GESTALT PSYCHOLOGY

BY

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To

MAX WERTHEIMER

Preface

I OFFER this book to the patient reader, feeling more than ever how much I have been inferior to my task. Nothing is easier than to criticize it. *Gestalt* psychology, the subject matter I have tried to present, resembles in 1928 a promising start more than a complete achievement. At all events it would have been difficult to introduce it adequately. To present it skillfully in a foreign language was far beyond my linguistic ability. I have done all that I possibly could. The greatest difficulty, however, is that only certain simple and almost crude concepts of *gestalt* psychology would yield to my painful efforts to convey them through the medium of English. Under these circumstances the optimism of my excellent publisher is responsible if the incomplete portrait of an incomplete thing is here recommended to public consideration.

Certain other trends in contemporary psychology are discussed in connection with *gestalt* psychology. The first question a foreign psychologist has to answer in America is, of course: How do you like behaviorism? This vigorous school has energetically drawn certain lines beyond which a psychologist should never go. And, then, indicating the only direction that all psychologists must take, it has left little place open for free discussion of theory and research; in other words, it has developed into such a fundamentalism that I felt myself forced to clear my way for a certain liberalism to which I am accustomed in science.

With introspectionism things are not entirely different. In this psychological school a scheme of procedure, which is just the opposite to my own, has become dominant. This is unfortunate. If behaviorists should refuse to take account of this book because in it direct experience is employed and referred to without apology, introspectionists would not treat it any better, since my use of experience, from their point of view, is not of the right sort. I have had to explain, then, not only how I could eat the forbidden fruit in spite of all commandments, but also why I took the common ware from the street and from the market-place instead of buying the standardized and sterilized products which introspection would furnish from its cultivated orchard. It was my aim, however, to make this discussion productive and, in addition to criticism, the reader will find some of our own rules of procedure expressed in these first chapters. If the fighting spirit should be found to prevail in some paragraphs, I beg you to remember that, as in politics, we sometimes strike at a system though we are fond of its defenders.

Certain psychologists, both in the United States and in England, will recognize their own ideas or, at least, find some similarities to them, in these chapters. Instead of treating all the instances of this convergence and agreement in detail, I only mention the theory of "emergent evolution" and the doctrine of Professor Whitehead's imposing work, with all the respect which is due them both. I should be happy to call Professor R. M. Ogden a *gestalt* psychologist. I have not been able to see quite clearly to what degree there is any agreement with certain other eminent psychologists. In Professor Bentley's well-known book, for example, his term "organization" sometimes seems to come near the concept of *gestalt*. But there must be some difference, since on the other hand, the basic doctrine of introspective psychology is preserved in a manner incompatible with *gestalt* psychology. Future development will undoubtedly show whether or not our views can be brought to a full congruence.

Dr. Mortimer Adler deserves the reader's thanks as well as my own for the skill and tact which he displayed in the correction of my text, which was finally revised by K. Koffka.

My thanks are due to Mrs. L. Köhler, without whose help my work would have been all but impossible.

To Max Wertheimer I should like to dedicate something of which I could be prouder than these ten chapters. I hope he will accept them, however, as a testimony of my good will and of our friendship.

W. K.

Ormor, Sweden,
August 12, 1928.

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GESTALT PSYCHOLOGY

I

The Viewpoint of Behaviorism

THERE seems to be a single starting point for psychology, exactly as for all the other sciences; the world as I find it, naïvely and uncritically. The naïveté may be lost as we proceed. Problems may be found, which were completely hidden from our eyes at first, and for their solution it may be necessary to devise concepts which seem to have little contact with uncritical experience; nevertheless the whole development began with a naïve picture of the world. This origin is necessary because there is no other basis from which a science can arise. In my case, which may be taken as representative of many others essentially similar to it, that naïve picture consists, at this moment, of a blue lake with dark forests around it, a big, gray stone, hard and cool, which I have chosen as a chair, a paper on which I write, a faint noise of the wind which hardly moves the trees, and a strong odor characteristic of boats and fishing. But there is more in this world: somehow I now behold, though it does not become fused with the blue lake of the present, another lake of a milder blue, and I find myself, some years ago, looking at it from its shore in Illinois. I am perfectly accustomed to beholding thousands of pictures of this kind which arise some way or other when I am alone. And there is still more in this world: my hand and fingers moving lightly over the smooth surface of the paper; now, when I stop writing and look around again, there is a feeling of health and vigor, but in the next moment I feel something like a dark pressure somewhere in my interior which tends to develop into a feeling of being hunted—I have promised to have this manuscript ready within a few months.

Most people live in such a world permanently, which is *the world* for them, and hardly ever find problems or difficulties in its properties. Crowded streets may take the place of the lake, a cushion in a sedan that of my stone, some serious words of a business transaction may be remembered instead of Lake Michigan, and the dark pressure may have to do with tax-paying instead of book-writing. All these are minor differences so long as one takes the world at its face-value, as we all do except in those hours when science disturbs our natural attitude. There are problems, of course, even for the most uncritical citizen of this first-hand world. But, for the most part, they do not refer to the nature of it; they are of a practical or an emotional sort and they usually mean that, this world being taken for granted, we do not know how to judge or how to behave in that actual part of it which we face as our present situation.

Various sciences, most of all physics and biology, began centuries ago to destroy the simple confidence with which human beings take that naïve world as *the reality.* Though hundreds of millions still remain undisturbed, the scientist has found it, in some respects, full of almost contradictory properties. Fortunately he has been able to discover another world, as it were, behind it, whose properties, highly different from those of the everyday world of naïve people, do not seem to be contradictory at all. No wonder therefore that now, as psychology begins to be a science, some of its most energetic students should wish to make it go the way of the exact sciences at once. Indeed, if other sciences have found the naïve world of everyday reality impervious to and unavailable for scientific method, what hope of better success can we as psychologists have? And if the enormous and almost impossible feat of jumping out of the world of direct, but confused, experience into a world of clear and hard reality, has already been achieved by physical science, it would seem wise for the psychologist to take advantage of that great event in the history of science and begin to construct psychology on the solid basis laid down by the physicist and the physiologist *outside* the field of uncritical experience.

A few words about the history of scientific criticism will help to define better the material or field to be given up by psychological investigation and to indicate what shall be taken as a better object of research. Our naïve experience consists of objects, their properties and changes, existing quite independently of us. It does not matter to them and their existence whether or not we see and feel and hear them; they remain exactly as they are when we state their properties, when we are not present or are occupied in other matters. It was a great step when men began to ask how seeing and feeling and hearing occur, and it was a revolution when they found that colors and noises and smells, etc., were merely products of some of the influences exerted on man by his surroundings. Still these surroundings seemed to subsist in their primary characteristics and to remain "the real world," if only you subtracted those "secondary qualities" as purely subjective ingredients. But finally even the "primary qualities" of naïve realism were assimilated to the secondary as subjective. The form, weight and movement of the objects of immediate experience were treated as colors and sounds, i.e., as functions of the experiencing organism, as end results of its complicated processes. And therefore we cannot hold that these objects excite the processes, since, according to this point of view, their very existence as parts of immediate experience is only a consequence of these processes. So the picture of the objective and independent *physical world* of physical things, of physical time, of physical movement and physical space, differs radically from the world-picture of direct experience. Physical objects, which *as such* we can neither see nor feel, exert upon each other a great variety of physical influences, which *as such* similarly do not occur in the world of immediate experience.

There is one physical object more interesting than all the others but in every way as foreign to naïve experience as they are: that is my physical body, an astounding system of electromagnetic entities. If the right influences be exerted upon this system by other physical objects, those processes will occur in it, the consequences of which I know as the exterior part of the world of naïve experience, the so-called external world of the senses. But an enormous amount of physical activity is constantly going on inside my body. We have sufficient grounds, therefore, to assume that there are still other processes with the occurrence of which the *inner* side of our immediate experience is connected: such are the feeling of strain, when I contract the muscles of my arm, also that of hunger and fatigue, my "memory image" of Lake Michigan, fear and hope and so on, almost *ad infinitum.*

At present we shall not consider *how* the physicist accomplishes the gigantic task of investigating the properties of a world which does not appear directly in any part of immediate experience. But there can be no doubt concerning the remarkable success of his procedure. Whereas the world of naïve man is somewhat confused, revealing its subjective character in any critical discussion of its properties, the world of the physicist gives short shrift to confusion and contradiction. And though we find ourselves surprised by the rapid changes which physical theory undergoes in our times, we still have the feeling that progress is made with most of these changes. Eventually there will be agreement about all the more important properties of the physical world.

For a while psychology was supposed to be the science of the events of direct experience, both in its external and its internal aspects, as contrasted to physical objects and occurrences.[1] By description of direct experience the psychologist hoped to get not only an orderly registration of all the possible varieties of such experience, but also a great deal of information about the functional relations among these events. He even aimed at formulating laws governing the flow of direct experience.

The well-known psychological school of behaviorism has leveled against the older psychology the criticism that it was misguided both with respect to its subject-matter and its aim. It has not been possible, according to the behaviorist, to register the varieties of direct experience in a convincing manner, nor has anything come of the attempt to

[1] It seems more cautious to talk about direct experience than about "consciousness." For some people consciousness is rather a function by or in which we become aware of "immediate experience." In my terminology, if some one has a "feeling of becoming aware," this is only *one special case* of direct experience.

describe their functional relationships or to formulate the laws of so-called "mental life." In no respect, the behaviorist holds, do we have the impression that there exists a real and progressive science of direct experience, clear in its methods and results. On the contrary, we find endless discussions of minor and, less frequently, major items, different psychologists giving quite different descriptions of "facts" which are supposed to be more or less the same for both of them. Take the example of "images"! One psychologist claims to have them in numbers, many of them almost as lively and concrete as perceptions. Others tell us that there are no real images at all in direct experience and that probably the first man was deceived by having words or other motor phenomena correspond to objects and facts not actually present in experience. If the famous method of "introspection," i.e., observation of direct experience,[1] cannot give better results in such a gross investigation, what shall we expect from it when we are confronted by questions of equal or even greater importance, and also more subtle and intrinsically more difficult? Unfortunately, the adherents of introspection do not seem to trust their own procedure. At least they must have agreed upon facing important problems as seldom as possible, since they are occupied mainly with describing the more remote

[1] The opposition between introspectionism and behaviorism discussed in this chapter is the opposition between those who use and those who reject the observation of direct experience. In Chapter III introspectionism as a special interpretation of the use of direct experience will be discussed.

and less exciting corners of experience, as e.g., minor nuances of sensations, or the "glassy" appearance of certain parts of space, and so forth. If all I have to do to develop the science of direct experience is to describe it, why then not attack the central facts of "mental life" at once, instead of scratching a little at the surface, or periphery, of it? There was a time in Germany, not very long ago, when people began to joke about psychology's ponderous discussion of trifles. And, indeed, it was strange to see how the description of a direct experience, what happens, for instance, during a single comparison of two tones or colors, could fill hundreds of pages without giving us the slightest positive idea about how the occurrence and accuracy of such a comparison might be explained. Even in a state of perplexity a science can be highly interesting. But psychology, so far as it deals with direct experience by the method of introspection, has not only been a complete failure; it has also become boresome for all those who are not professionally connected with it.

And the behaviorist is not slow in giving us reasons why this must be so. The meaning of "direct experience," through opposition to the world of physics, becomes associated with concepts like "the mental," "mind" and "soul," almost without our being aware of this association. Thus gradually this term becomes the expression for "spiritual life" or the name for the effects of a special mental substance, existing independently of the facts of physics and biology.

Consequently, all the old prejudices and superstitions of religious or metaphysical extraction slip into the concept and into the treatment of direct experience. The psychologist loses his impartial judgment; what he has heard about "mind," or "soul," and their miraculous properties since the early days of his childhood, creeps into his statements and makes "introspection" an ill disguised rhetoric in favor of prescientific mythology and medieval darkness.

However, if this were the only argument against introspection, the introspectionist might answer that if the behaviorist were right in this point, his criticism would not apply to the description of direct experience itself, but rather would indicate an accidental source of defect in the method. Then, some increase of self-criticism and a careful elimination of religious or metaphysical interests in students of psychology might be a remedy for these defects, and a very welcome gesture towards sober behaviorism at the same time.

But the behaviorist has other reasons for not accepting direct experience as a field of scientific research. First of all, as a procedure, introspection lacks the chief methodological virtue of physical research, a position of observation external to the system under observation; on the contrary, it is a special process *in* this system itself, and all chances are against its leaving the "observed" parts of it undisturbed. *Hinc illæ lacrimæ!* An example of this is the attempt to study introspectively the direct experience of sorrow and joy; they do not remain the same, but rather tend to disappear when the selfsame person who has or had the sorrow and the joy must assume the attitude of introspection.

But even supposing that this difficulty might be overcome by some scarcely imaginable improvement of the method, we still should find it useless, the behaviorist argues, because of its miserable and inevitable subjectivity. What is the first characteristic of an objective statement as the result of an objective scientific procedure? That whoever happens to be interested in it can be obliged to understand it in one determinate meaning, if only you give him exact definitions of the terms you are using in your statement. So we define the atomic weight or the atomic number of an element, so analogy and homology of morphological structures, and there is no physicist or biologist who does not know the exact significance of those words. If now you listen to psychologists talking, let us say, about the "confusion" which they observe in peripheral vision, what exact meaning can be conveyed by this word so long as it is not fixed by definition? A real definition, however, seems to be impossible wherever one has to do with the ultimate data of direct experience. If you are asked for the definition of "confusion," you may attempt to define it negatively, say in terms of "lack of clearness." But that does not help you very much, since your friend will then ask what you mean by "clearness." You may tell him that a rather high degree of clearness is a normal property of the central parts of a simple and orderly visual field. But such a field might have more than one normal property and you did not give a *differentia specifica* in your pseudo definition, in addition to which the words "simple" and "orderly" need definition as badly as did "confusion" and "clearness." In any case, you have now resorted to the only thing which seems feasible where, as in this field of direct experience, a true definition cannot be achieved, viz., a kind of *demonstration.* You do not define your term, but you give your friend a hint about what conditions are necessary in order to have the specific kind of direct experience about which you are talking. Supposing that he understands the words by which you describe those conditions, there is some chance now that he may attach your undefined term to the precise phase of his actual direct experience that your use of the term intended. But what a crude and altogether vague procedure this is, if you compare it with the elegant definitions of the exact sciences!

And still we have supposed that, given the same conditions, your friend, who cannot make statements about any direct experience except *his own,* must find in it the same properties, objects and occurrences, which you have found in *your* direct experience. You cannot, as the physicists seem to do, make your two statements about one and the same event, neither do you make readings from one and the same apparatus or scale. You have *two* events in *two* experiences. What is your evidence for assuming that under the same conditions the ultimate data of experience are the same for both of you? Unfortunately you shall never know whether this is the case or not. On one hand, color blindness and similar phenomena show conclusively that such an identity does not exist; on the other, you have no proof of that identity even in those cases where all the tests you might imagine give the same description, i.e., the verbal report you both make. Your friend may report "red" where you say "red." Still you do not know more than that your friend always has the *same* quality wherever and whenever you have your "red," without knowing at all that he has *just that* quality which you call "red." Neither does it help you to a real identification of qualities in the two direct experiences that what he calls "red" seems to have (more or less) the same "stimulating" value as you find verified in your own description, because you will see at once that he may use the same term "stimulating" in quite a different meaning, so that he does not have the same kind of experience when he uses it.

This is subjectivity in its extreme form. If every one has his own direct experience and if he is forever excluded from that of all other persons, direct experience is the private affair of each of us, and with respect to it a common science cannot possibly be achieved. Indeed, so little can be derived from the direct experience of one man, concerning similar experiences in others, that, in epistemology, it has become a truism that I shall never know whether or not there exists any "direct experience" at all even in my best friend. Whatever I see or hear when we talk together is a part of *my* experience. Certainly, what I call his attitude, or his voice, in my experience, is determined by physical events in the muscles and in the nervous system, etc., of his physical body. It is not only possible but even necessary to understand these physical events from the viewpoint of pure physics or physiology. Consequently, there is no proof that some of these processes in my friend are accompanied by "direct experience" in him.

The behaviorist might add to this criticism that he does not deny the great contributions which, before his time, the older forms of psychology have made to the advancement of this science. But he will say that, looking upon those achievements from the present point of view, one will easily discover a simple fact: viz., that nearly all of them are to the credit not of introspection and description but of the "objective experiment." The meaning of this word may be explained as follows: Instead of making a subject observe and describe his direct experience in a given case, we place him in a well-defined situation to which he has to react by certain performances. These we can state and measure without his giving us any description of his experience. So Weber's law was discovered; this was the kind of experiment by which Fechner made psychology an experimental science; memory and the formation of habits were investigated by research of this type with almost no introspection; and since Binet and Simon we have learned to treat intelligence in the same manner. If we are not mistaken, even the introspectionist gives us "descriptions" of colors and tones, pleasure and volition, only so long as he has not found a method by which he might transform them through objective experiments into measurable achievements or reactions. The introspectionist himself seems to accept the "descriptive" observation of a fellow introspectionist, according to the measure in which the other has been able to give objective experiments as a verification and corroboration of his descriptions. What, then, is the use of "direct experience" and its description?

From their common criticism all behaviorists do not draw quite the same conclusions about "direct experience." None of them, it is true, find direct experience a matter of interest for science, since as the private affair of those who have such experience it is not accessible to the objective observation undertaken by others, as it ought to be were it a proper object of scientific research. A few members of the school seem to go so far as to deny the existence of direct experience altogether; they simply abominate the notion. But these minor differences of opinion are of no importance for us, because in the question of *method* all behaviorists have not only the same negative but also the same positive opinion. Their program is a simple consequence of the foregoing argument. In his objective experiment the psychologist has tacitly accepted the procedure of the exact sciences, even if he has not become fully aware of the difference in principle which separates this attitude from that of introspection. Both the physicist and the chemist are interested in knowing how a system which they are investigating will react when exposed to a certain set of conditions; they also ask how the reaction will change with a variation of those conditions. Both questions are answered by objective observation, registration and measurement. This is exactly the form of real experimentation in psychology, too: a subject of a certain type (child, adult, man, woman, animal) is the system to be investigated; certain conditions are given and objectively controlled; the most important of these we call "the stimuli"; and a reaction occurs which is observed quite objectively.

The only thing which psychologists need, then, is to recognize, generally and in principle, that this form of procedure is the only possible one, and also that it is precisely the same as that of all other exact sciences. Behavior, i.e., the reaction of a living system, is its only subject-matter, and behavior in no way includes any direct experience. In the experimental work of the future, even the highest forms of behavior will be studied and described in purely objective terms. They *must* be, since personal, direct experience does not occur at a single point in the whole experiment. For some this truth is somewhat hidden by the fact that in most of our experiments language reactions are of a special importance. If the experimenter himself enjoys what he calls direct experience and if this experience includes a great many things associated with words, he will be inclined to take the words of his subject as signs of similar direct experience on his part. Nevertheless, the words are registered as responses of the subject, and as such are purely objective, physical facts, produced by certain processes in the larynx and the mouth of the subject. Though the experimenter knows that other objective processes (of innervation, inhibition, and so forth) will have occurred before these sound waves are produced by the action of muscles, he will never be able to decide, according to our analysis, whether there was any direct experience accompanying those inner processes. Perhaps we should discipline ourselves to a less frequent use of language reactions in our experiments, until finally we overcome the danger of associating language with direct experience, and introspection will have disappeared from psychology as an exact science.

Of course, not all the reactions of our subject are observed with equal facility in the objective manner. Sometimes even strong stimulation will not produce any overt behavior which can be registered externally with our present methods. But in the majority of these cases the physiologist can give us highly valuable information about the functioning of the autonomic part of the nervous system and about the subsequent reactions which occur in the most important visceral organs, including the endocrine glands. One of the main tasks of psychology will be to develop and to adapt the physiological techniques until these visceral reactions can be registered with perfect ease under the conditions of behavioristic experimentation. We also have some evidence for assuming that, in addition to the reactions just mentioned, so-called "thinking" consists in slight innervations of the muscles concerned in speech reactions.

So far I hope that I have given a fair statement of the opinions which prevail among behaviorists. It ought to be the more correct, since I sympathize with these opinions in several points, most of all in the criticism of "introspection." Indeed, much of current introspection seems to be rather sterile and, in an odd contrast to its ambitions, to lead research away from the more urgent problems. Whether this be an intrinsic property of introspection or merely a consequence of some accidental mistake usually associated with it, we shall discuss later on.

At present, we have a simpler problem before us. In the natural sciences, so the behaviorist tells us, methods deal with objective reality, whereas in the treatment of direct experience—if there is such a thing—they deal with something entirely subjective. Is that true? Is that the real reason why natural science has won the admiration of the world and psychology is still in an embryonic state? I cannot admit it. It seems to me that, starting with an admirable enthusiasm for exactness, behaviorism has been misled completely at this point and that, consequently, the energy spent by behaviorism in its fight against "direct experience" and "consciousness" has been spent in the wrong direction. So, at least, the situation appears to me because, whatever may have happened to the keen leaders of behaviorism in their individual development, I must give the following report about myself. It is a well-known story, but some psychologists seem to have forgotten it.

There cannot be the slightest doubt for me that, as a child, I had "direct experience" before I even dreamed of a world like that of physics. Of course, I did not know that term then, which could acquire its meaning later on only by the very fact that the world of physics became opposed to it. There were innumerable varieties of experiences appearing as "objective," i.e., as existing or occurring independently and externally. There were other experiences which belonged to me personally and privately, and insofar were "subjective," such as, among others, dreadful fear upon certain occasions, and an overwhelming, warm happiness at Christmas.

In the following chapters we shall be occupied mainly with "objective experience." This term, however, may be misinterpreted rather easily. So I shall try to specify its meaning a little more. Doing so, I shall even run the risk of repeating some arguments several times, because this is the point where most of our difficulties arise.

The name "experience" seems to indicate that, though appearing as "objective," those things and events were felt by me as "being given in my perception" and therefore as "subjective." Certainly, they were not. They simply *were there outside,* and at that time I had no suspicion whatever of their being the effects of something else upon "me." I must go further. There was not even a question of their depending upon my presence; upon my keeping my eyes open, and so forth. When I went around an object it remained the same identical thing. So absolutely "objective" were those objects and events, that there was no place left for a more objective world. Even now, this "objectivity" is still so strong and natural, that I am constantly tempted to attribute to the interior of these self-same "things" all those properties which, in the meantime, physics has taught me to attribute to objects of the physical world. When, in these pages, I talk about "direct experience" of the "objective" type, the word is always used in this meaning: e.g., a chair as something there outside, hard, stable, brown, generally without any taint of "being perceived" by me, of being "a subjective phenomenon."

> In some cases, it is true, the discrimination between the "objective" and the "subjective" side of direct experience may become dubious: as with after-images or with the prick of a needle in my finger. That does not make the discrimination less important in itself. In physics the discrimination of the substance conducting the electric current and the isolating material remains of high value, though between the extremes we find a great many intermediate cases. For us, the main point is the fact that in "things" and their "movements," etc., an almost unsurpassable degree of "objectivity" is reached.

At the time when I began to understand physics I did not learn only about the physical world. Another lesson was necessarily connected with studying physics, and it was this lesson that gave the term "direct experience" its connotation or, better, which introduced me to a manner of thinking that made the meaning of "direct experience" clear. The physical world could not be the same as that "objective" world which I had had around me the whole time. Certain physical objects were found to influence a particularly interesting physical body, viz., my physical organism, and what has been called "objective" experience here, was shown to depend upon subsequent and very complicated processes in that physical organism. Though that influence, exerted by other physical objects, starts these processes in the organism, there is no possibility of identifying the final product, "things" and their "changes" as I have them in immediate experience, with those physical objects from which the influences came. If a wound is not the gun which emitted the projectile, then the "thing" which I have before me, which I see and feel, cannot be identical with the corresponding physical object. This object, influencing my physical organism, produces certain disturbances in it, *the final outcome* of which is the "thing" before me in direct experience.[1]

Such "things," however, were the first items *I knew*. Therefore, a picture of the physical world had to be *constructed* after I once appreciated the necessity of it. Hence it is built up by inference as something found indirectly or mediately; and, *in contrast to it,* the world around me, as I had it before and still have it, quite apart from the physicist's construction, is now called the world of direct, or immediate, experience.

[1] By the way, the same warning applies to the relation between my organism as a physical system and "my body" as I have it in direct experience. Obviously "my body" is the outcome of certain special processes in my physical organism—started in the eyes, muscles, skin, etc.—exactly as the chair before me is the final product of other special processes in the same physical organism. And if the chair is seen "before me," the "me" of this phrase means my experienced body, of course, not my organism as an object of the physical world. Even accredited psychologists do not always seem to be clear about this point. Later on we shall come back to it.

But how can I say that a "chair," for example, is an "objective experience" if I must admit that it depends upon certain processes in my organism? Does not the chair become "subjective" then? It does and it does not. At this very moment the meaning of our terms has changed. In the last paragraphs "objective" has denoted a certain experienced property which some parts of my direct experience, in contrast to other experiences, possess *as such* (exactly as they have size, color, hardness and so forth); the term "subjective" in this paragraph means their genetic dependence upon my physical organism. "Subjectivity," in this latter meaning, is not itself a directly experienced property, but a relationship which we ascribe to "objective" experiences after we have learnt to regard them as the outcome of organic processes and, therefore, as distinct from the physical reality external to the physical organism. Sometimes the two connotations of the term are confused in the most deplorable manner as though what is "subjective" genetically ought also to be "subjective" in experience. So most introspectionists seem to think that, properly speaking, the chair before me must be a subjective phenomenon *in experience,* appearing "before me" only in consequence of some illusion. And since there is no "subjective" chair to be discovered, the behaviorist derides the introspectionist for dwelling in a world of imaginary ghosts. The simple truth is that some of the experiences depending upon processes in my physical organism have the character of "objectivity," whereas others, depending upon other processes in the same organism, have a "subjective" character, this contrast being something altogether indifferent to the *genetic* "subjectivity" of *both* types of processes and experiences, as depending upon the physical organism. After this, I hope that no misunderstanding of the term "objective experience" will be possible. When I talk about "a chair," the chair of my everyday life is meant and not some "subjective phenomenon" to be observed perhaps by highly trained introspectionists, but utterly unknown to me.

On the other hand, we have seen that we cannot identify the chair of "objective experience" with the chair as a part of the physicist's world. Under these circumstances, the world of direct experience being the first I had, and since all that I know about the physical world being inferred later on from the properties of the experienced world, how can I deny this experienced world which, for me, is the only basis upon which I can continue to guess about physical realities? No one can prevent me from thinking, if I choose, that after all the physical world may be the more important and more essential one. But even then I must confess that the other world has existed first and always *for me,* and that I still can see no other way of discovering the properties of physical reality but by observing "objective experiences" and drawing my conclusions about the physical world from them. With future progress of physiology I may become able to discover even the nervous processes underlying my "observing" and "guessing" and so be able to give a physical theory of these events. But even then, since the world of physiology is part of the physical world and *as such* is not directly accessible to me, any progress whatever along this line will depend upon my observing what I call a body or a nervous system as parts of "direct experience." It may be otherwise for behaviorists, so that the physical (and physiological) world *as such* is directly known to them, and that for them the term "known" has nothing to do with direct experience. But that cannot change my report about my own case in which affairs are as I have described them. It is in the terms of this description that I must decide whether I shall become a behaviorist or not.

What then about the behaviorist's statement that observation in physics deals with objective reality whereas, in the treatment of direct experience, it deals with something subjective?

Let me describe my own procedure when I investigate the physical properties of a physical object. Is there any considerable amount of $H_4C_2O_2$ in a given mixture of chemical elements? I know about these substances by the corresponding "objective experience" before me, and I find the positive answer to the question by smelling, i.e., in direct experience.

This being a rather crude procedure, let us consider a case of fine measurement. What is the intensity of an electric current which, under certain conditions, must exist in a given wire? The position of a needle on the scale of a definite apparatus will tell me, but it will tell me in direct optical experience, the whole apparatus being an "objective" part of my actual experience, exactly as the wire and the given set of conditions must manifest themselves either as parts of direct experience or by a sufficient testimony of their existence in some part of experience which I have learnt to regard as a sign for them. And this is true of all possible objective statements or measurements which I shall ever be able to make in physics. They are statements about my actual objective experiences in these different cases. I shall never be able to make a direct statement about a physical event *as such*. Therefore my observation of physical facts will always remain of the same kind in principle as that of an after-image, or of the "confusion" which I find so characteristic of peripheral vision, or of my feeling healthy. Therefore, the exactness of my physical observation cannot depend upon my avoidance of "direct experience" in physics. I do not avoid it, for I cannot avoid it. Yet the procedure *works*. *Some* observations of direct experience must then be an adequate basis for science.

If all the concrete statements which I can make in physical research are observations of direct experience, there are some inevitable consequences. How do I define my terms as a physicist? Since my knowledge of physics consists entirely of concepts and observations derived from or contained in direct experience, all the terms which I use in discussing physical problems must refer either to concepts or observations or, more probably, complicated combinations of the two—in any case, products of my direct experience. If I try to define them, my definitions must refer to other concepts or observations, and so forth. A gestural denotation toward the locus of the phenomena about which I am talking, as, for instance, a hint where to make certain observations or have certain direct experiences, will be the ultimate step in any process of definition I may attempt. Even the most abstract concepts of physics, as, for example, that of entropy, can have no meaning without a reference, indirect though it may be, to truly direct experiences. I shall never be able to give a definition of my terms in physics or to understand a definition given by others, which, in this respect, differs in principle from what I may use as a definition in psychology. Nevertheless the method of physics is successful. I never have difficulty with definitions when I talk with physicists about their science. Hence some definitions, or hints about cases of direct experience, must be sufficiently safe for exact science. Therefore the exactness of my definitions in physics cannot be the result of their being independent of "direct experience," for they are not independent.

But the behaviorist tells me that observation of my direct experience is a private affair of mine, whereas in physics, two physicists can make the same observation on the same galvanometer. I deny the truth of this statement. If another man observes the galvanometer, he observes something other than the galvanometer as a physical object, since the object of his observation is the result of certain organic processes, determined by the physical galvanometer. Again, the galvanometer I am observing is the final result of a different series of processes occurring in *my* physical organism. By no means do we "observe the same instrument" then, though physically both series of processes are started by the same physical object. Still, our statements about our observations seem to agree so well in a great many cases, that we do not discuss the question very much, whether there is an absolute philosophical proof for sufficient similarity between my galvanometer and his. Again the procedure works. Since I, at least, do not know about a galvanometer except through "objective experience," and yet almost all the statements which the other man makes agree with my statements, the privacy of direct experience does not disturb us at all—in physics. Again, in principle, there is no difference here between physics and psychology. All physicists, when working with others in such a case, are convinced naïvely that their fellow-physicists "have a galvanometer before them," and that their observations refer to it. In this sense they assume that their fellow-workers have a very definite "objective experience," highly similar to their own experience, and they quite naïvely understand the words of their colleagues as statements about that experience. That, however, is allowing private affairs in exact science. It does not seem to disturb scientific procedure, as it does not disturb the affairs of everyday life, wherein it is the most general and natural occurrence. In certain cases, therefore, belief in the "direct experience" of others must be innocent and is not a hindrance to scientific progress. If psychology does not make rapid advancement, the reason for it cannot be that belief as such.

There remains one consequence of the elementary fact that observation in physics involves observation of direct experience. As a physicist, observing my apparatus, I do not fear that my activity as an observer has any serious or distorting influence upon the essential properties of what I observe, if only I keep myself, as a physical system, at a certain distance from the apparatus, as another physical system. Now, as direct experiences both the apparatus to be observed and my "activity" of observing depend upon processes in the same system, viz., my physical organism. Again the behaviorist must be wrong when he declares that, because of the fact just mentioned, direct experience when observed introspectively is not being observed scientifically, since in this case of observation in physics, the situation is similar: the material to be observed and the process of observing belong to the same system. Thus we see that the physicist and the psychologist are in exactly the same situation in this respect. It does not matter at all whether I call myself a physicist or a psychologist in observing a galvanometer. In both cases my observation is directed towards the same "objective experience." The procedure works in physics. Why not in psychology? There must be some cases in which my observation of direct experience does not disturb the observed facts seriously.

This argument contains in itself a most remarkable limitation of the range of its own application. It does not mean at all, that all forms of so-called introspection are justified, even less does it mean that *generally* the products of so-called introspection are independent of introspection. Here the critical position of behaviorism has exaggerated the scope of a fair argument and unjustly applied it to *all* statements about direct experience, although the critical point is thoroughly justified in a great many cases.

I have described how, as a physicist, I must deal with direct experience. Evidently some epistemological difficulties with respect to the objectivity of procedure in that science may be derived from that description by a very extreme purist, as the behaviorist seems to be. But, fortunately, those dilemmas had not yet been discovered, when in the time of Galileo, Newton and Huyghens the first really important steps were made in physics. Those great investigators carried on, pragmatically naïve and happily undisturbed by a "behaviorism" of physics which might have impeded or blocked the whole development for the sake of "epistemological purity." Somehow the procedure has worked in spite of the fact that logically, perhaps, it is not possible to certify it as absolutely objective. Sciences which wish to carry on their researches and be productive often show a certain healthy disdain of such scruples. It might be better for psychology, too, if, after listening to a very wholesome critical lesson from behaviorism, it returns to undertaking productive work with some naïveté, using all possible means which yield results.

As a scientific attitude the Homeric assault of behaviorists against "direct experience," "consciousness" and so forth, appears very strange to me. They do not generally show an undue interest in epistemological considerations. It is just *one* question which suddenly arouses their interest: How can I know about the direct experience of others? I shall never have a definite proof of the validity of such knowledge. But physics, that is another matter! There we are safe! The behaviorist, however, forgets that it is a truism in epistemology that I shall never be able to "prove" conclusively the existence of an independent physical world. As an extreme purist I might argue this point exactly as the behaviorist disputes the assumption of direct experience in others. Somehow it does not occur to the behaviorist to apply his epistemological criticism to the assumption of the physical world. He does not say: Thou shalt not work upon a physical world, the existence of which will always remain a mere assumption. On the contrary, he assumes its reality with all the healthy naïveté which he lacks in psychology. The reason is, perhaps, that the achievements of physical science are imposing and have become the ideal of behaviorism. But to the mind of a methodological purist that cannot be taken as a satisfactory proof of the existence of the physical world. Of course, personally and practically I am as convinced of its existence as any behaviorist has ever been, and I am fully aware of the fact that sciences may and must believe and postulate, where the epistemologist, if he likes, may doubt. But then I shall believe and postulate the direct experience of others, as well as the existence of physical entities, if this makes my procedure simpler and more productive. And I feel the more justified in this attitude, since I have seen that all work I may do in physics is founded on direct experience and that, therefore, the enormous superiority of physics over psychology in accomplishment, cannot follow from any differences in this respect.

