# Perception: An introduction to the *Gestalt-theorie*

By Kurt Koffka (1922)

# Perception: An introduction to the *Gestalt-theorie*

## Kurt Koffka (1922)

When it was suggested to me that I should write a general critical review of the work recently carried on in the field of perception, I saw an opportunity of introducing to American readers a movement in psychological thought which has developed in Germany during the last ten years. In 1912 Wertheimer stated for the first time the principles of a *Gestalt-Theorie* which has served as the starting point of a small number of German psychologists. Wherever this new method of thinking and working has come in touch with concrete problems, it has not only showed its efficiency, but has also brought to light startling and important facts, which, without the guidance of this theory, could not so easily have been discovered.

The *Gestalt-Theorie* is more than a theory of perception; it is even more than a mere psychological theory. Yet it originated in a study of perception, and the investigation of this topic has furnished the better part of the experimental work which has been done. Consequently, an introduction to this new theory can best be gained, perhaps, by a consideration of the facts of perception.

Since the new point of view has not yet won its way in Germany, it is but fair to state at the outset that the majority of German psychologists still stands aloof. However, much of the work done by other investigators contains results that find a place within the scope of our theory. Accordingly I shall refer to these results as well as to those secured by the *Gestalt*-psychologists proper; for I wish to demonstrate the comprehensiveness of our theory by showing how readily it embraces a number of facts hitherto but imperfectly explained. For the same reason I shall occasionally go farther back and refer to older investigations. On the other

hand, I cannot hope to give a complete survey of the work on perception, and I shall therefore select my facts with reference to my primary purpose.

Since my chief aim is to invite a consideration of the new theory, I shall try first of all to make my American readers understand what the theory purports to be. So far there exists no general presentation of the theory which marshals all the facts upon which it rests; indeed, the general field of psychology has not, as yet, been treated from this point of view. For this reason the understanding of the theory has met with serious difficulties, and numerous misunderstandings have occasioned a great deal of the disapprobation which the theory has met. And yet, a theory which has admittedly inspired so many successful investigations may surely, claim the right to be at least correctly understood.

My plan in detail is the following: After giving a short sketch of the chief concepts of current psychology as they present themselves to the mind

of a *Gestalt*-psychologist, I shall introduce the newer concepts by demonstrating how appropriate they are in the solution of a very old psychological problem. I shall then proceed by developing a fundamental distinction made by the new theory which is quite contrary to the traditional view, and I shall also show the wide application of this distinction. This is all I shall attempt in this paper. In a second one I shall hope to be able to review the rest of the experimental evidence in support of the theory which has been gained in the various fields of perception, such as movement, form, etc. The reader, therefore, will have the complete case before him only after reading the second paper. I have preferred to write the essay in English in order to avoid the misunderstandings which always result from translation; and Professor Ogden has kindly undertaken to correct my manuscript.

When I speak of perception in the following essay, I do not mean a specific psychical function; all I wish to denote by this term is the realm of

experiences which are not merely "imagined," "represented," or "thought of." Thus, I would call the desk at which I am now writing a perception, likewise the flavor of the tobacco I am now inhaling from my pipe, or the noise of the traffic in the street below my window. That is to say, I wish to use the term perception in a way that will exclude all theoretical prejudice; for it is my aim to propose a theory of these everyday perceptions which has been developed in Germany during the last ten years, and to contrast this theory with the traditional views of psychology. With this purpose in mind, I need **a** term that is quite neutral. In the current textbooks of psychology the term perception is used in a more specific sense, being opposed to sensation, as a more complex process. Here, indeed, is the clue to all the existing theories of perception which I shall consider in this introductory section, together with a glance at the fundamental principles of traditional psychology. Thus I find three concepts, involving three principles of psychological theory, in

every current psychological system. In some systems these are the only fundamental concepts, while in others they are supplemented by additional conceptions; but for a long time the adequacy of these three has been beyond dispute. The three concepts to which I refer are those of *sensation, association,* and *attention.* I shall formulate the theoretical principles based upon these concepts and indicate their import in a radical manner so as to lay bare the methods of thinking which have been employed in their use. I am fully aware, of course, that most, if not all, the writers on this subject have tried to modify the assertions which I am about to make; but I maintain, nevertheless, that in working out concrete problems these principles have been employed in the manner in which I shall state them.

# I

[1.] Sensation: All present or existential consciousness consists of a finite number of real, separable (though not necessarily separate) elements, each element corresponding to a definite stimulus[1] or to a special memory-residuum (see below). Since a conscious unit is thus taken to be a bundle of such elements, Wertheimer, in a recent paper on the foundations of our new theory, has introduced the name "bundle-hypothesis" for this conception (65). These elements, or rather,-some of them, are the sensations,[2] and it is the first task of psychology to find out their number and their properties.

The elements, once aroused in the form of sensations, may also be experienced in the form of images. The images are also accepted as elements or atoms of psychological textures and are distinguishable from sensations by certain characteristic properties. They are, however, very largely a dependent class, since every image

presupposes a corresponding sensation. Thus the concept of image though not identical with that of sensation, rests upon the same principle, namely, the bundle-hypothesis.

In accordance with the method by which sensations have been investigated, it has been necessary to refer to the stimulus-side in defining the principle which underlies this concept. More explicitly, this relation of the sensation to its stimulus is expressed by a generally accepted rule, termed by Köhler the "constancy-hypothesis" (34); that the sensation is a direct and definite function of the stimulus. Given a certain stimulus and a normal sense- organ, we know what sensation the subject must have, or rather, we know its intensity and quality, while its "clearness" or its "degree of consciousness" is dependent upon still another factor, namely, *attention.*

What the stimulus is to the sensation, the residuum is to the image. Since each separate sensation-element leaves behind it a separate

residuum, we have a vast number of these residua in our memory, each of which may be separately aroused, thus providing a certain independence of the original arrangement in which the sensations were experienced. This leads to the theory of the "association mixtures" *(associative Mischwirkungen)* propounded by G. E. Müller (44) andcarried to the extreme in a paper by Henning (14).

2. Association: Even under our first heading we have met with the concept of memory. According to current teaching, the chief working principle of memory is association, although the purest of associationists recognize that it is not the only principle. It may suffice to point out in this connection that Rosa Heine (12) concludes from experiments performed in G. E. Müller's laboratory, that recognition is not based upon association; for she failed to detect in recognition any trace of that retroactive inhibition which is so powerful a factor in all associative learning. Likewise, Müller himself, relying upon experiments by L. Schlüter (54)

acknowledges the possibility of reproduction by similarity. Yet, despite all this, association holds its position as the primary factor governing the coming and the going of our ideas, and the law of association is based upon the sensation-image concept. Our train of thought having been broken up into separate elements, the question is asked by what law does one element cause the appearance of another, and the answer is, association, the tie that forms between each element and all those other elements with which it has ever been in contiguity. As Wertheimer (65) again has pointed out, the core of this theory is this, that the necessary and sufficient cause for the formation and operation of an association is an original existential connection -- the mere coexistence of *a* and *b* gives to each a tendency to reproduce the other. Meaning, far from being regarded as one of the conditions of association, is explained by the working of associations, which in themselves are meaningless.

Another feature of this theory is its

statistical nature. At every moment, endless associations are working, reinforcing and inhibiting each other.[3] Since we can never have a complete survey of all the effective forces, it is impossible in any single case to make accurate prediction. As the special laws of association can be discovered by statistical methods only, so our predictions can be only statistical.

3. Attention: It is a recognized fact, that, clear and simple as association and sensation appear to be, there is a good deal of obscurity about the concept of attention.[4] And yet, wherever there is an effect that cannot be explained by sensation or association, there attention appears upon the stage. In more complex systems attention is the makeshift, or the scapegoat, if you will, which always interferes with the working out of these other principles. If the expected sensation does not follow when its appropriate stimulus is applied, attention to other contents must have caused it to pass unnoticed, or if a sensation does not properly correspond to the

stimulus applied, the attention must have been inadequate, thus leading us to make a false judgment. We meet with like instances over and over again which justify the following general statement, that attention must be added as a separate factor which not only influences the texture and the course of our conscious processes, but is also likely to be influenced by them.

Modern psychology has endeavored to give a physiological foundation to its psychological conceptions. Let us therefore glance at the physiological side of these three principles. The substratum of sensation (and image) is supposed to be the arousal of a separate circumscribed area of the cortex, while the substratum for association is the neural connection established between such areas. Again attention holds an ambiguous position, for some see its essence as a facilitation and some as an inhibition of the nervous processes. Without going more into detail, let us examine the nature of this psycho-physical correspondence.

Methodologically the physiological and the psychological aspects of these three principles are in perfect harmony; the cortex has been divided into areas, the immediate experience has been analyzed into elements, and connections are assumed to exist between brain areas as between the elements of consciousness. Furthermore, the nervous processes may be altered functionally and their corresponding psychological elements are subject to the functional factor of attention. Evidently the psychological and the physiological are interdependent, and are not sensation, association, and attention, factual? Do not cortical areas exist, and likewise nervous tracts, and the facilitation and inhibition of excitations? Certainly facts exist which have been interpreted in these ways, but we believe it can be proved that this interpretation is insufficient in the face of other and more comprehensive facts. Furthermore, we maintain that the insufficiency of the older theory cannot be remedied by supplementing the three principles, but that these must be sacrificed and

replaced by other principles. It is not a discovery of the *Gestalt-psychologie* that these three concepts are inadequate to cover the abundance of mental phenomena, for many others have held the same opinion, and some have even begun experimental work with this in mind. I need but mention v. Ehrenfels and the Meinong school as one instance, Külpe and the Würzburg school as another. But they all left the traditional concepts intact, and while trying to overcome the difficulties by the expedient of adding new concepts, they could not check the tendency involved in these new concepts to modify the old ones. I must, however, warn the reader not to confound the old term of *Gestalt-Qualität* with the term *Gestalt* as it is employed in the new theory. It was to avoid this very confusion that Wertheimer in his first paper avoided the term (64) and introduced a totally neutral expression for the perception of movement -- the phi-phenomenon.

Just a line at this point upon certain recent tendencies in American psychology. Behaviorism,

excluding as it does all forms of consciousness from its realm, strictly speaking denies the use of these three principles altogether. Therefore we do not find the terms attention and sensation in the behaviorist's writings, and even association has disappeared from the explanation in the sense of a tie that can be formed as an original act. And yet, as I have shown in a paper which discusses the fundamental differences between Wertheimer's theory and that of Meinong and Benussi (26), despite the restriction in his use of terms, the outfit of the Behaviorist is essentially the same as that of the traditional psychologist. He says "reaction" where the latter said "sensation," and in so doing includes the effector side of the process, but apart from this he builds his system in exactly the same manner, joining reflex arcs to reflex arcs entirely in accordance with the method of the "bundle-hypothesis."

However, I find a radical abandonment of this hypothesis in Rahn's monograph (52) and also

in a recent paper by Ogden (48). With both of these I can in large measure agree, and both of these writers, it seems to me, could readily assimilate the fundamental working principle of the *Gestalt-Psychologie.*

## II

In order to demonstrate the clash of the old and new methods of thinking, I have chosen a very elementary example, which I have discussed in a recent paper (30). No field of psychological research, perhaps, has been better clarified than that pertaining to the differential threshold and Weber's Law. Yet when we come to the theory, we are far from finding unanimity among psychologists. I need but recall to the reader's mind Stumpf's famous old argument (60) which, abbreviated, may be stated in the following form: It is always possible to produce three sensations, a, b, and c, so that a and b are judged equal, likewise b and c, whereas a and c are judged to be different (either a b or b a). Stumpf concludes that in reality a ǂ b and b ǂ c, that is to say, our judgments of equality were based upon our incapacity to *notice* very small yet actual differences, the consequence of this conclusion being that the differential threshold as measured by our methods appears to be a fact, not of sensation,

but of our capacity of perceiving. Others, such as Cornelius, Ebbinghaus, Titchener, have not been so ready to abandon the sensationalistic interpretation. The explanations of Ebbinghaus and Titchener may be summarized in the word "friction." The nervous excitation corresponding to sensation a has a certain amount of inertia, so that a second but slightly different stimulus is incapable of arousing a slightly different sensation, but only the first sensation *a*. If, however, we apply a stimulus that is considerably different, the inertia will be overcome, and a different sensation result. This, at the first flush, would appear to be a sufficient explanation, but for the following result: When we apply two slightly different stimuli a b a great number of times, we get four different kinds of judgment: (1) a equals b; (2) a b; (3) a < b; (4) uncertain. Now the " friction" theory, although it covers 2 and 4, does not explain case 3.

Two attempts have been made to overcome this difficulty. The first is G. E. Müller's theory of the

"chance-error" (43) which maintains that the final result of a stimulus is never the effect of this stimulus alone, for there are external or internal processes always at work to modify either the sensation itself or our apprehension of it. (In so far, Müller's theory is in harmony with Stumpf's unnoticed sensations.) Therefore it may well happen that though a b, a-DELTA < b+DELTA. According to Müller, one of the causes of these chance processes is attention.

To understand the second attempt, made by Cornelius (4), we must analyze the "friction" and the "chance-error" hypotheses in their interpretation of Stumpf's paradox. Stumpf introduced his "function of perceiving" in order to avoid a contradiction. If a=b and b=c, it is contradictory that a ǂ c. However, the whole argument rests upon a tacit assumption. We have three different stimuli in a b c. According to the classic theories a sensation corresponds to each of these; let us call them *a, b, c.* Now in reality we have also three different sets of experiments (or

groups of experiments): *a* compared with *b, b* compared with *c*, and *a* with *c*. Stumpf's contradiction arises only if a sensation is regarded as being a function of its stimulus alone, that is, if the constancy hypothesis holds in its strictest form. If, however, a sensation is also a function of the general experimental setting, then the contradiction disappears. Should stimulus a correspond in accordance with the special experiment to one of the sensations *a*1, *a*3, stimulus b to *b*1 *b*2, and stimulus c to *c*1, *c*2, then as a result of our experience we might have the following non-contradictory facts: *a*1=*b*1, *b*2=*c*2, *a*3 *c*3. Long ago this was pointed out by Cornelius and has been admitted, since, by Stumpf, who nevertheless maintains his position, *viz: a*l=*a*3, *b*1=*b*2, and *c*2=*c*3, because it seemed to him ever so much simpler than any other assumption.[5] Yet the "friction" and the "chance-error" theories both abandon Stumpf's position. Friction requires that *c*2, at least must be different from *c*3, and the chance-error theory, insofar as it

touches sensation and not apprehension merely, allows variability to all sensations. But both these theories strive to remain as close as possible to tile constancy-hypothesis, the latter even more so than the former; for according to it, the true stimulus always evokes the same sensation although additional processes may increase or decrease its effect.

