the time, we had been shocked by the thesis that all psychological facts (not only those in perception) consist of unrelated inert atoms and that almost the only factors which combine these atoms and thus introduce action are associations formed under the influence of mere contiguity. What had disturbed us was the utter senselessness of this picture, and the implication that human life, apparently so colorful and so intensely dynamic, is actually a frightful bore. This was not true of our new picture, and we felt that further discoveries were bound to destroy, what was left of the old picture.

Soon further investigations, not all of them done by Gestalt psychologists, reinforced the new trend. Rubin called attention to the difference between figure and ground. David Katz found ample evidence for the role of Gestalt factors in the field of touch as well as in color vision, and

so forth. Why so much interest just in perception? Simply because in no other part of psychology are facts so readily accessible to observation. It was the hope of everybody that, once some major functional principles had been revealed in this part of psychology, similar principles would prove to be relevant to other parts, such as memory, learning, thinking, and motivation. In fact, Wertheimer and I undertook our early studies of intellectual processes precisely from this point of view; somewhat later, Kurt Lewin began his investigations of motivation which, in part , followed the same line; and we also applied the concept of *Gestaltung* or ,organization to memory, to learning, and to recall. With developments in America, Wertheimer's further analysis of thinking, Asch's and Heider's investigations in social psychology, our work on figural aftereffects, and eventually on currents Of the brain, we are probably all familiar.

In the meantime, unexpected support had come from natural science. To mention only one Point: Parts of molar perceptual units often have charac- [p. 729] teristics which they do not exhibit when separated from those units. Within a larger visual entity, a part may, for instance, be a corner of this entity, another part its contour or boundary, and so on. It now seems obvious; but nobody in psychology had seen it before: the same happens in any physical system that is pervaded by interactions. These interactions affect the parts of the system until, eventually, in a steady state, the characteristics of all parts are such that remaining interactions balance one another. Hence, if processes in the central nervous system follow the same rule, the dependence of local perceptual facts on conditions in larger entities could no longer be regarded as puzzling. Comparisons of this kind greatly encouraged the Gestalt psychologists.

In America, it may seem surprising that enthusiastic people such as the Gestalt psychologists were intensely interested in physics. Physics is generally assumed to be a particularly sober discipline. And yet, this happened to us most naturally. To be sure, our reasoning in physics involved no chan-es in the laws of physics and no new assumptions in this field. Nevertheless, when we compared our psychological findings with the behavior of certain physical systems, some parts of natural science began to look different. When reading the formulae of the physicist, one may emphasize this or that aspect of their content. The particular aspect of the formulae in which the Gestalt psychologists became interested had, for decades., been given little attention. No mistake had ever been made in applications of the formulae, because what now fascinated us had all the time been present in their mathematical form. Hence, all calculations in physics had come out right. But it does make a difference whether you make explicit what a formula implies or merely use it as a reliable tool. We had, therefore, good reasons for being, surprised by what we found; and we naturally felt elated when the new reading of the formulae told us that organization is as obvious in some parts of physics as it is in psychology.

Incidentally, others were no less interested in this "new reading" than we were. These other people were eminent physicists. Max Planck once told me that he expected our approach to clarify a difficult issue which had just arisen in quantum physics if not the concept of the quantum itself. Several Years later. Max Born, the great physicist who gave quantum mechanics its present form, made almost the same statement in one of his papers. And, only a few weeks ago, I read a paper in which Bridgman of Harvard interprets Heisenberg's famous principle in such terms that I am tempted to call him, Bridgman, a Gestalt physicist.