At this moment I see the behaviorist smiling ironically: With all his medieval philosophy Mr. Köhler will never make any progress against the sound scientific basis of behaviorism. I answer that this basis of behaviorism is as philosophical as anything could be: it is purely epistemological. In this connection the only difference between the behaviorist and me is one of completeness: the behaviorist sees a single theorem of epistemology and, as an extreme purist, he dwells exclusively on this one point, ignoring the context from which it has been taken. I am fully aware of this context; it is stated in the foregoing argument; and I find myself therefore with a profound aversion and guard against the behaviorist, or any other one-sided and impractical purism in science.

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II

Psychology as a Young Science

IN the first chapter I became slightly autobiographical in order to show why I have found it impossible to become a behaviorist. If I am ever to become one, some adherent of that school will have to effect the conversion. I do not myself see the way.

In the preceding pages direct experience was shown to be the raw material of both physics and psychology. If the physical sciences are so far ahead of psychology in their development, and if we wish to imitate their achievements, what shall be our method since, in one respect at least, there is no difference in the material?

Contemporary physics has a real advantage in the careful selection of the experiences which play a decisive rôle in the crucial moments of physical research. Of course, the physicist ignores all "*subjective* experience," as defined in the previous chapter, as far as possible, because apparently it has nothing to do with the properties of the physical world. All the difficulties of psychologists who try to observe and describe subjective experience are simply avoided by the physical sciences.

But the selection and exclusion does not stop there.

If most "objective experience" was taken as evidence of physical existences and physical events in early investigations, a more critical view of the situation tended to eliminate large parts of this material as well. The process of selection has become particularly severe, since the advancement of science has made it possible almost everywhere to transform qualitative observation into quantitative measurement. At the present time almost all physical measurements are made in an extremely indirect way. We do not observe some experiential object corresponding directly to the physical variable in question; we observe a related but different object the virtue of which is that it lends itself to more exact statement and is shown, or supposed, to be in a well-known functional relationship to the given physical variable. Of all possible objective experience nothing seems better to fulfill the requirements of the physicist than the localization of a single line (a needle) on a scale of other lines, especially if this localization becomes the coincidence of the first line (needle) with one of the others (or inclusion between two which are near to each other). The range of direct experiences used in measurement has now reached a minimum value, indeed. It almost seems as if the same scale and the same needle were used universally; it tells a hundred altogether different stories about the physical world. It means "atmospheres" or "volts," or "ampères" or "temperature" and so forth, as the case may be. In addition to observing a definite coincidence, and controlling the connection of the apparatus with the system to be observed, the physicist has only to recognize certain words and figures on the scale. That is all the direct experience which enters into the procedure. There is not much opportunity for serious blundering. Even so simple a property as physical size is not measured directly. No physicist measures the length of an object by comparing it with the experienced length of a standard object directly. That would not be accurate enough. Rather he prefers the method of coinciding lines or points. Defining physical length by this method, he measures the length of a line by observing the coincidence of its ends with certain points on a scale.

If now we ask ourselves whether we shall imitate the procedure of the physical sciences in psychology, two answers are to be given to our question, because that procedure has two aspects. In the first place, the procedure involves making statements about physical systems by means of objective experience. As the behavior of men and animals is observed in our objective experience—the direct experience of the *subjects* not playing any rôle in the matter—such a study is perfectly legitimate and undoubtedly will be much more extended in the future. Though it existed before the days of behaviorism, this school is fundamentally right in praising the advantages of objective procedure as against prevailing introspection. Although behaviorists have gone too far through failing to appreciate that, even in the objective method, direct experience remains the raw material as "the objective experience" of an observer, their error is of no practical importance as long as the second part of their answer to our question is proper. Unfortunately, behaviorism has here tended to take the wrong position.

As we have seen, in present physics objective procedure is characterized by the use of a small group of selected experiences and, consequently, by the exclusion of the great variety of objective experience, the exclusion being made to satisfy the requirements of quantitative measurement. Shall we do the same thing in psychology as a science of behavior? Evidently that will depend upon the nature of behavior itself. It is difficult to judge about a method as such. A method is not good or bad in itself. It is good if it is properly adapted to the essential aspects of my problems and my material; and it is bad if it lacks regard for those essentials or misdirects research. Therefore, what has been shown to be an excellent procedure in one science or for some problems may be altogether useless or even a hindrance in another science or for other problems. In this respect, behavior is easily seen to have distinctly different phases and to give the psychologist different tasks correspondingly. Wherever indirect and quantitative methods can be applied in conformity with the task, they should be applied. When C. P. Richter and his collaborators at the Phipps Psychiatric Clinic have once discovered the way in which the various drives of animals, and their temporal changes, can be investigated by registering amounts of general or of special activity,[1] we shall be keenly interested in the future development of the method. It is the correct procedure in cases in which total amounts of activity in relation to outer and inner conditions can yield us valuable information.

But what about other cases where either our problems are not of the quantitative type or we have no way as yet to transfer our observations from the direct experiences with which we are concerned to a special region, indirectly connected with the first, but better adapted to finely accurate statement and registration? Obviously, qualitative types of behavior are not less important than quantitative differences of some sort. Where we know about the qualitative varieties which occur in behavior and where, in a concrete case, we are sure about the special type before us under observation, we may be interested in finding ways of quantitative measurement. But the discrimination of qualitative types must be primarily accomplished. During the observation of a puppy under a given set of conditions, this important ques-

[1] Cf. C. P. Richter, *Animal Behavior and Internal Drives. The Quarterly Review of Biology*, II, p. 307, 1927.

tion will frequently arise: whether certain behavior of the animal represents playful activity or "serious" reaction to the situation. Such a question does not imply "consciousness" in the puppy; it refers to some very characteristic differences in what is being objectively observed. The difference is one of "quality" of behavior. Again, while observing a man in a somewhat critical situation, it may be decisive to observe whether he talks to us in a "steady" or in a "shaky" voice. At the present time this is necessarily a qualitative discrimination; even should we find in the future a method to measure the steadiness of a voice, this method would forever presuppose that in direct observation we know what is meant by unsteadiness as a definite characteristic of the voice.

In a similar way, we are restricted in the application of indirect methods to many other forms of behavior. I take it that the behaviorists hold that we can investigate the "emotional behavior" of subjects without being involved with their "consciousness" or "subjective experience." Certainly, psychologists have tried to transfer observation in this case to a region of more accurate registration and measurement. Much work has been done to develop pneumographic, plethysmographic, galvanographic methods, and so forth. The result is not altogether encouraging, however; for again, our interpretation of the curves recorded by all these apparatus depends greatly upon simultaneous *direct* observation; by no means do we feel justified to draw conclusions from the curves alone. All these procedures seem to be more problematic in themselves at the present time than an aid for the solution of psychological problems. In most cases it is easier to see "anger" directly in a subject's attitude than to state and measure, let us say, the adrenin in his blood.

Why does this difficulty beset behavioristic psychology, and not occur in physics? The answer is simple enough: Physics is an old science and psychology is in its infancy. Over centuries physicists have succeeded in eliminating a great many forms of more direct and qualitative observation, relying upon a small number of indirect and very accurate experiences as decisive in observation. Their success depended on previously acquired knowledge about the physical world. Most indirect methods and sound measurement presuppose a vast background of knowledge. Qualitative observation, experimentation and analysis have provided the primary knowledge which enabled the physicist to discover where he could measure an important factor, and what physical relation makes an indirect observation capable of indicating occurrences not directly observable. Oersted had to discover the deflection of a magnet in the neighborhood of an electric current before exact measurement of intensities of current became possible. His was a qualitative, direct observation; indirect and quantitative procedure was the fruit of it. Even in our days, Röntgen did not *measure* at the start of his investigation of X-rays. He had to discover and to analyze their properties in qualitative experimentation first of all. Later on his rays could become a means of measuring the constants of crystals. Too easily do we forget the simple truth that in their origins as well as at the opening of a new field of special investigation later on, the natural sciences, physics, geology, biology, rely upon qualitative statements almost completely. Certainly, the quantitative and the accurate indirect methods are the most imposing features of exact sciences *now* when we review them or consider them superficially. But we ought to be aware of the fact that in most cases this procedure is a mere refinement and end result of the underlying free and direct observation, without which there would not have been a basis upon which to build the refined superstructure. In the eighteenth century Cavendish measured the resistances of different materials by comparing the shocks he received in his arm when conductors of those materials were used to connect him with a battery, the other pole of which was in his other hand! Was that improper? On the contrary, it was perfectly sound in a young science, which was trying to find that first knowledge of facts upon which more refined methods may afterwards be founded.

From this it follows that where we have a good quantitative problem in psychology and an accurate method of measuring and registering, as in the case of Richter's excellent experiments, we can immediately apply the quantitative and indirect procedure, as used in physics. The problems which Galileo attacked in the seventeenth century could be solved quantitatively at once, the qualitative experience of everyday life having sufficiently provided the necessary basis. But for the majority of psychological problems this is not the case. Where do we have that first more or less qualitative knowledge of important functional relationships in psychology which might become the basis for indirect and exact measurement? It does not exist. Since the development of more "exact" methods presupposes its existence, our main task must be the gathering of that knowledge. In most cases our preliminary advance in this direction will have to be crude and qualitative. Whoever protests that conclusion in the name of purism does not understand our actual situation in psychology; he sees neither the nature of, nor the historical background prerequisite for, special quantitative methods. If we wish to imitate the physical sciences, we must not imitate them in their contemporary, most developed form; we must imitate them in their historical youth, when their state of development was comparable to our own at the present time. Otherwise we should behave like boys who try to copy the imposing manners of full-grown men without understanding their *raison d'être,* also without seeing that in development one cannot jump over intermediate and preliminary phases. A survey of the history of physics is certainly illuminating. Let us imitate the natural sciences, but intelligently!

Behavior is enormously rich in forms and nuances. Only acknowledging this wealth, and studying it directly as it is given in all its fascinating varieties, shall we become able gradually to find those forms of more quantitative, and perhaps more accurate, procedure which may become as adequate for us as are the methods of physics in its realm. At present, and in this broader historical perspective, qualitative observation and analysis may be, in a sense, more exact, i.e., adequate to our subject-matter, than much blind measurement. We shall press forward towards more refined methods, of course; but owing to our situation as beginners, we can go forward only through the use of less refined methods for the time being.

If organisms were more similar to the systems investigated in physics, a great many methods of the physicist might be introduced in our science exactly as the physicist uses them. But the similarity is not very considerable, practically. One of the greatest advantages which makes the physicist's work so much easier, is the simplicity of his systems. And his systems are simple because in physics the experimenter determines their main properties himself. He prepares them, more or less. I am far from believing that organic processes are of a supernatural kind. On the contrary, the most startling difference between the organism and a simple system investigated by the physicist is the enormous number of physical and chemical processes which, depending upon each other in the most intricate manner, occur in the organism simultaneously. And we are utterly unable to create simple organic systems for elementary study. An amœba is a more complicated system than all known systems of the inorganic world. We also know that, in studying the properties of a nerve-muscle-preparation, we are not investigating "a part" of natural behavior; because, physiologically, its properties are not only simpler but also radically different in some respects from what they would be if the same nerve and muscle were contributing to normal behavior. It is the *whole* organism, as some behaviorists have rightly said, the behavior of which is our subject-matter. But in the whole organism one can seldom follow the change of one variable alone as the consequence of one change in outer conditions. The change of one factor usually involves concomitant changes in a great many others and these affect the first retroactively. Now, isolation of changes and reduction of variables are the great artifices by which the physicist makes exact measurement possible in his simple systems. Since this procedure does not work in our case, since we have to take the organism more or less as it is, any kind of observation that gives us an expression of its total attitude and activity, of the integrated tendencies and states which it undergoes, will be the right observation to begin with. Such is typical behavior as we see (or hear) it before us, changing, with the conditions we give our subject, in innumerable phases, and much too attractive to be already excluded for the sake of narrowing ideals, taken prematurely from quite another field of research.

In the meantime, however, the outward aspect of physics has been too seductive. Since experimental psychology first became a science, every now and then a wave of blind imitation has swept it off its feet. Fechner himself was the first to copy *adult* physics when psychology was an embryo. He seems to have been convinced that measuring in itself would make a science out of psychology. We have seen the result: If flowers are impossible without a root and a stem, measuring, which is fruitful only as the most refined consequence of previous qualitative observation and experimentation, necessarily becomes a dead routine without it. Hundreds of thousands of quantitative psychophysical experiments have been made almost in vain, because no one knew just what he was measuring or what were the processes upon which the whole procedure was built. In Fechner's own day, however, psychology came into existence as a science, not as a result of his psychophysics, but rather casually and *in spite* of the premature quantitative program.

The lesson seems to have been forgotten, however. Sometimes, when viewing the admirable energy with which "intelligence" is tested quantitatively by hundreds of able psychologists, one is almost reminded of Fechner's time. In this case, it is true, there seems to be a positive result for practical purposes at least.

Apparently something like a crude total ability for certain achievements is measured by those tests which have been invented and applied since Binet and Simon. And since the test-scores show a rather satisfactory correlation with certain general abilities in study or other work of subsequent life, we have in these tests a first approach towards a practical prognosis. Still there is a grave danger in this very success. Do we know or do we learn by those tests what processes and factors, which are masked by the gross scores, coöperate in the test performance? We do not know, nor do we learn much, concerning these factors. Figuratively speaking, a given total score may mean: degree 3 of "intelligence," together with degree 1 of "accuracy," with degree 4 of "ambition" and degree 3 of "quickness of fatigue," or it may mean "intelligence" 6, "accuracy" 2, "ambition" 1, and "quickness of fatigue" 4,—and so forth.[1] Quite a number of different factors may combine in various proportions in order to give the same I.Q. And that fact matters, even for practical purposes. In the educational situation a teacher should treat the child according to the actual nature and strength of the various factors constituting the total I.Q. of a given child. This is not a new criticism, of course, but it must be repeated for the sake of science. We seem

[1] If it can be demonstrated more generally that the tests measure "speed" primarily—as Boring and Teak have found in their subjects—the value of the method would be more restricted than I have here supposed.

to be too well satisfied sometimes by our testing enterprise because it is a quantitative procedure, and as such it appeals to us as thoroughly scientific. This is superficial, however. If we compare the nature of testing with the main traits of our ideal, viz., the procedure of physics, we find a difference which may be decisive. What does the physicist ask when he must face a new field of research? In such cases his questions have been: Is light an oscillatory process? If it is, does it oscillate in the direction of propagation or vertically to it? Is magnetism produced by the magnetic fields of elementary currents in molecular structures? Why does surface tension produce *regular* forms of liquids and liquid films? How can the spectrum of one element contain thousands of different lines? This is the type of question with which we find the physicist occupied. They are his fundamental problems, in the treatment of which quantitative work appears at definite stages as an aid toward the solution, eventuating finally in the determination of laws, but directed and governed throughout by those questions about the *nature* of states and processes. What are the phenomena? Are they of one type or another? Such are the main problems of experimental science, in the *service* of which we find measurement. If now we ask ourselves what problems about the processes underlying intelligent behavior we are solving by our tests, not very many of us will have an answer ready. But some will answer that from now on, intelligence shall be defined as the X which is measured by those tests and that measuring is more scientific than grubbing among the unknown functions of the nervous system. The fact is that we are not in the habit of asking questions about underlying processes in psychology, questions similar to those in which the whole interest of physicists is centered. Instead of imitating the very kernel of physics, we assume merely the outer, quantitative form of exact science. Think of a physicist interested in all types of motors who would restrict his study of them to the following tests to be applied to all the various types: measurement of their volume, of temperature at their surface, of ionization of the air in their neighborhood, of their actual frequency of revolution, and of the total weight of the bodies; who would calculate from these data an average "power-coefficient" for each of them, define "power" by the method, never ask a question about the working process in their interior, and remain satisfied by this quantitative procedure for many years. I know I am exaggerating in some respects if I compare intelligence testing with such an attitude. I do it intentionally in order to call attention to the main point, viz., that admiration for quantitative method leads us almost exclusively to those research tasks which immediately afford us an opportunity for measurement. Given the natural narrowness of human interest, the next consequence will be our failure to see those problems which do not lend themselves immediately to quantitative investigation, problems which may be, however, the more essential and the more scientific, in a deeper meaning of that word. In such problems qualitative observation and experimentation would constitute the first step; but since in adult physics (as it appears from without) these methods seem to be despised, we dare not undertake these fundamental tasks. Wishing to be utterly scientific, we may just miss those opportunities for research which, in our stage as an immature science, would be the most scientific of all. The nature and the actual development of our problems should decide about the methods to be followed. What actually happens, however, is that an imposing method blurs our view of the subject-matter. This method then begins to decide what problems we shall see. I do not object in principle to testing as a preliminary mode of diagnosis and prognosis. But just as the engineer learns from the investigations of processes in physics, even practical diagnosis in applied psychology will never go very far without an eager interest in the inner nature of intelligent behavior. Eventually, if all psychologists occupied with "intelligence" are engineers in the field of testing, who shall investigate the "physics" of intelligence, without which we shall never have a true and sensible engineering?

Occasionally, in animal psychology, the situation has tended to become somewhat similar to that just described. In experimentation on animals almost the only quantitative method is statistical. We do not measure in the proper meaning of that term, but we count positive and negative cases at least. In order to have cases which are comparable, and therefore may be counted, we put our animals in situations where their behavior is restricted to a few types of reaction. These we count and the result of our experiments is given in frequencies enumerating these few possible alternative reactions. I do not criticize the method as such; I have used it myself. But we should never forget that, when used exclusively, it may make our knowledge of behavior narrower. With a given problem in mind we exclude a great many behavior possibilities by our choice of experimental conditions. We do not see them any more. Some psychologists will at least observe the remaining forms of behavior during the experiment as an aid for the interpretation of quantitative results; but others will not, because in their opinion what is not quantitative cannot be scientific. In this case, what is left of the material are abstract figures. Even then the procedure may be valuable, if the experimenter is one of those who always find new and productive problems for experimentation. For most of us, however, conservatism may become the consequence of such an attitude, since, admiring figures and curves, we keep away from the true source of new ideas and new problems in our youthful science: i.e., from a broad outlook upon our subject-matter.

Once more I venture to defend broad and qualitative information as a necessary supplement to quantitative work. Otherwise behavioristic psychology might become as sterile as supposedly it is exact. Too great an interest in present quantitative methods and in their standardization is not a hopeful response to the fact that the development of psychology depends upon the discovery of *new* methods and problems in the future, and not upon the monotonous repetition of a few standardized methods which fit only our primitive problems. If the reader says that behavioristic psychology does not need this advice, I reply that Watson has been criticized because his well-known observations on children were not made in the sanctified form of quantitatively controlled experiments. I do not think that those observations give quite an adequate account of autochthonous reactions and of learning in children; but, on the other hand, they reveal more interesting facts than we might have learnt from columns of abstract figures. An eminent psychologist was friendly enough to say a few approving words about my own work on intelligent behavior in apes. Still, he added, you have missed the main point by not applying the statistical method. To my mind this means an inability to see the problems which I had tried to treat in a preliminary way. Those problems concerned the characteristic forms of intelligent behavior in a special type of animal, as influenced by various situations. What may be valuable in those observations would disappear in an abstract statistical treatment of the "results." Am I right then in warning against a one-sided glorification of quantitative procedure *as such?* Let us apply it, but not before we know by qualitative analysis where to find good problems for quantitative research.

A certain broadmindedness and a clear appreciation of our present predicament would be wholesome, then, in the application of behavioristic methods. There is more in a scientific method than observing, experimenting and measuring. In physics, at least, observing and measuring usually result from definite questions, and these questions are at first prudent forms of hypotheses about unknown properties of nature. Observed but enigmatic aspects of nature may be deduced and explained from assumptions about unknown parts of it. Taken as such, however, an assumption is not what the physicist wants. Fortunately, a concrete assumption made in order to explain certain observations will always lead to consequences other than those facts for which it was made. These new consequences are to be tested in further observation and experimentation. What is the hidden basis of electrolytical conduction? Arrhenius makes the hypothesis of molecules being dissociated into independent ions. By their charge conduction is explained. But from their assumed independence definite conclusions are to be drawn about the optical properties of the electrolyte. These are tested in new experiments. Thus, productive method contains a bold hypothesis as essentially as it requires adequate observation, and the growth of physics is a constant oscillation from the first to the second and vice versa.

In behavioristic psychology we cannot complain about a lack of unknown parts of the organic systems we study as a source of productive hypotheses. We know a little about the elementary effects of stimulation upon the sense organs of our subjects; we observe their responsive behavior when it becomes overt. But between the two terms of the sensori-motor circuit there is more *terra incognita* than was on the map of Africa sixty years ago. If behavior is to be understood as depending upon inner organization as well as outer conditions, we must try to imagine the modes and traits of these inner processes, which are either started from without by environmental stimulation or aroused intra-organically by inner dynamics. For those who know the history of physics and wish to emulate it intelligently in psychology, this task of finding productive assumptions about the hidden parts of behavior will probably appear the most important of all. The whole future development of psychology depends upon it. Here all the creative force of behaviorism ought to be concentrated in a fine emulation of physics. Behaviorism's critical attitude toward introspection and direct experience is an absolutely negative feature of the movement, at a time when *positive* ideas are needed. If I feel a little disappointed by the work of behaviorism, the reason is not so much a certain innocence in its treatment of direct experience and in its imitation of adult physics, but its astounding sterility in the development of productive concepts about functions underlying observable behavior. As an imitation of physics it is scarcely a satisfactory achievement for the behaviorist to have taken the old concept of reflex action from physiology (including the reflexes of inner secretion) and to give us no further comprehension into the formation of new individual behavior than is offered by his concepts of positive and negative "conditioning." Why should behaviorism be so utterly negativistic in its characteristic statements? "Thou shalt not acknowledge direct experience in science" is the first commandment and "Thou shalt not conceive of other functions but reflexes and conditioned reflexes" is the second. I do not see how the behaviorist can reconcile this creed with our actual knowledge of organic processes and of behavior. Nor do I understand why, from the standpoint of "exactness," the organism should be conceived as such a crude and poor affair. In its effort to imitate the technical procedure of physics, behaviorism often shows a stubborn narrowness; the same attitude becomes quite striking in the exclusion of all but two types of processes from its assumptions about the inner side of behavior. What a strange conservatism and dogmatism in a revolutionary school! Even now, as an adult science, physics is allowed to have at least one new idea about the atom every year. Though *our* science is so very young, most behaviorists do not even dream of any possible change in the nature of their two fundamental ideas. The truth was revealed to them in its perfection at the birth of behavioristic psychology.

The two behavioristic concepts in question show one remarkable property: If you compare them with various types of processes in the inorganic world of the physicist, you will find that even simple physical systems are by far richer in the variety of their kinds of function than is the nervous system of man in the eyes of a radical behaviorist. A soap-bubble does not show us reflexes, it is true; therefore we cannot expect to find conditioned reflexes in it. Nevertheless those functional properties which the soap-bubble does exhibit are decidedly superior in some respects to the monotony of reflexes and conditioned reflexes. The same thing holds for innumerable structures in the inorganic world. Sometimes their behavior will at once remind us of the behavior of organisms, although they lack reflexes and conditioned reflexes. However, by the standard of exactness as the behaviorist understands it, it would be heresy to follow this hint. His imitation of physics excludes most of the functional possibilities given in physics itself. In organisms there shall be nothing but reflexes. Some members of the school even are beginning to protest cautiously against this strange situation.

It is not probable that an observer, looking upon human and animal behavior without prejudices, would find reflexes and conditioned reflexes as the most natural, or as the only, types of function by which his observations might be explained. For those, however, who are deeply convinced that the theory of original and acquired reflexes is the whole truth about the nervous system, there is no real incentive for the further observation of natural behavior, since they do not feel the need of any new information or new functional concepts. On the other hand, the restriction of observation to counting the frequency of the few reactions retained in standard experiments will protect those conservative concepts; and so one narrowness keeps the other alive.

But even with a more impartial interest in all the various forms of behavior, how shall we find more productive concepts, if the gap between the observable stimulating environment and observable overt responses is as large as we find it in the present stage of physiology? Of course we shall use whatever hint may be given by nerve physiology and endocrinology.

But even the more recent discoveries about the all or none law and the metabolism in nervous activity do not give us just that basis which we need for our purposes. In such a situation anything will be valuable upon which we may base a working hypothesis. In a critical predicament even a stone or a chair may become a weapon, though the strategist would despise it in exact warfare. There is no serious danger in developing a working hypothesis upon a somewhat wavering ground, since, in empirical science, such an assumption will be tested and continually corrected in further observation and experimentation. Either it serves as a path toward new explorations, its consequences being verified by success, or it is shown to lead us the wrong way. In the first case no one will have scruples about the previous legitimation of its birth; in the second, we shall discard it as soon as observed facts are against it, or as soon as we find it bare of real productive power.

The basis upon which I propose to build a working hypothesis is not altogether bad. In some points it is just as solid as objective observation is in behavioristic psychology, and in others, it is not insecure enough to raise doubts.

In an experiment in behavioristic psychology the "direct experience" of the subject does not play any rôle. If he has such experience I as experimenter do not assume that it has the slightest influence upon the course of the physiological events. My assumptions about those events must be able to explain thoroughly the overt behavior of the physical organism of the subject. Direct experience is not a "force" interfering with the chain of physical causation. (The opposite hypothesis is made by dualists. But I do not think that it, on the other hand, is productive, or that it illuminates the innermost nature of things.)

I cannot exclude *my own* direct experience in observing behavior, as I cannot avoid dealing with direct experience even when employing the most "indirect" methods of physics. Moreover in behavioristic psychology I have to use objective experience in a great variety of forms which no longer are material for observation in adult physics. If I can rely upon objective experience for my statements about the behavior of the subject I observe objectively in a behavioristic experiment, why should I hesitate to utilize it to develop a working hypothesis in the following manner?

Suppose I am used as a subject in a behavioristic experiment. With me as a subject, behavioristic research would have the same difficulty in developing a working hypothesis about the hidden processes underlying my behavior. But in this case I can help to build a bridge over the "large gap" mentioned above. During much of my own behavior I have direct experience. Now there is no doubt that this experience depends upon some of those processes about which we wish to formulate a working hypothesis. It is more than probable that, under these circumstances, I may use my direct experience in order to guess about these processes. It is granted that direct experience does not accompany *all* the unobservable events which produce the behavior observable from without; hence the hypothesis I am about to offer does not cover all cases. But in our crucial situation we have not much to choose. Confident as we are of further help from physiology, we are so utterly in the dark at present that it would be pure folly not to profit by any light that may fall upon some cases at least.

Let me declare that I do not propose to "introspect" in the technical meaning of the word. The same kind of simple and naïve statements about experience which I make as an observer of other people, of animals, of instruments and so forth, shall now be used for another purpose. Objective experience depends upon physical events outside my organism and upon physiological events *in* it concurrently. As depending upon physical events *outside* my organism, objective experience leads to the construction of the surrounding physical world; as depending upon physiological events *in* my organism, it gives me hints about these processes. *There is no reason at all why the construction of physiological processes directly underlying experience should be impossible, if experience allows us the construction of a physical* *world outside, which is related to it much less intimately.*

Obviously, I must have some leading principle in making my inferences from given properties of direct experience to the properties of concomitant physiological processes. In a more special form such a principle was established by Hering many years ago. Its content is the following: My various experiences can be ordered systematically according to the sorts and degrees of similarity found between them, exactly as animals are ordered in zoölogy and plants in botany. The processes upon which these experiences depend are not known directly; but if I knew their properties, I should be able to order them as systematically according to similarities as I do order the experiences actually. It is possible to imagine a great many different relationships between the two systematic orders, that of experiences and that of physiological processes. I should have ever so much difficulty in trying to relate definite experiences to definite processes so long as I failed to assume one specific relationship between the two orders, viz., that of *congruence or isomorphism in their systematic properties.* This principle is sometimes formulated more explicitly in a number of "psychophysical axioms."[1] Here it will suffice to give some examples of its application.

A sound of given qualitative properties is pro-

[1] Cf. G. E. Müller, *Zeitschrift für Psychologie, 14*, p. 189.

duced in various degrees of experienced intensity or loudness. The systematic order of all these different experienced intensities may be represented by a straight line, which means that, following that order, we have the impression of moving continually in the same direction. What may correspond to loudnesses in the underlying processes? The principle decides that whatever the concrete nature of these processes, the physiological fundaments of all the experienced degrees of loudness must show the same order as these show themselves, i.e., that of a straight line. More especially: if a definite loudness is situated between two other loudnesses in the systematic order of experiences, the process corresponding to the first shall be between the processes corresponding to the two others in the order of underlying physiological events. That gives the congruence or one-to-one correspondence between the two systems, which I mentioned above.

At the present time the all-or-none law does not allow us to choose "intensity of nervous activity" as a physiological correlate of experienced degrees of loudness. But our principle could be equally well applied if frequency or density (number) of elementary currents were taken as the correlate of loudness.

As another example, colors in their relation to underlying processes have been considered so thoroughly by G. E. Müller,[1] that there is no need of

[1] *Loc. cit.*

repeating his theory here. In some respects his assumptions go far beyond the principle we are discussing here, insofar as he draws conclusions about the *retinal* processes, whereas the principle *as such* applies only to the processes underlying visual experience directly. In his hypothesis it also happens that these processes are chemical reactions. Such a transgression of the principle is perfectly sound, however, and shows the productive force of it. If there is congruence between the system of color experiences and that of underlying processes, not all kinds of physical events will appear fit to give a system of so much variety (of so many "dimensions") as the system of colors is found to possess. Chemical reactions may easily be the only type of event which can be taken into consideration here. So the principle of systematic congruence serves to restrict the number of those physical processes which we may draw upon in developing the concrete hypothesis about the "large gap."

On the other hand, we have begun to apply the principle itself in a much more general, and at the same time much more concrete, form than either Hering or Müller would have done. As used by these authors it is based upon the *logical* order in which we arrange certain experiences after abstracting them from their context and judging about their similarities. It attributes a congruent logical order to the underlying processes, similarly abstracted from their dynamic context like dead specimens in a museum. But what about the real concrete order of experiences, which itself is experienced? At this moment I have before me three white points on a black surface, one in the middle of the field and the others in symmetrical positions on both sides of the center one. This is an order again; not an abstract, logical order, however, but a concrete, experienced order. As experienced it depends upon physiological processes in my organism, so that some functional processes must correspond to it. And applied to this concrete order our principle says that correlated with the visually experienced symmetry there is an homologous symmetry of dynamic context in the underlying processes. Or, in the same example: one point is seen *between* the others, this relation being experienced exactly as the white of the point is. Again in the underlying processes there must be something functional which corresponds to that "between." Applied to the *concrete* order our principle claims that the experienced "between" is accompanied by a functional "between" in the concrete dynamic context of concurrent physiological events. If the principle is applied strictly in this manner to *all* cases of spatial order in experience, it will lead to the general statement that *all experienced order in space is a true representation of a corresponding order in the underlying dynamical context of physiological processes.*

This is a bold hypothesis. It has far-reaching consequences, if it is taken seriously enough. Its significance will become clearer in the following chapters where we shall also discuss some misinterpretations which may easily arise. For the present I will offer another application of the same principle. The temporal position of one experience as "between" two others is frequently experienced, in the same way that spatial "betweenness" is. Now as far as it is experienced, time must have a functional correlate in underlying physiological processes no less than experienced space. Again, then, the temporal "between" is correlated with a functional "between" in the concrete dynamic context of physiological events. And if, in this manner, we apply the principle throughout, we will arrive at the proposition that *experienced order in time is a true representation of the corresponding concrete order in the underlying dynamical context.*

Both temporal and spatial orders do not exhaust the field of application of this principle. *More* order is experienced than that of space and time. Certain experiences "belong together" in a concrete manner, whereas others do not, or, at least, belong together less intimately. And again this "belonging together" may be itself experienced. The very moment I form this sentence of my manuscript, a shrill, disagreeable voice begins to sing in the neighbor's house. The context of my sentence is something which, though extended in time, is experienced as belonging together, whereas those sharp notes are experienced as something foreign, as not belonging to that context, though they are experienced *at the same time.* In this case our principle applied formulates the proposition that *to a context, experienced as "one thing" belonging together, there corresponds a dynamical unit or whole in the underlying physiological processes.* In this respect again, the order of experience is a true representation of a corresponding functional order in the processes upon which it depends. Perhaps it is this last application of the principle which has the greatest importance for gestalt psychology. In this form, at least, it becomes a radical physiological hypothesis about common sensory experiences as well as about the most subtle and complicated processes producing observable behavior.

In the last case I have taken an example from outside the realm of *objective* experience in the more restricted meaning of this term. A sentence I may be forming is not a part of objective experience, as is the chair before me. Still my statement about that experience is no less simple and obvious than were earlier ones about spatial or temporal orders. I admit that this is not always so, however. I do not wish to recommend the observation of *subjective* experience for our purposes without some limitation; as yet it looks as though only very simple statements about one's own *subjective* experience can be made with a high degree of certainty. Moreover, we have enough cases of objective experience to provide an adequate basis for developing a working hypothesis.

In the last paragraphs my own direct experience was taken as a basis for constructing a general working hypothesis about the relation between experience and unobservable elements of behavior. The only way in which I can bring my observations in that field of direct experience before the scientific public is through spoken or written language, which, as I understand it, refers to my direct experience. However, in terms of the position already taken in this chapter, all language as a series of physiological events is the peripheral outcome of other purely physiological processes in my organism, particularly of those upon which my direct experience depends. My general hypothesis states that the concrete order of actual experience is a true representation of the dynamic order of corresponding physiological processes. Therefore, *if, to me, my language is an adequate "symbol" for my own direct experience, it is an objective symbol for those physiological processes at the same time.* We may also conclude, then, that it does not matter very much whether I take it as a symbol for one or the other, since in the respects in which they correspond, there is no difference between them.

If I now go back to behavioristic psychology I have to deal with language as one form of behavior observed in my subjects. There is no reason now why I should not take language as a symbol, since only the most superficial view would treat it merely as a phonetical event. The behaviorist himself, when listening to a scientific argument, will find himself reacting not to the phonetical properties of speech, but to its symbolic meaning: he will react in the same way practically to the nouns "experiment" and *"Versuch,"* to "animal" and *"Tier,"* though in each case the first and second words are utterly different phonetically. Why should we change that attitude when the other person is called a "subject," and talks during an experiment?

The language of my subject may be taken either as indicative of direct experience in my subject or of those processes which underlie his direct experience. If the subject says, "This book is bigger than that," his words may be interpreted as stating a definite direct "comparison-experience" which he is having or the corresponding dynamic relationship in his physiological process. Since, for my hypothesis the same "order" is meant in both cases, the alternative is not important. From the viewpoint of behavioristic psychology the physiological interpretation is necessary, but the other meaning is not excluded. This second interpretation does not fit into the causal explanation of observable behavior, but if it is suggested by the words of the subject, it can do no harm.