Now Cornelius excludes the constancy-hypothesis from his theory. He assumes that to a single definite stimulus there corresponds, not a single definite sensation, but one of a number of several different ones (he denies also the continuity of the sensation-series). His theory therefore implies the general rule that sensation is not a function of the stimulus alone, and again it is attention that determines which of the many possible sensations will be aroused.

We have therefore a number of different explanations, which, however, apart from the role ascribed to attention, all possess one common

element: namely, they all start from the relation between a single stimulus and a single sensation, though this relation is modified by the friction-theory and still more by Cornelius. This modification, however. involves an addition of new factors, and accordingly we get a sum of different effects instead of a single effect.

Shall we then say that all in all the problem has been solved; that the minor differences of opinion are negligible? My answer is no, for with no one of the existing theories can we predict a single case. Therefore, if we accept them. we must either exclude single predictions altogether from our programme -- as chance can be only statistically predicted -- or we must await a discovery of the laws of attention, the outlook for which is not very hopeful when we consider how ill-defined the concept of attention now is.

Let us, therefore, try another method, and, returning to the simplest facts, without prepossession, look the data underlying all these

theories in the face. What is my experience when I say this gray is lighter than that, this line longer, or this sound louder, than that? The old theories assume without question that we are dealing with gray *a* and gray *b*, line *a* and line *b*, sound *a* and sound *b*. Whenever the bare existence of two sensations have seemed insufficient to explain a judgment of comparison, psychologists have searched, and not in vain, for other elements. Schumann (56) long ago attacked this problem, and was able to supplement the descriptive side of comparison, but he could only find what he was seeking, and it was in this way that he discovered the accessory impressions (*Nebeneindrücke*) -- those transitional sensations *(Übergangsempfindungem)* which have not yet ceased to play an important part in psychological theory. Other authors have turned to the *relations* as separate autonomous or dependent *(unselbständig)* elements,[6] and these again have been either rejected or reduced by the analysis of

other psychologists. Thus, current teaching has reached no agreement concerning the descriptive side of this problem.

Let us, therefore, turn to the experience itself. Upon a black cloth two squares of gray cardboard lie side by side. I am to judge whether or not they are of equal grayness. What is my experience? I can think of four different possibilities. (1) I see on a black surface one homogeneous gray oblong with a thin division line which organizes this oblong into two squares. For simplicity's sake we shall neglect this line, although it has varying aspects. (2) I see a pair of "brightness steps" ascending from left to right. This is a very definite experience with well-definable properties. just as in a real staircase the steps may have different heights, so my experience may be that of a steep or a moderate ascent. It may be well-balanced or ill-balanced, the latter *e.g.* when there is a middle gray on the left and a radiant white on the right. And it has two *steps*. This must be rightly understood. If I

say a real stair has two steps, I do not say there is one plank below and another plank above. I may find out later that the steps are planks, but originally I saw no planks, but only steps. Just so in my brightness steps: I see the darker left and the brighter right not as separate and independent pieces of color, but as steps, and as steps ascending from left to right. What does this mean? A plank is a plank anywhere and in any position; a step is a step only in its proper position in a scale. Again, a sensation of gray, for traditional psychology, may be a sensation of gray anywhere, but a gray step is a gray step only in a series of brightnesses. Scientific thought, concerned as it is with real things, has centered around concepts like "plank" and has neglected concepts like "step."[7] Consequently the assertion has become true without qualification that a "step" is a "plank". Psychology, although it is concerned with experiences, has invariably taken over this mode of procedure. But since the inadequacy occasioned by the neglect of the step-

concept is much more conspicuous in psychology than it is in physics, it is our science that first supplied the impulse to reconsider the case. And when we do reconsider, we see at once that the assertion "a sensation of gray is a sensation of gray anywhere" loses all meaning,[8] and that the assertion that a real step is a plank is true only with certain qualifications.

But our previous description must be still further supplemented, or, rather, amplified; for, speaking of "steps" I mean not only two different levels, but the rise itself, the upward trend and direction, which is not a separate, flighty, transitional sensation, but a central property of this whole undivided experience. Undivided does not mean uniform, for an undivided experience may be articulated and it may involve an immense richness of detail, yet this detail does not make of it a sum of many experiences. The direction upward or downward under certain conditions, *e.g.*, under brief exposure, may be the chief moment of the total

experience; in extreme cases, this direction may be present and nothing else, the plank-character of the steps having entirely vanished. In this connection I may refer to a result of Seifert's. He worked with tachistoscopically exposed figures that were composed either of full lines or of isolated dots. But this made no difference in the appearance of the total figure, and although Seifert accepts the distinction of a fundamental and a superstructure, he is constrained to acknowledge an "ungratefulness" towards the elements (57, p. 74). To return to our own case, we may say that the experience described as *direction* may be entirely dynamic, and that it is always partially so.[9] Let us now return to the remaining possible experiences which can arise in the comparison of two gray squares. (3) I see a pair of brightness-steps with the reverse direction. (4) 1 see neither the uniform oblong nor the steps, but something indefinite, vague, not tending towards uniformity, nor towards an ascending or descending step, since it never

quite consolidates itself.

It is evident what judgments will follow from each of these experiences: (1) Judgment of equality; (2) left darker (or right [ 542] brighter)[10]; (3) left brighter (right darker), and (4) uncertain. Thus the four types of judgment which we met previously are reduced to four different experiences.

While in the former passage we made the four types correspond to the same pair of (subliminally different) stimuli, we shall now consider cases in which typically different pairs of stimuli provoke these different judgments. What, then, follows theoretically from our pure description? We find that our description *explains* the comparison. Comparison is no longer a new act supervening upon the given sensations. The question how the two sensations can be compared no longer exists, because the two sensations themselves do not exist. What we find is an undivided, articulated whole. Let us call these

wholes "structures," and we can then assert that an unprejudiced description finds such structures in the cases underlying all psycho-physical experiments, but never any separate sensations.

Our theory finds confirmation in a crucial experiment, which shows, moreover, that these simple structures, far from being a peculiarity of the human species, are a very primitive form of reaction. As the question is put by Köhler (36), if an animal is confronted with two stimuli and is trained to react positively to the one and negatively to the other, what has it learned? The traditional theory would reply: the animal has formed a connection between the one sensation corresponding to the first stimulus and the positive reaction and likewise between the other sensation and the negative reaction; our theory, however, would say that the animal has learned to react to a certain structure. Köhler then introduced an experimental variation to solve the dilemma as to which explanation is the more apt. His method was as follows: b and c, one

lighter, the other darker, were placed before the animal, their spatial arrangement being varied. From the one, say b, food could be taken, but not from the other. The training was continued until the animal, in a fixed number of trials, invariably chose the positive b. Then this pair of stimuli was replaced by another pair a and b, a being lighter than b. According to the old theory the behavior of the animal should be as follows: Since it bas to choose between the well-known and positive b, connected by previous training with a positive movement, and a new and neutral a with which it has formed no connection at all, we should expect that in the majority of cases b would be chosen. From our theory, however, we should make a contrary prediction. Having learned to react positively to the higher step of a brightness-scale, the animal will do the same thing when confronted with a new pair, and choose a. The experiments were performed with fowls, chimpanzees and a three-year-old child, In the vast majority of cases gray a was chosen, while

further variations in the experiment indicated the reason for every b reaction. In exceptional cases the absolute factor, b, was dominant, though even then it could not be regarded a sensation in the traditional meaning of the term, but only as a structure of a kind to be discussed in the next section. As compared with the structural component the absolute factor as a cue to reaction has a very weak hold upon the memory, and with an increase of the time-interval between the training and the critical experiments, the number of a-choices was found to increase. The same problem was attacked with different sizes of objects and yielded the same results. The experiments were very carefully executed, all possible errors being excluded, while certain objections, which were nevertheless raised, have been set aside by subsequent tests (35, 37)-

Though the results of these experiments are unimpeachable, psychologists have not all been ready to accept Köhler's theory. Jaensch, for instance, who reported upon similar experiments

with fowls two years after Köhler's publication (21), turns to Schumann's transitional sensations for an explanation of his results, as do Bühler (2) and Lindworsky (41) in their criticism of Köhler's experiments. I have shown at some length in my book on mental development (33) that this attempt at an explanation is quite unsatisfactory, but here I must pass the matter over. Structures, then, are very elementary reactions, which phenomenally are not composed of constituent elements, their members being what they are by virtue of their "member-character," their place in the whole; their essential nature being derived from the whole whose members they are.

Here the argument may be anticipated that, in the analysis, parts must determine the whole; you lay the lighter gray at the left and you have a different brightness gradation than when you lay it at the right! But what does this argument really prove? Remember, you must not substitute your sensations for your stimuli. If you are careful not

to do this, your argument must be that the arrangement of the single stimuli determines the whole structure. But you have not proved that the part phenomena have determined the whole phenomenon; for it you react at all by way of a stepwise phenomenon its nature must depend, of course, upon the stimuli which provoked the reaction. Very good, you may say, but what is the advantage of this new way of describing simple experiences? It seems on the face of it so much more complicated, so much less systematic, than the old way. This, indeed, is a fundamental question. But it cannot be answered by argument, only by facts. It must be shown that in all fields as well as in the field of choice-training (*Wahldressuren)* this new description explains the facts of experience more easily and better than they can be explained by the traditional view.

Let us, therefore, turn back to our threshold-problem, and to Stumpf's paradox which is now easily solved, while the solution leads us to two

important laws of structure. With the two subliminally different stimuli, a and b, what will be the O.'s reaction? Most probably experience 1or 4; which of these two will depend upon circumstances. If the observer is not acting as the subject of a psychological experiment and is neither suspicious of deception nor otherwise prepared to look for the finest shades of difference, he will react with experience 1; which means that the structure corresponding to two very slightly different stimuli will be one of uniformity. Next you present the supraliminally different stimuli a and c and he will react with experience 2 or 3, as the case may be, that is, he will experience a true stepwise phenomenon. Mathematically, a plane surface can be defined as an aggregate of steps of infinitely small gradation; in mathematics, therefore, we can have a continual transition from steps to plane-surface. But not so in our experience, for here a plane is never a step nor is there any mediation between the two -- our experience 4 being neither a

step nor a plane but a very labile and indefinite experience. This means that if we neglect for the moment experience 4, we shall have either one of two totally distinct experiences, each of which is a "good" structure. A real ladder with steps one mm. high would not be a good ladder, and, excepting under artificial conditions, such scales do not as a rule exist in our experience nor in the real world either. If, on the other hand, the difference between two stimuli is too great to permit a plane-experience, then we shall have a good stepwise-phenomenon; loosely expressed the experienced difference is exaggerated as compared with the stimulus-difference, and this can be proved wherever we have organs that are adapted to reproduce the stimuli.

We can sum up these facts in two special laws of structure: the law of leveling or assimilation, and the law of emphasis. Later on we shall see that these are both special cases of a more general law.

From these two laws we can infer that the

"goodness" of the scale has also a maximum or upper limit. Therefore, with an increasing stimulus-difference the step-height-experience will become less and less emphasized until an indifference point is reached, where the objective and the phenomenal difference coincide. At this point the emphasis will be replaced by an assimilative leveling, since the phenomenal difference has become less than the real one. If in a real scale we raise the height of the steps more and more we come at length to the point where we no longer have a scale. Two planks at levels ten meters apart are no longer two steps, and the same thing may happen on the phenomenal side. From the chirping of a cricket to the thundering of a sixteen-inch gun there is no scale, for they cannot be compared in the same sense in which we compare two strokes of a hammer.

To complete our survey by answering some other questions, let us turn to *attention.* Attention influences the differential limen which is lowered by a high and raised by a low degree of attention. What

does this mean? (1) We see that assimilation is a less developed reaction than emphasis which demands special conditions and a special readiness on the part of the reacting organism. Accordingly, fatigue raises the threshold and reduces the efficiency of the organism. (2) What is it that a high degree of attention really does in such cases? I mentioned above that, under normal condition;, where we are not called upon to make comparisons, our reaction to subliminally different stimuli will be that of equality, whereas in psychophysical experiments equality-judgments are very rare, being aced by judgments of uncertainty, or even those of "greater," "smaller." So Fernberger (7) reports of a subject, who, in a series of twelve hundred judgments, did not judge a single pair to be equal. How is this difference of behavior explained? We may describe the facts by stating that judgments of equality or "level-experiences" which are descriptively clear are interfered with by experimental conditions, since these conditions

always favor some sort of emphasis. must therefore endeavor to find out the specific character of the experimental conditions. The O. has the task of comparing and judging, *i.e.*, of asserting a relation. So far we have not distinguished between the relational and the structural consciousness. This was in the interest of a simplification which must now be corrected to some extent. A pure stepwise phenomenon would lead us to a judgment of "crescendo" or "diminuendo," which, in accordance with the experience will refer to an undivided whole. The judgment "A is greater than B" presupposes a somewhat different experience, for the two steps of the scale are more prominent, more independent; they are not only steps in the scale but also its limiting platforms. Somehow, they stand apart and a greater "tension" between these two members of the whole is a consequence; a tension which does not exist at all in an assimilative phenomenon of the level-type. This, as Köhler (36) has pointed out, is, grossly speaking, our comparing experience. A

comparing attitude in itself will therefore tend to separate the two members by producing a tension, which decreases the chance that a phenomenon of the level-type will occur. This explains the preponderance of judgments of uncertainty over those of equality, in psychological experiments.