We will now return to psychology. More particularly, we will inspect the situation in which American psychology finds itself today. The spirit which we find here differs considerably from the one which characterized young Gestalt psychology. Let me try to formulate what members of this audience may have been thinking while I described that European enterprise. "Enthusiasm?" they probably thought. "Feelings of relief when certain assumptions were found less dreary than those of earlier psychologists in Europe? But this is an admission that emotional factors and extrascientific values played a part in Gestalt psychology. We know about the often pernicious effects of the emotions in ordinary life. How, then, could emotions be permitted to influence scientific judgments and thus to disturb the objectivity of research? As we see it, the true spirit of science is a critical spirit. Our main obligation as scientists is that of avoiding mistakes. Hence our emphasis on strict method in experimentation and on equally

strict procedures in the evaluation of results. The Gestalt psychologists seem to have been guilty of wishful thinking. Under the circumstances, were not some of their findings unreliable and some of their concepts vague?"

I will at once admit two facts. Almost from its beginning, American psychology has given more attention to questions of method and strict proof than Gestalt psychology did in those years. In this respect, American psychology was clearly superior. Secondly, sometimes the Gestalt psychologists did make mistakes. Not in all cases was the reliability of their findings up to American standards, and some concepts which they used were not immediately quite clear. I , myself once used a certain concept in a somewhat misleading fashion. I had better explain this.

What is insight? In its strict sense, the term refers to the fact that, when we are aware of a relation, of any relation, this relation is not experienced as a fact by itself, but rather as something that follows from the characteristics of the objects under consideration. Now, when primates try to solve a problem, their behavior often shows that they are aware of a certain important relation. But when they now make use of this "insight," and thus [p. 730] solve their problem, should this achievement be called *a solution by insight?* No -- it is by no means clear that it was also insight which made that particular relation *emerge*. In a given situation, we or a monkey may become aware of a great many relations. If, at a certain moment, we or a monkey attend to the right one, this may happen for several reasons, some entirely unrelated to insight. Consequently, it is misleading to call the whole process a "solution by insight."

This will be particularly obvious when the solution of the problem is arbitrarily chosen by the experimenter. Take Harlow's excellent experiments in which primates are expected to choose the odd item in a group of objects. "Oddity" is a particular relational fact. Once a monkey attends to it, he will perceive it with insight. But why should he do so during his first trials? His first choices will be determined by one factor or another, until he happens to attend, once or repeatedly, to the oddity relation just when he chooses (or does not choose) the right object. Gradually, he will now attend to this particular relation in all trials; and he may do so even when entirely new objects are shown. Surely, such a process should not simply be called "learning by insight." If Harlow were to say that, under the circumstances, it is learning of one kind or another which gives the right relation and corresponding insight their chance to operate, I should at once agree. What, I believe, the monkeys do not learn is insight into which object in a given group is the odd one; but they must learn to pay attention to the oddity factor in the first place. I hope that this will clarify matters. They have not always been so clear to me.

When the solution of a problem is not arbitrarily chosen by the experimenter, but more directly related to the nature of the given situation, insight may play a more important role. But, even under these circumstances, it is not insight alone which brings about the solution. The mere fact that solutions often emerge to the subjects' own surprise is clear proof that it cannot be insight alone which is responsible for their origin.

But I intended to discuss some trends in American psychology. May I confess that I do not fully approve of all these trends?

First, I doubt whether it is advisable to regard caution and a critical spirit as the virtues of a scientist, as though little else counted. They are necessary in research, just as the brakes in our cars must be kept in order and their windshields clean. But it is not because of the brakes or of the windshields that we drive. Similarly, caution and a critical spirit are like tools. They ought to be kept ready during a scientific enterprise; however, the main business of a science is gaining more and more new knowledge. I wonder why great men in physics do not call caution and a critical spirit the most important characteristics of their behavior. They seem to regard the testing of brakes and the cleaning of windshields as mere precautions, but to look forward to the next trip as the business for which they have cars. Why is it only in psychology that we hear the slightly discouraging, story of mere caution over and over again? Why are just psychologists so inclined to greet the announcement of a new fact (or a new working hypothesis) almost with scorn? This is caution that has gone sour and has almost become negativism -- which, of course, is no less an emotional attitude than is enthusiasm. The

enthusiasm of the early Gestalt psychologists was a virtue, because it led to new observations. But virtues, it has been said, tend to breed little accompanying vices. In their enthusiasm, the Gestalt psychologists were not always sufficiently careful.