The behavior of a chick can tell me without words that he is able to react to one brightness in its *relation* to another.[1] If in the course of an experiment, a human subject tells me that he sees one object as brighter than the other, the scientific value of this sentence is exactly the same as that of the chick's behavior, whether I assume direct experience in the human subject or not. Why then should I decline to accept language as one of the nicest and most instructive forms of behavior? Of course, we can obtain the same results in man as in the chick, without language, by applying the somewhat clumsy, statistical methods of animal psychology, and some behaviorists seem to prefer such a procedure. The only reason I can see for this attitude is historical. Introspectionists have used the "verbal report" in "introspection"; and I am ready to admit that their method was something like a blind alley. But, as is usually the case in such a situation, behaviorists have become negatively conditioned to much more than the "harmful stimulus" alone, so that they shy away not only from introspection but also from all things once connected with it, and primarily from language, though it is in itself utterly innocent.

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[1] Cf. Chapter VI.

III

The Viewpoint of Introspection

> Round about the accredited and orderly facts of every science there ever floats a sort of dust-cloud of exceptional observations.
>
> W. JAMES, *The Will to Believe*.

WILLIAM JAMES has well described how a vivid interest in the "irregular phenomena" of the "dust-cloud" will often mark the beginning of a new productive era in a science. At such times, what has been exceptional, and a part of the dust-cloud, may suddenly become the very center of scientific work. We shall see presently that introspection has developed a procedure by which an orderly though artificial system of psychology is *protected* against a similar revolution, a procedure in which the most interesting observations are continually exiled into the dust-cloud.

In referring to introspection I am not considering any special school, but all those psychologists who treat direct experience in the manner now to be discussed.

For the most part introspectionists will agree with what has been said about behaviorism in the first chapters; some, indeed, will have recognized their own arguments in parts of my criticism. Wherein lies the difference then between introspectionism and my position? This difference, I promise, will become startlingly clear as soon as we discuss the observation of direct experience. Since *objective* experience is so much more legitimated by its use in physics and behavioristic psychology, we shall first examine the treatment of it by introspectionists, with the result that, for all practical or concrete purposes, introspectionism will be found to be astonishingly similar to behaviorism.

The very moment we begin to move naïvely in the field of direct experience, protests will arise on all sides. If I say that I see a book before me on my desk, I shall be criticized because nobody can *see* a "book." Lifting the book between my fingers, I am inclined to say that I feel the weight, external to my fingers and chiefly in that portion of the object which is visible. This, a critic would remark, is an excellent example of a typical statement by an untrained observer, quite satisfactory, it is true, for the practical purposes of common life, but altogether different from the kind of description given by a trained psychologist. Even the character of being an "object," or "thing," which I have tacitly attributed to the experiences I have called "book" and "desk," is improper in correct psychological description. If observation is to give us simple and genuine data as the elements of psychology, we must learn to make the all-important distinction between *sensation* and *perception,* between the bare sensory material actually given to us and the host of other items which since early childhood have become associated with it. You cannot see a "book," I am told, since this term involves some knowledge about a class of objects to which this specimen belongs, and about their use, etc., whereas in pure seeing such knowledge cannot enter. As psychologists our task is to separate all these "meanings" from the seen material *as such,* the manifold of simple sensations. It may be a difficult task to effect this separation, and to behold the net sensations which are the actual data; but the ability to do so is precisely the special talent which transforms the layman into a psychologist. It ought to be evident that originally, when we lift a book, we do not have the experience of a weight external to our fingers in space. At first there are mere sensations of touch, and perhaps strain, in our fingers, whereas that "weight outside" has been connected with them in a long history and through the influence of other factors than those of pure sensation in our hand. A similar consideration will show us easily that genuine sensory data cannot present "objects." "Objects" cannot exist for us before sensory experience has become imbued with meaning. Who can deny that meaning fundamentally determines almost all experience? Does it not finally lead to a kind of illusion? The German noun *"Igel"* sounds to Germans as though no other animal but a hedgehog could have this name. "Eagle" however, which in English is acoustically the same as *"Igel"* is in German, sounds to an American as though only an *"Adler"*[1] could be called by this name. Here it is obvious that we have to discriminate between a genuine sensory experience, which is the same in both languages, and two different meanings connected with it in different countries. Again, the symbol + *looks* like the sign for addition, especially if you see it between numbers; but it might have been chosen as a symbol for division as well. If we at once feel a disinclination to accept the last statement, the only reason for it consists in just that sort of complexion which meaning, since we were children in school, has given to the innocent symbol. As soon as we realize how strong this influence is, we have to admit that probably *nothing* in the naïve experience of a normal adult can be free from it. Even the most lively characteristic of an experience we may try to describe, may be a property of that experience only in this acquired or accidental way.

Meaning depends upon personal biography; it has a highly complicated origin and represents a somewhat accidental trait of our material. Therefore we must get rid of it and learn to approach actual sensations in such a way that their qualities and laws may be discovered in their pure form. This procedure is called "introspection."

[1] The German word for "eagle."

Probably all psychologists who are teaching at the present time have learned this lesson thoroughly, though many perhaps implicitly and not as a well-formulated program. Its basis seems justified; its content seems sound scientifically. Therefore, if introspection is possible and can lead us to actual sense data, we should be inclined to follow in its footsteps. Direct experience as such, it is true, will not be worth so very much under these circumstances. Of all *objective* experience, at most only the part cleansed and selected by introspection will become the subject-matter of our study.

However, we should not form a definite judgment before knowing more about the criteria for the selection of some experiences as the genuine ones and the exclusion of those other parts of experience which are assigned to the influence of meaning. Let us consider a few more examples, then, which exhibit other properties than those to be found in the cases already mentioned.

Do you see that man on the street approaching us at an even gait? Now he is ten yards away, and presently five. What do you say about his size at the two distances? That five yards away it was practically the same as ten yards off? But that is impossible! A simple consideration of geometrical optics will tell you that during the man's approach his height must have doubled and his breadth too, so that his total size must have become four times the amount it was at ten yards. We transfer the observation from the street into the laboratory. Here I have two cardboard rectangles. The sides of the first have a length of two and three inches, those of the second are six and nine. If now I hold the first before you at a distance of one yard and the second at three yards, you will admit that, optically, they must have the same size approximately, since their linear dimensions vary exactly as do their distances. But you tell me that the further rectangle appears much larger to you than the nearer one? There you have it! Yours cannot be a statement about actual sensory experience. A slight change of conditions will make you realize that my opinion is right. Look through the opening of this screen, which will show you the two rectangles on a homogeneous background, but no more of their surroundings. Do they look different now? Still a little. Then we shall be even more cautious. I darken the room; then I turn the light on, allowing you to make your observation through the screen, only for a small fraction of a second, so that practically no eye-movements can occur during that time. Now both rectangles have the same size, do they not? After some practice you will be able, perhaps, to see them as equally large even without the help of my screen and the short exposure, as soon as you look upon them in the proper way—which is the method of introspection for obtaining pure sensory experience. If you do not accept my demonstration, you might even go so far as to believe that the after-image of an object changes its size according to the distance from which you "project" it upon a screen, because in untrained observation it certainly seems to change considerably, according to the distance of your fixation point. But that is absurd. How could it really change, if the area of the retinal after-effect remain constant?

My second example may be regarded as a natural consequence of the first. When dining with your friends, what forms do the plates have in your experience, those to the left, to the right, and opposite you? They are circular, you say, as is your own plate? Impossible again! They must be elliptical, as you will admit after you have once thought about their projection upon your retina; some of them must even be very flat ellipses, and even your own plate will become an ellipse as soon as your eye does not look down upon it vertically. It will be worth while to apply to this case a procedure similar to the one we used in the first example. On this plane which, as you see, is oblique to the direction of your eye I fix a circle; on this other plane, at right angles to the direction of your eye, I fix an ellipse, whose shape is chosen in such a manner that from its place it is projected upon your retina in the same form as the circle is from its oblique plane. Though you describe the first as a circle and the second as an ellipse, they are two almost identical ellipses in actual sensory experience. Take this screen with two holes in it, which lets you see both forms, but excludes, however, the data by which the angles of the planes became known to you at first. Both forms look alike now? All right. After a short while you will be able to see correctly without the screen, providing you adopt the attitude of introspection. Once more, the corresponding observation on an after-image of a given form, "projected" upon planes of different oblique positions with respect to your line of vision, will elucidate the case completely. Its form seems to vary according to the positions of the plane, though the retinal after-effect has the same form throughout. That being absurd, you see the importance of observing genuine sensory experience by way of introspection.

A similar paradox has been much discussed since Helmholtz wrote his *Physiologische Optik.* Here on this desk near the window, and parallel to it, a vertical screen is fastened. On the window side of the screen I place a black paper on the desk, and in the shadow of the screen, a white paper. These papers are selected in a special manner: the dark one exposed to the direct illumination from the window reflects the same amount of light as the white paper reflects under its low intensity of illumination. You call the first one "black" and the second "white"? Then again you have experiences which are not to be trusted, because under these circumstances the images projected upon your retina are both of them equally intense. So the sensations, i.e., brightnesses, of both must also be the same. Moreover this can be demonstrated. Take this piece of gray cardboard and look through the two holes in it so that one of them is filled by a section of the "black," and the other by a section of the "white" paper. Now, as the surroundings of the papers are excluded from vision, and thus you have pure sensations before you, their approximate equality is obvious. Again, with a little practice—most painters observe objects this way—you will be able, without the cardboard, to destroy the illusion of their difference—just by looking at the papers in the attitude of introspection.

All these observations are of an illusory sort, then, the purification of which exhibits the true sensory facts. In this and other respects they are comparable to a great many well-known "optical illusions," the diagrams of which fill many pages in our textbooks. Take the famous Müller-Lyer figures! When you observe them often and try to pick the main line out of its surroundings, you will find the illusion become unstable, and finally it may even disappear. It was not, then, a true sensory fact. The same thing may be shown in another manner. Accurately draw the two figures, one above the other. If now you fix your attention carefully upon the two left ends of the two main lines, you will find the one vertically above the other. Repeating the same observation with the right ends, you find the same result. It follows that both lines have the same length.[1] Similarly you will find all the other illusions variable, according to the attitude of the observer, and avoidable by practice. Therefore they cannot be classed as genuine sensory experience.

Recently, stroboscopic movement has been investigated by some German and American psychologists. Under appropriate conditions successive stimulation by two lights at two points not too distant from each other produces an experience of movement from the first to the second. But if in making your observation in the attitude of introspection, you find nothing but a "gray flash," must we not be very careful, indeed, before accepting these reports of movement? Did not the observers of Benussi[2] report similar experiences when their skin was touched successively at two different points? But in their description the movement did not usually occur upon the skin; it formed a curve through empty space touching the skin only at those points. Evidently, such an experience cannot belong to the world of touch alone; it is no genuine sensory fact, then.

If all these are examples of illusions which, for psychological purposes, have to be corrected by introspection, something must transform pure sensory experience so that for untrained people it assumes an

[1] This argument has been used by Wundt and Schumann.
[2] *Archiv f. d. ges. Psychol.*, *36*, 1917.

illusory character. This is the case indeed, and in most of our examples we can discover that "something" easily enough.

The man approaching us on the street does not grow larger, as for simple optical reasons he should. The circle observed on the oblique plane does not become an ellipse; it seems to remain a circle after its retinal image has become a very flat ellipse. The white object in the shadow remains white, the black paper remains black in full light, though the first may reflect much less light than the second. These three examples have one common property: the physical object *as such* remaining constant, while actual stimulation varies according to more or less accidental conditions (of distance, of position, of illumination), our experience agrees with the constancy of the physical object much better than with the varying stimulation produced by it. From this fact we draw the simple conclusion that all such apparent constancy in experience is the product of *learning*. Since our early childhood we have observed that a distant object is much bigger, "really," when we come near to it. In the same way we have learnt that an object situated obliquely in relation to our glance has quite another "real" form when we look at it vertically. Again in the same manner we have observed that an object which we see under abnormal conditions of illumination will show quite another "real" brightness or darkness as soon as conditions become normal. As these observations have been repeated millions of times, our knowledge of the "real" size, the "real" form, the "real" brightness, has become so intimately connected with what we see, that with time we seem to *see* those constant "real" properties instead of the ever changing ones corresponding to varying stimulation. In short, those facts, though strange at first, are explained by *meaning*, and then completely fit into our scientific system.

We have already seen that probably no experience escapes from the influence of meaning. Hence we do not here introduce a new hypothesis. If untrained people seem to "see" what from this viewpoint is a product only of past learning, that again is an illusion, to be found generally wherever we discover the indirect origin of the experience. Remember the symbol +, which *looks* like the sign for addition. Furthermore, meaning as an after-effect of past observations must be *reproduced* and reproduction presupposes reproducing factors in the actual situation. Obviously, if we do not see the distances, the oblique positions and the degrees of illumination, no reproduction of the "normal" size, form and brightness will be possible. It is precisely this which happens when the given objects are observed through holes in a screen: since, under these conditions, the definite surrounding properties being given, the *actual* distance, position and illumination are excluded from vision, and corresponding reproductions are made impossible. The objects must now be experienced exactly as, in pure sensation, they have been the whole time. The same proof is given by introspection. In our terms this procedure means an isolation of local size, form and brightness from their context in an actual situation. Thus it separates them from the factors determining the reproduction of "meanings," and under these conditions of isolation the effect is that of pure sensation determined by the actual stimulation.

If in the case of size and form, after-images show a surprising variation with the relative distance and position of the background, this also is a direct consequence of our explanation. After-images are localized upon the background. If the distance and the position of this background operate as factors of reproduction, as in the previous case, the same after-image will take on a great many apparently different sizes and forms, relative to the changes of the background in distance and position.

Finally, in terms of our explanation we can understand why, under extreme conditions, there is no "constancy of size," etc. Ten yards away a man is scarcely smaller than at a distance of five yards; but fifty yards off he looks smaller, and 1,000 yards away he usually becomes a tiny object, indeed. During most of our life we are interested in objects rather near to us; so we learn little about things far off and the consequence is that with increasing distance the relation between the actual sensory experience and the meaning changes, to the advantage of the former.

In all these arguments there is great persuasive force and cogency. Most psychologists do not for a moment doubt the truth of the explanation in terms of "meaning." It also seems to correspond to a very natural tendency in human thinking. Physicists who have never studied psychology will tend to give the same explanation, as soon as they become acquainted with the facts we are considering. And if you demonstrate the phenomena to a freshman, he will give the "meaning theory" at once.

The importance of this theory is not less considerable on this account. There is practically no field of vision which does not exhibit some of the facts we are discussing. When we open our eyes we behold sizes, forms and brightnesses, and, of these appearances, only a very few will be exempted from the verdict which, from the viewpoint of introspection, must be imposed upon them. The facts themselves are not exceptional; only the demonstration of their surprising deviation from what one should expect them to be is something unusual. This demonstration is a matter of psychological, one might almost say, of laboratory, sophistication; the facts themselves are affairs of every moment and of everybody.

Though the extent of objective experience which shall not be trusted thus increases in scope. we have not yet reached its limits. The simple localization of objects manifests similar properties. Looking at a point before me I see the objects around it in different places, corresponding to the different places which their images occupy on my retina. If I now fixate another point, the same objects will appear in other places according to the new positions of their retinal images. But actually the objects do not seem to have moved. Their localization in space is again more constant than my experience of it. Or take the speed of seen movement: the same physical movement may be seen from many different distances. When I am ten yards away from the moving object, retinal speed will be one-half of what it is at a distance of five yards. In direct experience, however, there is no appreciable difference of speed between the two cases of movements.[1] Obviously, the same explanation can be given here which has heretofore been applied to the constancy of size, form and brightness. Once more we are dealing with meaning. And ultimately we recall that first illustration, and indeed the most general case of the illusory character of direct experience: if we seem to perceive "objects" in our visual field, even those "objects," from the standpoint of introspection, must also be products of learning: *meaning transforms sensations into "things."*[2]

[1] Cf. T. F. Brown, *Psycholog. Forschung, 10*, 1927.
[2] Cf. v. Kries, *Allg. Sinnesphysiologie.*

From this it follows that of all objective experience, as both layman and psychologist enjoy it in the visual field of everyday life, very little is left as pure and genuine sensory fact. That such is the consequence we must accept, if we go the ways of introspectionism, will become clearer to us now at every step.

Introspection, however, draws a much more serious conclusion from the foregoing analysis. Not only shall all those experiences which are supposed to be influenced by meaning not be treated as primary sensory experiences; but, in addition, they shall be excluded from the subject-matter of psychology. Some introspectionists may hesitate to acknowledge explicitly such a radical principle, but in their researches most of them proceed as though they had completely accepted it. After they have classified a given experience as one that must be explained by the effects of meaning, they rest satisfied, and are no longer any more interested in it than if it were found to belong to astronomy. This means that most objective experience is excluded from the introspectionist's psychology as soon as he has found it susceptible to his explanation in terms of meaning. In the following chapters we shall see that wherever we touch upon a somewhat unusual and therefore interesting observation, the introspectionist will offer us this same explanation, and we shall always be urged to *discard* the observation because meaning is involved.

Whether the explanation is right or wrong, we continually face in common life the kind of first-hand objective experience which is discarded by introspectionism. Toward it all our interests are directed. There are millions of people who will never transform the "objects" of their environments into "true" sensations, who will always react to sizes, brightnesses and speeds as they find them naïvely, who will like and dislike forms as they appear to them in their objective world without recourse to introspection, and what are held to be the true sensory facts will never play an appreciable rôle in their lives. As long as the introspectionist's attitude prevails, however, psychology will never seriously study those experiences which form the matrix of our whole life. Instead, it will observe and discuss the properties of rare and unusual experiences which, though they are supposed to be continually present beneath our naïve experiences, seem to be so well hidden most of the time that their existence has nothing to do with life as we actually experience it. Even the best introspectionist will not confront or state these experiences unless protected by his special attitude which—fortunately for him—he does not assume except in the laboratory. Therein the enterprise of psychological observation is undertaken, far removed from common experience, and if we should ever learn from him the laws of pure sensory process, all of them together would not lead us back to the psychological world we live in, which is that of direct and naïve experience, and utterly foreign to those laws. If, therefore, the introspectionist's psychology does not treat the facts we have to deal with in life, if most of these facts are exiled from his science, he cannot complain about his own fate. It is only to be expected that such a psychology would not satisfy people for long. If you condemn interest in the experience of daily life and define genuine experience as a rare something only found by artificial procedure, it is inevitable that your professional as well as your lay audience will tell you that they do not care for your kind of psychology. And they will not stop there. Since you have asserted that your experience is the only genuine one, they will interpret your procedure as the only possible scientific approach to the problems of experience. Thus they will condemn *all* experience as material of psychology, after you have given away the greater part of it for the artefacts of sophisticated and sterile introspection. This reaction had to come. It has come in the form of behaviorism.

I do not feel justified in calling those artefacts *unreal.* When I apply the introspectionist's methods I myself can get those special experiences which corroborate his findings. But I do not attribute to them a rare value as though they were worth more, as though they were more "true" than everyday experience. If that common experience involves meaning, the experiences given by introspection depend upon the attitude of introspecting and cannot be shown to exist without it, as we shall see later on. Moreover, for actual life, common experience is found to be incalculably more important than any of the introspectionist's "true sensations." Let us grant for a moment that all the facts mentioned above are what they are for us because of the influence of meaning. Do they become less real or less significant psychologically because of this genesis? Is a certain amount of H_2O which I have before me no real chemical substance because I know that it has been formed by the oxidation of hydrogen? Would that hydrogen be a "true" chemical substance, but not the water? Is the water not worth the chemist's investigation? I do not see why an experience which is constituted by acquired meaning should be less interesting and important for psychology than experiences not so composed. Take the case of the symbol +, which every one will agree is a product of meaning. When seen between figures it *looks* like "plus," i.e., *its meaning appears localized in the visual field.* A strange fact, indeed, which may entail some very important questions! Why should such problems not be investigated in psychology? It is the same situation with regard to all the other cases which are explained by meaning. Why should we lose our interest in the problems they engender, because the fatal word "meaning" has been uttered?

Furthermore there is positive reason why, instead, we should examine them with particular attention. We have to discriminate between two types of cases among the examples considered. One, represented by the symbol +, is distinguished by the fact that child psychology offers an adequate account of the growth of meaning in the individual biography. For the second type, represented by most of the other cases, such an account is still to be given. It has been in no way demonstrated that the objectivity of things, the localization of weights, the constancy of size, form, speed, localization and brightness, and the variation of after-images, and so forth, are really produced by the influence of meaning. It may seem extremely plausible to most of us that such is the case, but none of the experiments and observations I have mentioned in this connection can be regarded by an unprejudiced psychologist as satisfactory proof. In our present situation it is merely an hypothesis that the second group of facts quite properly belongs together with the first class, and as an hypothesis it is to be treated.

The customary thing to do with an hypothesis is to test it. Does introspectionism test this hypothesis? Quite the contrary, since all interest in the experiences which it is intended to explain, is destroyed for the introspectionist by the explanation itself. Thus we have before us an assumption which would never be examined at all, if all psychologists were extreme introspectionists. If this be strange, it becomes the more surprising when you realize that at this point most psychologists would not listen calmly to our discussion of "meaning theory" as an hypothesis. At first they might express only an incredulous smile, as though they had been asked to believe something fantastical; but if you insisted upon this point they would grow impatient, because in their opinion you were disturbing the sound basis of careful science by an obtrusive paradox.

When a scientific argument tends to become rather emotional it undoubtedly prods some deep-rooted presupposition, the discussion of which is felt to be offensive. No one enjoys the discussion of what for him is secure beyond discussion. In the face of this exhibition of temper, the more calmly should we realize that the attitude of introspectionism, if taken so piously, becomes a real danger. Let us suppose for a moment that in the cases of objectivity, size, form, speed, localization, brightness, etc., the explanation by meaning is incorrect. We should then immediately draw the conclusion that all the facts in question are a part of genuine sensory experience. But these facts would tell quite a different story about sensory experience than that given by the introspectionists. Hence it would follow that what we learn about "true" sensations from them has to be regarded as distorted truth. Whether or not this be really the case, depends therefore upon the validity of the general explanation by meaning. And this explanation is not to be tested; it *shall* not even be examined! It will be obvious then that, used by the introspectionist in this manner, *the meaning theory serves as a defensive bulwark for his quite definite and special ideas about sensory facts.*

This makes us suspicious of the meaning theory. Introspectionists may be so utterly convinced of it, not because independently it is particularly attractive in itself, but because their prior firm belief about the nature of sensory facts will not permit them to acknowledge certain experiences. If these "irregular" experiences can be explained away by meaning, the belief will be reassured.

In order to show that such is the case, we have only to consider the arguments brought forward in favor of the meaning theory. They have little indeed to do with meaning, but very much with convictions about the world of pure sensory experience.

Take the "constancy of brightness" as an example. A white paper in the shadow appears as white, a black one in bright illumination as black, though the white may reflect less light actually than the black. Is there anything in this experience, in the white and the black *as such,* which tells us directly that we have to do with an effect of meaning? Nothing at all. The argument is of an indirect kind, viz., our observation in this case cannot be reduced to pure sensation; it must, therefore, be a product of meaning.

First of all, one can *change* the experience by looking at the papers in a special manner. Therefore, it is not a true sensory experience. A primitive conviction, then, which paradoxically both needs and supports the meaning theory is this: *True sensory facts are independent of changes in subjective attitude.* Evidently, however, such a statement is not a sufficient support of the theory. If, in the attitude of introspection, I can transform the apparent white into a dark shade, and the apparent black into a rather bright nuance, the opposite change will occur spontaneously as soon as I forsake that attitude and behold the papers as other people do. From the viewpoint of experience the "true" brightnesses, appearing during the period of my experimental attitude, are as changeable, then, as those which I had before and which I customarily have. Logically, I might as well condemn the experiences found during "introspection" and refuse to call them "true," because they disappear whenever I return to the everyday attitude. The introspectionist, however, is far from treating both experiences with the same measure. He thinks that what he experiences in his special manner is true experience, that, hidden somehow, it ever exists behind the veil of meaning whenever he falls back into a more naïve attitude. Thus it becomes clear that there is still another belief which makes him prefer his special and rare experiences.

His other belief is easy to find. Why should we be surprised by all those experimental findings on size, form, localization, speed, brightness, etc.? Why not take them quietly and as a matter of course? Obviously, because under the given experimental conditions of stimulation we expect to have experiences quite different from those which we really have. Naturally, we say, seen size should be proportional to retinal size, to changes in retinal form there should correspond changes in seen form, localization in the visual field should vary with retinal localization, seen speed with retinal speed, and seen brightness should follow the variations of retinal intensity. While the everyday experience of the layman contradicts these expectations in the most shocking manner, the special attitude cultivated by introspectionism succeeds in obtaining precisely those other experiences which, according to the introspectionist, we ought "naturally" to have always; or we at least *approach* that state of affairs which our understanding considers the appropriate one. *This* is the fact which makes the introspectionist prefer the special and rare experiences found by an artificial attitude, and which also makes him believe in a permanent, though hidden, existence of these "pure sensations" under the opaque cover of naïve experience. It becomes apparent, then, that the procedure and the results of introspection acquire a special sanction through their agreement with definite presuppositions about the relation between stimulation and sensory experience. The same suppositions lead to the condemnation of everyday experience. No one can understand the scientific trend of introspectionism who has not discovered, who does not see, this decisive point. How many times, as a student, have I read and heard that the Müller-Lyer phenomenon does not represent a "true" sensory fact, because by analytical observation and practice we can destroy the "illusion"! If this be taken as a proof, obviously one kind of experience is given a *higher value* than another. If you ask, why? you will ultimately get no other reason than the one already stated: that one experience agrees with the properties of peripheral stimulation whereas the other does not. Hence our original observation is rendered innocuous by the meaning theory.

A second fundamental conviction, then, underlying this theory and the whole scientific viewpoint of introspectionism is this: *The main properties of "true" sensory experiences follow the corresponding properties of peripheral stimulation.*

The introspectionist's belief takes a form still more concrete. How did he proceed in order to find the true sensory facts in the case of brightness, for instance? He tried to isolate the white and the black paper under observation so that they were taken out of their context as far as possible. The same thing is done by those who try to destroy the Müller-Lyer illusion, and similarly in all the other cases. Such an analytical and isolating attitude will have effects similar to those of a perforated screen, which, concealing the particular surroundings of the objects, gives them a new *homogeneous* environment. If now the "illusions" disappear, this effect of isolation is explained in terms of the exclusion of all those secondary factors, and in particular meaning, which before had distorted the true sensory experience. But the possibilities of pertinent explanation are not exhausted with this. The effect produced by isolation also proves that objective experience in one part of the field does not develop, independently of its neighborhood, as a function of local stimulation alone. If this be taken as a description of facts, we may explain the facts in one of two ways: (1) *Either* "true" experience depends upon local stimulation exclusively, whereas the meaning which almost always adheres to it depends upon (is reproduced by) the properties of the environing field; (2) *Or* natural sensory experience *itself* is not determined locally; i.e., the properties of any one part of the field depend normally upon the conditions given in the whole field, or, at least, in a larger area of it. In *both* cases isolation, or the introduction of a homogeneous environment, would make local experience correspond better to local stimulation. The introspectionist, however, does not consider the alternative. He only seems to see that one horn of the dilemma, according to which isolation gives the "true" sensation, and naïve experience is a product of meaning. Again we find him partial to an astounding degree; he *prefers* that hypothesis (although unwilling to regard it *as such*) which gives him local sensory facts determined by local stimulation. In a well-known example, when our subjects make eye-movements along the main lines of the Müller-Lyer figures, these movements are shown to have different extents for the two figures, corresponding to the difference of their apparent lengths. There you have it, we are told! The illusion is not an optical fact; it is produced by the accompanying motor phenomena, or at least by innervation tendencies if overt movements do not occur. This is an altogether partial statement because, supposing that the two lines *have* different lengths in "true" vision, nothing would be more natural than that the eye-movements should be different correspondingly. Only by prejudice can the fact in question be interpreted as though it proved the indirect genesis of the Müller-Lyer effect. It may prove instead that sensory experience is influenced by stimulation in a larger area and that, therefore, eye-movements, depending upon sensory experience, will also vary when surroundings are changed.

Recapitulating the foregoing analysis, we may conclude that: *A decisive motive in introspectionism is the belief that true sensations are independent of subjective attitude and depend only upon local stimu-*

lation as purely local experiences.[1] This is the only rule which helps us to understand on what occasions the introspectionist begins to introspect. Very seldom will you find him introspecting where, without any technique, he obtains certain simple relations between local stimulation and experience. But where these relations do not seem to exist *prima vista,* you will find him taking recourse simultaneously to the introspective procedure and to the theory of meaning.

Here is the remarkable result of our inquiry. The whole procedure of present introspectionism seems at first to be in full contrast to behaviorism's purely physiological viewpoint. If the introspectionist is not the advocate of direct experience, who else should be able to play that rôle? But we have found that there is some restriction upon his enthusiasm for direct experience. A foreign judge has the chair. He examines direct experience beforehand, convicts most of it, and sends it away for correction by a special procedure. This judge is a *physiologist!* His ideas about the sensory part of the nervous system are extremely definite. When the introspectionist mentions physiology, he seems to talk about a helpful servant; but when we look at the facts, the servant rules the introspectionist.

[1] This is the famous constancy hypothesis. Some introspectionists have said that *gestalt* psychology, as any science must, has also to acknowledge certain constant relations between given conditions and subsequent effects. Quite! We do not argue against constant relations existing between conditions and results in general, but only against a constant relation between *local* stimulation and *local* experience. For brevity's sake we call that "the" constancy hypothesis.

If introspectionism is much less guided by direct experience than by definite physiological principles which decide what shall be called "true" sensations, its viewpoint is not so very far from that of behaviorism. And this similarity of general ideas becomes even more striking if we compare in detail the physiological creed of introspectionism with that of behaviorism.

The main concepts of behaviorism are those of reflex and conditioned reflex. What is the characteristic trait of reflex action? That from a definite receptor organ nervous excitation is conducted along a definite path, through definite "centers" and along other definite paths towards a definite effector organ. The *order* of organic reactions in their dependence upon definite stimulation is explained by this conception: a preëxisting arrangement of more or less isolated conductors enforces that order. It is true that behaviorists do not suppose the anatomical arrangements to be *absolutely* rigid and constant. But, though a certain "irradiation" of excitement is admitted, almost the only biological value attributed to this plasticity consists in the fact that, if some other factor can make the connections practically rigid, this other factor will have a certain range of different possibilities to work upon. Hence, order of functions is enforced fundamentally in the reflex arc, but, at a higher level of the nervous system, connections may be built (or blocked) by another factor. This other factor is "conditioning."

If now we consider the definite physiological ideas which silently determine introspectionism's criteria for "true" sensory experience, we find the following two: *First,* local sensation depends upon local stimulation; it does not depend upon other processes in the nervous system, not even upon those issuing from neighboring points in the same sense organ. There is only one physiological assumption which would explain such an independence of local sensation, namely isolated conduction from one point of the sense organ toward one point of that region the activity of which is accompanied by sensory experience. But this is nothing but the first half of a reflex arc, so that in this respect introspectionism agrees completely with behaviorism. But experience does not seem to obey that principle universally. Of course not! Because we have a *second* principle: At a higher level of the nervous system at least, connections may be built during development, which did not exist originally at birth. A corollary of this principle is: that certain experiences will be followed and accompanied by others, or by reproduced material corresponding to them, in the form of "meaning." Though this term sounds a trifle more refined than "conditioning" in the physiological considerations of most introspectionists it purports the automatic formation of new conducting connections, just as conditioning does for behaviorists. Again there is no real difference between the one viewpoint and the other.

During their lively controversy over whether introspection, or the objective observation of behavior, is the true procedure, it does not occur to either party that some other theme might be more important and urgent, namely, that concerning their common convictions as to the basic type of nervous process underlying experience or behavior. This assumed basis appears self-evident to both of them. And since so much is taken for granted by both, we should not be surprised to find the same conservatism in introspectionism which is so striking in behaviorism.

That psychology is a very young science and that therefore its future and its progress will probably depend upon discoveries unsuspected at the present time, seems not to be realized by most introspectionists. In sensory experience, at least, the essential traits of all possible and acceptable observations are fixed for them before they begin to observe. Consequently, they show an almost negativistic attitude whenever an observation does not agree with that pre-established truth; and their experimentation tends to become a rather defensive and critical procedure. If others point to some new observation that does not exactly conform to the fundamental concepts mentioned above, they are eager to quell the disturbance, and to dismiss it by appropriate introspection and discussion. Criticism and open-mindedness to change are healthy signs in science; yet I have known introspectionists who spend their scientific life in the defense of what is forever true! In these circumstances one cannot expect them to have a youthful interest in free observation and naïve discovery. If the main points are already established, and if further observation cannot give us essentially new hints, why should we strain our eyes in the chaos of a large world behind which, anyway, the old truth *must* be hidden everywhere? As William James would say, whatever happens is a "nothing else but" for them; it can be reduced to an old story. In this attitude again there is no real difference between behaviorism and introspectionism, though each defends its conservatism by different methods. But there is a real difference between preferring to be at rest upon preëstablished ground or preferring adventure and intellectual curiosity which take you full-sail into the open sea of experience. If you choose the latter, perplexity will be your usual feeling, and a glimpse of clear-shaped coasts a rare reward on some Sunday of research. But the next generation will have more Sundays as a result of your exploration. Certainly, neither this nor the next generation is needed in psychology, if we stay at home and know beforehand that no other sites can be found in the future.

I do not see, then, why introspectionism should be preferred to behaviorism, or vice versa. They are so much alike in their fundamental opinions and in their general attitudes, that all their wrangling seems like a family quarrel to the onlooker. And it is among precisely those themes, over which they do not quarrel, that we shall find the problems of *gestalt* psychology. For the present, however, we shall pursue this question: Is it true that the processes underlying experience and behavior are determined mainly by the connections of given nerve-paths, and that some change in the conductivity of these connections is the secret of an individual's development?

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IV

Dynamics as Opposed to Machine Theory

SOMETIMES people are conservative and right at the same time. Still it seems highly improbable that in our very young science conservative opinions should be right, opinions which are opposed to almost all experience, and which have scarcely been examined, because they have been protected by the "meaning theory."

Some of the physiological presuppositions lurking behind introspection are hardly convincing if they are once examined critically. In one of our examples, apparent movement, produced by touching successively two different points on the subject's skin, is not admitted as a "true" sensory fact because usually it forms a curve through empty space, only the ends of which are felt on the skin (cf. p. 79). But why should all the experiences which depend physiologically upon the stimulation of a sense organ be localized in the same place where that sense organ is localized as an experience? In vision, this is not the case; forms or colors are not seen at the place where we feel our eyes. Neither are sounds heard generally where we localize our ears. Behind the introspectionist's argument there seems to be some primitive confusion of the peripheral processes produced by stimulation with sensory experiences depending upon it, of the physiological locus of the first with the sensory localization of the second.