But the experimental attitude is often still more specialized. Even if we include judgments of equality and uncertainty under the same head, they may be remarkably rare. Fernberger (7) has clearly pointed out the reason for this in the subject's attitude which makes him tend toward a specific judgment of "greater or smaller." In the terms of our theory, the instruction facilitates the stepwise and impedes the assimilative phenomena. This can be experimentally proved, and Fernberger has demonstrated how one of Brown's experiments furnishes this proof. Brown impressed upon his subject that he ought to be able to find a difference, *i.e.*, he emphasized the stepwise attitude, and the result was that practically no equality-judgments

were made in a long series of experiments. Fernberger himself arranged the following experiment with lifted weights: One group of seven subjects was given the customary instruction which presumably facilitates the stepwise phenomenon, while another group of seven received different instructions in which the three categories "greater," "smaller," and "equal" received the same value. Fernberger gives no tables to show the frequency of these judgments, nor does he differentiate between equality- and uncertainty-judgments, which for our present purpose would have been very advantageous. but he calculates the intervals of uncertainty and finds that "the interval of uncertainty for group two is considerably more than half as large again as the first group" (page 541).

I may in this connection refer also to Washburn's experiments upon the effect of verbal suggestion in tactual space perception (63). She stimulated twice successively the same region of the volar side of the O.'s wrist with rubber-tipped

compass points which were always I5 mm. apart, the O. being instructed to compare the distance between the two points in two successive contacts. In one group of experiments the O. was told the distance would always be smaller or greater, while in a second group the possibility of equality was also included. The results show a marked rise in the number of equality judgments in the second group over the first. Out of eighty judgments only five were of equality in the first group, while there were twenty such in the second.

What can we make of these facts? They show that the organism's structural reaction to a pair of stimuli depends upon its attitude. If we generalize from all the data the attitude may be such as to favor either a stepwise or an assimilative structure (each to the detriment of the other), or it may be indifferently advantageous to either one. From a consideration of the stepwise attitude we can now draw the following conclusions: before the subject is confronted with the stimulus, the

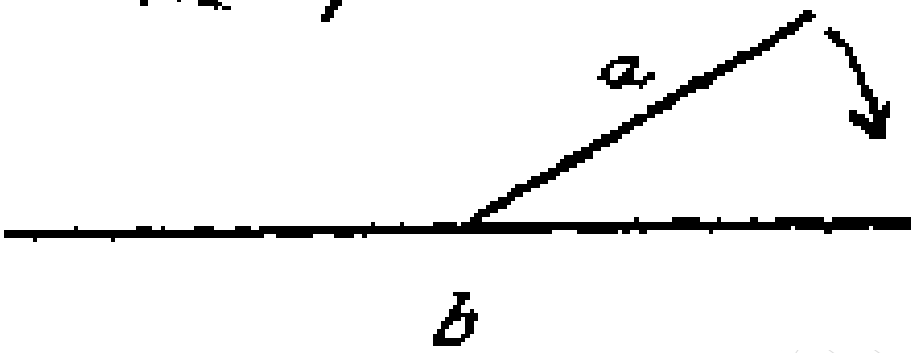
structure that eventually will ensue must be prepared for by a mental attitude, and this attitude consists mainly in a readiness to carry out a certain structural process. "Attitude" has now become a well-defined term as distinguished from "attention." It means that in entering a given situation the organism has in readiness certain modes of response, these modes being themselves what we have called " structures." Having such a process in readiness may be a mere nuisance, and it may not help the final response to the stimulus at all -- as when I am prepared for an ascending scale and receive stimuli that determine a descending one -- but the attitude may also be very effective. If a structural process is thus adequately prepared for, it may, come to its full effect under conditions which of themselves would have provoked a different structural process. This is a very important law, embracing as it does many of the facts imperfectly formulated by the ancient law of association. Take again the ascending scale attitude, with reference to

a pair of subliminally different stimuli a b. By themselves, these would provoke a structure of the level-type; now, however, they give rise to the ascending-scale phenomenon, a < b. In this way the typically false judgments are explained, or, at least, all those that cannot be explained by the absolute impression *(absoluter Eindruck*).

Thus we see that all chance means is that our customary experimental conditions leave room for an uncontrollable change of attitude inasmuch as they do not determine the status of the reacting organism. It is therefore an experimental task of the highest importance to fix the conditions so that they will also govern these attitudes.

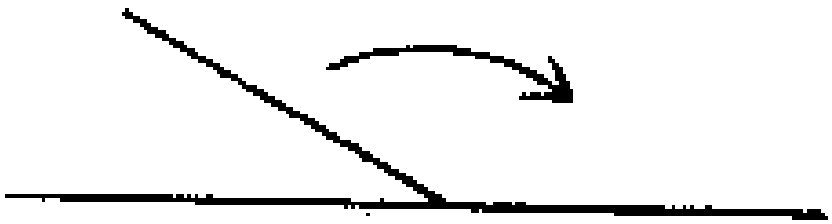
I owe the reader a proof of this general law, and I shall give it by a reference to two experiments of Wertheimer (64) which I have elsewhere considered from this point of view (31). In the tachistoscope Wertheimer exposes in succession, with a short interval them, the two lines, a and b, of Figure 1.

Fig. 1




The O. sees one line turning in the direction of the arrow. This experiment is repeated several times and then the position of line a is gradually changed, the angle between a and the right half of b becoming less and less acute until it is a right angle, and finally a more and more obtuse angles; let the direction of the turning movement remain constant, as indicated in the second figure.

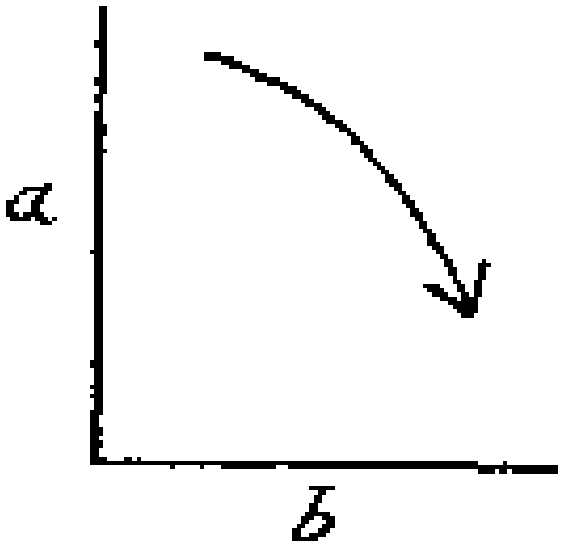
Fig. 2



Had the experiment been begun with the last pattern first, then, of course, the O. would have

seen the opposite movement. The effect is always produced by the O.'s attitude, and depends upon the strength of the original movement-structure. Again, expose a,b in the pattern Figure 3, and repeat it a great number of times. Then suddenly remove a so that b alone is exposed. What will the O. see? b resting in its true position? Not at all! The O. sees a line moving in the same direction as before only over a smaller angle, say like Figure 4, and if you repeat the exposure of b alone, at short intervals, this movement may persist several times, though each time the angle grows smaller. Now a single line like b exposed under no specific movement attitude will, of course, give rise to no experience of movement at all, yet in our last experiment the readiness of the movement-structure process is such that it can be

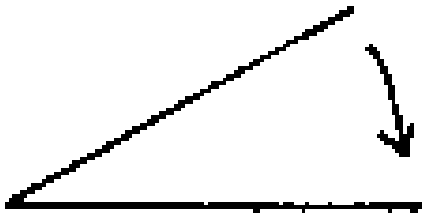
Fig. 3




touched off by a totally inadequate stimulus. This demonstrates the reality of structural preparations or anticipations. We find the same in cases of perseveration[11] and of suggestion. A striking example of the latter is found in the experiments of Edwards (5). Working in different sense-realms he employed the following method of experimentation: a stimulus was given and then gradually changed in some definite direction; the O. had to announce when he noticed the change, but in the suggestion-experiments he was always given a

false direction. So when a gray disc was darkened,

Fig. 4



he was instructed to give notice of tile first brightening. These suggestions were effective in a surprising number of cases, and the results are fully explained by our hypothesis.

With this concept of attitude as a readiness to carry out a structural process, we have explained a number of facts hitherto ascribed to attention; which means that we have been able to replace a non-specific, ill-defined cause by one which is both specific and well-defined. The explanation is also consistent with the rest of our theory, and this consistency of the descriptive with the functional concepts employed should not be overlooked. A

stepwise phenomenon, descriptively observed, and a stepwise process, functionally deduced, are thus brought into intimate connection. The structural process prepared by the attitude functions during the presence of a phenomenon as its physiological correlate, and this physiological hypothesis is determined by psychological observation; for we maintain that the physiological processes which underlie the structural phenomena must themselves possess the character of structures. This may seem to be a problem rather than a solution, but we shall presently see that even this problem has been successfully attacked.

No discussion of the differential threshold can pass by Weber's law; we shall therefore next consider the bearing of our theory upon this classic generalization. Although the theory of the Weber-Fechner law has long been controversial, we can now say that the physiological interpretation has won the field.[12] This supposes that the function connecting the stimulus with the nervous excitation

which underlies the sensation is the logarithmic factor. Since our theory abandons sensation, the usual interpretation of Weber's law must be remodeled; which again shows that we are not dealing with a mere change of names, but with a very active agent. In order to elucidate this part of our theory we must enter into certain details of physiological chemistry. Let us suppose, following Köhler's inferences (38, page 6 ff and 211 ff), that our entire field of vision is filled with a uniform gray, our whole optical sense-organ being homogeneously stimulated; we should then see a gray wall or nebula, but what may the process in our brain be like? Without entering the region of mere speculation, the following assertion can be made: the chemical reaction that will take place after we have become adapted to the stimulus will be a stationary one, that is, the concentration of all the substances concerned will be held constant during the whole time. It can further be shown that, owing to the chemical composition of our nervous system,

ions will take part in this process, so that a given degree of concentration would imply a definite amount of free ions. Let us now change our stimulus to one composed of two differently colored parts, say dark and light, meeting in an entirely arbitrary cue. Can the new process he fully described as two stationary processes corresponding to these two areas? An affirmative answer to this question would imply that no connection whatever exists between the two parts of the brain which are being differently excited, and since their border line was quite arbitrary it would also mean that each brain element is a miniature system insulated from each and every other element. This assumption is obviously untenable. Upon purely physicochemical grounds we must therefore conclude that between the two regions with their different concentrations, there must take place an adjustment (*Ausgleich*) of osmotic pressure, since with a certain concentration of substance there also belongs a certain concentration of ions. As ions must take part in this

process of diffusion and since different ions move with different velocities, there must arise, instantaneously, along the whole border line, a leap in the electrostatic potential. The absolute potential of each of the two areas is thus determined by the amount of this potential difference. It is not at all as though we had two areas independent of one another, each having its fixed potential, from which the potential difference arises. The opposite is true, since the fact of these two differently reacting areas coming together and forming one system is the cause for the arousal of the leap of potential and thereby determines the single potentials themselves. The term "potential difference" instead of misleading us, ought to furnish a striking analogy to our physiological stepwise phenomenon; for just as the step is a step only in a scale, so here each area has its potential only by virtue of the system in which it occurs, and just as the "upward (downward) direction " of the scale is a central property of the experience, so here the leap of potential is a central

factor of the optical function.

Let us go a bit further, and put this question: how does the potential difference PHI(1)-PHI(2) depend upon the two concentrations C1 and C2? From Nernst's theory of galvanic chains the following formula can be deduced: PHI(1)-PHI(2)=const. log. C2/C1, and this is precisely Fechner's formula for Weber's law.[13]

We can now state the structural theory of the Weber-Fechner law. The logarithmic law does not refer at all to single sensations, but to the whole structure; and from our deduction we must even infer that the concentration of ions in one area is a linear function of the intensity of the stimulus. Furthermore, what psychologists have called the process or function of comparing is not a third or "higher" factor accruing to the two sensations compared, but a moment inseparable from the whole structural system, which has been falsely singled out, just as the sensations have been falsely separated. In truth, comparison is always

determined by a system in which one step necessitates another.

The closest analogy in its essentials, even an identity, exists between our psychological description and our physics-chemical deduction, although the latter in no wise presupposes the former. We have, therefore, full justification for our previous assertion that the physiological process must also be structural, for the system of the two reacting areas with their potential difference is a true structure in the strictest sense. Von Ehrenfels, in his famous article (6) gave two criteria for his *Gestalt-qualitäten,* which, though imperfect, may be applied to our structures, both the psychological and the physiological. These criteria were (1) that structures cannot be composed out of elements, but (2) they can be transposed like melodies.

Our conception has now been further enlarged; for while our deductions are in no wise dependent upon physiological assumptions, they are found applicable to purely physicochemical facts. We

may therefore accept the fact that structures exist also in the realm of inorganic nature.

Before leaving the topic of the differential limen, I wish to mention a very interesting result from some experiments with lifted weights which Borak has recently published (1). Though his paper gives a mere statement of fact, and makes no reference to structural principles, it may be referred to here for two reasons. (1) The new fact puts a new problem before structural psychology, which, as I have reason to know, has been vigorously and successfully attacked. (2) It is very surprising that this fact has not been discovered before, since it ought to have appeared in almost any of the innumerable investigations made with the method of constant stimuli. The fact is the following: the sensibility to an increase in weight is greater than that of a decrease in weight, and, within certain limits, this difference increases with the time-interval between the two lifts. I quote the results from one of Borak's tables:

| Weights in Ascending Sequence | Number of Right Judgements | Weights in Descending Sequence | Number of Right Judgements |
|---|---|---|---|
| 400:420 | 16 | 420:400 | 2 |
| 400:430 | 27 | 430:400 | 10 |
| 400:440 | 30 | 440:400 | 16 |
| 400:450 | 33 | 450:400 | 20 |
| 400:460 | 45 | 460:400 | 35 |

n=50

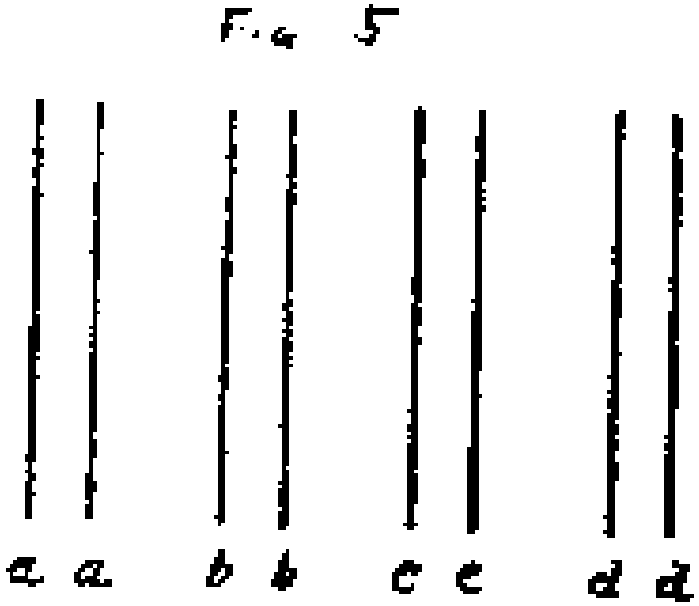
Both thresholds, the ascending and the descending, obey Weber's law.