In American psychology, it is rightly regarded as a virtue if a man feels great respect for method and for caution. But, if this virtue becomes too strong, it may bring forth a spirit of skepticism and thus prevent new work. Too many young psychologists, it seems to me, either work only against something done by others or merely vary slightly what others have done before; in other words, preoccupation with method may tend to limit the range of our research. We are, of course, after clear evidence. But not in all parts of psychology can evidence immediately be clear. In some, we cannot yet use our most exact methods. Where this happens, we hesitate to proceed. Experimentalists in particular tend to avoid work on new materials resistant to approved methods and to the immediate application of perfectly clear concepts. But concepts in a new field can only be clarified by work in this field. Should we limit our studies to areas already familiar from previous research? Obviously, would mean a kind of conservatism in psychology. When I was his student, Max Planck repeated this warning over and over again in his lectures. [p. 731]

Our wish to use only perfect methods and clear concepts has led to Methodological Behaviorism. Human experience in the phenomenological sense cannot yet be treated with our most reliable methods; and, when dealing with it, we may be forced to form new concepts which, at first, will often be a bit vague. Most experimentalists, therefore, refrain from observing, or even from referring to, the phenomenal scene. And yet, this is the scene on which, so far as the actors are concerned, the drama of ordinary human living is being played all the time. If we never study this scene, but insist on methods and concepts developed in research "from the outside," our results are likely to look strange to those who intensely live "'inside."

To be sure, in many respects, the graphs and tables obtained "from the outside" constitute a most satisfactory material; and, in animal psychology, we have no other material. But this material as such contains no direct evidence as to the processes by which it is brought about. In this respect it is a slightly defective, I am tempted to say, a meager, material. For it owes its particular clearness to the fact that the data from which the graphs and tables are derived are severely selected data. When subjects are told to say no more than "louder," "'softer," and perhaps "equal" in certain experiments, or when we merely count how many items they recall in others, then we can surely apply it precise statistical techniques to what they do. But, as a less attractive consequence, we never hear under these circumstances how they do the comparing in the first case and what happens when they try to recall in the second case.

Are such questions now to be ignored? After all, not all phenomenal experiences are entirely vague; this Scheerer has rightly emphasized. And, if many are not yet accessible to quantitative procedures, what of it? One of the most fascinating disciplines, developmental physiology, the science investigating the growth of an organism from one cell, seldom uses quantitative techniques. And yet, nobody can deny that its merely qualitative description of morphogenesis has extraordinary scientific value. In new fields, not only quantitative data are relevant. As to the initial vagueness of Concepts in a new field, I should like to add an historical remark. When the concept of energy was first introduced in physics, it was far from king a clear concept. For decades, its meaning could not be sharply distinguished from that of the term "force." And what did the physicists do? They worked and worked on it, until at last it did become perfectly clear. There is no other way of dealing with new, and therefore not yet perfect, concepts. Hence, if we refuse to study the phenomenal scene, because, here, few concepts are so far entirely clear, we thereby decide that this scene will never be investigated -- at least not by us, the psychologists.

Now, I had better return to Gestalt psychology. Let me try to show you how Gestalt psychology tends to work today by discussing, a more specific issue, an issue on which scores of American psychologists have worked for years. We shall thus be enabled to compare the way in which they approach this issue with the Gestalt psychologists' approach.