I think this case is typical because it shows that the apparent self-evidence of the argument from physiology prevents a critical consideration of it, whereas any examination of the case would efface the self-evidence of the assumption. In this chapter I shall try to show that even the main assumptions of introspection and behaviorism are by no means axiomatic, though they agree with a common prejudice which is thousands of years old.

We have seen that those assumptions are utterly dependent upon the explanatory force of the meaning theory. Unprotected by this theory those assumptions would be destroyed by any observations which contradicted them. Recent experimental work on some of the experiences discussed as examples in the last chapter bears upon the explanation of those experiences in terms of meaning. In order to *learn* that sometimes white is black and vice versa, an individual needs time and *much* experience, largely because he has to learn it so thoroughly that finally the products of learning will be "projected" into his field of vision as definite nuances of brightness instead of the "true" ones. We should expect, then, that young or primitive subjects would not show the "constancy of brightness" to any considerable degree. But when the experiment was made with young chicks under very severe conditions, they were found to possess approximately as good a "constancy of brightness" as I do.[1] Similar experiments made on the "constancy of size" with children (from two years up), and with young apes, also gave positive results.[2] Though it was difficult to exclude slight influences of meaning upon the phenomena in question, it seems altogether unlikely, even without further work, that *in principle* these phenomena are produced by meaning. I do not deny that objective experience is imbued with acquired meaning in many respects. But, as has been said before, where this influence is not demonstrated in the history of individuals, or somehow demonstrated independently, no indirect argument is strong enough to be accepted in lieu of such a demonstration.

Since in these instances the meaning theory begins to relinquish the field, a radical change in fundamental principles becomes unavoidable. The phenomena we have here discussed, such as constancy of size, form, localization, speed and brightness, the stroboscopic movement, the well-known illusions and so forth, are just as decisive for our understanding of genuine sensory experience as the so-called "normal" cases of introspection, i.e., as the observations that at

[1] *Optische Untersuchungen am Schimpansen und am Haushuhn. Abhandl. d. Preuss. Akad. d. Wiss.*, 1915.
[2] *Op. Cit.* and Frank, *Psychol. Forsch.*, *7*, 1926; *10*, 1927. Beyrl, *Zeitschr. f. Psychol.*, *100*, 1926.

a given distance and on a homogeneous background seen size depends mainly upon retinal size; that (apart from contrast and other exceptions) in a given illumination seen brightness depends upon retinal intensity, and so on. In these cases, usually regarded as normal, size, brightness, etc., are found to vary with the properties of local stimulation, because conditions of surrounding stimulation are practically constant. For the same reason, by assuming the introspective attitude, we may find experiences corresponding to local stimulation even in those other cases where *naïve* experience shows the contrary, because by an analytical attitude the influence of surrounding conditions temporarily can be suppressed to a high degree.

But we do not suppose such isolation to be the normal state of affairs. If in all the examples given we accept direct experience at its face-value, our fundamental assumption about the processes underlying experience and behavior must be opposite to the assumptions of both introspectionists and behaviorists; i.e., instead of reacting to local stimuli by local and mutually independent events, the organism reacts to an actual *constellation* of stimuli by a total process which, as a functional whole, is its response to the whole situation. This is the only viewpoint which can explain how to a given local stimulus there may correspond altogether different experiences as soon as surrounding stimulation is changed. We face this dilemma: *Either* we take the traditional horn, in which case direct experience and everyday behavior are irrelevant to our psychological analysis, *or* we trust all kinds of experience impartially, in which case we have to accept the suggested radical change in physiological theory.

"Total process" and "functional whole" are, however, terms which sound rather vague, or even mystical, to most scientists. It would be well to present our physiological assumption in more definite detail. This may be done. And at the same time we shall obtain a more general view of our problem if we ask ourselves just why the prevailing viewpoint should have appeared so utterly convincing to the last generations.

The chief reason seems to be that sensory experience is in most cases quite orderly, and that the behavior, integrated with it, is no less orderly. Now since the early days of European science man has been deeply convinced that the processes of nature, if they are left to their own "blind" play, will never produce anything like order. Why should they? Does not the accidental intercourse of inorganic natural forces around us produce chaos and destruction everywhere? We have been able to formulate some of the rules according to which isolated processes occur in nature necessarily, but where many processes influence each other without control, no reason can be found why confusion and general disorder are avoided or why the whole complex develops toward an orderly distribution. *Without control*—this appears to me to be the decisive point. For as soon as man began to restrict the possibilities of natural processes by appropriate rigid arrangements, the same forces which apparently would have produced chaos, destruction or disorder without control were guided toward orderly function in the service of mankind. This has been man's conception of nature for thousands of years; in modern times the engineer still enforces in the same manner the functional order in his machines. The "blind" forces of nature are allowed to make the machines *move*, but the *order* of the movement is enforced by those special arrangements which are the essential traits of the machines and the pride of their inventors.

From this viewpoint, which prevails even in current theory, a young science will tend to presuppose the existence of "special arrangements" wherever the distribution of forces or processes in nature is found to be orderly. Aristotelian astronomy is a good example. The movements of stars in the sky maintain a remarkable order, so different from what one would expect to occur in "free" nature, that something special must control those celestial events. Obviously, the possibility of a star running wild or a planet going astray must be excluded by rigid arrangements. Therefore, in Aristotelian theory, the stars are fixed upon those famous crystal spheres, the turning of which forces the stars to move in their apparently regular orbits. There are even engineers, viz., the stellar deities, who watch and control the machinery. Three hundred years ago this conception was still entertained in pious awe. The functional value of its crystal spheres in the orderly sky was precisely the same as that of any rigid arrangement enforcing orderly processes in our machines. There is a deep longing for rest and safety in man. Early in history it was satisfied by such a primitive belief, the content of which may appear crude, narrow and absurd to modern eyes. What was so shocking in Galileo's astronomical discoveries? That there was so much going on in the sky and the astronomical order was so much less simple than one could happily believe before! If the heavens begin to show a lack of rigid institutions, similar to the flexibilities we observe on this planet, who can feel secure in his most important beliefs? There was much exceedingly human, miserable fear in the furious attacks directed against Galileo by the Aristotelians of his time, and the fear was evoked by the amount of change and dynamics Galileo found in the world as the home of mankind. One may well suppose that the excitement produced by Harvey's discovery of the circulation of the blood contained a similar element of fear, because the conception of man as a rigid structure was suddenly disturbed by the idea of internal unrest, accompanied by the possibility of its fatal cessation at any moment.

In any case the same general motive expresses itself in the well-known tendency of early biological thought to explain all remarkable properties of organic life, and most of all its orderly course, by special arrangements enforcing order. In the time of Descartes, his so-called mechanical interpretation of organic functions may have been bold enough; still he was absolutely conservative in assuming without a moment's hesitation that—apart from the influence of one engineer, the soul—all delicate, vital processes were enforced by special arrangements, connections and channels throughout the whole body of man. Figuratively, the organism was for him what the sky had been for Aristotle—full of crystal spheres. He did not know the laws of dynamics, it is true. But though we know much more about them since his time, the main changes in biological theory seem to have been refinements of his type of thinking rather than discoveries of new general concepts about organic order. Even in our own day, though doubting the validity of that machine-conception as a satisfactory explanation of *all* organic order, biologists do not give us any other clear general principle whereby to understand the orderliness of organic function.

We shall see at once what the possibilities of alternative explanation are, if we try to get a more concrete picture of the characteristic trait in Aristotelian and biological machine theory.

In physical processes we find two sorts of factors determining events at every moment. In the first class belong the actual forces of the process itself; they represent the *dynamical* side of it. In the second class we have those properties of the system which may be regarded as *constant conditions* of its events. So, at the present time, the elementary charge of one electron seems to be an unchangeable condition of all physical events. In a conducting wire the specific conductivity of its material will be regarded as such a given condition. On the other hand, the electrostatic forces of the current itself driving an electron along that wire are an example of a dynamical factor. But among the constant conditions two further classes may be discriminated. In the case of electric current it is one given condition that all moving charges have the amount of electronic charge or multiples of it. Another condition of the current is given by the mode of its distribution, the connections and the spatial order of the conducting material, i.e., a network of wires.

Immediately the fact becomes clear that, whereas in all systems of nature, in addition to their own dynamical force, processes depend upon conditions of the first class, the influence of special conditions of the second class may dominate in one case and be at a minimum in another. When we consider the play of electrons in an atom or the integration of atoms forming a molecule, we do not find a special topography laid down beforehand which determines those processes. At every moment, in addition to constant properties of the material (first class of conditions), the actual *dynamical* situation develops the next event out of itself. If we wish to refer to the second class of conditions in such a case, it would be to point out that the topographical situation consists of physical space without any special determining or limiting feature in it. On the other hand, the equilibrated distribution of electric charge on the surface of a conductor depends upon the rigidly given form of that conductor as a limiting topographical condition; and in a steam-engine the piston can move only one definite way, determined by the rigid walls of the cylinder.

This leads to a classification of physical systems which is decisive for our problem. In all of them process is to be regarded as necessarily determined, but among the various cases we find enormous differences in the relative influence which limiting topographical conditions, on the one hand, and the play of actual forces, on the other, exert upon the course of events. Wherever we have given topographical conditions, preëstablished and not changeable by the process itself, their existence means the exclusion of some dynamical possibilities and the restriction of the process to only the possibilities compatible with those conditions. Electric charges may move through the conductor in various ways, but they are *prevented* from leaving it, and so their final distribution depends upon its form. Again the electric current may have one direction or the other, its distribution may vary enormously; but, if the wires are surrounded by isolating material, the dynamics of the current itself will remain restricted to the interior of the wires. The most extreme case will consist in a system where preëstablished topographical arrangements exclude all processes except only one; and an example of this type is given by the piston's motion strictly confined between the walls of the cylinder.

In this case the steam in the cylinder exerts its pressure in all directions, but, owing to the topographical conditions, it is not allowed to do work except in *one* direction, in that, namely, in which the piston is free to move for a certain distance. Consequently, nothing but the motion as such is determined dynamically in such a system, whereas its direction is strictly enforced by topographical arrangement.

Now this is exactly the relation between dynamics and preëstablished topographical conditions which we find in typical industrial machines. The number and forms of special one-way functions which may be enforced in such systems are enormous and varied. Still the general principle is everywhere the same. Sometimes a little more than the minimum may be left to dynamical determination, but at present no one would construct mechanical systems for industrial purposes where the form and distribution of process would be to any considerable degree a matter of dynamics.

It is the same idea again which occurs to Aristotle when he views the remarkable order of celestial movement. His spheres are topographical conditions enforcing that order. And since Cartesius, neurologists have worked with the same concepts wherever they have dealt with orderly organic function in higher animals and in man. It is not the dynamics of nervous processes as such which they suppose tend toward coördinated function. Vitalists may have such a mystical idea! Rather, special anatomical topography is the only explanation for order; and by it the dynamics of process are compelled to produce orderly results.

Once more we return to point out that it is the same conception which forms the common basis of both introspectionism and behaviorism, so far as their physiological principles are concerned. Take vision, for example. How many things may be present simultaneously in one actual field! Still, excluding extreme peripheral regions, there does not seem to be any confusion for the most part. One object appears separated from all others, and the sharpness of its contours is evidence of a high accuracy of function. But the field is not only clear as such; it also corresponds admirably to the physical realities. Points which are neighbors in physical space are neighbors also in the visual field; the center of a circle in physical space appears as the middle of a symmetrical figure in vision, and so forth. All this order is as remarkable as it is necessary for our response to the objects which, in the form of bodily movement, must be adjusted properly to the physical world. The order of projected images upon the retina is easily explained by the properties of the pupil, the lens and so forth. But what about the processes which, streaming from here into the brain, will eventually determine experience and behavior? Since experience and behavior show a similar order, this order must have been enforced or preserved throughout the entire process. Only one kind of explanation seems, then, to be possible: In the nervous system we have a topographical arrangement, preventing confusion and mixture during conduction. Indeed, if from each point of the retina local processes are conducted on definite and isolated paths toward their final cortical termini, and if the totality of these termini somehow reproduces the geometry of local retinal processes, then the dynamics of the process are excluded completely from the determination of its own direction and distribution. In terms of such a construction of the facts we can feel secure that the most important property of vision, i.e., its accurate order, is guaranteed by so trustworthy a factor as preëstablished anatomical conditions.

Similar considerations would lead to similar results in the case of touch and hearing. But what about learning and habit formation?

At the present time most psychologists would answer this question by saying that in some parts of the nervous system, between its optical and acoustical sectors, for instance, paths of nervous activity are not fixed once for all in the youth of an individual. Either no paths will be ready for conduction at first, or else from one point of the tissue several paths will conduct processes equally well in several directions, so that disorderly diffusion will occur. In the adult, however, we observe a great many associations, let us say, again, between the optical and the acoustical centers, and very seldom is there any confusion in the play of reproductions. The thing we have before us now is called a book, its parts pages, and so forth. It is a serious symptom if some one does not call things by their right names. Normally, the connection between definite visual processes and definite acoustical and motor processes works astonishingly well. What other explanation can be offered except that where we had no conducting path originally or several indifferent ones, with time one single path has become the mode of conduction, or at least is so much more susceptible to excitement than all the others, that now processes are obliged to travel along this one path? This means that, without explaining the *genesis* of this dominant one-way function, the perfect order of association and reproduction is again explained altogether in terms of the properties of topographical conditions. Though these conditions are not supposed to exist in the same manner in infancy, and though the changes by which they are produced remain obscure at the present time, when once they are established the direction and order of processes is as rigidly enforced by them, and any influence of dynamics will be as utterly excluded, as is the case supposedly in simple sensory conduction. As the railroad train remains on its tracks because these determine one way of least resistance, and as the enormous power of the engine has no influence upon direction, so in reproduction as well as in sensory processes all order and direction is a result of regulative prearrangement, independently of the actual properties and dynamical forces of the process itself.

If for the sake of order dynamics shall not take part in the distribution of processes, and if distribution shall be merely an effect of given topographical conditions, important consequences follow.

First of all, excluding only the somewhat obscure *genesis* of associations and habit, what happens in the nervous system will depend either upon inherited mechanical arrangements or upon secondary *acquired* mechanical arrangements. Therefore, where an actual performance is not an instance of learning as such, it must be explained either by original topographical conditions or by past learning, i.e., by acquired changes in those conditions.[1] Now, this alternative is nothing but the old dualism of nativistic and empiristic explanation. No reader of all the famous discussions between nativists and empirists can have a serious doubt that a nativistic explanation has always involved the assumption of a given anatomical basis for the actual fact in question. If such explanation did not seem to be acceptable, then only one other possibility was left open, that of learning. These authors never entertain the idea that some specific and orderly function might occur without being controlled either by special arrangements preëstablished *ad hoc* or by arrangements acquired in learning. What may this third alternative be? Vitalism? We shall see.

In well-established one-way streets what happens at the end of them will depend in part upon what has happened at the entrance. Sensory experience will therefore consist of purely local elements of experience, the genuine properties of which must depend upon local stimuli exclusively. If the isolation of processes in each pathway and in each final cell (of the brain) is supposed to suffice for the maintenance of order, no influence of processes in other parts of the nervous system will be able to alter sensory experience, and so it must remain the same whatever the changes of attitude. By enumerating the actual

[1] Among the first we may count those anatomical arrangements which, though not ready in embryonic life and at the time of birth, will develop to their final form by maturation.

properties of all elements at a given time we give an exhaustive account of the presented field. This is what has been called the summative, or mosaic, character of sensory experience, as it is understood from this viewpoint. Evidently, in terms of it, the sensory field becomes "inflexible," exactly as its physiological basis is mainly determined by rigid topography. It also becomes "poor," because the variety of experiences is restricted to those indifferent patterns of elements which we may find by varying independently the properties of the local elements. The organization of processes in the field is excluded because order must be explained by functional separation. Specific function, dynamically extended over an area of the field, is excluded for the same reason; we have to deal with a purely *geometrical* pattern of *local* processes.

How "empty" and "dead" does the organism appear in this theory! Dynamically, it has nothing to contribute to the monotonous elementary currents conducted compulsorily from a point of stimulation to a point of reaction. So it becomes an indifferent stage for actors indifferent to the stage as well as to each other. As an object of research in dynamics it is less interesting than a molecule or a soap-bubble which are both of them functional wholes.

If between the field of sensory processes and the effector organs conduction is determined in the same manner as, in this theory, it is between local stimuli and the elements of that field, a thoroughly adequate formula for research in psychology will be: to find out what stimuli produce what reactions in the effector organs. The statement that stimuli, on the one hand, and reactions, on the other, are the only points of interest in psychology, corresponds absolutely to the picture of the organism, and especially the nervous system, as lacking any characteristic process of its own as a whole. Since other schools do not pretend to have new positive ideas about the functioning of the nervous system, that seductive formula of behaviorism has found a rather general assent as expressing the viewpoint of natural science in psychology. Unfortunately, in its present usage, it is, as the seductive usually is, both ambiguous and superficial.

From the fact that in this theory dynamics is excluded from the determination of order and distribution, one more consequence follows immediately. Everywhere in nature dynamical events depend upon the properties of those processes and materials which exert influences upon one another. In a solution, containing Na_2SO_4 and $BaCl_2$, $BaSO_4$ will be precipitated because of certain properties of Ba, SO_4 and H_2O which, in their mutual relations, determine the dynamics of the process. Two electric currents will produce mutual attraction of their conductors if both have the same direction; repulsion, on the other hand, if the direction of one current is opposite to that of the other. The rule is general, that "relative properties" as exemplified in these cases are decisive for dynamical interaction. A theory, therefore, which excludes dynamics from the determination of distribution, allowing it the production only of elementary nervous current, will have to draw the inference that the properties of local processes have no influence whatever upon the total distribution in the field. A given local process will be altogether indifferent to its neighbors, running its course uninfluenced by their existence. All possible patterns may be produced by appropriate sets of peripheral stimuli; no mutual forces are admitted which, in the whole field, would produce certain definite distributions rather than others. A similar consideration in the case of association and reproduction will occupy us later on.

When confronted with such an unretouched picture of current assumptions about physiological functions, most psychologists will protest. They will declare that one should not take too literally what has been used rather as an analogy in a preliminary endeavor to develop ideas about the processes of the nervous system. Every one admits, moreover, that there are cases of "irradiation" in some parts of the tissue! To this criticism I must answer that if *all* analogies chosen, in the first tentative picturing of orderly nervous function, are of the same type, using topographical arrangements as the basis of order, this may be taken as evidence that other analogies do not occur to these authors. Preliminary though it may be, it remains a machine-picture, and no other has been developed, which is different in principle. As to irradiation of current, this concept as such does not mean more than a lack of definiteness and accuracy in the machine; it presupposes order enforced by strict isolation as the normal case, a slight deviation from which is the whole content of the idea. I admit that it makes our ideas about nervous functioning a little more nebulous than they should be according to the extreme viewpoint of machine theory; but I deny that in this manner *order* of distribution can be or has been explained by any one. Granted that all conductors may "leak" a little at certain points and that, therefore, local processes may become interfused mutually to a certain degree, I am unable to deduce orderly distribution or organization from such an indifferent "spreading" or interpenetration of essentially isolated units.

Let us compare the consequences of the theory with observation. Some relevant data have already been mentioned. Other and much more important data will occupy us in the next chapters.

That constancy of brightness and of size cannot be explained by the assumption of one-way conduction determining local sensory experience in terms of local stimulation does not need further discussion, since, because of their incompatibility with that assumption, those facts are commonly believed to exemplify the influence of meaning. But now that experimentation has shown that the meaning theory does not seem to explain them either, neither the empiristic nor the nativistic assumptions help us in these cases. So we must try to conceive a third kind of nervous function other than the types in which processes are strictly directed either by inherited or by acquired arrangements. If there is a third reasonable assumption, it will be necessary to apply it also in those other cases, such as constancy of form, speed, localization, and so forth, which are so similar to constancy of brightness and size, that the same explanation should obviously be given in all these instances. Quite generally then the alternative between empiristic and nativistic hypotheses must be misleading.

The introspective theorem that changes of attitude cannot influence "true" sensory experience does not agree better with the facts. It seems rather to be an arbitrary definition of "true" sensory experience. In observation, at least, if I transform the white in the shadow and the black in full light into two similar grays, *by "introspection,"* there can be no more radical influence of attitude upon sensory experience than this transformation which occurs under constant conditions of stimulation. And the same holds wherever introspection, destroying natural experience by an artificial technique, finds its "true" sensations instead. Fortunately in one part of our science at least, this seems to be the prevailing opinion. When we analyze a clang we hear several notes appear successively in the mass which previously we heard as a *unity*. If in this case we agree that under constant conditions of stimulation our attitude transforms one sensory reality into others, and that the clang heard as one is not less real than the partial tones apparent during analysis, we have no right to contradict similar observations in other cases.[1]

As to the statement that sensory experience is a purely local affair, each point of a sensory field depending upon "its" local stimulus exclusively, we must reiterate that no grounds have ever been given for such a radical assumption. Rather it seems to be an *a priori* belief about what *ought* to be the nature of things, experience to the contrary notwithstanding. As far as observation goes the properties of local retinal stimulation do not simply determine the size, the form, the localization and the brightness of local experience; neither does retinal speed determine seen speed, as it probably ought to according to the thesis that the geometry of retinal facts deter-

[1] If I say that "attitude" will sometimes have an influence upon sensory experience, this statement does not mean, that "mental power" can change the sensory field arbitrarily. First of all, strictly speaking, it is not the attitude, as an experience, which changes the sensory facts; the physiological process underlying it changes the sensory process. Also, such a change is not produced arbitrarily; it necessarily follows definite antecedents. Natural objective experience does not yield to all slight changes of attitude. Apart from introspection our attitude will not tend radically to alter sensory experience. Some very important exceptions will be considered later on.

mines spatial experiences. As a matter of observation, all the well-known "illusions" may be cited as evidence of the fact that local processes depend upon *sets* of stimuli. To some degree this controversy will be settled by pragmatic principles: the triumph will go to that side whose principles prove more fruitful in the further development of psychology. In the meantime, however, we may carry on our analysis of the polemical situation at present.

Almost all psychologists agree, in the case of one experience, upon the fact that local sensory experience is determined by more than merely local stimulation. This case is that of color-contrast, which most psychologists suppose to be an effect of interaction in the nervous system. If here the point-to-point correlation between retinal stimuli and sensory experience is surrendered, because the determination of local experience by conditions in a larger area is too evident, how can we proceed hereafter as if this discovery and concession had not been made? It took science some time before it would accept simple observation even in this case. Helmholtz refused to do so and, of course, he applied the meaning theory in order to save its fundamental belief, i.e., the point-to-point determination of local sensory fact by local stimulation. But after the first step has been made, we should realize not only that one theory of contrast has taken the place of another, but also that instead of applying persistently one general principle to all sensory experience, we have begun to accept a new one. In the future, wherever experience does not correspond to local stimulation, we should, at least, consider the possibility that such an experience may depend upon the total set of conditions in a larger field, exactly as contrast does. *If* it does, this may help us to understand why changes of attitude affect sensory experience in some cases. Sensory experience, which depends upon the constellation of stimuli in a larger area, and therefore does not correspond to merely local, inflexible units of process, may be influenced by "processes of attitude" as well. The most important point, however, seems to be that if we find a type of process depending upon *sets* of stimuli instead of single ones, this process may be that "third possibility" we are seeking. Certainly, it would neither be like the elementary processes of nativistic machine theory, i.e., absolutely determined by *inherited* arrangements, nor like the processes of empiristic theory, upon which a similar compulsion is exerted by *acquired* arrangements.

In the next chapters I shall try to show that other facts, much more important than those we have considered hitherto, point exactly in the same direction. For the sake of order in nervous function the machine theory excludes *organization of process* in the field. But we shall see that organization may be regarded as a typical fact in sensory experience. Again the theory excludes the assumption of any specific process dynamically extended over an area of the field. But it will not be difficult to show that there are experiences in great number the specific properties of which belong to *extended wholes* and do not exist in local isolation. This means also that the real variety of processes occurring in the sensory field is simply enormous when compared with those essentially indifferent patterns of local elements which may occur according to the machine theory.

If dynamics are excluded from the determination of distribution, local process will correspond in every case to its stimulus; the actual properties of stimulation in their mutual relationships will play no rôle in the whole affair, as they would undoubtedly in the case of dynamical interaction. Reviewing our observations, however, we find that everywhere the aspect of sensory experience depends upon the properties of stimuli in their mutual interrelationship. This is well known in the case of contrast and tonal fusion. but we may cite as well all those observations which were discussed at length in the last chapter. Constancy of brightness, for instance, depends upon the relation of the illumination and brightness of the neighborhood to the brightness of the field in question. That *organization* in the field depends upon the definite "relative properties" of local conditions, will be shown in the next chapter.

All these facts make it probable that something is wrong with the machine theory; they look as though they might be better understood by dynamical theory. Sometimes the observer finds dynamical events quite obviously occurring directly within the field. This is particularly the case, when sudden stimulation or change of stimulation is followed by a *development* of process. If we cause a bright figure suddenly to appear in the dark, this figure will have at the time neither its full size nor its "right" place. Instead it will appear with an energetic movement of extension as well as of approach. At the moment of abrupt disappearance, it will appear to have a movement of contraction and recession. Obviously, such observations would be exceedingly strange if considered in terms of machine theory. Or again, take the surprising fact that in touch, as well as in vision and hearing, the distance of objects and events may vary considerably if the stimuli are given in a certain manner. In some beautiful experiments of von Frey two distant points of the arm touched at the same time will appear to be one-half the distance apart which they appear to be when touched in slow succession. Scholz and Kester have both measured the mutual "attraction" which lights as well as sounds show when presented under adequate conditions. If the machine theory did not have its inordinate historical prestige, no one would hesitate to take these observations as evidence of dynamical interaction within the field. The stroboscopic movement, which belongs to the same class of observations, has acquired a unique importance by the fact that experimentation in this field led Wertheimer explicitly to discard the purely summative theory of sensory experience, not only for the problem in question but generally.[1] If at some distance from each other two stimuli are successively projected upon the retina of a subject, a movement will be seen, starting from the locus of the first and ending in the region of the second. Under favorable conditions there will not even be two "impressions." One "thing" will move from one place to the other—certainly a case of dynamics which it would be difficult to understand by the theory of isolated local processes. After the more important properties of stroboscopic movement were investigated by Wertheimer, Koffka and others, there was much discussion, mostly about minor points, which almost tended to obscure the main facts. As was to be expected also, meaning was rather liberally offered as an explanation. Nevertheless, if conditions and the attitude of the observer are not too inadequate, we undoubtedly *have* movement in the visual field. Those who do not believe direct experience, where it contradicts the supposed properties of "true" sensation, may deduce the "reality" of that movement from the fact that, when repeated, it produces an after-image of movement in the opposite direction, exactly as "real" movement does. Though, historically, Wertheimer's investigation was the be-

[1] *Zeitschrift für Psychologie, 61,* 1912.

ginning of *gestalt* theory, in the proper meaning of that phrase, the following considerations of sensory process will take another slant, because to me another way seems more advisable as an introduction.[1]

What may be said in favor of the machine theory? Sometimes I have heard the preliminary argument that it gives us a picture of nervous function which, clear and simple in itself, is the more easy to understand since in practical life we enforce order everywhere in the same manner, i.e., by arrangements *ad hoc*. I must confess that such a policy of the least scientific effort seems to me unacceptable. The comfort and the habits of the scientist do not count where he must deal with the properties of his subject-matter. Furthermore, only the psychologist, neurologist and physiologist will save time and effort by an assumption which explains order by arrangement. They simply hand their problem down to somebody else; wherever, in theory, a problem of function is reduced to one of special arrangements, the science of "morphogenesis" in its ontogenetic and phylogenetic branches is amicably asked to solve it, i.e., to explain the origin of the arrangements. So the avoidance of difficulties in our science means that others have proportionally more difficulties. And, by the way, at some point functional problems must be treated as truly functional. If it is barely possible to understand the ontogenesis of anatomical structure by the working of "special arrangements"

[1] Benussi has contributed to our knowledge of these problems by excellent experimental work. His investigation of similar facts in the field of touch has been mentioned above. Recently certain extremely important properties of stroboscopic movement have been investigated by Wertheimer and Ternus (*Psycholog. Forschung*, 7, 1926).

in the egg and germ, it would be ridiculous to explain the processes of phylogenesis by arrangements enforcing it.

Yet we do have special arrangements guaranteeing definite function in a great many organs of the body. Nobody can deny it, and I shall admit at once that the existence of the optic nerves as a conducting system between the eyes and the brain may be regarded as an example. Still, in our body, there is another conducting system in which a great many substances are transported along with the *blood*. And here, though the conduits are a general arrangement "for transportation," we do *not* have special arrangements for carrying each part of the fluid to its right place. What a definite part of the tissue pours into the blood or takes away from it at a given time is not determined by isolating conductors; everywhere it depends upon the actual relationship between the state of the tissue in question and the chemical properties of the blood, and yet normally we have order in the whole affair. The example shows that the existence of "organs" does not allow us to draw any inference about elementary processes being kept in order entirely by machine-arrangements.

However, you forget that in physiology and pathology we have much evidence for assuming a "projection" of retinal points upon definite points of the *area striata* in the brain! I am not certain whether the facts in question prove that between the retina and the *area striata* conduction is completely a point-to-point affair. But that does not matter, since at the present time neurologists no longer believe that the *area striata* is the terminus of optic processes and that visual experience is concomitant with processes in *this* area. If up to this nucleus conduction should be a matter of strictly isolated paths, the functional problem we are occupied with would have to be solved in parts beyond the *area striata*.

The nervous system consists of cells the fibers of which *are* isolated from each other. But in its gray ganglionic and nuclear fields conditions are different. Here mutual influence is not only possible; it is necessary.

The all-or-none law proves that elementary conduction is a matter of each single nerve fiber, occurring in one definite manner as long as the properties of the conductor remain the same. Experimental investigation of the all-or-none law is not yet complete and, so far as I know, we have no evidence as yet of its application to central fields. Supposing, however, that it holds in brain tissue as it seems to hold in peripheral nerves, the alternative between a strict machine theory of distribution and dynamical conceptions would remain as open as before, because, then, in each part of the nervous system the number of elementary organs taking part in any actual process, i.e., the density of the process, would have to be determined somehow, as would also the frequency of current in each single organ. Both properties of the process may either depend upon given local arrangements or upon dynamical intercourse in the ganglionic tissue. As distribution of energy is without exception a dynamical problem in physics notwithstanding the quantum theory, so the all-or-none law, as a sort of quantum theory in nerve physiology, does not exclude dynamical distribution of process.

The argument that distribution of process by pre-established and isolating conductors is the only way to explain the order of the field in its relation to the external physical world, seems to have two parts. *First*, it presupposes that dynamical interaction, not controlled by special arrangements at each step, must produce chaos and confusion. We have before us the old human opinion which, for a great many cases of dynamical interaction, is absolutely wrong. *Secondly,* as to the relation between the external physical world and the sensory field, the theory of isolated conductors enforcing order would explain a strict correspondence of sensory facts to *retinal stimulation.* But in the last chapter we saw that much experience is exiled "into the dust-cloud" of psychology by introspectionism just because it does *not* agree at all with the properties of stimulation. To be sure, there is a high degree of correspondence between the constancy of size, form, brightness, localization and speed of *physical objects* and the sensory field; but this agreement is *not* explained by the machine theory of vision which refers to the relation of *retinal stimulation* and experience.

If we are not satisfied by the alternative between order enforced by preëstablished arrangements and order determined by acquired arrangements, what else can produce order? For the moment we restrict our discussion to sensory processes and return to our statement (cf. p. 111) that, in physical systems, the relative influence of topographical conditions, on the one hand, and of the play of actual forces, on the other, may vary enormously. In typical man-made machines the rôle of topographical conditions prevails to such an extent that the only rôle of dynamics is to drive processes along a path laid down by those conditions. This means that typical machines are essentially a *special* type of physical system and that outside the little world of man-made machines there exists an immense world of other physical systems, in which the direction of processes is not completely determined by topographical arrangements.

Let us consider a drop in a current of water which moves through a narrow pipe. Why does it move? In addition to inertia, because pressure is higher on one side of it than on the other. But this difference of pressure works in one direction only, the walls of the pipe excluding all its other effects. Now let us suppose that the pipe disappears and that the drop (and the whole column of water in the pipe) becomes a part of a larger volume of water. The drop will probably also move in its new environment. But now it is exposed to forces on all sides, and its movement will be in the direction of the *resultant* vector of force. Obviously this movement is no less necessary than was the movement in the pipe, but here there is no local arrangement which can determine a single direction as the only one possible. Therefore the path of the drop will be determined *dynamically* in the new case, i.e., by resultant force at each moment. It follows that in this situation the path which our drop takes will depend upon the dynamical situation it encounters at each stage, and that it will change when this situation changes. This is one simple example out of millions. *In all of them not only movement, or process as such, but also the direction and distribution of process is determined* *dynamically by interaction.* It is events of this type which are excluded almost completely from machines, and the same type of process is practically excluded by standard neurological and psychological theory. *Gestalt* psychology asks to know the ground for its exclusion and defiantly proposes to give this type of process a fundamental rôle in psychological theory.

In the pipe the drop of water moves because movement, under differential pressure, is an approach toward equilibrium. Such is the effect of forces at all points of all systems. When surrounded by water, the movement of the drop will still be an illustration of the same rule.[1] The only difference is that now the direction of movement, according to the rule, will depend upon the actual *dynamical* situation. If we consider all the drops in the given volume of water, we will find the distribution of water gradually changing from one moment to the next. But whereas in pipes the distribution of the movement in space depends upon the form and the spatial position of the pipes, in the "dynamical case" it depends only upon the play of actual forces. In pipes, order is produced by *exclusion* of dynamical interaction; whatever distribution may result in the "dynamical case" is *produced* by dynamics itself.

At this point our interest concentrates upon this

[1] I am neglecting here the influence of inert velocities which may be neglected indeed in all discussion of the nervous system.

question, whether Aristotelians and modern theorists are right in assuming that *anything* may happen in dynamical interaction and that, therefore, dynamics is to be regarded almost as a synonym for disorder. What we see around us in inorganic nature seems to corroborate that opinion, since the blind meeting of forces and processes usually leads to chaos and destruction. What we have before us in these cases, however, may be described as follows: There is a thing at rest or a process going on uniformly; suddenly a new factor impinges upon the first thing or process from without, and after a short while, another new influence, again independent, is exerted from without, and so forth. Anything indeed may happen under these circumstances, and in most cases the end result of such fortuitous concurrence is disorder and destruction. This, I think, is the picture most men have in mind when they refer to dynamics, as though accidental impact were its only form!