## III

In the last section I have tried to give an impression of what "structure" means, descriptively and functionally. In this part of my essay I shall report a number of experiments performed in various fields, which show the fruitfulness of our conception. First of all, let us turn to a special structure of great significance. Keeping close to the discussion of the last section, I put this question: What are the phenomena which appear when we investigate an absolute threshold, say in the auditory field? Is it not correct to say in this connection that we try to find the smallest stimulus-energy that can give rise to a single sensation? Let us seek our answer in a pure description of the phenomenal data observable daring the course of the experiment. The O. sits in a noiseless room and awaits a faint sound. Is there anything auditory in his consciousness? The question would have appeared very different if we had chosen the visual field, for then the O. would be sitting in a dark room

waiting for a faint light, and darkness is admittedly a visual phenomenon. But is "stillness" auditory? Let the following rhythm be beaten: _ . . _ . . _ . . _ . ., do we hear anything between the dactyllic groups? Our question now appears to be more difficult, but my answer is that the intermetric intervals belong quite as much to the whole experience as do the intrametric intervals, only they belong to it in a different manner. Or take a visual analogy: In Figure 5 the intervals ab, bc, are different from the intervals aa, bb, cc, though both belong to the "fence-phenomenon." In trying to describe this difference we find one very, striking feature which we shall here single out. The white space; in the intervals ab, bc, cd, form part of the total white space, whereas the white spaces in the other intervals are limited to the regions between their respective black lines; they do not extend beyond these regions, nor do they form a part of the white space round about. Practised observers can even describe the curves that mark off these white

stripes, which are slightly convex toward the interior. We see, then, that the white surface of our pattern, though objectively the same throughout, gives rise to two different phenomena, one being limited to the "stripes," while the other comprises all the rest of the experience. We have two expressive terms to indicate this difference: we call




the one phenomenon a "figure" and the other its "ground"; on recognizing at once that no visual figure can occur without a ground upon which it appears.

Let us return now, to our auditory example.

The situation is very similar, for we have two kinds of intervals, the inter- and the intra-metric. Does our distinction apply here? Clearly the intrametric intervals belong to the rhythmic group itself, *i.e.*, to the "figure," but can we say that the intermetric ones belong to the "ground" in the same sense in which the intervals between the stripes constitute a visual ground ? My observation tells me that we can, and that there exists a groundin the auditory field as well as in the visual field, or in any other sensory field. This ground may be "stillness" or it may be the mixture of street-noises which, in a city, never cease during the day-time. And now mark this: When you leave the city for the country, and sit down to work at your desk, you may be startled by a strange phenomenon, for you may "hear" the stillness. The auditory ground of your work has altered and this alteration strikes you forcibly.

To show that this is not a description made up in accordance with a predetermined theory, I may quote an unprejudiced witness. At the

beginning of Ibsen's last play, "When We Dead Awaken," Mrs. Maja says, "Do listen how still it is here," and Professor Rubeck replies a little later, "One can, indeed, hear the stillness."

Returning to our threshold problem, we may therefore conclude that when the O. awaits the appearance of a faint sound, he is conscious not of auditory nothingness, but of an auditory ground; and what he is looking for is the appearance of an auditory figure, though in this case, because of its faintness, the figure may be ill-defined.

If we consult experimental procedure, this is strongly confirmed. In measuring auditory thresholds the chief consideration is not always to have the room as quiet as possible, but to have it as uniformly noisy as possible. If both postulates can be combined, well and good, but as a rule we are not able to exclude irregular outbursts of faint noises. Therefore, instead of keeping the room still, the experimenter fills it with a constant noise which is intensive enough to drown all irregular incoming

sounds; as, for instance, Peters has done (50). The O.'s task is then well defined. Upon this auditory ground he is instructed to await the appearance of a circumscribed noise-quality which does not belong to the ground.

An artificial ground has been created because a constant and uniform ground is a most important condition in testing absolute thresholds. But does not this mean a reduction of absolute to differential limens? Are not the objective conditions quite similar in the two cases-a constant stimulus, and a slightly greater test-stimulus? For just as I compare the weight N with the weight N plus , so here I compare the constant sound-intensity A with the slightly increased one (fall of a shot) A plus . This interpretation, however, misses the psychological point; for it overlooks the characteristic phenomenal difference between the two experiences. In absolute-threshold experiments we do not work with stepwise phenomena, as we do in differential limens, for our experience oscillates

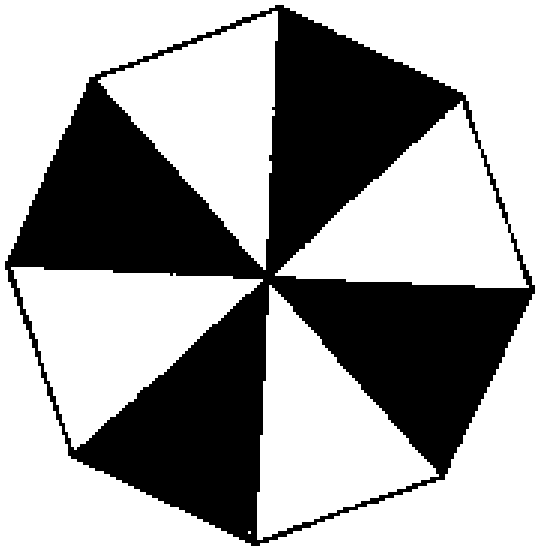
between one of a uniform ground alone, and one of a quality that stands out from the ground. Our assimilative phenomenon of the "level" which lies at the basis of all quality-judgments in the differential tests, is different from what we now call a pure ground experience. The "level" phenomenon is always experienced with a figure lying on a ground, and although the figure itself may be inarticulated, it is nevertheless distinct from its ground.

The difference between absolute and differential thresholds is therefore well-founded, and our principles of structure enable us to comprehend it fully. The distinction is also corroborated by experiments which indicate that the two function quite differently. Specht (59) has shown that alcohol lowers the absolute and raises the differential threshold, and we can infer from this a functional difference between the two structures-the one, a figure against a ground, and the other, a part against another part of a figure.

Having discovered this figure-ground-

structure in the absolute threshold, we must now consider it more closely. Let us revert to our fence-phenomenon. We found that the white intervals belonging to the figure were bounded, while those belonging to the ground were not, though objectively there was no border line in either case. Here we have a very general characteristic, namely, that the ground is always less "formed," less outlined, than the figure. Rubin (53) was the first to investigate these facts systematically,

FIG. 6



and the following statements are largely taken from his work. His method was peculiarly well-adapted to bring out the differences of figure and

ground, in employing geometrical patterns which are phenomenally equivocal as to their figure-ground structure. A simple example of such a pattern has already been discussed by Schumann (55). If we make the distances in our fence aa, bb, . . equal to ab, bc, . . we have a striking instance. For now bb may be a stripe, bc a piece of the ground, or inversely, bc may be a stripe, and bb a piece of the ground. In either case we find our old difference, that the stripes are always bounded, whether they are formed by bb or bc, while the intervals are not. Another example is offered by the so-called subjective rhythm, whether auditory or visual, which corresponds to an objectively equal series of beats or flashes.

In such a phenomenal series we again meet with the difference of inter- and intrametric intervals, and again their coordination with the objective intervals is ambiguous. The cross in Figure 6, reproduced from Rubin, may be experienced either as a white cross on a black ground, or as a

black cross on a white ground (neglecting other less important effects). Compare either cross with its ground and you can clearly recognize that the latter is always less definitely structured than the former; either the ground has no distinct shape at all, or else it approaches the comparatively simple form of a square.

Hand in hand with higher degrees of structure there goes a greater "liveliness" or vividness of the figure. As Schumann observed, the white space inside a figure is "whiter" than that outside, which can also be easily seen in the equidistant fence-design. A striking example of this is afforded by a certain kind of drawings, used frequently for advertising posters, where the contour is not fully drawn, but where, nevertheless, no gap appears in the figure. I may refer the reader to Jastrow's Editor, reproduced in Pillsbury's textbook (51, p. 158).

These last examples show what has already been pointed out, namely, that phenomenal figures

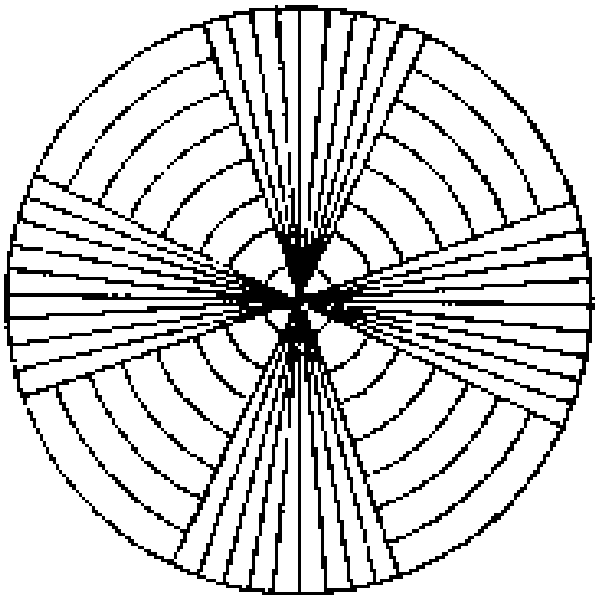
have boundary lines even when the corresponding objective figures have none. A good figure is always a "closed" figure, which the boundary line has the function of closing. So this line, separating the fields of figure and ground, has a very different relation to each of these, for though it bounds the figure, it does not bound the ground. The ground is unaffected by the contour and is partly hidden by the figure, yet it lies without interruption behind the figure. The cross of the accompanying figure (Figure 7) will make this description clear. Look at the fields with the arcs for filling. When forming a cross, these become true arcs, *i.e.*, cut-off pieces of circles, but when forming the ground they look quite differently, for they are no longer cut off, becoming now the visible parts of a phenomenal series of complete circles.

This property of the ground, that the figure's contour does not affect it, is closely related to the first characteristic we mentioned, namely, its lesser degree of structure. In our last instance this fact is

revealed by the observation that the whole circles when they constitute a ground are simpler structures than the arcs which are necessary to the formation of the cross; for in place of each single circle there appear four arcs. The lesser degree of structure leads also to another indication noted by Rubin of the difference between ground and figure: the ground has more of a substance- and the figure more of a thing-character.

Let us return to the boundary line. From its variable relation to figure and ground there follows the inference that it must have two different sides, an inside and an outside; the one includes, the other excludes, or to use terms in this more general sense which have been suggested by v. Hornbostel (19), the one is concave, and the other is convex. Though these words are not psychological terms they are meant to indicate true psychological descriptions. Look at the left line-b in our fence-figure and you will understand what is meant by this description, for its left side is hard and

Fig. 9



repelling, whereas its right side is soft and yielding. Very full descriptions of these properties are given by v. Hornbostel who reduces the illusions of reversible perspective to a change in these properties: to reverse a figure is to make concave what was convex, and convex what was concave.

One remark here to the reader who may raise the objection that our terms do not designate the existential properties of visual phenomena, but only their intentional meanings. I have said that I wished to point out true properties. Now consider that these properties need not be like those of

traditional psychology, "dead" attributes, possessing a "so-being" only, but that many of them are alive and active, possessing a "so-functioning." A beam of wood, lying unused on the floor, may look like a beam carrying weight, yet an accurate description would have to note this fact by giving heed to the state of tension which must then exist. More generally speaking, a state of rest with an absence of force is different from a state of rest with an equilibrium of force, and the same thing holds true, in the writer's opinion, for phenomena. The border line of a figure performs a function. and this performance is one of its visual properties. Traditional psychology has defined the term "visual property" so as to include "dead" properties only. Consequently in looking for visual properties it has found only these. But this definition was arbitrary, and it proves to be inadequate, since it makes the investigator blind to facts of the highest significance.

Lest the reader should be inclined to consider the distinction hitherto offered as trivial,

artificial, and secondary, we may turn to experiments with ambiguous patterns, where the different structures

FIG. 8

correspond to two totally different forms, whereas in the previous examples the same form, a fence or a cross, appeared in both cases. Well-known puzzle pictures fall under this head, one of which is produced by Titchener (61, p. 278) -- a brain with fissures which assimilate as babies, while another example is given in Pillsbury's book (51, p. 162) as a duck's or a rabbit's head. The best example of this which I know was used by Rubin. It is a goblet, whose contours also form the profiles of two faces. Many similar patterns were employed by Martin (42). We need not, however, search for

examples, since everyday life supplies us with any number of them. The simplest, perhaps, is an ordinary chessboard pattern, where at least six different phenomena may be aroused, and many others are frequently found in lace or wallpaper designs. Figure 8 is reproduced from the edging of a table cloth. You can see either the black T-shaped forms or the white leaves. On the actual frieze it is hardly possible to see both at the same time, though in our sample this is easier. Whenever you see one of the figures only, the remainder becomes a ground of the simplest possible description.

This difference has not escaped the psychologist, but has been discussed at length. The clearest statement is given by Titchener (61), whose report I shall closely follow in my interpretation. He would say that in the beginning the black T's are at the upper level of consciousness, while the rest is at a lower level. Suddenly a change takes place, the T's drop clear away from the upper level, and the white leaves stand out with all imaginable clearness, while

the form of the T's is no clearer than the feel of the book in your hand. Had he written the last sentence only there would be no disagreement between us, for the "feel of the book" belongs truly to the "ground" of the whole situation. But what he does say leaves the existence of the T-phenomenon untouched by the change in its phenomenal aspect. It has merely shifted its level, having dropped from the crest of attention to its base, from whence the leaves have now risen.

In objecting to this interpretation (which has also been vigorously attacked by Rahn), and at the same time arguing against Wundt, Rubin states most emphatically that when the T's have disappeared and we see in their place a mere ground, the T's have indeed no clearness at all, for they have become nonexistent.