The issue in question refers to the concepts of conditioning and motivation. One school seems to regard conditioning as almost the process with which *the* psychologist has to deal. In a famous book with the general title *Principles of Behavior,* the late Clark Hull, then the most influential member of the school, actually dealt with little else -- although he often used other terms. He felt that even such facts as thinking, insight, intentions, striving, and value would eventually be explained by a consistent investigation of the various forms of conditioning. We are all familiar with the basic concepts of his theory. Hence I will say only a few words about it. When conditions in an animal's tissue deviate from an optimal level, a state of need is said to exist in this tissue. Such needs produce, or simply are, drives -- which means that they tend to cause actions in the nervous system, some more or less prescribed by inherited neural connections, others of a more random nature. Drives are also called motivations. None of these terms is to be understood in a phenomenological sense. They always refer to assumed states of the tissue. The main point is that, for biological reasons, states of need must, if possible, be reduced and that this may be achieved by certain responses of the organism to the given situation. In case first responses are of a random character, learning or conditioning will often select such responses as do reduce the needs in question. In a simple formulation, the well-known rule which governs such developments is as follows: when a response has repeatedly occurred in temporal contiguity with the neural effects of a certain stimulus, then this stimu- [p. 732] lus will tend to evoke the same response in the future -- provided the response has caused a reduction of the need. I will not define such further concepts as habit strength, reaction potential, afferent stimulus interaction, reactive inhibition, and so forth, because they will play no role in my discussion.

But one term seems to me particularly important. Many recent, and important, investigations are concerned with so-called "learned drives," an expression which has, of course, this meaning: if a neutral stimulus is repeatedly followed by conditions which cause a primary state of drive such as pain, and the corresponding fear, then the fear with its usual effects on behavior will gradually become connected with that neutral stimulus, so that the stimulus alone now evokes the fear and its overt consequences. Certain drives are therefore said to be "learnable" in the sense that they can be attached to facts which, as such, are not related to the drive and hence would originally not evoke corresponding responses.

Some experiments in the field of conditioning in general are most interesting. I will only discuss the concepts used in the interpretation of this work and the conclusions which it is said to justify.

To begin with these conclusions: They refer to certain human experiences which, if the conclusions were justified, would have to be regarded as strange delusions. I mean our cognitive experiences. Suppose somebody discovers by accident that, every time he subtracts the square of a given integer from the square of the next integer in the series, the result is an odd number. A more learned friend now explains to him why this is a necessary rule, undoubtedly valid beyond any tests ever done by a person. The explanation refers to simple relations and to relations among relations -- all readily understandable -- and the final outcome is convincing. Now, is the understanding of the relations involved to be explained in terms of conditioning? Nothing in conditioning seems to give us access to the psychological fact which I just called understanding; and, since an understanding of relations is essential to, all cognitive achievements, the same applies to the whole field.

Explanation of our intellectual life in terms of conditioning would simply mean: its reduction to the operations of an often most practical, but intrinsically blind, connection of mere facts. Promises that such an explanation will nevertheless be achieved cause in the present speaker a mild, incredulous horror. It is not the business of science to destroy evidence. Behaviorists would perhaps answer that arguments which refer to human thinking as an experience are irrelevant, because science is only concerned with facts observable from the outside, and therefore objective. This answer would hardly be acceptable. The Behaviorist's own objective observations are invariably observation of facts in his perceptual field. No other form of objective observation has ever been discovered. Consequently, the Behaviorist cannot, without giving more particular reasons, reject reference to other individual experiences merely because

they are such experiences.