There are, however, other cases, much more interesting for our present discussion. If in a basin, for instance, water is somehow distributed, perhaps in full movement, at a given moment there is a definite pressure at each point, and everywhere differences of pressure will tend to alter the distribution and the direction of local drops of water. Supposing now that the basin itself does not change and that no outer influences accidentally impinge upon the system during its redistribution, what will the result of continual internal interaction be? If we try to find the answer by imagining an indefinite number of drops, each moving under the resultant force in its immediate proximity, and each again influencing its proximate neighbors by that movement,—if we notice that this picture changes continually, as the distribution, and, therefore, the actually resultant force at each point changes in the smallest fraction of a second,—then we are inclined to dismiss the task as beyond our efforts, and to treat the events of this field in the light of the confusion or destruction that is attributed to nature in the case of accidental impacts.

But in this we are wrong. We are projecting our own confusion into the course of objective events. We become guilty of anthropomorphism. The physicist has quite a different attitude toward the problem. By observation, as well as by theoretical calculation, he is led to the conclusion that, generally, undisturbed dynamical interaction will produce a definite orderly distribution.

Let us review an example we entertained at the beginning of this chapter. To Aristotelian theorists the striking order of astronomical movements appeared inexplicable without the assumption of special arrangements controlling them. Now in modern times no one believes in those crystal spheres. But the order is there! And since the stars did not "*learn*" to move so orderly, some factors other than preëstablished and acquired topographical arrangements must be able to produce and to maintain order in distribution and movement. And in the prevalent conception of the solar system, it is continuous dynamical interaction, without any topographical arrangements, which produced and still maintains the order.

Other examples may be found in all parts of physics and chemistry: when two atoms come into their sphere of mutual influence, the play of dynamic interaction immediately begins, and, as the case may be, depending upon their "relative properties," they either separate again or they form an orderly molecule,—an architectonic structure,—without the aid of any arrangements *ad hoc*.

If we suspend a number of straight wires so that they form different angles with each other, the whole distribution being irregular, electric current entering the wires turns them into parallel lines. This is an orderly result of electrodynamic interaction.

Or again, we pour oil into a liquid with which it does not mix. In spite of the violent interaction of molecules at their common surface, this surface remains sharply determined, not by any arrangement enforcing this orderly distribution, but just by the play of surface dynamics between the oil and the other liquid. If specific density is the same for both liquids, these surface forces will change the distribution until the oil forms a regular sphere swimming in the other liquid.

I might go on to describe hundreds of examples. In all of them the situation would be the same in principle. Dynamical interaction, undisturbed by accidental impacts from without, leads to orderly distribution, though there are no special regulative arrangements.

And what is the explanation of this general tendency in undisturbed dynamics? It is simple enough. In all these systems we have one resulting force at each point at each instant of time. All the resultant forces together form one texture of stresses. From the principles of physics one can deduce, therefore, that, *for the system as a whole,* the immediate effect of all those forces will have one definite direction. At each point the forces will produce changes of movement or process which, when considered in their totality, bring the system nearer to the balance of the forces themselves. The factor of inertia may cause the real course of events to deviate from the ideal exemplification of this principle. But where, as in most organic systems, inert velocities not corresponding to actual forces are destroyed by friction, the real distribution of processes will exhibit the principle perfectly, and will finally reach a state of stability, of rest or of stationary process. The fact that this state will be an *orderly* distribution has been explained by Ernst Mach as follows: In orderly and regular distributions the totality of internal stresses will be more balanced than in a state of disorder. Therefore, by undisturbed interaction a system approaches order. For all details I must refer the reader to the literature quoted at the end of this chapter.[1]

Dynamical self-distribution is the third kind of functional concept which I propose to add to psychological theory, in addition to distribution enforced by inherited arrangements and order determined by acquired arrangements. More concretely and for the visual field, my assumption is that the order and distribution in this field is in each case the result of dynamical interaction. From this viewpoint the processes underlying the visual field in a state of *rest* represent the equilibrated distribution of sensory dynamics under actually given conditions. When not at rest, sensory dynamics will be in a state of *developing dynamical distribution.*

Though the direction of local process in a system is not altogether determined by local arrangement, the result of dynamical self-distribution as a whole may still depend upon topographical conditions (cf. pp. 112-113). Thus the electric current in a network

[1] The reader is asked not to judge the physical side of *gestalt* theory on the basis only of this short report. The concept of equilibrium is less fundamental in biology than is that of dynamics. But the *direction* of dynamics cannot be defined without it. By no means are we allowed to treat the sensory field (or even the organism) as though it were an isolated system. Therefore the laws of dynamics do not apply here in their simplest form.

of wires is distributed dynamically; yet the actual distribution as a whole depends upon the position of the electrodes and upon the conductivities in all the conductors. Similarly, the totality of processes in the optical part of the nervous system will depend upon given conditions in each case. For the moment, though this will prove to be an inexact assumption when it is examined more closely, I shall suppose that in the interior of the optical network the general conditions of conduction remain constant. But, then, as a set of peripheral conditions, we have the patterns of different chemical reactions on the retina, as they are produced in each case by actual stimulation. Upon these varying conditions the self-distribution of process will depend primarily. If neurologists are correct when they assert that between the retina and the *area striata* of both hemispheres, conduction is a matter of isolated pathways, then the *area striata* will be a sort of "central retina," in which the pattern of retinal stimulation is copied by a pattern of central processes. In this case *dynamical* distribution will begin here, depending upon the actual pattern of processes in the occipital lobes.

It will be evident that the task such a theory must face is enormously more difficult than that of machine theory. Where arrangements are altogether responsible for distribution, the concrete dynamical properties of process are of little concern. If we do not know about them, it does not matter very much.

In dynamical theory, on the contrary, the whole development of the theory requires that we know those properties of the process. And since as yet physiology is not very instructive concerning them, the extension of the theory depends instead upon the assumptions which we make. Experimentation on visual experiences will have to give us the necessary hints, and thus the consequences of our hypotheses will be tested. At the present time only the first steps have been made in this direction, and it will take us a long time before we feel firm ground under our feet. Let us remember, however, that all the perplexities we may find on our way, and all the mistakes we make in its course, are not to be referred to the fundamental concept of self-distribution by interaction; they should be referred to the particular types of process, force and interaction, which occur in the optical part of the nervous system.

As an example which will make our analysis more concrete, I shall introduce one special problem. If from a given pattern, either on the retina or on the "central retina," processes start into the conducting network beyond, and if their distribution is determined dynamically, why should differences among the processes, corresponding to differences in color, be preserved on the way? This is generally the case: to the accurate retinal contour of the image of a letter on a white page corresponds the sharp outline of the letter in my visual field. If the totality of processes issuing from the area of the letter remains detached somehow from the processes surrounding it, we may understand, perhaps, how even in dynamical distribution the most essential and crude geometrical properties of retinal pattern may be sustained. But how can one set of processes remain detached from the rest in dynamical theory? This embarrassing question certainly could not occur for the assumption of isolated pathways. My answer is based on the fundamental principle of the theory. If under these circumstances one set of processes remains detached from the rest there must be dynamical factors at work. Generally, processes corresponding to a definitely colored area will have definite properties as a class of processes, different from the properties of a class of surrounding processes which corresponds to another color. They will remain isolated in the nervous network if we suppose that in the ganglionic fields, where they "touch" each other, their differential properties act as *separating forces of contact,* so that they mutually exclude each other. Take as an example the contact of oil and water. Here interaction is so strong that the form of the surface is determined by it; this surface as such remains a sharp boundary and the drop of oil remains detached from the water by those same molecular forces which, at the same time, mold the form of the drop. I shall assume, then, that, in optical processes, contours are preserved by similar forces of antagonistic contact, depending upon differences in the properties on the two sides of the contour. From this viewpoint, interaction will thus determine general distribution; it will also have an influence upon size and form, and so forth, and it will not generally "dissolve" processes, corresponding to a homogeneous area of color, in the surrounding processes. This may seem a bold hypothesis. But recent experiments have shown that this assumption is in the right direction. It would be surprising if *all* process differences should act as separating surface forces of the kind just described in the case of color, for in physics such forces exist between oil and water, for instance, but not between water and alcohol, or a great many other pairs of different materials. Indeed, when reëxamining the results of older experimental investigations, Liebmann[1] proved that, whereas very slight differences of brightness gave sharp contours, neighboring areas of different color, even with maximal differences of chroma *alone,* will show mutual diffusion.[2] We may conclude, then, that only differences of brightness act as effective forces of separation. Under normal conditions neighboring areas of different chroma will almost always be different in brightness. Therefore, sharp contours on the retina will in most cases give accurate contours in the visual field.

Without further discussion we can draw a rather

[1] *Psycholog. Forschung, 9,* 1927.
[2] It is well known in photometry that without differences of brightness figures lose their definite form by diffusion into the ground.

important conclusion. We are accustomed to regard order enforced by rigid arrangements as exceptionally secure. If, however, the order of sensory experience be conceived as the result of sensory dynamics, this will seem to most people like explaining the quiet life of an orderly citizen as the outcome of many moral struggles and catastrophes. But, in this connection, the chief point is that in those cases in which the machine theory of visual order seems to be most convincing, dynamical theory gives exactly the same results. As an example we may take the symmetry of a seen circle, corresponding to the symmetry of its retinal image. Is it necessary to explain this correspondence by isolating conductors which maintain the geometrical properties of the retinal image in all the processes underlying visual experience? By no means. Assuming that surface forces of separation keep the processes of the circle distinct from the surrounding processes, the dynamical influences exerted upon the processes of the circle will be the same in all directions,[1] if, in the immediate neighborhood of the circle, surrounding processes are homogeneous. Therefore, if the network has the same conducting properties throughout that part of the nervous system, i.e., if it is functionally "homogeneous," there will be not the slightest reason why

[1] If the surrounding field is not homogeneous the symmetry of the circle will be distorted as in the well-known illusion in which two lateral vertical lines make the circle oblate without, of course, changing the retinal image. A number of other illusions illustrate the same principle.

the symmetry of the process should be disturbed. In this case, dynamical theory gives the same result, then, which until now has been rather clumsily explained by preëstablished arrangements *ad hoc*.

Since the rôle dynamics plays is so slight in contemporary theory, it may seem very strange to some psychologists. Hence I wish to make the following statement expressly: *these concepts do not contain a single thought in the direction of vitalism.* It is true that a number of difficulties may be removed by them, difficulties which vitalism can still raise quite properly against the claims of unqualified "mechanistic" ideas. But, in our field of research, "mechanistic" ideas have almost always involved explanations by topographical arrangements, and "dynamical" ideas, the type of idea prevalent in theoretical physics, are no more the discovery of the vitalists than of the mechanists.

I shall not try to develop more detailed theorems about sensory dynamics until we become further acquainted with the main facts and their significance. Some consequences of dynamical theory will be obvious at once. From this viewpoint, sensory experience is allowed to be as *fluid* and *manifold* as, to observation, it reveals itself. Furthermore, as local processes are not isolated,—their existence and their actual properties depending upon the dynamical context in a larger area,—the visual field may prove to be *organized*. In this case, as everywhere in physics, organization will be found to depend upon what I have called the "relative properties" of stimulation. We may eventually find specific properties in experience characteristic of extended areas and not analyzable into local sensations, just as existence exclusively as *functional wholes* is a property of many dynamical states in physics. The next chapters will be concerned with the development of this hypothesis.

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V

Sensory Organization

A DYNAMICAL distribution will be rightly regarded as a functional whole. Take, for example, a simple electric circuit: the differences of potential and the densities of current distribute themselves along the conductors in such a manner that a stable or stationary state is produced and maintained. No part of this distribution is self-sufficient; local processes depend throughout upon the totality of the distribution.

If a similar conception is to be applied to the processes underlying sensory experience, we must avoid a mistake. Protesting against the atomism which had been introduced into the treatment of sensory experience, William James once said that, in the sensory field, local experiences are interwoven with their neighbors in a manner which is beyond the grasp of purely intellectual theory. He seems to think that, even in original sensory experience, there is uniform continuity and that all cuts and boundaries are introduced later on for pragmatic reasons.

From the viewpoint of *gestalt* psychology such a statement does not correspond to the facts. In the last chapter we saw that, notwithstanding the general dynamical interdependence throughout the field, there may be boundaries in it where forces of "segregation" and "separation" take the place of those of "coherence" operating elsewhere.

Indeed, in the visual field, for instance, we have two kinds of order. One of them is the order with which we found the machine theory occupied in its effort to explain how a given local process is properly placed between its neighbors, and not confused therewith. However, there is another order in the field which has escaped the attention of many psychologists, though it is not less important than the first. In most visual fields the contents of certain areas "belong together," so that we have circumscribed, or bounded, units before us, from which surrounding elements are excluded. If James did not admit this organization of the field as a sensory fact, the reason for it was the enormous power of the theory of meaning, which has been an obdurate obstacle to our seeing important problems, more in this connection than elsewhere. For the same reason there may be only a few readers who will not quarrel with the next paragraphs as long as they see any possibility of so doing.

When I look at the desk before me I find quite a number of circumscribed units which appear detached and segregated in the field: a piece of paper as against the surface of the desk, a pencil, an eraser, a cigarette, and so forth. In all these cases there are two mutually dependent conditions. The existence of a unit involves its segregation from its surroundings. In order to satisfy myself that I am here talking about realities, I may try to form other units consisting of parts of those objects and parts of their environment taken together. In some cases my attempt will be a complete failure. In others, where, for some reason, there is greater success, the result is so strange that it indicates by contrast what a specific reality the original organization was.

But the reader says: Of course, you are somehow talking about realities. Still, something may be real psychologically without belonging to sensory experience proper. How can you forget for a moment that a piece of paper, a pencil, a cigarette, are objects known by use! You have handled them during many years and so you had more opportunity than you needed for learning that they *behave* practically as units. This previous experience being projected into your field of vision, why do you lay so much stress upon a simple fact which is widely known and well explained, and which has been accounted for ever since, or probably even before, Aristotle wrote his textbook of psychology. My answer will be more extensive than this argument requires. Until we are able to prevent such superficial applications of the meaning principle, there can be little agreement about the most elementary problems of *gestalt* psychology. Who in the world would deny that a piece of paper, a pencil, and so on, are well-known objects? That I know their uses and their names by previous experience and that hence they are full of "meaning," shall also be granted without hesitation. But from these facts there is a large step to the statement that neither the paper nor the pencil would exist *as segregated units* in my visual field without that previous knowledge about their practical behavior and use. It may be that before I had that knowledge the same things occurred in the sensory field as units, unknown and unnamed, but still as segregated wholes. When I see a *green* object, I can tell the name of the color immediately; yet I know that green is used as a signal on railroad tracks and also a symbol of hope. But I do not believe that, therefore, the color green *as such* must be explained by meaning. Existing independently it has acquired several secondary properties in my lifetime and I agree with the reader in praising all the advantages which this kind of learning holds for all of us. In exactly the same manner, sensory units may have acquired names and may have become richly symbolic in the context of our knowledge, while existing, nevertheless, as segregated units in the sensory field prior to such accretions. Such is the conception which *gestalt* psychology offers to defend. It even goes so far as to hold that it is precisely the original organization and segregation of circumscribed wholes which make it possible for the sensory world to appear so utterly imbued with meaning to the adult, because, in its gradual entrance into the sensory field, meaning follows the lines drawn by natural organization. It usually enters into segregated wholes.

If the explanation by meaning were correct, wholes should be segregated in the field only insofar as they are recognized as definite known objects. But this is not the case. Looking into a dark corner or walking through mist in the evening, the reader will frequently have found before him an unknown something, detached from its environment as one whole, the use or the meaning of which he did not discover until after a more detailed observation. Walking through an unknown country at night, I have often had such an unrecognized whole in the field for several minutes. It is evident to me, therefore, that my knowledge about the practical behavior of things does not determine their existence as detached units. The same argument may be restated in a more general form. Whenever we say to ourselves or others: Now, look here! What may that something there be, at the foot of that hill, just to the right of the next tree, between those two houses, and so on?—we ask about the meaning or the use of that something, demonstrating by our very question that segregation is independent of knowledge and meaning. As, in physics, a molecule is segregated as a functional unit, so definite wholes seem to be dynamically detached in the sensory field.

But so fond are we of our empiristic convictions that, in this predicament, the explanation by meaning will immediately assume another form. Your unknown whole, seen in the mist, so the reader will say, appears as something separate, because it is darker, for instance, than the gray mist around. I admit that no *special* knowledge about this definite group of sensations, as meaning a particular object through past experience, is needed for unifying and segregating it. But you under-rate the wonderful achievements of previous experience if you restrict its effects to particular cases. We have always observed that a set of adjacent sensations, possessing almost the same quality, different from that of the environment, "behave together," i.e., move and are moved, appear and disappear, at the same time. This is the case with stones, with papers, with hats, with boots, with many animals, with leaves. As physical objects they are bound together, so that physically they move as units. It is only one example of the well-known generalizing power of memory if now we treat as units and even believe we *see* as units *all* groups of adjacent sensations which are more or less homogeneously colored and sufficiently different from their surroundings. So we must not be astonished by the fact that, in the mist for instance, an area of darker nuance is seen as one individual something, though we may be unable to tell its *special* use or meaning.

I am not satisfied, however, by this form of the theory either. Units are formed and segregated in the field in a great many cases where this rather bold explanation does not apply. Take all units consisting of separate parts! If we look up at the sky on a clear night, some constellations of stars are seen immediately as belonging together and as detached from their environment. Cassiopeia is an example, the Dipper is another. In past ages people saw the same groups as belonging together and at the present time

Fig. 1

children do not need instruction in order to perceive them as units. In Fig. 1 the reader has before him two definite groups of patches. Why not merely six patches? Or two other groups? Or three groups of two members each? Looking passively at the figure every one beholds those particular groups in the field, two units being segregated, each containing three definite patches. What about generalized meaning in these cases? No previous experience could separate Cassiopeia from the other fixed stars around it. As far as everyday experience goes, they all move together. And no hint at the generalizing properties of memory will help here. We cannot possibly assert that we have *learnt* to see a number of separate patches, similar to each other and different from the environment, as one thing or one group, because they move together regularly. They are very far from doing that. On the desk in my room are sitting five flies which are five black dots as I see them from where I am. These dots begin to move separately and to move in different directions. So do three yellow leaves which a breeze lifts from the ground separately; so again three stones which my hand moves, one after the other. My general experience is that, at least as often as not, similar members of a group, which are separated by the common background, are movable and move *independently*. If, nevertheless, in this case definite groups are formed and segregated, this happens despite our general previous knowledge about the behavior of their members.

Investigating *which* separate "patches" tend to become included in one group, we find that, among other factors, their equality or similarity and their common difference from other "patches," favor their becoming grouped together and their segregation from others. So we see that in the case of separate members the same rule holds, without the influence of meaning, which was said to explain unknown *continuous* wholes under the influence of generalized previous knowledge (cf. p. 153). Consequently, in this latter case, the indirect genesis of groupings through previous experience is not needed any more than it is where separate members form groups. The grouping of separate members is used in one of our tests for color-blindness: A rectangular field is filled with dots at equal distances from each other. For the normal eye certain groups of them belong together and are seen segregated from the rest at once, and since these groups form written numbers, normal persons will read the numbers without difficulty. The dots in question have similar chroma and are sufficiently different from the others to appear as one group which, as a whole, is recognized immediately. But for those color-blind people who do not perceive the given differences of chroma, no group will be segregated spontaneously, so that they do not see and cannot read the numbers. In this example, the general acquaintance with numbers is the same for both normal and color-blind subjects. Therefore, the striking difference as to grouping depends directly upon sensory conditions.

Groups consisting of separate members have a special interest for theory insofar as they also prove that one unit segregated in the field may at the same time belong to a larger unit. One dot in our last example represents a continuous detached area; still it is a member of a larger whole, the number, which as a larger unit is again segregated in the whole field. There is nothing peculiar or mystical in such a subordination, since, in physics, a molecule as a larger functional whole contains several atoms as subordinate wholes. The atoms belong to the molecule functionally; still they do not altogether lose their functional individuality in that dynamical whole.

Following the casual observations of others, Wertheimer was the first to see the fundamental importance of spontaneous grouping in sensory fields and to demonstrate, by a great many examples, the main principles upon which it depends. Most of his illustrations entail the grouping of separate dots or lines, because, by using meaningless constellations in this manner, it is easier to guard the demonstration against disturbing arguments and criticism in terms of previous knowledge. He has pointed out most clearly, however, that the same principles may hold for all formation and segregation of wholes. I do not know a better introduction to these problems than is given by Wertheimer's paper of 1923.[1] The reader is asked to make himself acquainted with them by studying it. Some of the principles are easy to understand. We have already considered one of them, which claims that the equal and the similar tend to form units separated from what is dissimilar to them. Where we have no differences of quality, or other properties, among the members, relative distance will often be decisive. In one of our examples *two* groups (of three members each) are formed, because among the six patches some distances are small as compared

[1] *Psycholog. Forschung*, *4*, 1923.

with others, those patches, the distances separating which are relatively small, belonging together in one group. In some cases, perhaps the most interesting ones, it seems more natural to define the rule of grouping not by given conditions, but by the tendency toward certain results. As the physicist is accustomed to say that surface tension works in the line of reduction of surface, so in the sensory field grouping will produce certain wholes rather than others. We may say that simple and regular wholes, or closed areas, are formed more easily and more generally than irregular, or "broken," and open wholes. It becomes evident here that, in contrast to the indifferent mosaic of sensations assumed in older theory, this order of the field shows a strong "predilection" for certain general kinds of organization as against others, exactly as the formation of molecules and the working of surface forces in physics operates in certain definite directions.[1]

Recently the nature of grouping as a sensory and elementary fact has been demonstrated in the most convincing manner by experiments which Hertz has made on birds (*Garrulus glandarius*).[2] A number of little flower-pots are put upon the ground upside down. If the rather tame bird, sitting high up on a

[1] One form of empiristic explanation would say that we have learnt to regard as wholes whatever always moves together. Wertheimer has pointed out that, here, we have rather to do with one more principle of sensory segregation and organization: when some parts of the field begin to move at the same time and in a somewhat uniform way, they will become one moving whole at once and *ipso facto*.

[2] *Zeitschrift für vergleichende Physiologie*, 7, 1928.

branch, sees that the experimenter puts some food under one of the pots, he will come down very soon, lift the pot and take the food. This is a simple form of "delayed reaction" as Hunter investigated it years ago. In these experiments, however, the main point was not the *delay* of reaction as such, but its dependence upon the actual constitution of the field. The bird reacts without difficulty if there is one pot only.

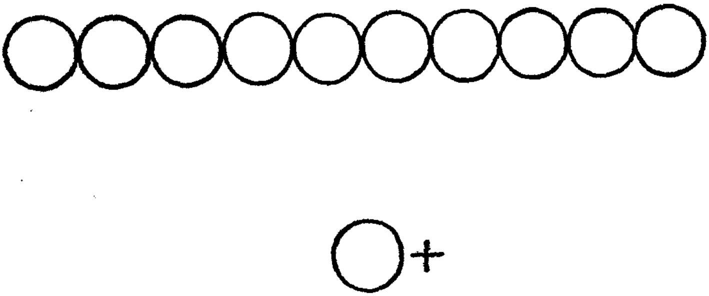

FIG. 2

But when there is more than one everything depends upon whether the "right" pot in some way stands out from the rest in the aggregate. If it is put in a regular line with the others so that, for human vision, it becomes absorbed as one indifferent member in a series, the bird lifts one pot or another in a haphazard way, even if the distances between them are as large as 25 cm. As soon, however, as, by grouping, the right pot becomes something strikingly apart, i.e., segregated from the rest for the human observer, the bird selects the right object at once. So in the case of Fig. 2, in which the right pot is physically only

10 cm. apart from a straight line of other pots, it is chosen at once. Obviously, here, the line of other pots is a well-bound whole and the one pot, a segregated thing by itself for the bird as it is for man. Even in the situation of Fig. 3, where the right object

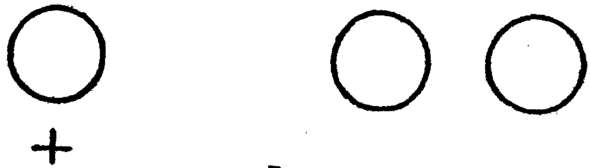

Fig. 3

is 6 cm. from the next and this 2 cm. from the last, grouping is definite enough to allow an accurate reaction. But in the case of Fig. 4, where the right

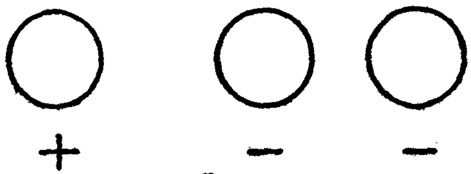

Fig. 4

object is 3 cm. apart from the next and this 2 cm. from the last, reactions become a matter of chance. The bird cannot keep the right pot apart before reacting, unless definite grouping helps him to do so. If the grouping is very well determined for man, however, the bird will have no difficulties at all, though the right object may be in immediate contact with its next neighbor. In the situation of Fig. 5 16 pots are arranged so that they form a closed ellipse for human vision. The right pot is put close to one of the other sixteen. The human observer will have before him one closed whole and one object outside. The bird chooses the right one at once. This example seems to me particularly valuable, because it shows that single objective distances as such are not decisive, but rather the grouping which results from

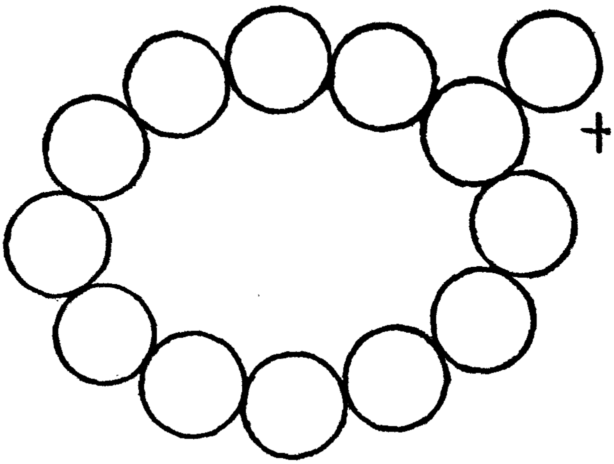

FIG. 5

the *total* constellation. How Hertz was able to demonstrate similar effects by the application of other principles, as, for instance, differences in size or chroma, may be better appreciated by reading the original paper. To me these experiments seem to open an altogether new field of research in animal psychology, providing we give up our somewhat conservative and negativistic mood in this branch of science, and begin to believe in the possibility of new problems. But as that negativism has not yet disappeared completely, I should remark that, if grouping is demonstrated in those birds as a "sensory fact," it does not imply "consciousness" in the animals. From our viewpoint, it is true, this does not matter very much (cf. above Chapter II). The only thing proved is that grouping occurs in sensory processes.

By further experimentation it must be possible to find out how far birds and other animals see *continuous* wholes, segregated in the field, as man does; though even now it would be difficult to understand the behavior of those birds, and their reactions to grouping, if the pots themselves were not detached units in their field. In any case the elementary nature of continuous wholes is demonstrated by certain observations on the first reactions of congenitally blind persons after they have been operated upon. Generally, the problems most interesting to the ophthalmologist in those cases are those of visual depth and of an original similarity between forms in vision and forms in touch. Results have been construed in several ways, but in most of the cases one side of the observed facts is not given adequate attention. It is generally true that when the patient is asked about an object, known by touch from previous life, but given him the first time optically and without the help of touch, there is no satisfactory answer; with a very few exceptions the patient does not recognize those forms directly. Still there is something very positive in his reactions: When asked about "that something" which he has before him, *he understands the question.* Obviously he has before him some thing as a segregated unit, to which he refers the question and which he tries to name. At least, if the object is a simple and compact form, the patient does not have to *learn* what "aggregate of sensations" he shall "treat as one thing." Thus elementary organization is an original sensory fact.

In Wertheimer's paper on sensory grouping one finds the same problem discussed in the case of wholes of a somewhat different sort. As we experience *time,* it has some properties in common with space, particularly with one dimension of it, namely its sagittal axis with man as the center. Therefore, words referring to relations on this axis are used as terms for temporal relations everywhere and in all languages. In English we have something "before" or "behind" us in both meanings; we look "forward" in space as in time, and death will come "nearer" in time, as one place is nearer to me in space than another. Perhaps, physiologically, there is a corresponding similarity between the two, because, with respect to the organization and segregation of extended units, we find the same general principles determining temporal order which are known to us from the visual field in a state of rest. By approaching my hand now and then for two-second intervals to the opening of an organ pipe which is sounding continually, I can lower the pitch slightly. In hearing, the effect will be a segregation of the corresponding number of acoustical units, the constant tone appearing as general background and the somewhat different notes as so many "patches" singled out from it. Of course, there are no such units in the physical stimulation. Physically we have thousands of waves of equal or different wave-length, all of them following each other indifferently. It hardly needs to be mentioned that in the same manner an appropriate succession of visual stimuli will be found to result in temporal unification and segregation. The same thing is true of touch and other senses. Again, "temporal dots" will readily form "temporal groups" in all these cases, the groups containing the dots as subordinate units and so forth.

It is in these *groups* that the principles of temporal organization are recognized most easily. With my finger I tap on the desk three times at very short intervals and, after waiting for a second, I repeat the tapping. People who hear this sequence for a while get groups in time. Physically each sound is indifferently related to every other; they are independent events as the stars of Cassiopeia are practically independent. Logically, other forms of grouping are quite possible which do not occur, however, in the experience of an observer who is listening calmly. Therefore, the groups, as we observe them, represent an example of physiological or, if one likes, psychological organization. The principle determining it in this case is relative distance in time exactly as relative distance in space was a principle of grouping in the simultaneously presented visual field. If all the intervals are made equal, we still get groups when introducing differences of intensity or quality in the series, especially when the tapping is done according to a simple regular scheme. In temporal sequences equality or similarity plays the same rôle as against differences of properties, which we have found in the visual field.

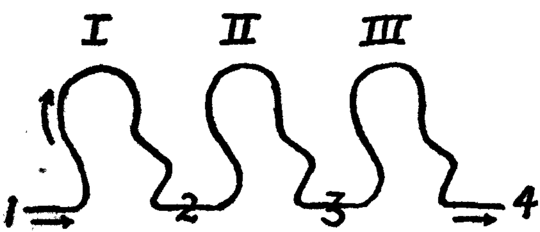

FIG. 6

In the most general case of sensory organization both space and time are involved in the same experience of grouping. A simple example will show what is meant: In a dark room we move a lamp hidden in a box, so that one bright point is the only thing visible on the dark ground. Let us suppose that the point moves in the following form without a change in speed: *vide* Fig. 6. A naïve observer will describe what he has seen as three curious figures, or three movements (I, II, III); perhaps he will correct himself after a while and say that there were seven movements (1, I, 2, II, 3, III, 4). But he will not say that he saw 53 or 16 or 29 movements! Now, as applied to the number of optical stimuli which impinge serially upon his retinæ as quite independent events, there is no reason why one of the larger numbers should be less correct than three or seven. But, in experience, instead of an indifferent series we again find a definite product of organization. The reader will have realized already that with respect to a more complex experience such as I may have if I see some one "nodding twice" or "shaking his head a few times," there is much in the experience that is not covered by our present discussion; but, neglecting the meaning of those particular movements, he will also realize that there is a sensory organization of the given movements into "two" or "a few" sub-wholes.

This seems to be an appropriate place to mention an indirect explanation of organization and grouping, preferred by some of the best psychologists in America. If I understand them rightly, a few of them claim that the overt movements which we make when responding to stimuli will produce the facts in question. Others would say that one particular kind of experience, namely, the kinesthesis occurring during such movements, is responsible for organization and grouping. In order to answer certain obvious objections, the first will say that in adults mere *tendencies* to movement will suffice instead of the overt movements which occurred originally. Similarly, the other hypothesis assumes that even faint reproductions of past kinesthetic experiences will be sufficient to explain organization of other sensory experiences in the adult.

In either case, whether the tendency toward movement, or kinesthetic experience, is taken as decisive, the fundamental question will be how these factors produce a definite organization, apparent in the visual field, or elsewhere. As far as I can see the only answers are, in the first case, that our overt movements are organized in exactly the same manner physiologically which, to naïve observation, is plainly *given, for instance,* in the visual field; or, in the second case, that kinesthetic experiences, and perhaps *their* reproductions, exhibit that organization. Whatever the process may be, by which organization is supposed to be introduced into the chief sensory field, which for most persons is that of vision, it cannot be imported without existing beforehand in whichever region it is said to have its origin. As long as we consider peripheral movements or sequences of kinesthetic experiences as a series of instantaneous events which follow each other independently and indifferently, it will hardly be possible to use them to explain the occurrence of definite segregated wholes and groups anywhere. Let us take the bright point moving in dark space as an example. If we say that the observer talks about three or seven movements in this case, because he makes or experiences three or seven eye-movements, we are presupposing the same organization in the temporal and spatial sequence of eye-movements, or the experiences of them, which I personally seem to have in the visual field *as such*. Otherwise the observer might as well report 53 or 29 or any other number of events, for with regard to stimulation such an enumeration would be no more arbitrary than that of three or seven parts.

I was once told that all the observations of *gestalt* psychology are very old and have long been explained by the kinesthetic experiences which occur during eye-movements. This sounds as though a hint about the kinesthetic experiences accompanying vision were satisfactory as an explanation of the phenomena of visual organization. But we see that, instead of having solved the problem, we have only shifted it from one place to another, for now we have to solve the problem of the segregation of wholes in the temporal and spatial extension of kinesthetic experiences.

I shall not deny that the problem of organization exists in the field of movements and kinesthetic experiences. On the contrary, I am convinced that the problem in either of these fields cannot be understood rightly without taking our point of view. But why should movements and the processes underlying kinesthetic experience be the only material capable of being organized, or of being treated from the viewpoint of *gestalt* theory? If organization is physiologically possible in one field, why not in others? In order to explain the apparent organization of visual experience by accompanying kinesthetic experiences, one must assume spatial as well as temporal organization of motor phenomena. I do not see any reason why such organization should be excluded from optics and acoustics. If there are some more general grounds for the exclusion, I am not yet aware of them. In the meantime we shall not discuss the difficulties which would arise, were we to try to explain *concrete* particular cases of visual organization by motor phenomena, for in the next chapter we shall again return to the question of indirect explanations of organization.