In Titchener's report we recognize the typical attempt of traditional psychology to elucidate phenomena by means of the cardinal concepts stated at the beginning of this paper. Something

which ought to be there phenomenally, since a corresponding stimulus does exist, is not observable, and this contradiction is overcome with the aid of attention. Yet this is no longer a description of fact, but a hypothetical interpretation.[14] For I can describe only what I can observe, what is there before me, and to say that a figure is at so low a level of consciousness that it is not observable is not a description of what is present, even though in the next moment I can reëxperience what at the time was nonexistent. If I wish to describe truly I must report positively what that part of the total phenomenon looks like which lies at the so-called basis of attention; for it is not a description of it to tell how it does not look.

To infer how something looks when it is not observable from the data of its appearance when at the crest of the attention-wave, meansthe acceptance of the constancy-hypothesis and a final abandonment of every effort to obtain a factual verification. As Köhler has pointed out (34), if we

stand by description proper, *i.e.*, by verifiable description, we must recognize that the T's have ceased to exist the moment we see the leaves, and that the T-phenomenon has been replaced by a totally different ground-phenomenon, which corresponds to the same part of the stimulus-complex. We see now what an enormous change has been effected when a figure "emerges" from its ground. Rubin gives a striking description of the shock of surprise felt again and again in such a transition, even when he tried to imagine in advance what the new phenomenon would be like.

We have seen how the concept of attention has prevented the recognition and vitiated the pure description of a very marked phenomenal difference. Yet a connection exists between the figure-ground consciousness and the attention, so-called. But by observing the facts, what we find is a functional dependency, instead of a descriptive identity. As a rule the figure is the outstanding kernel of the whole experience. Whenever I give attention to a particular

part of a field, this part appears in the figure-character. have frequently performed the following classroom experiment: using a photographic shutter I project Figure 8 for a short time upon the wall, and instruct beforehand one-half the audience to watch the white, and one-half the black parts of the picture. I then ask the whole audience to make sketches of what they have seen. Invariably the "black" half of the audience draws the T's, and the "white" half the leaves.

Is it possible to describe the attitude of the observer which is produced by the instruction to "watch"? Again we may refer to v. Hornbostel's inversion experiments. He finds that it is more difficult to invert the convex into the concave than the concave into the convex, because whatever I am looking at, watching, acting upon, stands forth grows fixed, becomes an object, while the rest recedes, grows empty, and becomes the ground. He also adds that since the objects obtrude themselves upon me, and come toward me, it is *they* I notice

and watch rather than the holes between them. (19, p. 154.) We need only to apply this general description to our special case, and we shall see that attention has now a very definite meaning; for, in attending to the black parts, we adopt a "figure attitude" toward them by making them the center of our interest. At the same time, the part that has become the figure itself strives to become the center of our experience. This notion of the "center" will play an important part in the later expositions of our theory; here we have simply replaced the vague concept of attention with one which is well-defined.

The functional connection of figure- and center-consciousness is not absolute. Though it is natural to "attend" to the figure we can, for a time, at least, attend to the ground, and let the figure recede. If we continue this attitude too long, however, we run the risk of a change in the phenomenon; but that such an attitude is possible -- and many observations reported in the foregoing prove that it is -- demonstrates that the figure-

ground distinction cannot be identified with a mere difference of the attention-level.

All good psychological descriptions must find their justification in functional facts. Phenomena that are different in description must also prove to be different in function, if the description is tenable.

Fig. 9

So we turn to the functional facts which underlie the figure-ground distinction.

Two sets of experiments have been performed by Rubin, both employing patterns of the type of Figure 9. These patterns are ambiguous, either the enclosed white space or the enclosing black space may appear as the figure. Let us call the

first the positive, the second the negative reaction. According to the instructions given, it is possible for the O.'s to assume either a positive or a negative attitude before the exposure of the pattern. After some practice the attitude assumed will in most cases be effective, *i.e.,* a positive reaction will ensue from a positive attitude, and *vice versa*. In his first series of experiments, Rubin presented a number of such patterns with either positive or negative instructions. After a certain interval the experiment was repeated with instructions prescribing an indifferent attitude, neither positive nor negative. The result was that in the majority of cases a pattern once reacted to in a certain manner was reacted to the next time in the same manner. Rubin calls this a "figural after-effect" *(figurale Nachwikung)*. It proves that the structure by which we react to a given stimulus-complex remains in the memory of the individual, a fact of paramount importance for the theory of learning, as I have elsewhere (33) shown. The problem of the second

series was to find out if a pattern seen the first time under one attitude, positive or negative, will be recognized when it is seen the second time under the reversed attitude. The procedure was similar to that of the previous experiment, except that the instruction of the test-series was either positive or negative. The result was in full accordance with the descriptive distinction, for when the reverse instruction was effective no recognition took place. By overlooking this fact many troublesome mistakes are committed even in everyday life.

We have assigned to the figure a "thing"-character, and to its ground a "substance"-character. This description has also been justified by experiments, for we learn from Gelb's investigation (11) that the color-constancy commonly called memory-color is dependent upon the color's "thing"-character and not upon its "surface"-character. This was clearly proved by two patients with brain lesions who saw no surface-colors (*Oberflächenfarben*) and yet they made the same brightness-equations

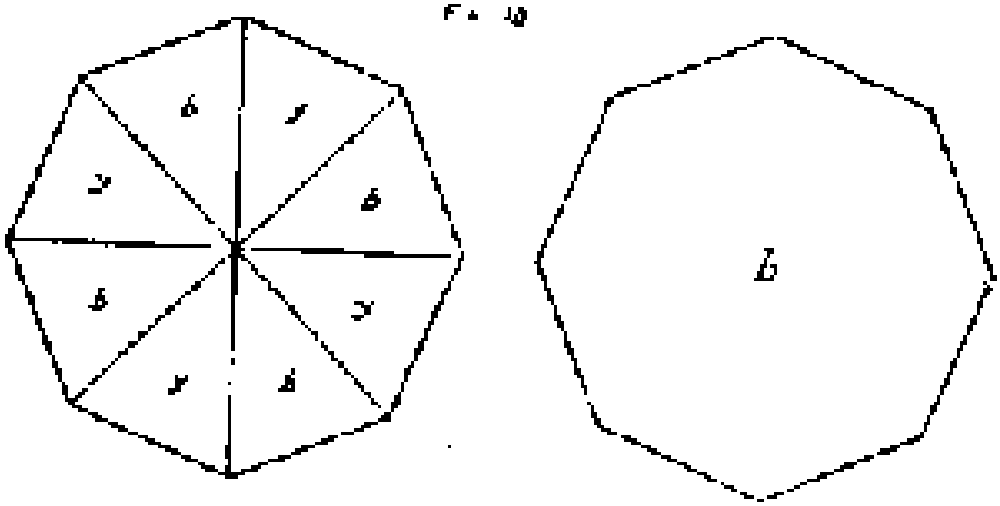
between a lighted and an unlighted color as did normal O.'s. They reacted differently only in case of a shadow, and this was because their visual apprehension was not sufficiently restored to enable them to recognize a dark spot as a shadow cast upon an object.

Before Gelb's paper had been published the connection between color-constancy and "thing"-character was suggested to Rubin by the researches of Katz (24), and Rubin concluded that because of this connection the figure-ground difference ought also to appear when the color-constancy is altered. To test this conclusion he planned two ingenious experiments. In the shadow-experiment he used a cross of the type of Figure 6, and cast a light shadow upon one of the white sectors. His O.'s reported this shading to be stronger when the white sector was part of the ground than when it was part of the cross. In the color experiment the cross was colored and observed through differently colored glasses. The result was again that the figure offered

a stronger resistance to change of color than did the ground.

Starting from the greater vividness of the figure as described, I devised the following experiment (32). I tested the power of figure and ground to resist so-called retinal rivalry. On the left side of a stereoscope I put a Rubin cross like that of Figure 6, composed of alternate blue and yellow sectors, while on the right side there was a regular blue octagon of homogeneous surface (comp. Figure 10). The left cross can appear either as a blue cross on a yellow ground, or as a yellow cross on a blue ground and in looking through the stereoscope it is easy to see either, since the left image, with its richer detail is superior in rivalry to the right image. Beginning with the yellow cross which is a very stable phenomenon, you can accentuate the right image by moving it, or by pointing at it with a pencil, without disturbing the yellow cross. But let the blue cross on the yellow field involuntarily appear, and then accentuate the right image but

slightly and the cross will disappear as the blue octagon emerges. The



explanation is simple enough- There is a constant rivalry between the yellow sectors on the left and the corresponding blue space on the right, yet so long as the yellow forms the figure in the left image the structure is so strong and so fixed that it resists attack. When the yellow is ground, however, it is but loosely formed and can therefore b easily defeated by the right image. So the better formed the field is, the more vivid and more impressive *(eindringlich)* it will be, a fact which has been theoretically explained by Köhler (38, p. 206f). Discussing the electrical processes occurring in the

optical system during stimulation, and making the well-founded assumption that the entire optical sector, periphery, optical tract, and cortical area together form one system, Köhler comes to the conclusion that the density of energy is always much greater in the figure-field than it is in the ground-field, and that the current (*Strömung )* is much more concentrated in the former than in the latter. It is this condition of energy which helps figures to attain their phenomenal vividness, and also, as we say after our last experiment, their superiority in rivalry.

Phenomenally, the figure is always a stronger and more resistant structure than the ground, and in extreme cases the ground may be almost formless, a mere background. For this distinction we have also found a functional counterpart. Kenkel (25) has discovered that figures, when briefly exposed, appear with specific movements which expand with their appearance and contract with their disappearance. I have (27)

advanced the hypothesis that this movement, called by Kenkel the *gamma movement*, is the expression of a structural process. This hypothesis has been tested and proved by an investigation of Lindemann (28) which will be more explicitly discussed in a later article. However, one experiment of this investigation belongs in the present context. Lindemann worked also with patterns that were ambiguous in their figure-ground structure. His figures were of the type of Figure 9 and of the goblet pattern described above. If Figure 9 is positively apprehended the O. sees violent outward movements of the white teeth, whereas, if observed negatively, the black indentures, particularly the lower claw-like one, move vigorously inwards. The goblet pattern behaves similarly. If the goblet is seen, it performs extensive expansions and contractions, whereas, if the profiles appear they tend toward one another, the direction of the movement being reversed, but, on account of the close proximity of these two structures movement is

in this case notably checked. These experiments show that the gamma movement takes place in the figure and not in the ground, and since they reveal a constructing process, they prove that functionally the figure is better formed than the ground.

I shall repeat here another experiment performed in the Giessen Psychological Laboratory, which has not yet been published. Hartmann (29) has investigated the laws governing the fusion of two stimuli separated by a dark interval. The O. looked through a telescope, or in most of the experiments through a blackened tube, behind which the Schumann tachistoscope was rotating. In the rim of the wheel there were two slits, separated by a variable interval. Behind the wheel was the object which in this procedure was twice exposed during one revolution. The objects were transparent figures getting their light from the rear. In accordance with the facts known about the Talbot fusion (for instance, rotating discs), Hartmann found that the critical speed of the wheel was a

direct function of the intensity of the stimulus which could easily be regulated by varying the amount of light passing through the exposed objects. By "critical" speed is meant that speed which is just capable of bringing about a complete fusion, after the last bit of flicker has disappeared. Hartmann then worked with Figure 6 as one of his objects, and he found a marked difference in the critical speed for the two phenomena, black cross on white ground and white cross on black ground. For instance, the time of revolution in the first case was 1.65 seconds while in the second case it was 1.3 seconds. Now the black sectors are no blacker than the dark interval, hence the flicker is produced by the white sectors alone; consequently the same field fuses under Hartmann's conditions more easily when it is a ground than when it is a figure. This proves again the close connection between construction or "formedness" and vividness or intensity. And this proof seems all the more convincing because it is based upon an effect which has hitherto been

considered a purely physiological process of the retina. Besides, this experiment is not only qualitative, it is also quantitative, since the difference of critical speed for the two different phenomena corresponding to the same stimulus-intensity can be matched with another difference in critical speed between two corresponding phenomena (black vs. white cross) with different stimulus-intensities.

I believe that the functional facts I have adduced are sufficient to prove the essential difference between the figure and ground phenomena. This difference is fundamental and the figure-ground structure must therefore be considered one of the most primitive of all structures. I have (33) defended the view that this structure is also the first phenomenon experienced by the human infant; for instance a light patch on a dark ground instead of the various sensations with which, according to the traditional view, the baby's consciousness is supposed to be filled. This genetic

consideration raises still another question. We have said that a figure cannot exist without a ground. Can a ground exist without a figure? In another connection (33, p. 97) I have tried to prove that it cannot, and that mere ground would be equivalent to no consciousness at all.

So far our observations have shown a superiority of the figure-phenomenon over the ground-phenomenon. This, however, must not lead us to disregard the latter, for the ground has a very important function of its own; it serves as a general level *(niveau)* upon which the figure appears. Now figure and ground together form a structure, consequently the former cannot be independent of the latter. On the contrary the quality of the figure must be very largely determined by the general level upon which it appears. This is a universal fact, observed in such products of culture as fashion and style. The same dress which is not only smart, but nice to look at, almost a thing of beauty, may become intolerable after the mode has passed.

Again, put a heavy modern leather club-chair into a rococo salon and the effect will be hideous. Music offers any number of examples as to the influence exerted by the general level. Each tone, each harmony, has a specific meaning, inherent in its "sound" for a given key only; but this meaning changes with the key, so that G is the tonic of G major, but the dominant of C major.