Thus we are justified in considering a further example of human experience. A need or drive, we are sometimes told, is a motivation. I do not entirely agree with this statement for the following reasons. A need or drive, we remember, is supposed to be a particular state in the tissue. There is no indication in Hull's writings that such a state "points beyond itself" toward any objects -- although it may, of course, cause movements, or actions of glands. Now it is true that the same holds for certain needs as human experiences; because, when a need is felt, it does not always point toward an object, attainment of which would satisfy the need. At the time, no such object may be in sight: in fact, no such object may yet be known. But when the proper object appears, or becomes known. then the situation changes. For, now the subject feels attracted or (in certain instances) repelled by this object. In other words, an object may have characteristics which establish a dynamic relation between the subject and that object. According to common experience, it is this dynamic relation which makes the subject move toward, or away from, the object. We ought to use different terms for a mere need *per se* and the situation in which a subject is attracted or repelled by an object. Otherwise, the dynamic aspect of the latter situation might easily be ignored. I suggest that we reserve the term "motivation" for this dynamic situation. Here we are, of course, on familiar ground. Motivation as just described was Kurt Lewin's main concern in psychology. He clearly recognized the part which certain characteristics of an object play in establishing the dynamic relation between this object and the subject. He called such charac- [p. 733] teristics of objects *Aufforderungscharaktere,* a term which then became "valences" in English.

So far as I know, there are no valences in objects no attractions and no repulsions between objects and subjects in the Behaviorist's vocabulary. I am afraid that, in this fashion, he misses a point no only important in human experience but also relevant to what he regards as true science.

How would a Gestalt psychologist handle motivation in the present sense? He would be-in with the following psychological facts. I do not know up to what point Lewin would have accepted what I am now going to say. My facts are these: (*a*) In human experience, motivation is a dynamic vector, that is, a fact which has a direction and tends to cause a displacement in this direction. *(b)* Unless there are obstacles in the way, this direction coincides with an imaginary straight line drawn from the object to the subject. (*c*) The ,direction of the experienced vector is either that toward the object or away from it. In the first case, the vector tends to reduce the distance in question; 1 the second, to increase it. (*d*) The strength of both the need present in the subject and of the valence exhibited by the object can vary. Both in man and in animals it has been observed that, when the strength of the valence is low, this reduction can be compensated for by an increase of the need in the subject; and, conversely, that, when the need is lowered, an increase of the strength of the valence may compensate for this change when considering these simple statements, anybody familiar with the elements of physics will be reminded of the behavior of forces. *(a)* In physics, forces are dynamic vectors which tend to change distance between one thing (or event) and another. *(b)* Unless there are obstacles in the way, force operates along a straight line drawn from first object (or event) to the other. (c) The action in which a force operates is either that of attraction or of a repulsion of a reduction or of increase of the given distance. (*d*) The formula which the intensity of a force between two objects is given contains two terms which refer to the sizes of a decisive property (for instance, an electric charge) in one object and in the other. It is always the product of these two terms on which, to the formula, the intensity of the force depends. Consequently, a reduction of the crucial term on one side can be compensated for by an increase in the term on the other side.

We have just seen that the behavior of vectors motivational situations is the same as the behavior of forces in nature. Gestalt psychologists are, therefore, inclined to interpret motivation in terms of such forces or, rather, of forces which operate be between certain perceptual processes and processes another part of the brain, where a need may be physiologically represented. We have no time to discuss the question how cortical fields or forces would cause overt movements of the organism in the direction of these forces.

Now, not everybody likes the term "force." Its meaning, it has been said, has anthropomorphic connotations. But, in human psychology, we simply must use terms which -- if I may use this expressions -- "sound human." If we refused to do so, we would not do justice to our subject matter which (to a high degree) is human experience. To be sure, in physics, Heinrich Hertz once tried to do without the concept "force." He actually wrote a treatise on mechanics in which he avoided this term. And what happened? He had to populate the physical world with unobservable masses, introduced only in order to make their hidden presence substitute for the much simpler action of forces. Ever since that time, physicists have happily returned to the old concept "force," and nobody has ever been harmed by the fact.

The present reasoning leads to a conclusion which distinguishes this reasoning from the treatment of motivation in the Behaviorist's system. Clark Hull was a great admirer of science; but, to my knowledge, he hardly ever used the concepts characteristic of field physics. The fundamental distinction between physical facts which are scalars (that is, facts which have a magnitude but no direction) and vectors (which have both an intensity and a direction) played no decisive part in his theorizing. His main concepts were obviously meant to be scalars. There is no particular spatial direction in a habit strength, none in a reaction potential, and none even in what he called a drive state. Hence, the core of modern physics as developed by Faraday and Maxwell had no influence on his system. For this reason, and also because he refused to consider motivation as an experienced vector, he could not discover that the operations of motivation appear to be isomorphic with those of fields or forces in the brain.