After what has been said about organization, we cannot be surprised to learn that serious lesions in the optical center of the brain may produce a sort of "blindness" in persons, who at the same time are not totally blind. Careful examination of such a case by Gelb and Goldstein [1] has shown that, here, the field of vision has undergone a radical change, organization having disappeared almost completely, so that the field shows a more or less chaotic character. Where he fixes his attention, the patient is able to grasp some small fraction of a line, for instance, but he can no longer see extended wholes as clear-cut forms. It is particularly interesting to observe that this patient begins spontaneously to rely to a great

[1] *Zeitschrift für die gesamte Neurologie und Psychiatrie*, 41, 1918.

degree upon motor experience instead of vision. Following the fractions of contours, which are clearer to him, with movements of the eye, he is able in time to build up motor wholes and to recognize them. So, if his name is written on a blackboard, he will follow the first letters and soon guess the rest. But it is possible to exclude this procedure by a simple trick: draw a few lines, which have the same color as the letters, across the name. Since to the patient the name is never given optically as one simultaneously and well-organized whole, and since now he does not see it as one thing and the crossing lines as another pattern apart from it, he will follow parts of a letter or parts of the crossing lines indifferently. The result is that he cannot read the name under these circumstances. The example shows, by the way, how much motor function, accompanying vision, depends itself upon normal visual organization. Organization being a matter of extended regions of the field, wherever only local fractions may become organized to some degree, the *control* which organization in a large area normally exerts upon the motor function, is made impossible, and results like those I have just mentioned become inevitable.

But why should wholes, detached by the operations of sensory dynamics, correspond so generally to objects, or things, in the practical meaning of the words? Do we have to assume that a surprising harmony is established between the laws of sensory dynamics and the area or the limits of physical things around us? No such assumption need be made, for there are exceptions to the correspondence of sensory organization and physical units. Take all the cases of groups of separate members, the constellations in the sky, the examples of dots forming definite groups (Fig. 1) above, the instances of grouping in ornaments, the parts of which are, in their physical nature, indifferent to each other, i.e., without functional interrelation. In countless cases organization is a sensory reality without there being a corresponding physical unit. Also, continuous sensory wholes may occur in the absence of an homologous physical unit. The reader himself at some time has seen a strange object, perfectly unknown to him, which later on, perhaps after some movement of his head and eyes, metamorphosed into a single well-known thing and some part of another one, these two together having been, at first, unified and segregated as one unknown whole. The same example shows that sometimes to a definite physical object there does not correspond a sensory unit, because in vision its parts have been absorbed by surrounding areas which happen to have qualities appropriate to them. This was the case in the puzzle-pictures which years ago amused the readers of magazines. And in the last war it became a real art to make things, guns, cars, boats, disappear at some distance by painting upon them an irregular design, the parts of which would form indifferent spots by intermingling with parts of their environment. The objects themselves are destroyed as optical realities and in their place appear meaningless patches which do not arouse military suspicion, since similar patches are produced constantly by the accidental properties of country and sea.

On the other hand, it is easy to see why there generally are visual units which correspond to definite physical objects. The things around us are either made by man or are products of nature. Objects of the first class are prepared for our purposes. Therefore, we give them a form and surface, etc., so that they are likely to be seen without difficulty. Without knowing the principles of sensory organization in an abstract form, man works in conformity with them, and so the physical units which are the products of his art will appear as visual units. On the contrary, it is not easy to create a somewhat compact object which, when placed in a simple environment, would not fulfill the general conditions of visual segregation. Camouflage is a difficult art.

With objects produced by nature, the situation is not altogether different. One class of surface properties in contact with the average character of other surrounding surfaces, is a condition fulfilled by most natural things, since the degree of similarity of the fractions of their surfaces will often show evidence of their common origin, and in the surroundings the surface properties will generally be of a variant character. Therefore, one condition of visual segregation is given in the case of most things. Even if a stone lies half-embedded in the sand, which is nothing but tiny fractions of the same kind of stone, the difference of coherence, and therefore of "inner detail," between the surface-elements of the stone and those of the sand will be sufficient in most cases to make the stone optically one thing. At least, at the boundary between a natural object and its surroundings some discontinuity of properties almost universally prevails. This discontinuity separates the environment from the interior of the object by a closed outline. Since, as a rule, that will suffice to make even a meaningless area appear as a segregated whole in the sensory field, it will certainly have that effect where the boundary of a physical object is concerned. If there are no such differences and no discontinuity whatever between the object and its surroundings, no visual unit will exist, it is true. But try to find objects which, without fulfilling any of the conditions of sensory segregation, are still plainly before you, because of the influence of meaning! You will have a hard task. Our general experience shows that wherever the conditions of visual segregation work against a unit, it will not exist in the sensory field of a naïve observer, even if it is well-known as such and is camouflaged only momentarily by special circumstances. A more detailed discussion would have to treat here the problem of visual depth and the segregation of things as three-dimensional wholes. But, though this question is of the greatest importance for the correspondence of physical units and sensory segregations, I must leave it untouched for the present, because as yet in this field experimentation as well as theory is in a rather undeveloped state.

In the last paragraphs I have laid some stress upon the fact that organization in a sensory field is something which originates as a characteristic achievement of the nervous system. This emphasis has become necessary because some psychologists have recently said that, according to *gestalt* psychology, "*gestalten,*" i.e., segregated sensory wholes in this connection, exist outside the organism and simply extend or project themselves into it. This is so absolutely wrong that I cannot comprehend how the misunderstanding arose.[1]

But after what we have seen, it is quite another problem to ask how far sensory organization, though being a characteristic achievement of the nervous system, may have an objective value at the same time. Between the physical objects around us and our eyes waves of light are the only means of communication. These do not bring the "*gestalten*" ready-made into

[1] One chapter of *Die physischen Gestalten in Ruhe und im stationären Zustand* has the title: "Denn was innen, das ist aussen." Should these words of Goethe have produced the misapprehension? Who reads the chapter will see at once that that title refers to the similarity between sensory experience and the physiological processes accompanying it, not to the relationship between organic processes and the environment (cf. § 181).

the organism; rather, the segregation of wholes occurs in the nervous system; but the result may tell more about some of the objective properties of the world around us than the rays of light would be able to do. We do not always learn more about an object, the nearer we approach it. For instance, when a lens is put in the way of the light reflected by a bright object, it would not be wise, for the purpose of getting a clear image of the object, to bring the screen as near as possible to the lens (and thereby the object); at a certain distance the projection tells much more about the object than nearer by. Similarly, sensory organization may give us a "truer" picture of the world around us in some respects than the rays of light, though these are the first messengers coming from the objects and sensory organization occurs later on and farther off.

Indeed, the waves of light do not contain the slightest indication of any organization or any "belonging together" which may exist among the parts of the objects by which they are reflected. Each element of the physical surface reflects light independently and, as reflectors, two elements of the surface of a sheep, for instance, have no more to do with each other than one of them has to do with a surface element in the animal's environment. In reflection, therefore, no trace is left of those units which exist in the physical world; they are dispersed completely into an indifferent mass of rays, all equally independent of each other. By the refractory properties of our eye those rays, which come from one point in the outside world, are made to converge upon one point of the retina; also the geometrical relations of the points on the surface of an object are reconstructed here, in large measure. Still, each local stimulus thus achieved is an independent affair, and rays coming from elements of the surface of one physical object, the sheep for instance, are as indifferently related to each other as they are to stimuli from the sheep's environment. So we have no organization at all in retinal stimulation, no wholes, no groups, no segregation. It cannot be asserted, in opposition, that there is one definite area or patch on the retina as the image of the animal, for the elements of this area are as independent of each other functionally as any one of them is independent of an element outside the image. In psychology much has been said about the stimulus-error which consists in our confusing our knowledge about the physical conditions of sensory experience with experience as such. But another mistake, which I propose to call the *experience-error,* is not less unfortunate. It occurs when we unintentionally attribute certain properties of sensory experience to the actual constellation of stimuli, properties which are so very common that we tend to apply them to whatever we are thinking about. This is the case primarily, wherever we have not yet learned to see the *problem* contained in those common properties of experience. No wonder, then, that neurologists and some psychologists still talk about "the retinal stimuli" corresponding to an object, as though there were something like detached functional units on the retina. Whereas as a matter of fact the whole retina is a mosaic of indifferently related spots, and this is the case until sensory organization begins physiologically.

Once we have realized, however, that stimulation, as such, is completely unorganized, the enormous biological value of sensory organization will become apparent. Since the rules governing this organization conform to the structure of objective units, to objective divisions, to objective "belonging together," in very many cases the result of their operation is a kind of reconstruction of those aspects of the objective physical situation which are temporarily lost on the way between the objects and the sense organ. It is true that continuous wholes are sometimes segregated, and groups of separate members are often formed, which do not correspond to objective physical units. But that is not a serious deficiency when compared with the indefinitely large number of cases in which organization is a picture of objective facts. If all the content of the sensory field were indifferent grains of sensory stuff, it would be a hard task to orientate ourselves in and to react to such a world. I am not sure that even after years of trial and error with regard to such a field, a child would *learn* to organize it. Considering the situation impartially, we may come to the conclusion that organization of the field, as an original sensory fact, is much more important biologically than the properties of local stimulation are. Color-blind people are perfectly able to adjust to their environment although their experience has fewer nuances of stimulation than the normal. This is so because their lack is not a serious impediment with respect to the practically important similarities and differences in stimulation. Differences of chroma are usually associated with differences of brightness (cf. p. 144). That is enough for *organization,* and since most of our behavior will be determined by this property of the field, even a large deficit as to qualities does not matter very much.

Organization is no less important for the procedure of science than it is for practical life. We saw in the first chapter that, as a physicist, sensory experience is my only primary material. But what experience? The system I am investigating, the apparatus of research, its scale, the needle, and so forth, are all of them segregated wholes, or sub-units, in my sensory field. If they were not given to me in such an order of "belonging together," physical research would be all but impossible. About this phase of "objective method" we do not hear very much, when behaviorists recommend the procedure of the natural sciences to us. But we should, since, if we consider physical research as a series of physiological events occurring in the physicist, we are still confronted with the problem of organization as an aspect of those events, which is absolutely indispensable for their success.

At the same time we can understand why the formula of "stimulus and response," though sounding well at first, is quite misleading as long as the term "stimulus" is used as carelessly as most behaviorists do use it. One stimulus, when taken in the strict meaning of the word, is not followed by one definite reaction in a great many cases (cf. Chapters III and IV). In optics, for instance, the organism will respond to an objective constellation of millions of stimuli by developing, first of all, an organized field, many and perhaps the most essential properties of which have no physical partner among the single stimuli (cf. Chapter VI). Reactions of the effector organs may and will begin very soon, in many cases; but, as the eye-movements show, even the first of these reactions will depend upon the developing organization of the field, because the laws of optically determined eye-movements refer to the boundaries of segregated wholes, to the situation of these wholes on the retinæ, and to lines, but not to independent "sensations." Apart from eye-movements, what is called "acting" in man will be a reaction to a well-developed field and in most cases to some definite whole in it. If, therefore, we say that in psychology the right formula is, *Constellation of stimuli—Organization—Reaction to results of organization,* such a statement fits the facts incomparably better than the usual one. The organism is not barren functionally; it is not a box containing conductors each with a separate function; it responds to a situation, first, by dynamical events peculiar to it *as a system* and, then, by behavior which depends upon the results of that dynamical organization and order. Suppose that somewhere in a factory HNO_3 were produced out of its elements and that in another part of the factory the product of that chemical organization were used to dissolve silver,—would you say that the silver reacts to nitrogen, hydrogen and oxygen? You certainly would not, because what happens to the silver depends upon that chemical organization, and it cannot be understood as a reaction either to those elements separately or to the sum of them. If that is so, we should also be very careful before we refer to types of behavior as being reactions to "a stimulus" or to "some stimuli." Even the last expression would often be quite ambiguous, because it might mean that the behavior in question is the consequence of several stimuli working independently at the same time, whereas it may depend upon a product of sensory organization.

Once I tried to convince a behaviorist that referring to "a female" as "a stimulus" for a male bird is equivalent to completely closing one's eyes to the problem of *gestalt* and organization. It was not possible. Though (or because?) he treats sensory experience as something without any interest for psychology, the behaviorist committed the "experience-error" so continually and pervasively that "the female bird" remained "a stimulus" for him. How often has "a mouse," "a door," "the experimenter" and so on been called "the stimulus" in animal psychology! Innocent though this expression may be, if it is used as an abbreviation by those who are fully aware of the problem of organization, it will hide this problem in a most unfortunate manner when used by an author who is not free from the "experience-error."

I mentioned above the great biological value of sensory organization as a reconstruction in nervous process of objective "belonging together." But how can it be a reconstruction if, on the way from the objects to the sense organ, the waves of light are an unorganized mass of independent events? There must be something in this transmission of rays which determines the "right" organization in most cases. We have seen, indeed, that the relations of neighborhood, of similarity and difference among the stimuli, though these are indifferent to each other dynamically, are in some respects a copy of the corresponding relations among the surface-elements of surrounding objects. Some definite relations among the stimuli determining sensory organization, and the preservation of these relations in transmission, seems to be the essential condition for that reconstruction of objective "belonging together." But if organization of the field depends upon the relations of stimuli as these are distributed on the retinæ, we must draw the conclusion that sensory organization cannot be understood by considering independent local processes as such. Now we know that, in dynamical self-distribution, process-in-extension exists as a functional whole. So we must assume that sensory organization, as we have considered it in this chapter, is a property of such a dynamical distribution, occurring under definite conditions of stimulation. At the same time we must remember that everywhere in physics dynamics depends upon the *relations* between given conditions. The more probable will it appear to us that sensory organization, as depending upon the relations of stimuli, is to be explained as an effect of sensory dynamics within the field. By studying organization, therefore, we may be able to discover what particular kind of dynamical events is responsible for sensory order.

Gestalt psychology is said by some critics to repeat the word "whole" continually, to neglect the existence of parts and therefore to sacrifice that wonderful tool of all scientific procedure, analysis. Nothing could be a more misleading statement, as may be judged from the fact that we found it necessary to mention *segregation* wherever we were dealing with a unit or a definite whole. In dynamical distribution, as we have seen, the functional "interwovenness" of a field is altogether compatible with dynamical segregation. We may even say that in *gestalt* analysis we find the *genuine* "parts" of the field as segregated wholes and groups and, in these wholes or groups, their genuine "parts" again as subordinate wholes and members, whereas the so-called sensations of introspective analysis are parts existing only in construction and theory. For this very reason analysis as a statement about "real" parts, existing in consequence of organization, is a perfectly legitimate and necessary procedure in *gestalt* psychology, probably much more valuable than any analysis into sensations which certainly no one finds segregated in his visual field.

One remark is needed here about another kind of analysis. I may passively accept what I find before me as the sensory field. Then analysis is possible in the meaning just defined. I may, however, adopt a special attitude with regard to the field, selecting some of its members and more or less suppressing the rest. In many cases a change of organization will be the consequence of such an attitude, and hence "analysis" of this sort involves a real transformation of sensory facts in *gestalt* psychology (cf. Chapter IV, p. 124). Of course, an analytical attitude is not the only one by which a change of organization may be produced. When we select certain members of the field, we can keep them together at the same time and so favor one special kind of "belonging together" instead of that which would prevail without our interference. Again the change produced by our attitude will be a real transformation.

From the viewpoint of *gestalt* psychology a change of attitude involves a definite physiological stress exerted upon a sensory field by processes originating in

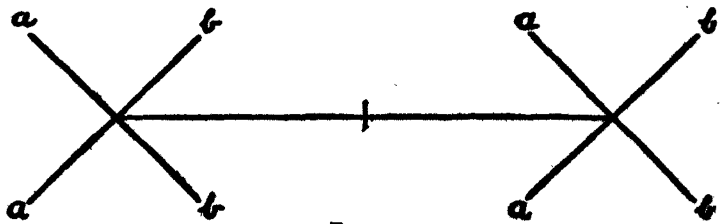

FIG. 7

other parts of the nervous system, and to some degree the organization of the field may yield to it. The figure 7, for instance, is seen normally as a symmetrical form. By picking out the lines marked "a"

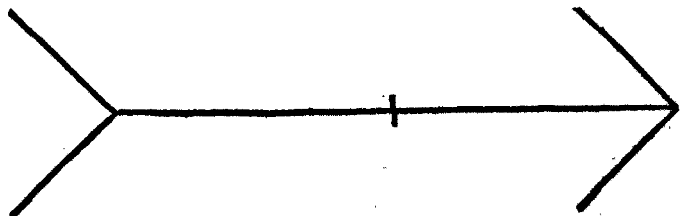

FIG. 7a

however and keeping them together I can almost see the figure 7a, the lines marked "b" being repressed. In the same way I may favor the lines marked "b" and, as it were, create the figure 7b. How concrete and real such a change is, will become apparent if we consider the point which is the center of the figure objectively. When we produce the figure 7a by favoring the "a," that point is shifted to the right, as it also is, of course, when the lines marked "b" are not drawn at all. It is shifted to the left when we produce Fig. 7b.

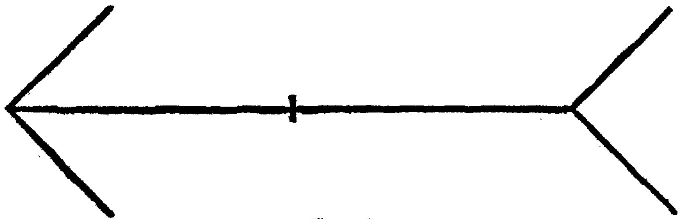

FIG. 7b

In some cases sensory organization seems to change without any influence being exerted upon it from without, simply because processes which remain the

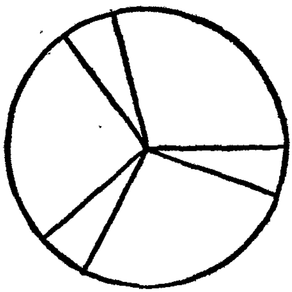

FIG. 8

same for some time in the same part of the nervous system tend to alter conditions in that part and to block their own path. We know that the same thing occurs in electrolytical cells in which the current polarizes the electrodes and thereby creates forces opposed to itself. In Fig. 8 we are confronted by a pattern formed by three narrow sectors. Looking at the center of it, most persons will suddenly see another pattern after a while. In the new shape those lines, which belong together in the first pattern as contours of one arm, belong to separate arms and vice versa. The organization is changed. By appropriate experimentation, making our subject fixate the center for a long while, we can reduce the time for either shape to a minimum value, so that the alternations follow each other very quickly.[1] If now the figure is turned around a little, so that the arms have another position, the figure becomes as stable again as it was at first, and only after some time the alternation will quicken, as in the first position. In my judgment, this fact may be taken as evidence for assuming that a local effect of organized processes itself produces the alternations of organization.

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[1] Not all subjects are equally suitable for this experiment.

VI

The Properties of Organized Wholes

THE dawn of the *gestalt* problem in modern psychology was not the idea of dynamical self-distribution as opposed to order enforced by arrangement; nor did it begin with the discovery that segregated wholes represent highly important sensory phenomena. The starting point was the observation that sensory fields are replete with qualities and properties which one neglects if one takes "sensations" as their sole content and which, indeed, may have a mysterious aspect when first viewed in this way. It was von Ehrenfels who, preceded by some casual observations of Mach, directed the attention of psychologists toward the fact that a great many, and perhaps the more important, properties of sensory fields do not fit into the scheme of concepts which is centered around the idea of "sensation."

If it is natural for a "sensation" independently to fill its local place in the field, determined by one local stimulus alone, the characteristic and surprising feature in those other qualities (which are usually neglected) is their existence only as properties of somewhat extended regions. From this fact it seems to follow that those qualities of von Ehrenfels cannot be determined by single local stimuli; their existence, then, depends upon several stimuli having a specific effect in a certain *area as such.*

As an example we may take a glass of water in which soap is dissolved. The aspect of such a liquid is called "trübe" in German, which in English means something like "dim" or "turbid." Now look at it through a little hole made in a piece of cardboard and you will see the hole filled with a certain hue of gray (perhaps a little bluish or reddish), but the quality of "dimness" or "turbidness" will have disappeared. It is the property of a more extended field and depends upon more than local uniform stimulation. Exactly the same is true of the dimness or diffusedness which appears as a quality of things seen in a dark corner. Again no local impression, "isolated" artificially, shows any dimness, but the extended area does so in a striking manner. "Clearness" and "definiteness" as qualities of a field or of parts of it, have the same supralocal character. I may also mention the property of a surface which we perceive by touch as "rough" (the German "rauh"). There is no character like "roughness" in purely local experience of touch. And further we see that von Ehrenfels' peculiar qualities occur in temporal extension as well as in space in that the German word "rauh" is used for certain acoustical phenomena, as it is for "rough" surfaces. When listening to rather rapid beats or to the "R" of human speech we get that peculiar sensory quality, which seems to be intrinsically similar to the "roughness" of a surface. Again, as this quality depends upon rapid beats, if stimulation is shortened below a certain limit, it disappears, whereas other acoustical qualities will subsist. In passing we may remark that in most cases words like "homogeneity" and "continuity," whether applied to optical or to other experiences, mean properties of extended sensory fields.

From a functional point of view, these observations are less surprising than was the general opinion at the time of von Ehrenfels' discovery. The processes directly underlying our experience of a color will be a certain chemical reaction, i.e., certain molecules are built up or destroyed. Now the chemist may analyze such a reaction, but there is a natural limit to his analysis, because at least one whole specimen of each atom or molecule taking part in the reaction, and the whole dynamical event thus comprised, must be included. Beyond that limit what is called "this specific reaction" will lose its meaning for the chemist and, therefore, for psychophysical theory which associates one definite color with one definite kind of reaction. Therefore, even before parting with the theory of "local sensations" we are obliged to accept dynamical realities existing only in somewhat extended areas of space. If that is so in chemistry, the same fact should not frighten us when we face it in direct experience.

According to the theory of dynamical self-distribution color as a quality is dependent upon chemical reactions in a brain-field, although the production of local chemical reactions in a certain distribution is only one side of the total process. There is much more that is characteristically spatial and dynamically real in such a coherent distribution than in the mosaic of local sensation-processes which are the "dynamical realities" of machine theory.

From the new viewpoint the Ehrenfels-qualities, corresponding to more extended dynamical realities than color, would be physiologically produced at the same time as color, since each is a phase of the same total "process-in-distribution."[1] In machine theory there must be "sensations" first of all, if upon this basis something else shall be founded as a secondary product. It would have been a superhuman achievement for von Ehrenfels to have gone so far as to sacrifice at once the machine theory and its sensations. To him his peculiar qualities remained new bits of experience simply added to the sensations, and in the school of Graz (v. Meinong, Witasek, Benussi) there was much discussion about the *fundierte*

[1] There is some suspicion that the color process, mainly as a chemical affair, may even *follow* other more physical sides of the whole dynamical event on some occasions. In an electrolyte some chemical reaction at the electrodes may begin after the distribution of current is established throughout. Similarly, we need not see colors or differences of color in order to experience definite results of dynamical self-distribution.

Inhalte (qualities founded upon the basis of sensation) as the product of intellectual faculties working upon the sensations. So, with regard to the fundamental concepts applied to the sensory field, not much was changed.

An enormous number of the new qualities discovered for psychological theory by von Ehrenfels are properties of those segregated wholes which were discussed in the last chapter. "Simple," "complicated," "regular," "harmonious," are words which *may* have a meaning when applied to a local or punctiform experience, though in most cases they refer to products of organization. But when we call something "symmetrical" this something is certainly a segregated whole. Similarly, "slender," "round," "angular," "clumsy," "graceful" are specific properties of definite wholes. And from these there is only one step to the more particular "form-qualities" given in the characteristic aspect of a circle, a triangle, a pear, an oak-tree, and so forth, all of them existing exclusively in their corresponding wholes. In German the word "gestalt" may be used as a synonym for "form," or perhaps "shape." So von Ehrenfels, taking the case of specific shape as the most important and evident among his qualities, applied the name of "gestaltqualitäten" to all of them. Therefore it will be clear that not only the different forms or shapes of objects and figures are included, but also qualities like "regular." Furthermore, we have seen that there are temporal *gestaltqualitäten* as well as spatial ones, since the definition applies to the specific properties of a melody, to its "major" or "minor" character, for instance, in the same way it does to the "angularity" of a figure. Finally, seen movement as a whole may have a *gestaltqualität* which is temporal and spatial at the same time. This is the case in the aspect of a definite form of dancing and in the characteristic movements of animals, such as "jumping" or "creeping."

At this point a general remark about terminology may be useful. For von Ehrenfels the new characteristic properties themselves were objects of outstanding importance; he was more interested in them than in those segregated parts of the field which exhibit the best examples of *gestaltqualitäten* as their properties. In the German language however—at least since the time of Goethe, and especially in his own papers on natural science—the noun "gestalt" has two meanings: besides the connotation of "shape" or "form" as a *property* of things, it has the meaning of a concrete individual and characteristic entity, existing as something detached and *having* a shape or form as one of its attributes. Following this tradition, in *gestalt theorie* the word "gestalt" means any segregated whole, and the consideration of *gestaltqualitäten* has become a more special side of the *gestaltproblem,* the prevailing idea being that the same general type of dynamical process which leads to the formation and segregation of extended wholes will also explain their specific properties.[1] Here the main stress is laid upon a characteristic type of process. This, indeed, is the most general concept of *gestalttheorie:* wherever a process dynamically distributes and regulates itself, determined by the actual situation in a whole field, this process is said to follow principles of *gestalttheorie.* In all cases of this type the process will have some characteristic which exists in an extended area only, so that a consideration of local points or local factors as such will not give us full insight into the nature of the process. From this viewpoint, even the segregation of circumscribed wholes becomes one more or less particular, though highly important, case among the various possibilities which are included in the most general idea of self-distribution and self-regulation, and in consequence the concept of *gestalt* may be applied far beyond the limits of sensory fields. According to the most general definition of *gestalt,* the processes of learning, of reproduction, of striving, of emotional attitude, of thinking, acting, and so forth, may be included as subject matter of *gestalttheorie* insofar as they do not consist of independent elements, but are determined in a situation as a whole. Quite apart from psychology the same will

[1] In the theory of F. Krueger, rather the opposite view was held originally under the influence of von Ehrenfels and Cornelius. Recently, however, the concept of *gestalt* in our meaning of the word is given more attention by Krueger.

be true of ontogenetic development, and other biological events, wherever they show the definite marks of self-distribution and self-regulation. And this extension of the term does not mean vagueness, as many seem to believe. If prior to detailed investigation the general use of concepts belonging to machine and mosaic theory has prevailed for such a long time and so widely, it cannot be forbidden to discuss the opposite principle of dynamical order and regulation, which certainly is less known among psychologists though equally well established by physical theory. By no means do we believe, however, that any problem is really solved by the application of the general principle *as such*. On the contrary, when the principle seems to apply the concrete task of research is just beginning because we want to know the manner in which each special kind of process regulates itself under the conditions of each particular case.[1]

If in the treatment even of sensory fields the real solution remains a task for the future, at least the first step may be made at once. Here, as everywhere, it will consist in the recognition of the reality and the concreteness of the problem. No one fails to see that there is a problem in visual depth as a property determined by conditions on the two retinæ, or—more generally—by stimulation on a two-dimen-

[1] Köhler, *Gestaltprobleme und Anfänge einer Gestalttheorie.* Jahresberichte über die gesamte Physiologie, herausgeg. von Rona, Berlin, 1924.

sional surface. To see the real problem in the case of "form," as a property of segregated wholes, seems to be much more difficult. The reason is the same as in the case of segregated wholes themselves (cf. pp. 175-177). When we consider retinal stimulation, for instance, our thinking operates with ready-made wholes which already have definite forms as we know them from perception. So we say innocently that "the form" of our pencil or of a circle is projected upon the retina. These words contain the experience-error. There is no factor in retinal stimulation which might pick out of the geometrical distribution of *all* local stimuli that circumscribed whole which will appear in the form of our pencil or of a circle, only after such a segregated whole has become a functional reality. On the retina we have the indifferent mosaic of millions of local stimuli, and nothing else. By arbitrary geometrical thinking, we may select and combine certain retinal spots; thus we may imaginatively impose all possible forms upon the retina, including, if we like, those of the pencil and of the circle. We must not forget, however, that this is mere play when compared with retinal reality and that the form of these objects is at this moment not more really there physiologically than that of an angel or of an Arabic letter.

Some concrete examples will show us what is meant by "concrete, real form" better than any amount of general discussion. Sometimes we look upon the map of a country the general form of which we have seen thousands of times on other maps. But what we really see remains a foreign and completely unknown figure for a long while, until finally something happens in our field of vision and the known form suddenly appears, the unknown figure disappearing completely at the same time. Excellent cases for this observation are the charts of ship captains, whereon the sea and its bordering regions are represented as land and the coast are on common maps. Now, the general contour of the land is the same on the maritime chart as it is on the usual map; the geometrical line separating land and water "is projected" upon the retina as always.[1] Notwithstanding that fact, when looking at such a map of the Mediterranean I may fail completely to see Italy; instead I see some "funny" figures (corresponding to the Adriatic and so forth) which are new to me but nevertheless "have a concrete form" in my visual field, whereas on the usual map it is the peninsula that "has a concrete form." So "to have form" as a concrete property is a peculiar feature distinguishing certain areas of our actual visual field from others which have no form. In our example, so long as the Mediterranean "has form," the area corresponding to Italy is formless and vice versa. One is tempted to say that even a form existing on the retina does

[1] Of course, even this expression is not justified. There is no "line" unified and segregated *as such* in the mosaic of retinal stimuli. There is not more than a definite series of spots, which qualitatively are stimulated in a manner different from their neighbors.

not necessarily determine a corresponding form in vision. But we have to insist upon the fact that *no* form at all is given on the retina as a functional reality, neither that of Italy nor that of the Mediterranean. There is only a certain geometrical constellation of different local stimuli. Whether the physiological process starting from the retina under these conditions will lead to a simple "line" as real

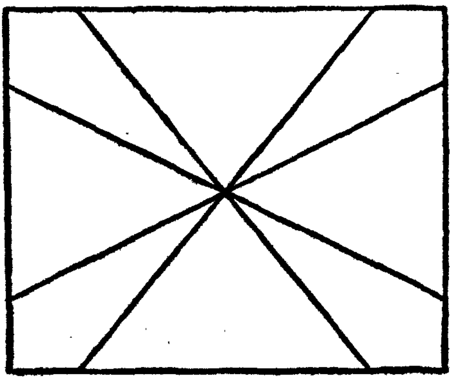

FIG. 9

form, or to the form of the peninsula with the contour as its border, or to that of the Mediterranean, the contour now being its boundary-line, whether it will produce any other or *no* form whatsoever, all this cannot be deduced from the constellation of retinal stimuli before we know by visual experience what area is segregated, and distinguished as having *real* visual form.

As a second example the following variation of Fig. 8 (or Fig. 8 itself) may be taken: Fig. 9. With exactly the same constellation of stimuli we may have two different forms, either a cross consisting of four slender arms or a cross (like that of an order) containing four large sectors. So long as we *really* have the first one, i.e., as existing in vision, the other will be absorbed in the general surroundings, which optically have *no real form* at the time. When the second form becomes a visual reality, the first disappears.[1] The observer will remark that the oblique lines are boundary-lines of the *real form* in both cases. They belong to the slender cross as *its* contours, in the first case, and to the large cross, in the second.

Simple observations of this kind were first made by Rubin,[2] who has given us a great many examples. That only definite special parts of the field have "real form" (as their most characteristic property) was shown conclusively by the fact that subjects who had seen *one* definite form at the first presentation of such an "ambiguous" pattern, did not recognize the pattern, if later on, at the second presentation, the other form happened to be seen. This second form had not existed as a visual reality when, previously, the first one was experienced; so the second one was something completely new when seen in the later presentation. Again, when one of the two crosses (Fig. 9) is "real," one does not see other forms which are quite as real

[1] Under certain abnormal conditions both forms may be seen at the same time.

[2] *Visuelle wahrgenommene Figuren*, Kopenhagen, Berlin, London, 1921.

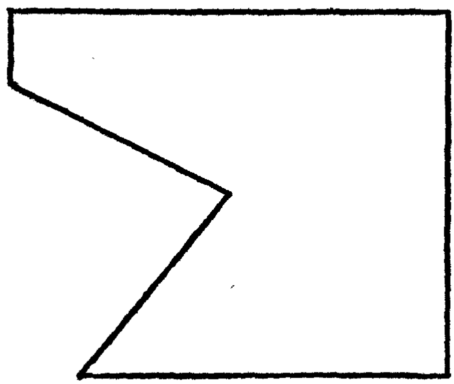

FIG. 9a

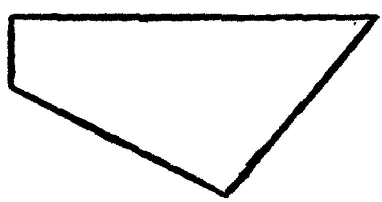

FIG. 9b

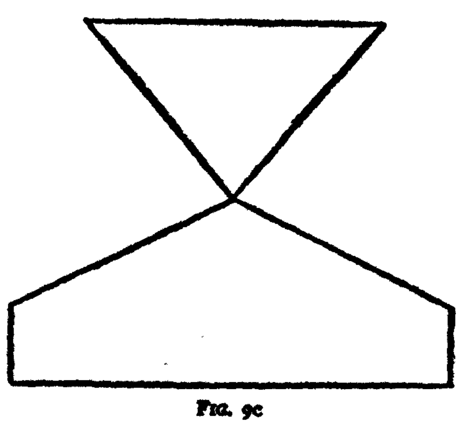

FIG. 9c

(or *unreal*) geometrically on the retina. So one does not see the form 9a or 9b or 9c.

In the case of Fig. 10 two unknown forms will be seen as a group through which a horizontal line is laid. When I tell the reader that the number 4 is before him in the field, he will undoubtedly find it; but if he is not influenced by theoretical prejudices, he will confess that the form of the 4 did not exist as

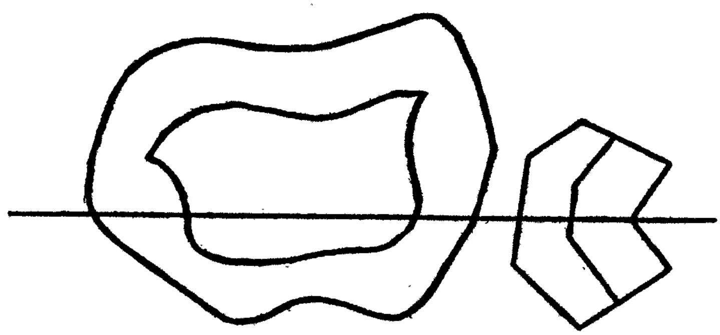

FIG. 10

a visual reality at first and that, if it began to exist later on, that meant a transformation of visual reality.

In the last example it will be obvious that the reality of a form depends upon the existence of a definite whole which, when segregated as such, *has* that real form, others, which would correspond to other wholes, being excluded from visual existence. So when we first look at Fig. 10 we have a definite organization, consisting of two unknown wholes with the horizontal line running through them. Consequently one part of the geometrical constellation of the 4 is absorbed by the whole on the left side, a second fraction by the angular whole to the right and the rest by the horizontal. And the destruction of the 4 as a whole also destroys its form. When after instruction, the observer really gets the 4 as a form in his visual field, a corresponding whole is produced at the same time. We formulate the general theorem that real form depends upon the segregation of corresponding wholes. This statement is confirmed by all observations of puzzle pictures, of camouflaged objects, and so forth. The existence of visual form presupposes the existence of wholes which, then, have a real form as their specific property. The reader will be able to convince himself that even the reality of partial forms depends upon the existence of corresponding sub-wholes.