The influence of the ground appears in many psychological experiments. As Hering (15) has shown, the question of the functional dependence of the brightness of a gray upon the amount of light reflected into our eyes is unanswerable because it is incompletely stated. To solve this problem we must determine the general level. If we allow the level to vary the same amount of light in the figure may arouse a black, a gray or a white, as can easily be proved by Hering's "hole method," and the same is true if we take color into consideration. Witness the following experiment with Hering's hole method which I have often used as demonstration in a

classroom. Put a white screen (of about 50 x 50 cm.) with a hole in it of about 5 cm. diameter before a white wall. Put one or two ordinary electric lamps between screen and wall so that they throw their reddish-yellowish light on the wall, and close the shutters of the room. The wall will look fairly white, so will the hole in the screen so long as you perceive it as part of the wall seen through the hole in the screen. But you can also see the hole as part of the screen, which then becomes its ground; in which case the hole will seem to protrude somewhat from the screen and will have a distinct yellow tinge. Now throw on the screen white light from the arc-lamp of a projection lantern and arrange the intensities so that the amount of light upon the wall and upon the screen is approximately equal. The filling of the hole is then forced into the plane of the screen and has a fairly saturated color of a warm reddish-yellow. It is much more colored than it was before, while the screen now looks to be a light gray with a slight blue-greenish tint. Cover and uncover alternately

the objective lens of the lantern and you can easily observe a great change in the hole's color. Now fixate the wall, looking right under the lower edge of the screen. Again open and close your lantern. In this case you will see the fairly white wall suffering but very little change, whereas when the screen is lit up with a clear blue-green color, the hole becomes invisible.

You may object that this experiment involves a combination of memory-color and contrast. But in the first place, Jaensch has proved that these are not two different effects, but special cases of one and the same law; in the second place these terms are not an explanation of the phenomena and the facts mentioned do not readily submit to the current theories of contrast.[15]

Let us describe the facts by means of our level-concept. Consider that objectively the filling of the hole is but slightly altered by the turning on of the lantern light, which only causes it to grow a little whiter. Since this effect is opposed to the

phenomenon we have described, we may for simplicity neglect it altogether. But why does the "white" wall, when illumined by the yellowish lamp, still look fairly white? There is but one sort of light in the room, excepting the traces of daylight that are not excluded by the shutters.

The light-level of the room is therefore solely determined by the lamps, and the lighted part of the room is homogeneously colored. Let us now make the assumption that every general color-level tends to look white, that, in other words, white (including gray and black) is the, characteristic level-color. This will explain our fact. Now, as to the hole in the dark screen: it remains white when it appears as part of the wall, for it then belongs to this general level. But, if it appears on the screen it lies at the screen's level, and since the screen reflects no lamplight but only certain traces of daylight, the screen will therefore look almost black- (white-level). As a consequence, the hole as a figure upon this ground, reflecting a light which is different from its ground,

can no longer retain the same color; accordingly it appears yellowish. The color-effect is not very marked because of the great difference in brightness between the ground and the figure. The explanation for this, which also involves a law of structure, will be given in the following article.

When the screen is illuminated by the white lantern light, it forms a pronounced level, and since by the conditions of the experiment the brightness-difference between ground and figure has been decreased, the figure now appears to have the color of the lamps behind the screen. The screen, reflecting white light only, does not look like a pure gray, but being much smaller than the wall, it is therefore influenced by the wall's general level, as the wall is also influenced by the "level" of the screen. Therefore the screen looks slightly blue-greenish, while the wall, in turn, is tinged with yellow. If the screen were larger, so as to cover the entire wall, it would look pure gray and the hole still more yellowish. The difference in illumination

between screen and wall determines, primarily, a color-distance or a system of color-steps, the actual position of the steps being dependent upon other factors.

Turn to the second experiment. Here one remains at the unchanged level of the wall. The screen becomes. now the figure upon this constant level, and since the objectively yellowish level of the wall looks almost white, the screen must appear of a pronounced bluish color, though it, too, is objectively white.

In other words, objective white looks white when it is the "ground" of the observation, and objective yellow looks yellow when it is a "figure" upon this "ground." Similarly, objective white looks bluish when it appears upon an objectively yellow ground, and objective yellow looks white when it forms a ground -- all of which may take place under the same objective conditions. From this we can draw the conclusion that a field, reflecting a certain amount and quality of light, depends for its

phenomenal color-quality upon the ground on which it appears.

Thus our experiments are arguments in favor of our initial assumption, and this assumption furnishes a true psychological interpretation of the observation of Helmholtz, who maintained that we are unable to recognize a true white without comparison. Since, according to his theory, a sensation of white is composed of the sensations of the three cardinal colors mixed in certain proportions of intensity, and since the comparison of the intensities of colors is difficult and uncertain, therefore, in the absence of a true standard, we are very often mistaken and judge a sensation to be white, when in reality it is not white, but colored.

Our conception (*Begriff*) of white, is thus subject to change, while the sensation remains constant (13, II, p. 223f, 1st ed., p. 396f ). This theory involves the constancy-hypothesis, deducing the actual though misjudged sensation from the nature of its stimulus. Furthermore, it draws a

distinction between the true sensation and our judgment of it. Having abandoned this position, we can resolve the statement of Helmholtz into our own terms by saying that if the general level is produced by a colored light, then we *see* it as white. Helmholtz characteristically bases his theory upon a number of experiments similar to those from which we started. Let us return to our experiments and leave everything unaltered except that instead of a white light, we throw saturated yellow light upon the screen. If the intensities upon the screen and the wall are fairly equal the objectively yellow hole will appear to be distinctly bluish. The explanation follows from what has already been said. In a third experiment we use the same arrangement as in the first-white light on the screen, both wall and screen receiving approximately the same intensity-and we see a hole slightly lighter or darker than the screen and of a different color. Now slowly change the illumination of the screen, for instance by moving the objective lens of the projection lantern, and a

distinct change will take place in the objectively unaltered hole, whereas a change upon the screen is hardly noticeable.

This experiment shows that the general level offers a greater resistance to changes in the objective conditions than does a single figure. The physiological explanation follows from the general physiological theory of the figure-ground structure. Since the density of energy is greater in the figure-field than in the ground-field, any change of the whole system will appear with greater strength in the figure than in the ground. This relative stability of a general level is probably the fundamental fact in all our so-called "color-transformations." Nor does this fact contradict the results of the experiments previously described, in which the figure, by virtue of its "thing-character" proved to be the more constant; for in these other experiments the general level of the whole experience was never involved.

In the realm of space the general level plays a role no less important. Witasek (66) has described

the following method of testing the single "space-values" of the retina. One single point of fight in a totally dark room is presented in different positions with head and eyes fixated. Under these so far unrealized conditions Witasek expected to secure an exact determination of pure space (local) sensations. Try this experiment yourself and you will find it altogether impossible; for after you have stayed some time in total darkness, a single point of light has no definite position at all; if continually exposed, it wanders about, even when fixated, making so-called autokinetic movements.[16]

If the exposure is only momentary the point of light is neither clearly nor fixedly localized, and the crudest mistakes in localization occur. After watching these autokinetic movements for some time, the floor under your feet, the very chair you sit on, begin to lose their hold.

All this means that a definite single phenomenal position exists only within a fixed spatial level. If the conditions for the formation and

conservation of such a level are absent, localization is no longer possible; for just as the level grows unstable so does the single point within it.

The spatial level has, however, a marked tendency to remain constant, together with the common directions of "above" and "below" "right" and "left." We shall see in the next article what a strong influence these common directions exercise upon the formation of structure. For our present purpose we need only point out that "above" is not necessarily something depicted upon the vertical meridian of the eye below the fovea, since this is true only when the eyes are in a special position with head erect and eyes looking straight forward. When writing at my desk, for instance, this same part of my retina gives the impression of that which is farther away. It comes, to be sure, from the upper part of my manuscript but this is not "above" me. As a rule, the general level remains unaltered, despite changes in phenomena produced by movements of the eye, the head, the whole body, or indeed

movements of the surrounding objects. But let yourself be rapidly turned around several times, or let the surroundings be revolved about you, and everything is changed; all orientation is lost and giddiness results. The effect when your surroundings revolve is produced by visual influences alone, but when you are yourself moved, the vestibuler organs play a part. This, however, does not impair our theoretical position for it only goes to show that spatial level is dependent upon these sense-organs.

A third system upon which our spatial level depends is formed by the sense-organs of skin and muscles. In a very ingenious investigation, Garten (9) has tested our capacity to recognize the position of the body relative to the vertical. He constructed a special tilt-table which could be immersed in water so that the effect of gravity could be almost totally neutralized. Under these conditions, orientation was considerably disturbed, which again indicates the importance of the sensory systems named.

The term "spatial level" *(Raumlage)* in the

specific meaning hereemployed was used for the first time by Wertheimer (64) who alsomaintained that the Aubert phenomenon (A-P) depends largely upon a shifting of this level. This phenomenon and a number of related facts have been extensively investigated of late by G. E. Müller (46), but before we turn to these facts we must introduce some of the concepts used by Müller to explain the A-P, which are also applicable in our determination of the spatial level. Müller, investigating the localization of visual images (45), found that an ego-centric localization can be referred to three different systems of coordinates: the visual (*Blick*) *system* (*V. system*), the head system (*H. system*), and the "standpoint" system (*S. system*). The V. system may be defined by the three main axes of Hering's imaginary "Cyclopean" eye; the H. system is represented by the head, one axis being the basal line, the other two lying in the median plane of the head at right angles to the first; while the S. system is determined by the normal position of the trunk. In

normal positions of trunk, head and eye the three systems fall together, while in other positions they may differ so that each in turn maydetermine the localization. Müller inferred these systems primarily from results obtained with images, but he could show also that they play a part in perception and recognition, for instance in reading.[17]

In the following consideration of the A-P we shall refer merely to the V. and S. systems. An O., inclining his head, say 90º sidewards, in a totally dark room, is shown a single vertical line of light (*Leuchtlinie*). He sees this line not as vertical, but inclined in a manner contrary to the inclination of his head. The inclination, considerable though it may be, never, or at least very rarely, reaches the full degree of the head's inclination, even when we deduct the effect produced by the compensatory swivel-rotation of the eye. This is the gross phenomenon of the A-P; it can be described, using Müller's terminology, in the following manner: "The apparent position of the vertical line lies between

the two positions which it would have if either the V. or the S. system were alone operative["]. The V. system would make the line vertical, since it is parallel to the basal line which now is vertical, while the S. system would make the line horizontal, since normally a line cast upon the horizontal meridian of the eyes, as this vertical line now is, would be a horizontal line. For simplicity's sake we shall neglect the swivel rotation. Müller explains the actual apparent position of the line as a compound effect resulting from the competition of the two systems, and speaks, therefore, of the V. and S. components. This explanation is corroborated by another form of the phenomenon appearing mainly with slighter inclinations of the head, which we shall here omit.

For MüIler these systems are a product of experience and they work according to the general law of association. They can also be expressed by ascribing to each retinal point, not one, but two, values, a V. and an S. value. We see that Müller

does not use our concept of a spatial level but operates with single elementary effects, the V. and S. components (resulting from corresponding space-values) which enter into an additive combination. But, like all theories of this sort, it must be supplemented, as we shall presently see, by the employment of such concepts as "apprehension" and "judgment." We may proceed by reporting from Müller's monograph (46) which contains an excellent summary of the existing literature, as well as a number of further facts.

1. We saw above that in eye- or head-movements our general level is not changed. Consequently objects do not seem to move when we move our head or eyes. But this holds only at a fixed level where the visual field contains points of "anchorage" *(Verankerungspunkte).* In the dark, where such points are missing, a single vertical line of light may appear to be moving about a vertical axis in a direction contrary to that of the head's movement. This shows that the effect of head-

movements on visual objects is a function of the fixity of the spatial level, since, as we have already seen, in total darkness this level loses its stability.

2. If we observe the line of light with head inclined in a lighted room it appears to be vertical when the light is turned out, and with many O.'s it maintains at first its initial vertical position, and then passes gradually into its final oblique position. Müller considers this to be the effect of a general spatial perseverative tendency. (*Beharrungstendenz*). But what is it that perseveres; is it the line itself, or is it primarily the initial space-level of the lighted room? All the facts here adduced speak for the latter interpretation and against the former which Müller accepts. (See particularly No. 5, below.)

3. Some authors maintain that the A-P also appears, though in a lesser degree, when the head remains erect, if the rest of the body is turned about its sagittal axis. This result, which has not been confirmed by all investigators, seems largely dependent upon individual differences and upon the

method by which the position of the body is maintained. Yet, like Müller, we have no reason to doubt that it may occur. Müller explains it by the associative law of substitution. I cannot here set forth the reasons why I am unable to accept this explanation, but the reader will understand that the *Gestalt* theory is fundamentally incompatible with the associationist's principles. According to our conception the fact under discussion signifies that the spatial level may be altered by unnatural positions of the body, even if the head remains in a normal position, and that this change of level is similar to that of the A-P proper. The individual differences, and the differences between the results of different authors, can then be also understood; for the stability of the spatial level is very different with different individuals, as has been clearly shown by Wertheimer (64), and different experimental conditions will therefore not be of equal effectiveness in producing a uniform change of level.

4. Many O.'s report that they feel very

uncertain in judging the position of the line, since they have lost their standard of the vertical, and the same O.'s show great variability in their final judgments. Apparently Müller considers this only as a matter of judgment, but again we cannot accept the distinction he draws between the phenomenon itself and the judgment of it, in which marked properties of the judgment are not considered to be founded in the phenomenon. We must ask, instead, what are the properties of the phenomenon, and what are the causes of these properties which lead to an uncertain and variable judgment? Our answer is that such judgments are based upon uncertain and variable phenomena. "Uncertainty" or "undeterminateness" may quite well be a property of visual phenomena, as Katz (24) maintained for the "distance" of his so-called " film" colors *(Flächenfarben),* and both the undeterminateness and the variability of this phenomenon are readily explained from our point of view. We have recognized the paramount importance of visual

points of anchorage for the spatial level. When these are lacking, the level loses its hold, since the position of any single object depends upon the general level, and if the O. no longer has this, the position itself is no longer fixed or unambiguously determined. Instead of employing this descriptively and functionally well-defined concept of the spatial level, Müller and his followers distinguish between the phenomenon and the means whereby we orientate ourselves to it, the absence of points of anchorage being for P. Busse (3, p. 19), the absence of objects that can give us information about the inclination of the observed line.

5. During a longer observation, the line of light does not, as a rule, maintain its position. In the majority of cases its angle of inclination is increased toward the vertical. Müller suggests several explanations for this, maintaining that the V. component loses in weight, since with the passing of time the impression of the head's inclination loses in intensity; since, however, this explanation is

insufficient, he also suggests a tendency to decrease the influence of the V. component which is purely visual in origin. Yet the observed fact fits very readily into our explanations; for the longer the points of anchorage are lacking, the more the spatial level will change, and in consequence of its great instability the more it will deviate from its normal standard. The rarer cases in which a change takes place in the opposite direction simply prove again the general condition of instability; for they can be fully explained only when we know in detail all the factors upon which the level depends. In this connection we are reminded of the vestibular and the skin-muscle systems, to both of which we would ascribe a direct influence upon the level, and not merely an indirect influence upon our judgment concerning the head's inclination, as Müller states the case with reference to the vestibuler organ.