But, if motivation is to be interpreted in this fashion, certain assumptions often made by Behav- [p. 734] iorists may no longer be acceptable. Take the concept of learned drives. As I understand this term, it means that learning can attach a drive state to a great variety of stimuli which, as such, are neutral facts. Now, so long as a drive is not regarded as a vector, this seems indeed quite possible. But, if the drive in Hull's sense is replaced by a motivational force which operates between a subject and some perceptual fact, no arbitrary connections of this kind can be established. For, now motivation becomes the experienced counterpart of a force in the brain, and this force depends entirely upon the relation between conditions in the subject and the characteristics of the perceived object. There can be no such force if the object is, and remains, a neutral object. Forces only operate between objects which have the right properties. Any example of a force in nature illustrates this fact.

How, then, are the observations to be explained which are now interpreted as a learning of drives? After all, some learning must be involved when an originally neutral object gradually begins to attract or repel a subject. >From the present point of view, only one explanation is possible. Supposing that the subject's need does not vary, learning must change the characteristics of the object, and thus transform it into an adequate motivation object. One instance would be what Tolman calls a sign Gestalt; in other words, the neutral object would become the signal for the appearance of something else which is a proper motivational object. This expected object would now be the object of the motivation. Or also, when a neutral object is often accompanied by facts which are natural motivational objects, the characteristics of such facts may gradually "creep into" the very appearance of the formerly neutral object and thus make it a proper motivational object. Years ago, comparative psychologists in England stressed the importance of such processes, to which they gave the name "assimilation." They regarded assimilation as a particularly effective form of an association. And is it not true that, as a consequence of learning, a coffin *looks* forbidding or sinister? I also know somebody to whom a bottle covered with dust and just brought up from the cellar *looks* most attractive.

As a further and particularly simple possibility, the subject might just learn more about the characteristics of the given object itself than he knew in the beginning; and the characteristics revealed by this learning might be such that now the same object fits a need. It seems to me that all these abilities ought to be considered before we accept the thesis that motivations in the present sense can be attached to actually neutral objects. Incidentally, similar changes of objects may also be responsible for the developments which Gordon Allport once regarded as evidence of "functional autonomy."

You will ask me whether my suggestions lead to any consequences in actual research. Most surely, they do. But, since I have lived so long in America, and have therefore gradually become a most cautious scientist, I am now preparing myself for the study of motivation by investigating, first of all, the action of dynamic vectors in simpler fields, such as cognition and perception. It is a most interesting occupation to compare motivational action with dynamic events in those other parts of psychology. When you do so, everything looks different, not only in perception but also in certain forms of learning. Specific work? There is, and will be more of it than I alone can possibly manage. Consequently, I need help. And where do I expect to find this help? I will tell you where.

The Behaviorist's premises, we remember, lead to certain expectations and experiments. What I have just said invites us to proceed in another direction. I suggest that, in this situation, we forget about schools. The Behaviorist is convinced that his functional concepts are those which we all ought to use. The Gestalt psychologist, who deals with a greater variety of both phenomenal and physical concepts, expects more from work based on such premises. Both parties feel that their procedures are scientifically sound. Why should we fight? Many experiments done by Behaviorists seem to me to be very good experiments. May I now ask the Behaviorists to regard the use of some phenomenal facts, and also of field physics, as perfectly permissible? If we were to agree on these points, we could, I am sure, do excellent work together. It would be an extraordinary experience -- and good for psychology.

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[1] Address of the President at the sixty-seventh Annual Convention of the American Psychological Association, Cincinnati, Ohio, September 6, 1959.

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