The *objects* around us are, in most cases, very stable wholes; therefore, we regularly see them with their definite forms, excepting in those rare instances in which the wholes and their real forms are disordered by chance conditions or by intentional camouflage. This is the reason why we so easily fail to see the problem of form and why we remain satisfied by "the form given objectively in retinal projection." There is, however, no visual form in the world to which our last considerations do not apply. Wherever we have "real form" in the visual field, we must assume a specific property of functional process in that area. But the processes do not have this special property in all parts of the field. If you walk along a street between high houses on a bright day, the sky is seen above you surrounded by darker contours of houses and towers. Do you see that bright area as a form? Generally not; it has no form, it is indifferent "background," though on your retinæ there does exist a homogeneous area surrounded by contours. These contours remain borders of the houses; the houses have forms, but the sky has not. If you wish to see a certain area of the sky as a definite visual form, look at it through a little hole cut into a box which you then put over your head. If the hole is cut like the letter H you will see an area of the sky in the corresponding form as a bright figure on a black ground. And now *this ground* has no visual form.

Who wishes to judge about the problems of *gestalt* theory ought to be well acquainted with these observations and with the consequences to be deduced therefrom. Exactly as we may either have color in a definite part of our visual field, or mere brightness of a certain nuance, so a definite area may have a certain form or it may be formless, i.e., merely extended.

Since "real form" presupposes a segregated whole, the existence of "form" depends upon factors of stimulation similar to those upon which the segregation and organization of wholes depend. Again, definite relations in the total constellation of retinal stimuli are found to be decisive for the existence of real form. But no abstract consideration of all the logical relations that might possibly obtain among local stimuli would make us able to forecast where or when a specific form will be seen. As for the existence of segregated wholes, i.e., organization, certain *special* relations again are important and others indifferent; which are the important ones can only be discovered by the observation of real forms appearing under a given set of conditions.

Since for some time to come it will be impossible to observe physiologically the inner dynamics of optical processes, we cannot do more at present than draw conclusions from the properties of the visual field in comparison with given retinal constellations. In the course of this attempt we find that "form," wherever it exists, is a *supra*local property of that part of the field; so the property of the underlying process must be a supralocal phase of it. We find also that "form" is a property exclusively of segregated wholes; so, with or after the organization of these wholes, that special supralocal property of physiological process must develop which is the basis of "real form" in direct experience. As it is an attribute of detached wholes, this phase of the process exists only in definite areas, which are determined by certain relations of stimulation. These relations must be decisive *dynamically* for "form," as they are for "wholes" which have "form" as their most important visual property. Evidently, one cannot understand "form" from the viewpoint of mosaic theory; dynamical self-distribution with its functional coherence and structure of process is the only principle known at present, which may give us an explanation of "form" in the future.[1]

I shall not try to demonstrate with the same care the concrete reality of definite forms in *time*. What has been said would have to be repeated more or less in the case of melodies, of rhythms, of seen movements and so forth. The "form" of a musical *motif* begins at a definite point and ends at another; then another *motif* may follow. But there is no real form extended from the second tone of the first figure to the third of the second figure, and between the two figures there is what is called a "dead" interval (corresponding, as "empty" time, to the mere extension or ground outside a visual form). Again when in the dark room a bright spot in motion describes the path of Fig. 6, we see certain definite forms of movement, $1 \rightarrow 2$, $3 \rightarrow 4$, $5 \rightarrow 6$, for example. We do not see other forms, such as, for instance, a form corre-

[1] Once, Bühler tried to give an explanation of at least one very characteristic form, viz., the straight line. As he assumed that all retinal points which form a straight line are connected with each other in a special manner anatomically and that, therefore, the straight line is something specifically determined, his hypothesis has the character of a machine theory. I do not think that this is the way in which we may hope to solve our problem. There are a great many particularly characteristic forms besides the straight line. Shall we assume for each of them a preëxisting anatomical apparatus? or rather, a great many for each single form, since it may be projected upon many different parts of the retina? If not, "form" must be explained *dynamically* in general, and the most characteristic or regular forms must be explained by the special regularity of dynamics occurring under appropriate conditions.

sponding to a fraction of $1 \rightarrow 2$, the horizontal, and a fraction of $3 \rightarrow 4$, taken together. Real form coincides with the wholes and sub-wholes segregated in this case as in that of melodies and rhythms. In passing we may remark that the behavior of others is continually seen in *forms* of events which correspond to the natural grouping of what we see or hear of them.

As form is a property of segregated wholes, what

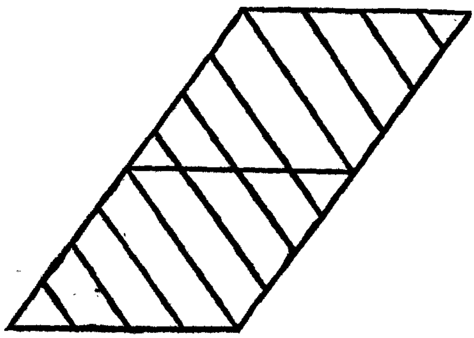

FIG. 11

has been said against the explanation "by meaning," or other indirect explanations, in the case of segregated wholes, will be more or less to the point with respect to their forms, though, probably, for most readers this is yet a rather critical issue. Therefore, the problem shall be treated once more in the following paragraphs.

1. What is the effect of our past experience of certain definite forms upon visual experience in subsequent life? Drawings like Fig. 11 and Fig. 12 con-

tain a great many geometrical lines the outline of which, when given alone, would make us see other forms than those which we see naïvely. Thus, in both of them the outline of Fig. 13 is present geomet-

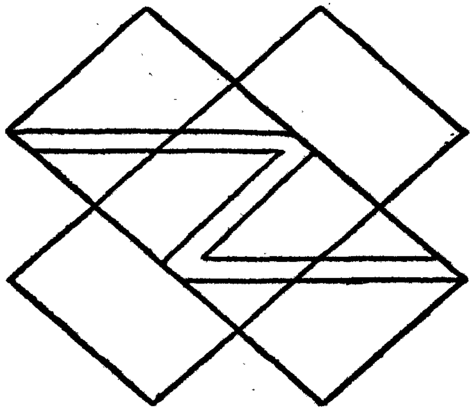

Fig. 12

rically. If now we have a large number of drawings, seen naïvely in a certain way, and besides them certain other figures geometrically contained in the first, will training or repeated experience with regard to

Fig. 13

the latter change the way we see the first, so that the learned forms become "real" when the larger drawings are shown? Gottschaldt has recently made such experiments. Since past experience is supposed to have its effects upon form independently of our knowledge about the presence of corresponding outlines, i.e., automatically, the subjects were not told to analyze or to look for the "learned" forms. The larger drawings were given them simply for description. Under these circumstances, with three previous exposures of the smaller forms, the training had no effect upon subsequent perception of form in more than 90 per cent. of the cases. When the number of previous exposures of the smaller forms was increased to 520 with new subjects, the result was still the same, the drawings being seen unchanged by the training in 95 per cent. of the trials. Not even the few cases, which showed a positive result, can be explained by the mere fact of previous training, because all those subjects, who occasionally saw the "learned" forms in the larger drawings, had some suspicion about the aim of the experiment and asked the experimenter whether they should look for the "learned" forms in the larger drawings. Though they were not instructed to do so, of course they went into the test with an attitude of definite expectation. So the few positive results do not prove an automatic after-effect of past experience upon forms seen subsequently, since, apart from training as such, there was this other factor of expectation.

Gottschaldt's larger drawings were "difficult," i.e., of a highly stable organization. In some of them, in spite of strong efforts, I cannot really *see* the smaller form, even when I know *about* its geometrical presence and position. However, if any one criticizes this "difficulty," by his own argument he recognizes the reality of visual form as something stronger in such cases than a very large amount of previous experience. Furthermore, the larger drawing (Fig. 12) cannot be said to have its stable organization in consequence of much experience in previous life outside the laboratory. What we naïvely see there is not better known from previous experience than the small figure 13. After these results, whoever defends the automatic influence of past experience upon our seeing definite forms, will have incumbent upon him the task of supporting his theory by other experiments. If such an influence exists, it must be restricted to rather special cases.

2. Where "form" *exists* originally, it acquires a meaning very easily. But here a whole with its form is given first and then the meaning "creeps into it." That meaning automatically produces a form where beforehand there is none, has not been shown experimentally in a single case, as far as I know. It may be that in a very *unstable* constellation, in which a certain form can be seen or organized, past experience of such a form will tend to produce it really, whereas without that previous experience, this would not happen. Even in this case, however, we should still have to explain what factors produced that form in previous life. It is only assumed, then, that condi-

tions were more favorable, and the question remains open whether they were not favorable for its originating directly. In any case, even granting that previous experience of a certain form favors its appearance in the future, we ought to realize that such an occurrence will be limited to definite cases, namely those in which the actual constellation does not tend decidedly toward other more stable wholes and forms. The number 4, for instance, is certainly a well-known form; but when Fig. 10 is shown to hundreds of people without any special instruction or warning, only a few of them will see the 4 and mention it in their description of the figure. If any one protests that we have never seen the 4 in such an environment before, he misses the point, because no explanation in terms of previous experience is needed when 4 is given upon a homogeneous ground, as it usually is. If past experience has an influence, it is exactly in such a case as that of Fig. 10, in which its influence ought to be effective. And after all, it is not the "unusual" environment as such which prevents our seeing the 4 immediately. In the following figure (14) the 4 *is* seen at once, though in my judgment the environment is not less unusual than in the first case. Why, then, is it seen now? Because the relation between the added lines and the geometrical parts of the 4 is not such that these parts are absorbed in the formation of other wholes. But this is a principle of original organization. So we may say that any influence of past experience upon the organization of forms in the future depends upon the degree and the stability of original organization in the actual

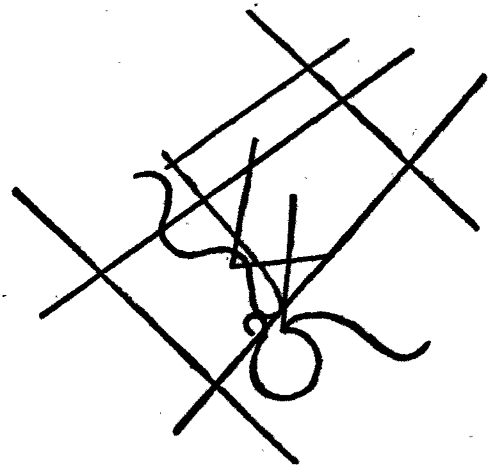

FIG. 14

case. I shall give a few more examples which show the absorption or destruction of a well-known form by actual organization into other less known forms.

FIG. 15

Look at Fig. 15 before you read the next lines. No one would use the name of letter E in describing it as long as he is a naïve observer, though the well-known E is present geometrically, and the form really seen is less known. Fig. 16 may be beheld as an ornament for months before one first finds two H's in it. Similarly the letter K is hidden in Fig. 17. If the reader is already a little sophisticated, he would be wise to show these figures to some of his more naïve friends, for five seconds perhaps, asking them only to give a spontaneous description of them. I

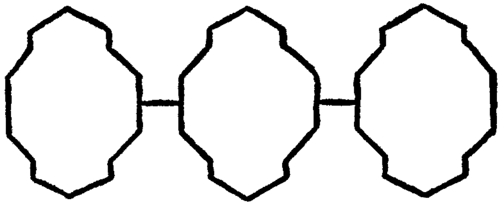

FIG. 16

do not think the enormous experience we all have with respect to these letters will produce any considerable effect upon the result, as long as the subjects do not begin to look for hidden forms. If they do, of course, the result will be produced by actual

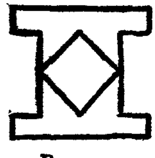

FIG. 17

attitude apart from previous experience. When we give our subjects a hint about the camouflaged form, we create in them what the physicist might call a *"special vector,"* favoring that concrete form. This vector does not exist when, merely informed by "past experience," but in an indifferent attitude, our subjects look unaided at some of the unknown figures. Without it, the hidden forms do not appear.

3. Again, some psychologists will go so far as to derive all form in the visual field from tactual or motor experience. Our answer in this case is once more simple enough: Real form is a concrete property of certain things. If its history is to be traced to other than visual experiences, we must have concrete real form in those other regions in which it has its origin. Nothing can be imported from other fields into vision without having originally occurred in these other fields. So the whole problem would only be shifted from one sensory field to another, and the explanation would have to assume original forms in the latter. This reasoning applies also to our sensations of eye-movements. If visual form shall be derived from them, real form must be a property of some of these kinesthetic experiences. Therefore, such an hypothesis does not seem to be a scientific gain, and we may as well acknowledge the problem of visual form *as such*.

4. I find a decisive argument against explanations of form in terms of past experience, in the following consideration. No doubt, parts of the visual field can reproduce past experiences associated with them in previous life. But when we ask what factors are the main reproductive properties in these cases, we see that in 99 out of 100 cases *reproduction depends upon the form of a segregated whole in the field.* Take

the concrete real form away, so that reproduction can be aroused by only the color or the place or, perhaps, the size of an area, and you will realize that for the most part, without form, visual realities are not specific enough to reproduce definite experiences. When we talk about the automatic influence of past experience upon actual vision, we are tempted to extend this idea to the creation of definite "form" by reproduction. But then we forget that there is not much reproduction without form itself as a reproductive factor, and that, from the viewpoint of such an hypothesis, form *as such* must be strictly excluded from the sensory field and, therefore, from the reproductive features of the situation, supposedly being present in the field *only by way of* the reproduction of past experience. The curious result is that we presuppose "form," erroneously and unawares, in order to get form reproduced from past experience! A naïve observer does not see the 4 in our example, although it is such a well-known figure. Why? Past experience cannot work if there is no specific factor present which might reproduce that definite part of past experience under the circumstances of the case. In our example the ever so well-known 4 will not be reproduced, as, in other examples, countless other well-known forms are not reproduced either, there being nothing in the actual optical processes which might effect that definite reproduction. I suspect that a great many explanations in terms of meaning and past experience fail completely to realize this side of the question. It is easy to say: This, of course, is so because in earlier life certain things have happened. But the time has come for more concrete thinking in this field. Reproduction does play a highly important rôle in mental life and in behavior. It can play it only if the sensory world is sufficiently endowed with concrete properties in those areas which are said to reproduce our previous experience. If it is not, because we regard it as a mosaic of sensations, by what factors can a definite reproduction be determined? "Transposing" shows that a form will remain the same independently of the color, the place and the size of its area. If, now, a definite form is said to appear in the field by reproduction, I do not see what characteristic of the mosaic can possibly determine the right reproduction, since we cannot rely upon color, place and size for it.

In order to show that "form" can never be explained by the existence of "sensations," von Ehrenfels laid great stress upon this very fact—mentioned previously by Mach—that a form can be "transposed," i.e., will remain the same as a visual property, after its brightness, color, size and place are changed. Of course, when shifted too far toward the periphery of the field, a form will change its character more or less, but, excepting this case, the possibilities of transposing a form are very numerous. Again in this respect temporal form behaves like spatial form, since a melody may be rendered in different keys and still remain the same, so far as its musical form is concerned. Indeed, nothing can show better than these facts that the specific supralocal organization of the processes underlying form as a sensory experience must be a physiological reality, certainly as much as are the processes underlying chroma as a quality of sensory experience. Since, in the case of vision, we can change not only chroma, but brightness, localization and size of a form, without disturbing the form itself, the usual categories of quality, intensity, position, extension do not help us when real form is to be explained. There is only one thing we cannot change too much without influencing form itself, viz., that special set of relations of stimulation which seem to be decisive for the segregation of a definite whole and for the specific dynamical structure underlying form.[1] We do not normally *experience* these relations in the sensory field. But in the determination of self-distribution of process, in the segregation and "formation" of wholes, they must become almost all-important somewhere between the retinæ and the psychophysical field.

[1] We may remark however that, for adults, one more condition must be fulfilled if we wish to transpose, without altering, specific form: most forms show a changed aspect *as* forms when they are turned around, for instance, upside down. This fact reveals a curious anisotropy of the visual field, which does not seem to exist in early childhood (Cf. W. Stern, *Zschr. f. angew. Psychol.*, 1909, and F. Oetjen, *Zschr. f. Psychol.* 71, 1915). On the other hand, Wertheimer has laid stress upon the point that, for a given form, not all relations of stimulation are equally important. Some of them may be changed considerably without any remarkable effect upon the actual form.

Up to the present time there has been a tendency to regard the remarkable properties of wholes, especially the possibility of transposing their translocal properties, as the achievement of "higher" processes. From the viewpoint of *gestalt* theory sensory organization is as natural and primitive a fact as any other side of sensory dynamics. If, for von Ehrenfels and the *Grazer Schule, Gestaltqualitäten* and form are treated as a superstructure built upon the foundation of sensations, this assumption would fail to account for the evident rôle of *gestalt* in such biological processes as ontogenesis. In psychology, too, the influence of *gestalt* has been demonstrated by Hertz (cf. Chapter V) in very primitive behavior, in which no one would assume the operation of "higher" processes. Lashley seems to have been the first to find that an animal, trained to react positively to one of two "stimuli," will shift his reaction spontaneously when he finds himself before two other stimuli of the same class, the new reaction being directed to that stimulus which plays the same rôle in the new pair as a whole, which another specific stimulus played in the pair used during the learning process. I wonder why these experiments failed to exert their due share of influence upon current theoretical formulations. Without knowing Lashley's work I have repeated the same experiments on apes and chicks, with special care to exclude all possibilities of indirect explanation. There is no doubt, now, that a chick, trained with two grays, I and II (II being darker than I), always to choose II, will, after a while, when II and the new (darker) gray III are given, in the majority of the trials not choose II but the unknown nuance III. The same experiments were performed on apes with size, and also with different hues of color. Several investigators have been able to confirm these experiments. We may conclude that animals react to such pairs as to wholes, either side of which has a definite character depending upon its "position" in the whole. So II is "the dark side" of the first pair; then, in the new pair, III assumes this rôle, and since the animal has learned to choose the dark side of the pair rather than a more or less definite gray, it tends to avoid the specific gray of the learning period and to choose the new gray after the transposition. Again it does not matter at all in such a case, whether or not we assume that the chick has direct experience. The difference between choice depending upon a more or less definite intensity of light and reaction depending upon one definite side of a structure, is the same for either assumption. Once more the formula of "stimulus and response" becomes altogether misleading, since it neglects the fact that between the stimulus and reaction there is the organization of definite wholes with specific properties. upon which the reaction so obviously depends.

In order to prove that dynamical self-distribution will explain "transposition," we have to show that "transposition" is possible in self-balanced physical systems. Nothing can be easier! First of all, if under certain conditions all forces of a dynamical distribution are in equilibrium with regard to each other, it is evident that the equilibrium is not disturbed if the intensity of all those forces decreases or increases in the same proportion. *All* forces being reduced to one-half of their first value or increased to three times that amount, they will balance each other as before. The supralocal dynamical structure is largely independent of absolute intensities. Now let the dynamical structure be the self-distribution of current in an electrolyte, depending upon the total form of the electrolytic conductor. Again the intensity of current has no influence upon its distribution. Also, if instead of ions like Na and Cl, K and Br or any others carry the electric charges, the distribution of current will not be changed. Or, take as an example the electromotive phenomena developed when two solutions (I and II) of different ionic concentration are in contact. Those phenomena, existing only in the pair of solutions as a whole, depend upon the *relation* of the ionic concentrations, the absolute concentrations being indifferent. If, in a definite case, the solution II with a concentration of $\frac{1}{20}$ n is the electropositive side of the pair, as against I with a concentration, say, of $\frac{1}{4}$ n, in a new pair with the concentrations $\frac{1}{20}$ n (II) and $\frac{1}{100}$ n (III) the new solution III becomes the electropositive side.

To be "the electropositive side" of such a physical system is no less a *gestalt property* in a definite electrochemical whole than to be "the dark side" is a *gestalt property* in a sensory pair.

In some respects form is the most important property which a whole may have; the introduction of it as a specific reality will help us to understand certain of the properties which occur only in wholes. In Figs. 8 and 9 we observed a change of form, now one cross or star being present as a form, and then the other. But something else changes at the same time. When we see the slender cross, the area of this cross has a character of solidity and coherence; we may even say that this slender cross has the substantiality of a "thing," whereas the environment appears as comparatively "empty" and "loose." Exactly the contrary is true when the other cross dominates the field; the large cross becomes "solid" and "substantial," whereas the narrow angles have become "loose" and "empty," like the environment. Because an area becomes "solid" when appearing as "form" or "figure," Rubin, who was the first to describe the difference, has called the "solid" quality the "figure" character, whereas to the "looseness" of the environment he gave the name of "ground" character. This term "ground," or "background," is the more appropriate since we find the "figure" protruding a little, as if in three-dimensional space, and the environment localized behind it, so that the solid "figure" appears as something apart, behind which the "ground" seems to extend without interruption like a simple homogeneous plane. The sky above the houses (cf. p. 202) has the character of ground.

I do not think we ever find that character of "solidity" where we do not have a segregated whole. That character, then, is a typical property of segregated wholes and belongs to von Ehrenfels' qualities. Some psychologists will try to derive this character from touch or other previous experiences which we have had with things, in the practical meaning of the word. We find that all visual things, which as such are clearly perceived in the field of vision, have the same character of "solidity." But as it appears as a visible quality, it may be such a quality *originally;* it may even belong, independently of other experiences, to the primary constituents of the meaning of the term "thing" in common life. Whether the empiristic explanation is justified or whether this quality is founded directly upon the specific dynamics of seen form, will be decided by further research. As yet it has been shown that, functionally, "figure" and "ground" behave differently. Constancy of color has been shown to be more striking for "figure" than for "ground" (Rubin); the limen for a patch of color projected upon the "figure" was found to be higher than upon "ground" of the same objective intensity (Gelb and Granit); after-images are more vivid when observed upon a "figure" than they are upon "ground" (Frank).

After all these considerations the reader will be prepared to accept some statements as concrete which, in consequence of their more general character, he, without that preparation, might be tempted to despise as "mere philosophy." If in our experiment on animals "the dark side" is something which, as such, exists only in a definite whole, the same thing is implied in a great many terms which we are continually using as trivial words in common life. We do not realize generally that all their meanings presuppose definite wholes in which something plays that rôle to which the words apply, as "the dark side" applies to the rôle one gray plays in a pair of grays. I give the following few examples, selected from a large store: The German "Rand" (English "brink" or "edge") is such a word; again "Anfang" (beginning), "Ende" and "Schluss" ("end" and "close"), "Stück" and "Teil" ("piece" and "part"), "Rest" ("rest" or "remnant"); and even "Loch" ("hole") and "Störung" ("disturbance"), which presuppose a definite experienced whole, with respect to which an area may appear as a "hole," or some factor as a "disturbance." Intentionally I do not restrict my examples to those cases in which the words apply to sensory facts. There is not the slightest reason why I should do so.

And the reader will easily understand that talking about a "disturbing" factor in the case of "thinking" presupposes a definite dynamical whole, as it does in sensory experience. There is no meaning in the word without it. Whoever is slightly acquainted with musical theory will recognize at once that a tone cannot have the character of the "tonic" without belonging to a larger musical whole, in which it plays a definite rôle; of course, the same is true of the leading-tone, which has its strong dynamical properties in the definite key of a sequence of tones, and not independently.

Obviously we must have similar cases in the world of adjectives and verbs. Indeed, "hohl" ("hollow") and "offen" ("open"), "complete" and "incomplete" belong in the same group, as terms referring necessarily to specific wholes. Finally, in the realm of activities and events we might give a long list in which there ought to be included: "starting" or "beginning," "ending" and "finishing" or "closing," "desisting" and "interrupting," also "proceeding" and "continuing," even "deviating," "bending," "retarding" and so forth. If we try to find out what we mean when we use terms like "hesitating" or "deviating" with reference to the activities of others or to heard music or to our own stream of thought, we shall find that their meaning depends upon extended dynamical structures, the change of which is described by those words. They may be applied to sensory experience, but their essential meaning remains the same in other fields of experience, because the principal aspects of *gestalt* are found in all of them. To say that they are imported into these other fields from the region of sensory experience, in a merely analogical way, would be no more than another vicious circle of the kind described above (cf. pp. 212-214).

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VII

Behavior

IT will be difficult to understand this and the next chapters unless we first solve a problem which presents serious difficulties to some psychologists.

When I referred to "objective experience" in an earlier discussion I laid great stress upon the fact that things, their movements and changes, are given as *outside* or *before* us. At the same time "objective experience" is regarded as depending upon processes in a certain field of the brain. How then can it appear before us? About the facts there can be no doubt. Under certain conditions a sound may be localized in my head, it is true; but that tree over there is seen as something far off and the window, though much nearer, is still *outside,* beyond any possibility of doubt. Functionally, however, their existence, as is the case of all the other surrounding objects, is a matter of processes in my brain, and therefore *in* me. So much can be proved by the simplest physiological considerations.

Let us attack this problem from the physiological side at once. For simplicity's sake we shall proceed at first as though the visual field were the only objective experience we have.

One thing is immediately obvious. Though we usually have many objects before us, their totality appears as ordered in one visual space, so that any one of them has definite spatial relations to all the others. This statement is rather superficial, because it neglects the actual grouping and "belonging together" of some objects rather than others; but for our present purposes it will be sufficient. The pencil is nearer to that book than to the lamp, the knife is between the book and the fountain-pen, and so forth.

As all properties of the field depend upon physiological processes in the brain or certain of their properties, so the relative position of experienced objects will depend upon a definite order of the processes which are their physiological basis; the mere geometrical localization of the processes in the brain cannot be the correlate of seen spatial order. I take it for granted that whatever is experienced has a *dynamical* basis physiologically, i.e., depends upon physical states and events. Process-in-extension, coherent functionally and self-maintaining dynamically, is supposed to contain those dynamical relations between "segregated wholes," which, in experience, appear as positions of objects in mutual relation and order. Of course, these dynamical relations, underlying experienced spatial order, exist in an aggregate of cells and fibers, which are extended in physical space. But only the dynamical relations count for our problem, not the geometrical distances and areas as such, through which that dynamical order extends. Still we may safely assume not only that, where two objects touch each other in visual experience, the *dynamical contact* of the corresponding processes is immediate and direct, but that the two processes take place in two *neighboring areas* of the brain. Again, if two objects are experienced as separated from each other in visual space, some process, *not* corresponding to the objects, will mediate *dynamically* between them; and at the same time the area of the first will not be in geometrical contact with the area of the second. We shall not go further. The *geometrical* distribution of processes in the brain will *not* be an adequate picture of visual order in all respects. Only with respect to those crudest properties of visual space, neighborhood and distance in general, will the underlying dynamical order of processes be extended homologously in brain-space.[1] But no more need be assumed in order to picture in imagination what follows, and to discuss our problem in simple terms. We must not forget, however, that, if in the next paragraph relative localization in visual space is connected with relative position of processes in the brain, this is meant as an abbreviation for the *dynamical* relations of process-in-extension.

In visual experience the pencil there is external to

[1] *Die physischen Gestalten in Ruhe und im stationären Zustand*, pp. 230, 231.

that book and at a certain distance from it. We know now, at least in a general scheme, what is the physiological basis of this separation. Our next task will consist in applying that scheme consistently. My hand is in the same visual field as an experienced object. Evidently, as this new segregated whole is external to the pencil and the book in visual experience, the process, corresponding to it in the brain, must be external to the processes corresponding to the pencil and to the book, dynamically and, as we have seen, geometrically. There is not the slightest reason why the hand, as a visual object, should be treated in a manner different from the pencil or the book, or the spatial relation between the hand and pencil or the book different from the spatial relation between the latter. Beside my hand I have other parts of myself in the visual field: my arm, for instance, very often my feet, my chest and, though in peripheral vision, the tip of my nose. They are visual experiences, exactly as are the pencil and the book. Therefore, in my brain as a *physical* system there must be processes corresponding to these experienced bodily members, as there are processes underlying those objective experiences which I do not reckon among the parts of what I call my body. And the processes underlying the seen book, the seen pencil and all the other surrounding seen objects will be external to the processes which underlie the experiences of arm, feet, chest and nose, for the same reason that the processes corresponding to those seen objects are external to each other.

Here we have, in principle at least, the solution of our problem. To some extent my body, to which common speech frequently refers as "I," is a group of visual experiences just as other things are. As these are seen external to each other, so "I" am experienced external to all of them in vision. If I find no difficulty in their appearing separately in different parts of the field, and if their mutual localization can be understood by the dynamo-geometrical relations of the processes underlying them, I am simply obliged to apply the same idea to myself as a visual experience in the same field and to the processes underlying this experience. No new hypothesis is needed to explain why "I" am external to those objects and they external to me. If there were a paradox in their relative localization with regard to "me," the same paradox ought to be found in their mutual localization. I have only to become aware of the fact that, as a visual experience, this "I" depends upon processes in a definite circumscribed part of my brain as a physical system, no less than do any other objects in the field.

Following this consideration, it becomes an almost impossible idea that things should be experienced as being "in me." The pencil might be experienced *in* the book just as well! Everybody is accustomed to discriminate sharply between the thing as a *physi-*

cal object and as the *experienced whole,* corresponding to it, which appears in the visual field. If we could only accustom ourselves also to make the same distinction as radically and consistently in the case of our body! In one meaning this body is a physical organism, certain processes of which underlie *all* experience. At the same time "my body" or "myself" is a particular one among all other sensory experiences, and, as such, it is also represented by definite particular processes in a definite part of the brain at a given moment. Dynamo-geometrical relations in the brain-field mean spatial relations in visual experience. In this respect, other visual things are evidently outside or external to the special thing called "my body." But, you say, all these processes are *in* the brain, i.e., *in me,* after all! That does not matter for our problem, because experience depends upon the processes of a definite field, and experienced space depends upon the totality of dynamo-geometrical relations *in* it; the only meaning which "in" and "outside" have in experience is determined by this dynamical context, and to the *physical* place which this context has in the physical world, in the physical body, and more particularly in the physical skull, *there does not correspond any experience at all.* All the possible spatial relations we can have in sensory experience are restricted to the inner dynamical properties and relations of that field; the anatomical relation between this field and its surroundings, which do not contain "psychophysical" processes, is an affair of the physical world which does not occur in my sensory experience. Therefore, "things" are "before" and "around" me, as far as experience is concerned. If somebody expects that "things" or "I" or both should be localized in my brain, he does not see that in the first half of this sentence he is talking about direct experiences whereas in the second half "my brain" is a concept referring to physical existence. So he confuses experienced space with the space of the physicist, which no one experiences directly, and he expects to see certain particular parts of *experienced* space localized in relation to parts of *physical* space, which is altogether impossible.

This is also the reason why every one is so astonished when he is first told that things, colors, etc., which he experiences as being external and distant, depend upon events occurring in himself! Of course, this is correct if meant physiologically, so that "himself" is the physiological organism, not experienced directly. It is far from correct and evident for some one who does not discriminate between the physical organism and "himself" as one particular whole in experience, because for the most part, distant things and colors do not appear as depending upon that particular experience, "himself," at all. Why should they? In experience a tree depends upon "myself" just as much or as little as in the brain the particular and local process corresponding to "myself" determines that corresponding to the tree. Such an influence will sometimes occur and will then be experienced, too, but in general the "tree" and "I" will not depend upon each other more than any other segregated wholes depend upon each other when, dynamically, they are distant from each other.

Perhaps all this is too well known to be discussed here. But four years ago a distinguished psychiatrist formulated this as the most difficult problem to be found in all the relations of "mind" and "body": that things appear as being outside of us, whereas we should expect them to be in our interior!

As yet we have taken "ourselves" and "things" exclusively as visual experiences. But the situation remains the same if we consider other experiences as well. Things, their properties, events and so on, may be experienced by touch instead of visually; things are warm or cold, they smell, are heavy, audible and so forth. But all these experiences are somehow localized in experienced space, either in a well-defined or in a more confused manner. All of them appear as localized more or less in relation to visual experiences. So a voice is heard as outside the window; this room, given visually, seems to have or contain the smell of a cigarette, and the cold surface of the safety-razor in my hand is felt where the thing is seen. Two different explanations may be given for the fact that all sensory experiences appear in one and the same space.[1] *Either* the reason is the same as for the special case of binocular vision in which, notwithstanding the duality of peripheral sense organs, one single visual space contains all visual experience. What has occurred here may have happened with respect to *all* sensory experiences as depending upon a large number of different sense organs: in phylogenesis, and also in early individual life, for dynamical reasons which we do not have to discuss here, the distribution of sensory processes in the brain may have developed in such a direction that, now, all of them occur in one psychophysical field and that, therefore, corresponding to their dynamo-geometrical relations, all sensory experience appears in one experienced space. *Or* the relative localization of experiences furnished by different sense organs may have been *learned* by all of us in early childhood, so that in consequence of that learning a sound, for instance, would seem to issue from a place in the same space in which we see objects. In any case, whether we accept the more "nativistic" or the "empiristic" explanation, all sensory experiences are now given to us in one space, the same space in which we have visual objects around us and, in one region of which, parts of "ourselves." Some of the non-visual experiences are localized outside, as

[1] Obviously it does not matter in this connection whether or not this localization, for instance of sounds in relation to visual objects, be *correct* in all cases. First, if it is *not* correct, the very fact that we are able to state and measure the error is in itself a proof that the two appear in the same space.

sounds are almost universally, and also weights; but other non-visual experiences appear *inside* that region of the visual field which we call ourselves, as, for instance, thermal experience and all kinesthesis. In the interior of this region we find eventually a great many experiences the origin of which may be a matter of much discussion, but which certainly are localized there somewhat diffusedly, as our feeling "tired," "nervous," "healthy," even "happy" and so forth, i.e., the more or less "subjective" experiences. From what we have said about the separation of visual experiences as belonging partially to ourselves and partially to our surroundings, it will be obvious that the same thing is true of all other experiences except certain particular experiences which are always localized in our interior. All this is not more astounding than is the external localization of visual things outside our body, itself a particular visual thing.[1]

After these preliminary remarks we are prepared for the treatment of an old problem: How does it come to pass that we ascribe to others experiences similar to those we have ourselves? We continually seem to do that, not only in general, but in particular; we attribute to others definite particular experiences at a definite time. To begin with, I shall not

[1] The boundary between "ourselves" and objective experience around us is not altogether sharp and constant. Experiences of touch, for instance, are rather variable in this respect. Sometimes they are felt as things and their properties; but they *may* also be experienced as "subjective."

discuss the question, whether such attribution can be legitimated in psychology as a science. But in common life something like sympathetic projection and attribution seems to be a most frequent occurrence, and may be regarded as fundamental for social psychology. What is its nature, and what are its modes of occurrence? A certain paradox is connected with the problem: sometimes other people seem to judge my subjective experience more soundly from without than I am able to do it myself "from within." What shall I say, for instance, in order to describe to others what I feel as a lack of inner determination or as hesitation in a certain situation? But others say that they "see" that hesitation most clearly on my face, and I am inclined to believe them, since I know that characteristic expression very well from my observation of others. I think it was Nietzsche who occasionally said that somehow the "you" is earlier than the "I." This seems to apply most of all to our knowledge of "character" and "personality," since it is extremely difficult to get a definite picture of our own character from our subjective experiences, whereas the main traits of the character of others may sometimes be strikingly apparent in their attitude.