6. Some O.'s, particularly those who report uncertainty of judgment, show a tendency to persist in judging the line vertical. Again Müller explains this

as a tendency to judge "without sufficient foundation," but he also admits the possibility of an illusory perception, caused by the O.'s imagination, which, in some persons, exercises a strong influence over the apparent position of the line. He calls this the "vertical tendency." From this vertical tendency he distinguishes such cases as those in which the O., with head but slightly inclined, judges the line of light, when momentarily exposed, as either uncertain or vertical. In cases of this sort he says that the line was not apprehended long enough. Apart from this method of interpretation the facts are as follows: When the level is unstable -- and the influence of the imagination is nothing but an expression of its instability -- the O. can see the line at will in different positions; the more prominent character of the vertical direction can then be influential. A momentary exposure is a favorable condition for instability because the peripheral (stimulus-determined) conditions of the whole (physiological) process, comprising the entire optical

system, are weakened. Such a weakening always increases the effectiveness of purely structural factors. This has appeared clearly in Lindemann's investigations (28).

The conclusion we draw from these facts is not that Müller's theory is altogether wrong; for when we discard from it the concepts of apprehension, judgment, imagination and association [18], the competition among the two or three components remains. Only we would refer their effect to the general spatial level and not directly to the line. The components, therefore, find a place in our system as functional but not as descriptive facts.

Experimentally, we can destroy a fixed spatial level; we can also makeone level give place to another as has been shown by an experiment of Wertheimer's (64). Put a mirror in an inclined position upon a table. That part of the room seen in the mirror will then look abnormal. Objectively vertical lines will be inclined, and if a person visible

in the mirror drops an object, it does not appear to fall vertically. Now hold a tube to your eyes excluding the whole "real" room from your vision and continue looking into tile mirror. Let other persons walk about and do things in the visible section of the room. Very soon everything will be all right again; the floor will assume its horizontal position, the chairs will stand vertically upon it and objects will no longer be seen in an angle smaller than 90º. You can measure the change by executing an apparently vertical line at the beginning and at the end of the experiment, and then determine the angle between these two.

In the three systems, the V., the H., and the S. system, we have found factors which enter into the constitution of our spatial level, and this last experiment has shown that the visible world itself is a concurrent factor. This is a fact of very general significance. Standing in a room of average size the direction "straight ahead" is not under all conditions the sagittal axis of my Cyclopean eye; for the most

part it is the direction toward the wall, with which the plane of my face forms the smallest angle. It is I who am turned out of the main direction when I gaze obliquely towards the wall. This influence of the objective room-structure upon the space level is very different with different individuals. Yet the normal effect for the majority of persons can be shown by the following experiment. Since the discovery of v. Hornbostel and Wertheimer (20), we know that the apparent direction of a noise depends upon the time-difference with which the sound-waves strike the two ears. By inserting pipes of variable length, like trombone pipes, between the source of the sound and each ear one can, by drawing out or pushing in these pipes, readily make the noise wander from one side of the head to the other. One can also try to bring it to the middle or straight ahead. After some practice, this can be done with great precision and subjective certainty if one sits in a "good" position, *i.e.*, if one of the walls of the room serves as a frontal-parallel orientation.

But if a wall is lacking, or if one sits Somewhat obliquely, the same task becomes very trying. When I had acquired an enormous practice after several thousand experiments, working with closed eyes, I was still unable to find a good middle position for the sound under these conditions. The auditory middle, the phenomenon provoked by the time-difference zero, coincides with the sagittal axis of the Cyclopean eye, but in an oblique position this was not "straight ahead" for me, since the walls of the room influenced my spatial level, and consequently the auditory cue failed. Referring back to the beginning of this paragraph, I may add that these experiments also indicate descriptively the existence of an auditory space-level; for when the noise of a metronome stroke occurs, it enters into a thus far empty, yet phenomenally existing, auditory space. We find, too, that the stability of objects within a given level depends upon the quality of the object. Thus Busse (3) found that fine black vertical threads were much less stable than thicker brown

ones which carried red and black wooden beads at fixed distances.

The conclusion is that normally we possess a general spatial level within which we are anchored. When we lose this anchorage, we are practically lost. Yet even this effect of optical vertigo has been explained by experience! When a room is rotated around us, "experience" should tell us that the room is fixed and that consequently we are ourselves rotating. Think of the man who daily, operates such a machine of deception and who knows by experience of long standing that the room does move; will this man entering the rotating chamber with his knowledge grow giddy, or will he not? Practice in the room may no doubt modify the effect, just as does practice on the merry-go-round. But it is only by practice and not by knowledge or experience that the individual can succeed in maintaining a fixed level of any sort under these trying circumstances. With the aid of our level-concept we can also understand the so-called

(physiological) relativity of movement. A person looking from a bridge into a rapid stream soon has the impression of being himself moved. Seated in a train which is standing in a station, we are often unable to decide whether it is our train or the one on a neighboring track which is beginning to move. One explanation of the former effect actually maintains that the movement of a small piece of the bridge which belongs to one's field of vision is *moreprobable* than the movement of so large a surface as that of the stream! But we should say that normally there is no choice as to where we shall place our anchorage; for, in most cases, even with thestrongest impulse of our will, we cannot alter this anchorage (see Wertheimer 64). Normally it is something quite independent of our will-a compulsory perception founded in properties of the objective field which determine for us what parts are to appear as figures and what parts as ground; as v. Hornbostel (19) puts it: *things* are not holes in the world of experience. On the other hand ambiguous

situations occur in which two or more anchorages are equally possible, though here, too, law reigns and not chance. The chief rule for these ambiguous cases is this: that the objects which form the (dynamic) center of our visual world are at the same time our points of anchorage. When I am playing cards in my compartment I see the train move on the next track even if it is in reality my own train which is moving, but when I am looking at the other train, searching perhaps for an acquaintance in the coach, then it is my own train which seems to be moving. Psychologically, *i.e.*, *phenomenologically*, there is no relativity of movement.19

But our level-concept has still a wider application. We have already referred to certain instances, such as fashion and style. Experimental psychology has also studied certain facts about the phenomenon of the level without recognizing them to be such. What I mean is best explained by referring to some of Hollingworth's experiments upon the indifference-point (I.P.) (17, 18). Many

investigators, testing a scale of magnitudes, have found the existence of an I.P.; that is, while most members of the scale were estimated with a constant error, positive or negative, small magnitudes being overestimated and large ones underestimated, there comes a point where no constant error occurs. Though the fact has been confirmed over and over again, and in very different fields, yet, strangely enough, there has been a wide divergence of opinion as to the absolute position of the I.P. This startling fact suggested to Hollingworth the idea that there must be a mistake somewhere in the way the question is put. Is there, he asks, an absolute I.P. independent of the position and extent of the test-series, or is this I.P. a function of the total scale? He was able to demonstrate by a number of ingenious experiments that the latter is the correct assumption. Working with the reproduction of hand-and-arm movements, he arranged three series of experiments; A, including magnitudes of 10 to 70 mm. (with increments of 10

mm.); B, magnitudes of 30 to 150 mm. (with increments of 20 mm.); and C, magnitudes Of 70 to 250 mm. (with increments of 30 mm.); each scale consisting of seven different magnitudes. Upon a given day only one of these series was used. The I.P. of series A fell at about 40 mm., of B, at about 75 mm., and of C at about 125 mm., that is, it was always found to be approximately at the center of the scale. Smaller magnitudes were overestimated and larger ones underestimated. There was no absolute I.P. The magnitude of 70 mm., being the upper limit of A, and the lower limit of C, and near the middle of B, was underestimated in A (minus 10.2), overestimated in C (plus 16.5), and reproduced fairly accurately in B (plus 1.7; p.e. 10.3). To check this result, four months later, the three magnitudes of 10 mm. (always overestimated), 250 mm- (always underestimated) and 70 mm. (variable with the series) were tested singly on occasions several days apart, but for none of these three did a constant error occur.

In still another very clear experiment, the shifting of the I.P. itself was demonstrated. A set of standard magnitudes was prepared ranging from 10 mm. to 60 mm. (by increments of 10), from 60 mm. to 150 mm. (by increments of 15) and on to 250 mm. (by increments of 20). The standards of the 10 mm.-60 mm. were now given and reproduced in chance order, five trials being given for each magnitude. Then, without the knowledge of the O., the next magnitude was added and again five trials made of each standard. This was continued until the whole series of seventeen standards had been offered. The success of this experiment was remarkable. The I.P. rose with the introduction of each new standard magnitude; constant errors which were positive from the beginning, increased throughout the series, while constant errors which were negative in the beginning likewise underwent a continual change, decreasing to the zero point and emerging again as positive increments.

Hollingworth concludes, "that the

phenomenon of the indifference point . . . is of purely central origin" (17, p. 21), and this theory is as close to the one we propose as the general theoretical position of psychology at the time of his investigation would admit. According to his results, the I.P.-phenomenon belongs not to memory but to perception, and as an analogy he refers to type-concepts, such as race and class (18, p. 468). He also speaks of a "mental set", meaning by this "that we are adjusted for or tend to expect the average magnitude, and to assimilate all other magnitudes toward it, to accept them in place of it" (17, p. 39). But he insists on employing the term "judgment"; the error to which this tendency leads, he says, "is distinctly an error of judgment, and is quite independent of sensory or physiological conditions" (18, p. 469).

Again the distinction drawn between sensory components and judgments of peripheral and central factors vitiates his theory. Leaving these out of account, and referring the reader to the next article

for a discussion of the difference between peripheral and central factors, which takes on a very different aspect in our theory, we may here draw the following conclusions. In reacting to a definite scale of stimuli we establish a general level which, in the case described, as in many others, is both motor and sensory. The effect of each single stimulus is dependent upon this level, much as the figure is dependent upon its ground. And secondly, the general level holds together the whole group of phenomena corresponding to the scale of stimuli. Although they may rise or fall from this level, the phenomena never lose their existential connection with it, and being attracted by the level, the result is often a wrong judgment or a false reproduction. This attracting or assimilating effect of the level is a special case of our general law of leveling (discussed above). We see further that this level adapts itself automatically to the scale and this process of adaptation must therefore be explicable in terms of our general physiological theory.

Hollingworth rightly gives a wide application to his results, comparing the I.P.'s of different investigators with the range of their scales, and he has himself confirmed his "law of central tendency" in a purely sensory field by experiments upon the size of gray squares. I may also add in this connection that what G. E. Müller calls the "absolute impression" (*Absoluter Eindruck*) is just such a rise or fall from a general level. Whenever an O. makes a judgment that is not based upon a comparison between two stimuli, he is reacting not to a stepwise phenomenon, but to an emergence from the general level. With this I must bring my first article to a close.

## REFERENCES

1. BORAK, J. Über die Empfindlichkeit für Gewichtsunterschiede bei abnehmender Reizstirke. *Psychol Forsch.*,1922, *1*, 374-389.

2. BÜHLER, K. *Die geistige Entwicklung des Kindes.* Jena: Fischer. 2 Aufl., 1921, pp. xvi+463.

3. BUSSE, P. Über die Gedächtnisstufen und ihre Beziehung zum Aufbau der Wahrnehmungswelt. Überr die Vorstellungswelt der Jugendlichen und den Aufbau des intellektuellen Lebens. Eine Untersuchung über Grundfragen der Psychologie des Vorstellens und Denkens. *Zeits.Psychol.,* 1920, *84*, 1-66.

4. CORNELIUS, H. *Psychologie als Erfahrungwissenschaft*. Leipzig, Teubner, 1897, pp. xv+445.

5. EDWARDW, A. S. An Experimental Study of Sensory Suggestion. Amer. *J. of Psychol.*, 1915, *26*, 99-129.

6. v. EHRENFELS, CH. Über Gestaltqualitäten. *Vierteljahrschr. f. wissewch.*

*Philos*. 1890, *14*.

7. FERNBERGER, S. W. The Effect of the Attitude of the Subject Upon the Measure of Sensibility. *Amer. J. of Psychol.,* 1914, *25*, 538-543.

8. FRINGS, G. Über den Einfluss der Komplexbildung auf die effektuelle und generative Hemmung. *Arch. f d. ges. Psychol*., 1913, *30*, 415-479.

9. GARTEN, S. Über die Grundlagen unserer Orientierung im Raume. *Abh. d. math.-phys. Kl. d. säichs. Akad. d. Wissensch.,* 1920, *36*, 433-510.

10. GELB, A. Theoretisches über "Gestaltqualitäten." *Zeits. f. Psychol.*, 1911, *58*, 1-58.

11. GELB, A. Über den Wegfall der Wahrnehmung von "Oberflächen farben." Beiträge zur Farbenpsychologie auf Grund von Untersuchungen am Fällen mit erworbenen, durch zerebrale Läsionen bedingten Farbensinnstörungen. Psychologische Analysen hirnpathologischer Fälle auf Grund von Untersuchungen Hirnverletzter. *Zeits. f.*

*Psychol.*, 1920, *84*, 193-257. Also contained in *Psychologische Analysen* u.s.w. Bd. 1. Leipzig: Barth. 1920, pp. 561.

12. HEINE, R. Über Wiederkennen und rückwirkende Hemmung. Zeits. f. *Psychol*., 1914, *68*, 161-236.

13. v. HELMHOLTZ, H. *Handbuch der physiologischen Optik*. 3 Aufl. Erg. u. her. in Gemeinschaft mit *A. Gulistrand und J. v. Kries* von *W*. *Nagel*.† 3 Bde. Hamburg & Leipzig, 1909-1911 (I. Aufl. 1856-1866).

14. HENNING, H. Experimentelle Untersuchungen zur Denkpsychologie. 1. Die assoziative Mischwirkung, das Vorstellen von noch nie Wahrgenommenem und deren Grenzen. *Zeits. f. Psychol*., 1919, *81*, l-96.

15. HERING, E. *Grundzüge* der *Lehre vom Lichtsinn*. Berlin: Springer, 1905-1920, pp v+294. Sonderabdruck a. d. Handbuch d. Augenheilkunde (*Graefe-Sänisch*) i. Teil, XII Kop.

16. HÖFLER, A. Gestalt und Beziehung-

Gestalt und Auschanung. *Zeits. f. Psychol.*, 1912, *60*, 161-228.

17. HOLLINGWORTH, H. L. The Inaccuracy of Movement With Special Reference to Constant Errors. *Arch. of Psychol.*, 1909, No. 13, Columbia Contrib. to Philos. & Psychol., *17*, pp. 87.

18. HOLLINGWORTH, H. L. The Central Tendency of judgment. J. *of Philos., Psychol. and Sci. Meth.,* 1910, *7*, 461-469.

19. v. HORNBOSTEL, E. M. Über optische Inversion. *Psychol. Forsch.*, 1922, *1*, 130,-156.

20 v. HORNBOSTEL,. E. M. und WERTHEIMER, M. Über die Wahrnehmung der Schallrichtung. *Sitzungsber. d. Preuss. Akad. d. Wissensch.*, 1920, *20*, 388-396.

21. JAENSCH, E. R. Einige allgemeinere Fragen der Pychologie und Biologie des Denkens, erläutert an der Lehre vom Vergleich. (Mit Bemerkungen über die Krisis in der Philosophie der Gegenwart.) *Arb. z. Psychol. und Philos.* Leipzig: Barth, 1920, *1*, pp. 31.

22. JAENSCH, E. R. und MÜLLER, E. A. Über die Wahrnehmung farbloser Helligkeiten und den Helligkeitskontrast. Über Grundfragen der Farbenpsychologie. Zugleich ein Beitra zur Theorie der Erfahrung. Hn. v. *E. R. Jaensch* I. *Zeits. f. Psychol.,* 1920, *83*, 266-341. (Auch separat bei Barth.)

23. JANSCH, E. R. Über den Farbenkontrast und die so genannte Berücksichtigung der farbigen Beleuchtung. Über Grundfragen u.s.w. III. *Zeits. f. Sinnesphysiol.*, 1921, *52*, 165-180.

24. KATZ, D. Die Erscheinungsweisen der Farben und ihre Beimflufsung durch die individuelle Erfahrung. *Erg. Bd. d. Zeits. f. Psychol*. Leipzig: Barth, 1911, pp. xviii+425.

25. KOFFKA, K. *Beiträge zur Psychologie der Gestalt.* 1. Untersuchungen über den Zusammenhang zwischen Erscheinungsgrösse und Erscheinungsbewegung bei einigen sagenannten optischen Täuschungen, von *F. Kenkel. Zeits. f. Psychol.,* 1913, *67*, 358-449.

26. KOFFKA, K. The same. III. Zur Grundlegung der Wahrnehmungspsychologie. Eine Auseinandersetzung mit V. Benussi, von *K. Koffka. Zeits. f. Psychol.*, 1915, *73*, 11-90 [Translated as "Reply to Benussi" in W. Ellis (Ed.) (1938). *A source book of Gestalt psychoogy* (pp. 371-378). London: Routledge & Kegan Paul.]

27. KOFFKA, K The same. IV. Zur Theorie einfachster gesehener Bewegungen. Ein physiologisch-mathamatischer Versuch von *K. Koffka. Zeits. f. Psychol.*, 1919, *82*, 257-292. Beiträge I-IV also separate, as Beiträge, etc. Vol. 1, Leipzig: Barth, 1919, pp. v+323.

28. KOFFKA, K. The same. VII. Experimentelle Untersuchungen üiber das Entstehen & Vergehen von Gestalten, von *E. Lindemann. Psychol. Forsch.*, 1922, *2*, 5-60. [Translated as "Gamm movement" by E. Lindemann in W. Ellis (Ed.) (1938). *A source book in Gestalt psychology* (pp. 173-181). London: Routledge & Kegan Paul.]

29. KOFFKA, K The same. VIII. Über die

Verschmelzung von zwei Reizen (title not definitely settled), von *L. Hartmann,* to appear in *Psychol. Forsch.* [Final title was "Neue Verschmelzung." *Psychol. Forsch.*, *3*, 319-396 (1923). Translated as "Further studies of gamma movement" in W. Ellis (Ed.) (1938). *A source book of Gestalt psychology* (pp. 182-191). London: Routledge & Kegan Paul.]

30. KOFFKA, K. Probleme der experimentellen Psychologie. 1. Die Unterschiedsschwelle. *Die Naturwissensch.,* 1917, *5*, 1-5, 23-28.

31. KOFFKA, K. The same. II. Über den Einfluss der Erfahrung auf die Wahrnehmung (Behandelt am Problem des Sehens von Bewegungen). *Die Naturwissensch.*, 1919, *7*, 597-604.

32. KOFFKA, K. Die Prävalenz der Figur. Kleine Mitteilungen a. d. psychol. Inst. d. Univ. Giessen, *3*, *Psychol. Forsch.*, 1922, *2*, 147-148.

33. KOFFKA, K. *Die Grundlagen der psychischen Entwicklung. Eine Einführung in die*

*Kinderpsychologie.* Osterwieck a/H: Zickfeldt, 1921, pp. vii+278.

34. KÖHLER, W. Über unbemerkte Empfindungen und Urteilstiuschungen. *Zeits. f. Psychol.,* 1913, *66*, 51-80.

35. KÖHLER, W. Die Farbe der Sehdinge beim Schimpansen und beim Haushuhn. *Zeits. f. Psychol.,* 1917, *77*, 248-255.

36. KÖHLER, W. Nachweis einfacher Strukturfunktionen beim Schimpansen und beim Haushuhn. Über eine neue Methode zur Untersuchung des bunten Farbensystems. Aus der Anthropoidenstation auf Teneriffa IV. *Abh. d. Preuss. Akad. d. Wissenschaft,* 1918, Phys.-math. Klasse. No. 2, pp. 101 (Einzelausgabe). [Translated as "Simple sturctural functions in the chimpanzee and in the chicken" in W. Ellis (Ed.) (1939). *A souce book in Gestalt psychology* (pp. 217-227). London: Routledge and Kegan Paul.]

37. KÖHLER, W. Zur Psychologie der Schimpansen. *Psychol. Forsch.*, 1922, 1, -46.

38. KÖHLER, W. *Die physischen Gestalten in Ruhe und im stationären Zustand. Eine naturphilosophische Untersuchung.* Braunschweig: Vieweg, 1920, pp. xx+263.

39. KROH, O. Über Farbenkonstang und Farbenstraus formation. Über Grundfragen u.s.w. (S. No. 22), her. v. *E. R. Jaensch.* IV. *Zeits. f. Sinnesphysiol.,* 1921, *52*, 181-186.

40. LEWIN, K. Das Problem der Willensmessung und das Grundgesetz der Assoziation. *Psychol. Forsch.*, 1922, *1*, 191-302, *2*, 65-140.

41. LINDWORSKY, J. Referat über *Köhler* (36), *Stimmen der Zeit,* 1919, *97*, 62-68.

42. MARTIN, L. J. Über die Abhängigkeit visueller Vorstellungsbilder vom Denken. Eine experimentelle Untersuchung, *Zeits. f. Psychol.,* 1914-15, *70*, 212-275.

43. MÜLLER, G. E. *Zur Grundlegung der Psychophysik.* Berlin: Grieben, 1878, pp. xvi+424.

44. MÜLLER, G. E. und Pilzecker, A.

Experimeiitelle Beiträge zur Lehre vom Gedächtnis. *Erg. Bd.* I, *d. Zeits. f. Psychol.* Leipzig: Barth, 1900, pp. xiv+300.

45. MÜLLER, G. E. Zur *Analyse der Gedächtnistätigkeit und des Vorstellungsverlaufes.*II. *Teil. Erg. Bd.* 9 *d. Zeits. f. Psychol.* Leipzig: Barth, 1917, pp. xii+682.

46. MÜLLER, G. E. Über das Aubertsche Phänomen. *Zeits. f. Sinnesphysiol.,*1916, *49*, 109-244.

47. OETJEN, F. Die Bedeutung der Orientierung des Lesestoffes für das Leseni und der Orientierung von sinnlosen Formen für das Wiedererkennen der Letzteren. *Zeits. f. Psychol.,* 1915, *71*, 321-355.

48. OGDEN, R. M. Are there any Sensations? *Amer. J. of Psychol.*, 1922, *33*, 247-254.

49. PAULI, R. *Über Psychische Gesetzmässigkeit, insbesondereüiber das Weber'sche Gesetz.* Jena: Fischer, 1920, pp. 88.

50. PETERS, W. Aufmerksamkeit und

Reizschwelle. Versuche zur Messung der Aufmerksamkeitskonzentration. *Arch. f. d. ges. Psychol.*, 1906, *8*, 385-432.

51. PILLSBURY, W. B. *The Essentials of Psychology.* New York: Macmillan Co., 1911, pp. ix+362.

52. RAHN, C. The Relation of Sensation to other Categories in Contemporary Psychology, A Study in the Psychology of Thinking. *Psychol. Monog,,* 1913, 16 (1), pp. 131.

53. RUBIN, E. *Synsoplovede Figurer. Studier i psykologisk Analyse.* I. del. Kobenhavn og Kristiania: Gyldendal, 1915, pp. xii+228. German edition: *Visuell wahrgenommen Figuren. Studien in psychologischer Analyse*. I. Teil. Kobenhavn, Christiania, Berlin, London: Gyldendal, 1921, pp. xii+244.

54. SCHLÜTER, L. Experimentelle Beiträge zur Prüfung der Auschauungs und der Übersetzungsmethode bei der Einführung in einen fremdsprachlichen Wortschatz. *Zeits. f. Psychol.,*

1914, *68*, 1-114.

55. SCHUMANN, F. Beiträge zur Analyse der Gesichtswahrnehmungen. I. Einige Beobachtungen über die Zusammenfassung von Gesichtseindrücken zu Einheiten. *Zeits. f. Psychol.*, 1900, *23*, 1-32.

56. SCHUMANN, F. Beiträge etc. III. Der Successivvergleich. *Zeits. f. Psychol.,* 1902, *30*, 241-291 and 321-339.

57. SEIFERT, F. Zur Psychologie der Abstraktion und Gestaltauffassung. *Zeits. f. Psychol.*, 1917, *78*, 55-144.

58. SHEPARD, G. F., and FOGELSONGER, H. M. Studies in Association and Inhibition. *Psychol. Rev*., 1913, *20*, 290-311.

59. SPECHT, W. Beeinflussung der Sinnesfunktionen durch geringe Alkoholmengen. I. Das Verhalten von Unterschiedschwelle und Reizschwelle im Gebiet des Gehörsinnes. *Arch. f. d. ges. Psychol.,* 1907, *9*, 180-295.

60. STUMPF, C. Tonpsychologie. 1. Leipzig: Hirzel, 1883, pp. xiv+427.

61. TITCHENER, E. B. A Text-Book of Psychology. New York: Macmillan Co., 1910, pp. xx+565.

62. TITCHENER, E. B. Functional Psychology and the Psychology of Act, II. *Amer. Jour. of Psychol.,* 1922, *33*, 43-83.

63. WASHBURN, M. F. An Instance of the Effect of Verbal Suggestion on Tactual Space Perception. Notes from the Psychological Laboratory of Vassar College. II. *Amer. J. of Psychol*., 1909, *20*, 447-448.

64. WERTHEIMER, M. Experimentelle Studien üiber des Sehen von Bewegung. *Zeits. f. Psychol.,* 1912, *61*, 161-265.

65. WERTHEIMER, M. Untersuchungen zur Lehre von der Gestalt. I. Prinzipielle Bemerkungen. *Psychol. Forsch.*, 1922, *1*, 47-58. [Translated as "The general theoretical situation" in W. Ellis (Ed.) (1938). *A source book of Gestalt psychology* (pp. 12-16). London: Routledge & Kegan Paul.]

66. WITASEK, St. Psychologie der

Raumwahrnehmung dcs Auges.-Die Psychologie in Einzeldarstellungen, her. v. *H. Ebbinghous* † und *E. Meumann,* II. Heidelberg: Winter, 1910, pp. viii+454.

## FOOTNOTES

1 The exceptions to this universal rule occasioned by factors such as fatigue, practice, etc., do not affect the general interpretation and may here be neglected. (return)

2 We shall set aside the concept of feeling, though in many systems feelings are taken to be specific elements just as simple as sensations. (return)

3 That the facts of reinforcement and inhibition are far from fitting into the theory can be mentioned only incidentally. The reader is referred to he work of Shepard and Fogelsonger (58), and to that of Fringa (8). (return)

4 Compare Titchener's recent discussion (62). (return)

5 Full quotations in (30). (return)

6 A full discussion of this problem of relation may be found in the papers by Gelb (10) and Höfler (16). (return)

7 The reason for this trend in the formation

of our concepts is discussed by Köhler (38, p. 48f). (return)

8 Rahn has developed the same view from an implicit criticism of current teaching (52). (return)

9 Wertheimer has introduced the distinction of static and dynamic phenomena (64, p. 227), recognizing that the latter are no less real than the former. (return)

10 These two judgments are psychologically different, and to each there corresponds a different stepwise phenomenon, as a rise to the right, or a fall to the left. We have, for simplicity's sake, neglected this difference, and shall continue to do so in what follows. The reader can easily supplement the discussion in order to make it cover this distinction also. (return)

11 This has been proved by Lewin who speaks of a "readiness to act" (*Tätigkeitsbereitschaft*) (40). (return)

12 A full discussion is given in Pauli's

monograph (49). (return)

13 The nature of the constants contained in this formula makes it possible to calculate the approximate value of PHI(1)-PHI(2) in volts, as Köhler has shown to be the case with respect to the brightness-threshold. (return)

14 Titchener, though he recognizes that he is interpreting, seem[s] not to be fully aware of the totally hypothetical character of his interpretation. (return)

15 Compare Jaensch (22, 23) and Kroh (39). (return)

16 Compare, for instance, Wertheimer (64). (return)

17 See Müller (46, p. 238f). Oetjen (47). (return)

18 That association is quite out of place here will appear when in our next article we are able to prove that the principal directions of space do not owe their prominence to experience and habit, but to an imminent law of structure, in consequence of

which Müller's three systems cannot be accepted as habitual tendencies. (return)

19 Nor of size either, as Wertheimer (64) has shown.