I do not think that the language of others is our main or most trustworthy cue, in the sense that the content of it might be taken as a description of their experience. People do not talk sincerely about their subjective experiences, and we ascribe to them pomposity or modesty, friendliness or coldness, without their telling us a single word about such traits. In a foreign country, we appreciate to a great extent that others are "provocative" or "kind," though we may be absolutely unable to understand their language. Where we do understand their words, their *manner* of talking is often a better cue, and we trust it more than the content of their talk. Also, a certain kind of silence can occasionally tell us more about others than any number of words could reveal in the same situation. Furthermore, the behavior of apes, for instance, shows the unprejudiced observer that they usually "understand" each other very well, though they do not use language, in the usual meaning of that word. For these reasons, our discussion will deal with a fundamental aspect of our problem, even if we exclude language from it as communication by the meaning of words and sentences.

The answer which philosophers have given to the question raised, is very well known. Since I cannot directly perceive the experiences of another, the only evidence available is his body or, more particularly, the changes and events which I observe as occurring on its surface, or issuing therefrom. These, however, have nothing to do with the non-apparent direct experiences of other persons, apart from that simple rule of concomitant variation that usually accompanying definite bodily events, observable to others, are definite direct experiences which, of course, are unobservable. Originally I learned this rule by observing myself alone, because I have observed that, in me, definite experiences are accompanied by definite movements, and so on, in *my* body. This observation, made millions of times very early in my life, induces me to draw an inference by analogy wherever afterwards I observe the same bodily events in others. From the fact that the Earth is inhabited by organisms the inference is sometimes drawn that there also are organisms on another planet, similar to the Earth, as Mars is, for instance. In the same manner we are said to infer that behind the several bodily reactions or "expressions," which we observe in others, there are occurring unobservably definite experiences similar to those we have experienced in ourselves when we have observed ourselves undergoing the same bodily changes. Evidently such an inference cannot be worth very much, as the parallel astronomical case shows. Furthermore, the whole theory seems to be an arbitrary construction, since no one explicitly draws such inferences by analogy in common life, though he may "understand" his fellow men to a considerable degree.

Therefore, a modification of that hypothesis is proposed by psychologists. The basis of it remains the same: In millions of cases we have had experiences when we could observe bodily reactions in ourselves at the same time. The constant repetition of these simultaneous occurrences has built up associations between them, so that in the future the mere observation of certain bodily events will forcefully reproduce the idea of the associated definite experiences. But we may go further: instead of "free" ideas we very often get an "assimilation" of the reproduced material by the reproducing material. So the symbol + looks like "adding," a coffin appears imbued with the gloominess of death, and the flag with the special value of our country. In the same way, by an enormous force of association, the bodily changes of others appear as imbued with those experiences which have so repeatedly accompanied those changes in ourselves. This is the reason why we seem to *see* friendliness in other people's faces or to *hear* anger in a certain animal cry.

Evidently we must deal once more with a special application of the meaning theory. I do not believe it to be in principle a solution of the problem. First of all, from its viewpoint we should be utterly unable to understand any behavior in others except that which we know because of its extremely frequent occurrence in ourselves. But this logical consequence of the theory is not quite corroborated by the facts, for sometimes I understand others as being highly different from me. The characteristic manliness of Douglas Fairbanks is something which impresses me very much, though unfortunately I shall never be able to achieve it myself. On the other hand, a certain kind of envy, mean and hideous to the highest degree, is plainly visible to me in certain definite facial expressions of others. But I am happy to state that it does not occur among my own subjective experiences. Again, some psychologists are right in protesting that the theory does not work in the most important of all cases, which is that of facial expression. We do see changes in the "expression" of other people's faces; but we usually do not see our own face. No one usually looks into the mirror when he is angry. We may "feel" some contractions of our face when we are angry, but since the angry face of others is given to us in *visual* experience, there does not seem to be a reproducing factor here which might have been associated with our subjective experiences in previous life. Finally, a chimpanzee reacts very quickly and adequately to the friendly or the angry attitude of another chimpanzee. Shall we assume that he projects into the other's face the reproduction of his own subjective experiences? This seems to be a rather bold assumption!

Let us consider for a moment what is implicitly presupposed in this hypothesis, as in that of the "inference by analogy." Why should our "understanding" of others be such an indirect procedure? Obviously, because we are convinced that "the mental life" of others is something radically different from and even incomparable with the bodily events which we can observe in their behavior. Nothing but an external correlation seems to exist, and to be possible here, between the facts of two different worlds. If that is so, we should expect that, in spite of assimilation, it must be extremely easy to analyze the product. Where things are combined, the natures of which are so different as to make them incomparable, the slightest effort toward analysis should separate them *at once*. But if in a friendly-looking face we try to separate the mere bodily configuration and the friendliness, we find the task rather difficult, as long as we look at the whole face and do not analyze the face itself as a source of colored spots.

This makes us wonder for a moment why philosophers and some psychologists should suppose that the facts of behavior, which we can observe in others, should be *toto genere* different from the experiences which these others may have. But the reason is obvious enough. The "stuff" and the "events" which occur in the "bodily" world, on the one hand, and in the "conscious" world, on the other, are believed to have incomparable properties since Descartes. This is philosophy. And we may perceive at once that, in this connection at least, it is bad philosophy. For this reason: in the introduction to this chapter we saw that the noun "body" is dangerously ambiguous. It may either mean a physical organism which, as such, cannot be experienced directly any more than the physical structure of an atom, or it may mean an experienced "thing," as a segregated visual whole.

Now, if we talk about the "bodily events" and so forth, as the only material we can have and observe in order to understand others, the meaning of the word "bodily" can refer only to the experienced thing and its changes as sensory experiences, since the physical body of another does not occur *as such* in any part of our direct experience. It is as remote from and incomparable with our sensory experience as is the "consciousness" of others. Therefore, when we discuss how we "understand" other people, we have to decide first of all, whether or not our sensory experience of others as *experienced* "bodies" contains a more direct cue for that understanding. Of course, our sensory experience of others depends ultimately upon stimulation issuing from their physical bodies. But in the fifth chapter we found that our sensory experience will sometimes give us more valuable hints about the realities in which we are interested than the physical events themselves, which constitute the stimuli for that experience, are able to give. So, though in the genetic series our sensory experience of others is more remote from their "mental life" than are the physical events on the surface of their bodies, we may find something valuable in the former which we cannot find in the latter. The most interesting question is whether our sensory experience of the behavior of others can give us a hint about those events in them which in ourselves we call *subjective* experiences. We would regard it as such a hint if we could discover a similarity between our sensory experience of the behavior of others and those subjective experiences. Suppose that there were such a similarity, we could then dispense with the indirect approach through inferences by analogy, or through association and assimilation, since theory has approached the *subjective* experience of others indirectly only because it had assumed as a prior dogma that there could not be any similarity at all between the events of the "bodily world" and of "mental life."

I will proceed cautiously. In its efforts to assort experiences into separate classes, psychology has introduced certain rigid distinctions and barriers among the several classes which, first of all, we shall try to remove.

One of these barriers, built by Helmholtz, is that between the qualities of the different senses, as though they were incomparable. I contend that they are comparable in more than one case. "Brightness," for instance, is an attribute of acoustic as well as of visual experiences. We may go a little farther and say that in the "coolness" of an object which we touch there is a certain affinity to brightness, whereas in "warmth" there is something similar to dark nuances. In the preceding chapter I mentioned the fact that the German word "rauh" is used for certain acoustic experiences as well as for a definite character of touched surfaces. Again, the German poet Morgenstern states that

Die Möwen sehen alle aus, als ob sie Emma hiessen.
(All seagulls look as though their name were Emma).

To the German at least, because of his pronunciation of the name, its sound seems to contain something similar to the aspect of the birds. But we may construct another example. If, looking at these two figures (18 and 19), the reader is asked to choose which he would rather call "takete" or "baluma," he will probably be able to decide with ease.[1]

I take it for granted, then, that there are some similarities between the experiences we have through different sense organs. In passing we may remark that in primitive languages one finds much evidence for assuming that the names of things and events often originate according to this similarity between their properties in vision or touch, and certain sounds or acoustical wholes.[2] In modern languages, it is true, most of these names have been lost.

For our main problem it is interesting to know what names are given to subjective experiences. First of all, most of us will agree with the statement that, if peripheral vision is "confused" in comparison with foveal vision, in this respect almost all sub-

[1] Usnadze, *Psychologische Forschung*, 5, p. 24 f., 1924.
[2] von Hornbostel, *Festschrift Meinhof*, 1927.

jective experience is more similar to peripheral than to foveal vision. And if the reader agrees with me here, he has admitted that in one point, at least, subjective experiences may be compared with objective experiences. Recently Klages has collected a large

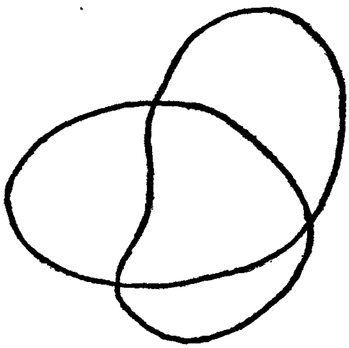

FIG. 18

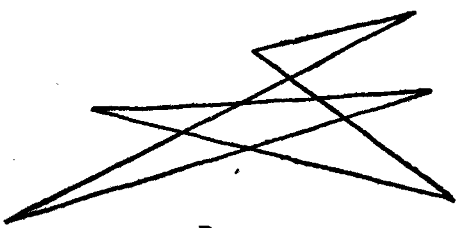

FIG. 19

number of words which we apply to subjective experiences and all of which have at the same time a meaning in the realm of objective experience.[1] Here a few instances must suffice. Something arouses a "bitter" feeling in us. In German one talks about being in a "soft" mood (*"weiche" Stimmung*).

[1] L. Klages, *Vom Wesen des Bewusstseins*, Leipzig, 1921.

"Sweet" love seems to occur in all countries, also "bright" joy or "dark" grief, and not only in my country is wrath called "hot."

More dynamically, one talks about a tense expectation, this subjective experience being compared with what we feel when we touch a taut string. A certain kind of thinking is called "straight," and every one knows immediately what is meant by it. We may have "rest" or "restlessness" in a visual field, but we apply the same terms to something similar in our subjective experience. When one feels nervous one might use "flickery" as an appropriate word for his subjective state, it being similar to visual flicker, for instance. Again, we feel "attracted" towards something or inclined to "reject" the idea of it; our spirits are "high" or "low," and so forth. The reader will be able to fill out the list by dozens of other terms which, as these few examples, are used both for objective and subjective experiences without creating difficulties of understanding in communication. Some psychologists may refuse to draw any conclusions from such verbal hints. They will say that here we have to do with mere analogies. I cannot accept this as an argument, however, because what else is an analogy but some kind of similarity? Language does not use the words generally applied to sensory experiences in a haphazard way as terms for subjective experiences. If one of these terms is applied to just one class of subjective states or experiences, and not to others, and a second of these terms is applied exclusively to a second class, and so on, there must be some principle which regulates this definite extension of meaning. Furthermore, there must be something at the basis of the correct *understanding* of this kind of verbal practice, when a *new* application and transfer of meaning is suddenly invented by some one trying to describe his subjective state in a lively manner. I contend that this *something* is a certain degree of similarity between definite experiences of the "inner" and "outer" world.

Though I realize that there is some truth in the James-Lange theory of emotional experiences, I do not wish to make my statements seem to depend upon this theory. Of course, if the so-called emotional aspect of subjective experience *is* sensory experience issuing from our muscles, viscera, and so forth, it will be a truism that emotional experiences are "similar" to certain "sensory" experiences, since they are declared even to be identical by that hypothesis. But we may recognize a similarity without accepting the identity. From this viewpoint, maintaining a certain cautiousness toward the James-Lange theory, we may find a similarity even between subjective experiences and those of vision or hearing, so that, in this respect, our theory would be more general than that of James and Lange.

With this preparation we can approach the problem, whether our objective experience of the behavior of others is capable of showing us something essentially similar to their "mental life"—a question which has a reasonable meaning if understood in terms of the last paragraphs. I shall discuss a few concrete cases in which unprejudiced observation seems to give a clear answer.

Two scientists in Poland are talking about the difference of their theories. I can observe their behavior as it develops before me in my visual and acoustical experience but, since they talk in a foreign language, I cannot understand their words. For a while the aspect of the scene is calm, both visually and acoustically. But suddenly the man to the left, as though "struck" by some of his colleague's words, makes but a slight receding movement of the head and, from this moment, the quality of his voice and the expression of his face show a certain character of hardness. To this change there seems to correspond immediately a transformation of the other's behavior, insofar as, from now on, the man to the right becomes a picture of what the musician calls "crescendo," both as I see and as I hear him. Soon the first, too, becomes increasingly active, and since the activity of either is directed toward the other, the whole scene approaches an extreme state of agitation, when presently I see the man to the right looking at a placard on the wall and smiling. He says a few words to the other, who also looks in that direction, a certain brightness appearing on his face at the same time; and from this moment their conversation resumes its initial smoothness and placid quality.

These scientists happen to know the German language. So, when the discussion is over, the man to the left tells me that a short while after the conversation started some unexpected words of his friend appeared to him as a personal offense or insult, and that he could not avoid having a bitter feeling as a result of it. The friend, on the other hand, reports that, beginning rather abruptly, the man to the left manifested such a closed and hard appearance, as though to indicate that he would never surrender his position or be open to persuasion, and that as a result he felt anger continually rising and increasing within himself. The first, the man to the left, reports that a similar wave of irritation occurred in himself after a while; and both of them tell me that the placard on the wall reads "Keep smiling" in their language, and that, almost at once after becoming aware of it, they felt much more relaxed and calm.

Is it really possible to deny that in such a case my objective experience pictures some essential traits of what the others experience subjectively? I see the two scientists as one visual group, the members of which are directed towards each other in all their attitudes. Thus, each of them feels that he is attacking the other or defending his own opinions against an attack directed toward them. Again, the sudden receding movement of one of them pictures very well the felt increase of social distance which followed from something like an offense. Neither will deny that, from this moment, there began an emotional crescendo, which found an adequate expression in the visual and acoustical crescendo of my sensory experience of the total scene. Finally, I could see the return to calmness, resulting from their looking at the placard for a moment. And this calmness itself, as seen and heard by me, is similar to what both tell me about their subjective state at that time.

But the lesson which this example teaches seems extremely general to me. It seems to teach that all forms of dynamical development occurring in subjective experiences may be expressed by similar forms of objective experiences in the sensory field of an observer. Quite generally the inner processes, whether emotional or intellectual, show types of development which may be given names, usually applied to musical events, such as: *crescendo* and *diminuendo, accelerando* and *ritardando.* As these qualities occur in the world of acoustical experiences, they are found in the visual world too, and so they can express *similar* dynamical traits of inner life in directly observable activity. Who has not found himself occasionally walking faster when thinking about the disagreeable attitude of an adversary! Again, to the increasing inner tempo and dynamical level there corresponds a *crescendo* and *accelerando* in visible movement. Of course, the same inner development may express itself acoustically, as in the *accelerando* and *reforzando* of speech. Or look at your friend on different mornings. Sometimes his movements will be even and calm, sometimes his whole visible surface, his face and his fingers, will be unstable and restless. He does not need to tell you in the first case that he is well and at rest, nor in the second that he feels restless subjectively, because both states are apparent directly in your objective experience. In the same way, hesitation and lack of inner determination become visible in a form obviously similar to them as subjective experiences, as *ritardando* of visible or audible behavior, or as slight, disorderly, starting movements in different directions, not organized into one whole. As long as human beings behave naturally, any *sudden* change of inner direction, any sudden event in their subjective life, will appear as a sudden event on their visible surface or in their speech-melody. In a moment of *sudden* fright, human beings *jump* back, or start. When some one experiences that flash by which a new idea or the solution of a problem comes to him, he will suddenly interrupt his walking or abruptly strike his head. Here both his inner experience and his outer aspect will exhibit the same interruption of continuity. For similar reasons the activity of a man, as it is seen by us, will often show an organization which corresponds to the organization of his doing and planning, as experienced subjectively. Actions which flow from one determining source appear as one stream of visible events in most cases. Where a new "idea" leads to new action in subjective experience, the observer will see one stream of visible development ceasing and another one starting independently. If one action consists of relatively segregated members, subjectively, the same "articulation" will frequently prevail in visible behavior. So with regard to dynamical structure, not only the so-called expressive movements but also the practical behavior of human beings is a good picture of their inner life, in a great many cases. The reader who may have lost the feeling here that I am discussing concrete things should review an earlier chapter in which, among other examples, the organization of seen movement was considered. In my book *The Mentality of Apes* the description of animal behavior includes a great many instances of this type.

It will be convenient, however, to treat in detail a few examples which are of a somewhat different kind. In objective as well as in subjective experience we find states to which the names of "tension" and "direction" may be applied. If my attention is attracted by a strange object, a snake for instance, I feel directed toward it and at the same time a feeling of tension is experienced. A friend, even if he has not perceived the snake, will see me and especially my face and eyes directed toward it; in the tension of my face he will have a visual picture of my inner tension, as in its direction he has a direct picture of the direction which I experience. It cannot be said in objection that vision will not apprise him in any way of my "connection" with the object, since there is nothing in space between my eyes and the snake, which might be a "stimulus" for his seeing that connection. Such an argument seems to forget what has been said about grouping in visual space. As soon as my eyes are seen looking in a definite direction—and in this respect our eyes are astonishingly expressive—a segregated whole or object, which is situated in that line of direction in the field of an observer, may become immediately connected with my gaze and my whole person. This case of grouping is not more problematic than all the others we have discussed. We may at once add the case in which the direction of attitude is the opposite, i.e., away from something. Again, grouping may become so striking that no one can see such a movement or reaction without referring it to a definite object, *from* which it is averted, exactly as the event is felt subjectively by the person whom we are observing. As an example, I give the following statements, taken from a famous experiment, literally:

"He started to reach for the head of the animal with the forefinger of his left hand but withdrew it suddenly before contact."

(Wool is presented to a child in a paper package). . . . "He then began to play with the paper, avoiding contact with the wool itself." Both statements occur in Dr. Watson's description of his experiments with children.[1] In the first, the movement of a finger is seen as *impulsively directed toward the animal,* though "objective" methods of observation, as Dr. Watson understands the term, would not find the slightest connection in space between the finger and the animal, and though, therefore, his description seems to imply an almost purposive psychology. In the second statement, the same criticism applies to the words "avoiding contact with the wool." "Avoid" is a term which may surprise us in a description governed by the ideal of the natural sciences and the fear of so-called "purpose." However that may be, the example shows that, in visual experience, the tendency toward grouping and directed reference is so strong that even the most radical behaviorist cannot resist it. Therefore, if I "avoid contact" with a definite object or if I "reach for" another, these facts of my experience must be excellently pictured in the organized field which an observer of my behavior himself beholds as his *objective* experience.

In the last few examples it is the spatial, more than

[1] *Psychologies of 1925*, pp. 52 and 54, Clark University Press.

the temporal, side of visible behavior which resembles the inner experiences of a person. This is also the case, where a state of inner depression expresses itself in a drooping carriage of the body as a visual whole, similar to the posture of the normal person in a period of extreme fatigue or sorrow, but even more decidedly in the attitude characteristic of certain insane patients. Just the contrary is seen in those patients who enjoy a supranormal euphoria or "elevation." Usually, their body as a visual thing will show most clearly a corresponding erectness, and in one case, which Dr. Janet described admirably at the last International Congress, the patient began to walk on tiptoe.

Most readers will be well acquainted with the next case which, by the way, is particularly interesting for social psychology. A man in a leading position, but perhaps a little too good-hearted for his high office, treats his inferiors as friends. When he sometime finds himself obliged to censure one of them severely, and to make him feel that friendly relations are over, such a man becomes a most suggestive object of observation. If he has not already trained himself on previous occasions, he will have the greatest difficulty in uttering those decisive words which will show the other quite openly and definitely that the superior has radically changed his attitude. In spite of the best intentions provided by a feeling of duty, those words will not be uttered, but in their place others which do not go to the center of the affair. They come near to it; the other man may guess the whole truth from them if he is sensitive enough, but they leave something like a screen around the main point. Viewed from without the official's activity is a picture of his inner perturbation. The stress of firm intention is in him, but social factors prevent his behaving entirely according to his intentions. Consequently you may see him walking up and down before the other, as a magnetic needle swings in the field of force. He himself feels the opposed strain and duty of acting, and externally his action and speech are "deflected" from the other. When he stops, his eyes are worth observing. All the inner direction and difficulties of a person become visible more easily in the sensitive play of the eyes than elsewhere, particularly under the pressure of "social forces." Of course, it is rather easy to say nice things (which one does not believe) to another man, while looking into his eyes steadily. Social forces are not opposed to that; on the contrary, they operate precisely in that direction. But try to look into another's eyes, i.e., toward what we experience for some reason as the center of the other man's personality, and then tell him what obligates you to be less friendly toward him, and, as it were, to reject him socially! For some persons this is an extremely onerous task. So the man in our example may well intend to look into the other's eyes; but his own eyes will either stop short somewhere, at the mouth or the nose, for instance, or when they reach the full glance of the other man, they will be deflected very soon as though by an irresistible force. Just as this man feels his intentions bending around and his words avoiding the socially decisive step, his visible behavior appears to us as persistently deflected from the main direction, which is toward the other, and especially from the visible center of his personality, i.e., his eyes.

This may suffice as a description of cases in which the similarity of the objective and the subjective experiences of the same behavior seems to me obvious. The reader will find a great many other examples, as soon as he begins to observe other people from this point of view. If that similarity is not a familiar fact to the psychologist, the reason may be the analytical tendency of our science. Indeed, when we think of other people's appearance in terms of local nuances of brightness, chroma, and so forth, we feel inclined to deny the idea of such similarity. But as soon as we look upon behavior more naïvely and are not afraid to recognize grouping, and a great many qualities of the von Ehrenfels type, as realities in sensory experience, the similarity becomes obvious in thousands of cases.

Our problem has a genetical side also. Why should behavior in the objective experience of an observer be similar to what is being experienced subjectively by the individual under observation? The answer is rather simple in the case of temporal and dynamical traits of behavior. A pianist while playing a sonata is living in a dynamical stream, the organization of which is very clear to him. In his experience he ends one "phrase" now and at a definite moment begins the next one. He starts a *crescendo* as the Ehrenfels-quality of one musical development and a *ritardando* as that of another. Whatever the laws of motor innervation may be, the impulses which are conducted to his muscles follow the organization of the scheme he "has in mind." The final results are sound waves, and these are not dynamically organized, because in the air one wave is independent of the previous waves and so forth. But something remains which is sufficient for organization in those who listen. Where he intends a *crescendo* as an Ehrenfels-quality, the product of his playing will be a series of waves of increasing intensity. In those who listen this series will produce an acoustical whole which again has the same Ehrenfels-quality of "swelling." Where, in his experience, he ends a "phrase" and afterwards starts a new one, the relations of the waves as to temporal distance, intensity, and so forth, will be such that in the acoustical organization experienced by the audience one whole ends and a new one begins at similarly related moments. In some respects the situation is the same as in the case of physical objects that appear as segregated wholes in visual experience. Though among the light-waves reflected by the surfaces of objects, and consequently among the stimuli projected upon the retina, there is no organization, the relations between the stimuli are so well preserved that, in the nervous system, organization, which depends upon those relations, will generally build up "adequate" wholes. As in this case, organization comes nearer the objective facts in some essential points than the indifferent mosaic of stimuli does, so in our example, acoustical organization will correspond to something in the pianist which does not exist *as such* in the sound waves between him and his audience. Once more the mediating properties of the stimuli are their relationships. If we assume that the pianist has experiences similar to our own, the knowledge implicit in our acoustical organization goes beyond the knowledge implicit in our having visual objects as segregated wholes. For the organization which we experience will correspond not only to the organization of his nervous processes, but also to the organization of his musical "intentions," as he *experiences* them. In order to understand this, we must remember the concluding remarks of the second chapter. If the pianist intends a musical phrase as a whole, shall we assume that the processes corresponding to that experienced development-as-a-whole are also one whole functionally, or shall we assume the contrary? Where, for his experience, the phrase ends and after a discontinuity a new one begins, shall we suppose that the corresponding processes of his brain are functionally continuous or shall we assume that the physiological organization is discontinuous, and hence similar to that of his experiences? *Gestalt* psychology assumes the similarity of organization in both cases; it makes this hypothesis quite generally and in all respects. And from this viewpoint the genesis of communication between the experiences of one man and those of another becomes simple enough. The experiences of the first, the pianist, are a picture of the corresponding processes in his brain, so far as organization is concerned. Innervation of the pianist's muscles occurs as something like a projection of that picture upon his muscles. The sound-waves which he produces are not organized, but the relations between them somehow preserve and sustain that previous organization. In a similar nervous system a new organization is built up depending upon those relations existing among the acoustical stimuli. To some degree it will be similar to that which exists or has just existed in the nervous system of the pianist. Therefore the experience corresponding to these processes will be correspondingly similar to the organization of the experience in the pianist. As in this example, the similarity between the inner experiences of one man and the sensory experience of another may be explained wherever we find such a correspondence existing with respect to temporal organization and dynamics.

It will not be necessary to repeat the argument in the case of "direction," of "'tension," of "lack of determination," and so forth. Even where we do not have full insight into the reasons *why* the peripheral overt behavior of a human organism pictures the organization of its experiences, or better that of the corresponding processes in his brain, we have to admit that this connection very often exists. The temporal and other relations of the stimuli which are produced by such activity in one organism are just the right ones, then, to determine the appropriate organization and similar Ehrenfels-qualities in other organisms receptive to those stimuli. A man who makes a great effort to find a name which he cannot immediately remember will, very probably, contract his face. The face thus contracted produces just the constellation of stimuli which, in the objective experience of another, will determine the character of "tension" as an Ehrenfels-quality of that face as a visual whole. We have also seen how the almost inevitable visual grouping of a man with those objects towards which he is directed, will complete the picture so that, in this respect too, we see something similar to what is going on "in" that man.

At this point the reader may protest that, after all, our understanding of other people remains an indirect affair. Though, in our objective experience, the other may exhibit properties similar sometimes to his inner experiences, our objective experience remains something different numerically from those inner experiences of another. The first gives us what we call "bodily events" in direct objective experience, whereas the second will never become an objective experience of ours. Therefore, in one point at least, we do not seem to be better off than other theorists! Something like a concluding step in our analysis is needed if we are to use our objective experiences as a basis for guessing about similar inner processes in others.

Indeed, the discussion has proceeded to this point as though in this final step the old and the new idea would become identical compulsively, the only difference being the acknowledgment of a much better basis for this last step in the new theory. It is now time, however, to change this aspect of the problem. It has to be changed in the direction of agreement with what is sound and justified in behaviorism.

If I review in reverie one of those days which is particularly filled with social contacts and the ever repeated task of understanding other people, I have to confess that I have scarcely, if ever, had an "image," or anything like it, of the inner experiences of those other people! To speak the truth: I have never even thought of the possibility of having one. I intentionally summon such an "image" now, in the hope of producing and "having" what, from the usual viewpoint, should always be the kernel of social understanding. But the resulting product is very poor and decidedly unusual. It becomes quite obvious by such a test that generally social understanding does *not* depend upon that procedure. What else happens then? The answer is simple, though it will surprise some of my readers.

In my objective experience of my neighbor's anger there is no "dualism" between the "movements of his body" and his "inner experiences." Even for the adult this dualism does not exist, so long as we remain naïve and do not theorize too much. To ask for the *final step* in the inference from certain of our objective experiences, viz., the behavior of others, to their inner life, is nothing but the introduction of a sophisticated, scientific and philosophical classification into a field which, to the unprejudiced observer, does not require such a treatment. Philosophy, in trying to build up a picture of the world, may be compelled, perhaps, to make a distinction between "mind" and "body." But that does not mean that our objective experience of others must appear to us as something insufficient and limited, *behind* which an altogether different kind of event occurs. This is not necessary because objective experience as such contains *all* the material needed for understanding others, at least in all those cases which we have treated here. When I refer to the calmness of another man standing before me in everyday life, I refer to his visible appearance, which is not something "merely" visible to me, but represents all the "calmness" I am interested in *without any further idea about something additional such as, for instance, his "consciousness."* If he becomes more and more excited, according to my eyes and ears, I behold a "purely" sensory *crescendo*. The man's excitement is nothing but that sensory *crescendo* in my objective experience of his behavior, and I do not think of the man as a locus of events belonging to an essentially different world. At least, before I begin to philosophize, I do not do so. In the same way, when I refer to other men's "hesitation," "restlessness," "determination," "depression," "avoiding," "reaching for," and also their "joy," "fear," "anger," "embarrassment" and so forth, I am usually far from inferring "inner experiences" in them, by making "a last step" from my sensory experiences into quite another field. On the contrary, what I denote by those terms *remains* entirely in sensory space. Though the terms may also be applied to *my* inner experiences, which often are strikingly similar to those objective experiences, they do not usually refer to the inner experiences of others when used in an everyday description of their conduct.

May I ask the reader to make an experiment before criticizing these statements? It is easy enough to produce embarrassment in some one. If the reader tries it and if he becomes convinced that he has been successful, he should ask himself whether what he calls the embarrassment of the other is not present to him in sensory space. He should also ask whether or not he imagines something out of another world, i.e., the subjectively experienced embarrassment of the other, when he "understands" that man's behavior, as we understand each other in daily life. Sometimes, it is true, we experience the other man's attitude as something issuing from "his interior." But what is here called "his interior" is a part of the same sensory space in which his visible face is seen. Even such a description does not refer to events in a world apart; it means an event in the self-same world in which we see the other "looking at a definite object," for instance. If in my objective experience the other man is a segregated whole and if to this whole there corresponds a functional whole of processes in my brain, we have no right to expect that the dynamics of that functional whole should be restricted to its boundary in all cases, the interior of it remaining altogether empty. So we may well understand the fact that sometimes a dynamical attitude seems to issue from another person's "interior."

The conclusion of this discussion is that the "numerical difference" between fear as experienced subjectively and fear as experienced objectively by an observer, shall not be denied. But we have to add immediately that this theoretical difference does not play an appreciable rôle in common life, because here, from the viewpoint of a naïve observer, the frightened behavior, as it is experienced objectively, *is the* fear of the other. Similarly, to this observer the pencil before him *is the* pencil, though in a moment of theoretical consideration he may realize that, in addition to it, there exists a pencil as an unexperienced physical object.

I mention one more consequence of this theory or, as one should better say, of this description: All physical events or states which send similar constellations of stimuli to our eyes and ears, as, for instance, the physical body of another person does, will look or sound "emotional," "restless," "directed towards something," "determined," and so forth, just as a living person does. No one can hear naïvely the rumbling *crescendo* of thunder without thus understanding it as "menacing." Different types of weather are experienced in the same manner, i.e., as calm or restless or friendly, and so forth.[1] No one thinks of imagining the subjective experiences of the weather in such a case. What surrounds us in sensory space, we describe as restless or menacing or friendly, because these are the dynamical attributes or Ehrenfels-qualities which characterize the sensory experiences in question.

I wish to state expressly, however, that sometimes the problem of "understanding" seems to present difficulties which cannot be solved in this way.

[1] In this case there is a complication, since weather influences our physical body and its nervous system, apart from our sensory experience of the weather.

Wherever the objective experience which we have of another person does not appear as similar to those inner experiences which he probably has, a new situation is before us. One might say, for instance, that, from the viewpoint of our theory, laughing as seen and heard by others is not an appropriate expression of that group of subjective states which are usually accompanied by laughing. Whether in such cases the theory of association and assimilation, or any other indirect explanation, may be accepted as a supplement, I shall not try to decide here. Suppose that such a supplement should become necessary; the facts described in this chapter would be an excellent basis for it, because where understanding has already occurred as something direct and obvious, this core of experiences might well tend to complete itself in the form of meaning acquired indirectly. On the other hand, we do not claim that what is going on within a person will *always* be evident in his sensory appearance. A theory that would pretend so much could not possibly be right. Most persons begin to conceal themselves to some degree early in life. A well-trained actor or pianist does not "look" his stage fright at all; he does not "sound" it either. To be sure, sometimes, calmness acquired as a social form will become transparent in consequence of some slight but treacherous nuance of effort in it. Real embarrassment, as, for instance, in the example considered above (pp. 253-255), is a thing which few people can conceal altogether. By no means can we deny, however, that innumerable passing events and traits of inner life remain hidden in social contact, because suppression of peripheral "projection" has become a facile automatism in adults. Moreover, I shall concede without hesitation that, on the way from organized processes in one person through the transmission by waves of light or sound to new organization in a second person, much may be lost or distorted. The theory gives us neither an altogether new nor an altogether perfect key to another person's inner life; it tries only to describe so far as it can that kind of understanding which is the common property and practice of mankind.

A special relevant task consists in considering what further effects those sensory experiences may have, which, possessing the qualities of calmness or restlessness, of tension or relaxation, and so forth, appear to us both in our fellow men and the weather. In our nervous system, the processes underlying those experiences do not remain isolated, the psychophysical field is larger, and other parts of it are very apt to develop processes which we experience as subjective. We have seen that these have much in common with the processes underlying our objective experience of the behavior of others. Therefore, a rather direct influence seems to be possible. Social contagion particularly, but also fear, as an effect of menacing objective experience, or haughtiness as a consequence of the humbleness of others, experienced objectively, must be considered from the new viewpoint.

These are problems which we may leave to a future time when the simpler facts described in this chapter will have found more general acknowledgment. One word more shall be said, however, about the application of the new viewpoint to scientific method. In this respect one might be inclined to say that such an understanding of others is all right for the purposes of common life, but not for those of science. It ought to be excluded, therefore, from behavioristic studies as something which originates in our sensory processes as a very indirect and complicated result of stimulation. We have seen, however, that even such a radical behaviorist as Dr. Watson does not keep himself free from its use, because too little would be left of the qualitative observation and description of behavior, if he should try seriously to exclude even the slightest trace of that "understanding." Take the example of the physicist! If he would exclude from objective observation whatever exists for him as a product of sensory organization only, he could neither rely upon the sensory segregation of *objects,* including his apparatus, nor upon their *form* as a visual experience, since it does not exist without the "indirect and complicated" processes of sensory organization. It is obvious that such an extreme procedure would make his scientific task an impossible affair. Quite the same is true of behavioristic psychology. At least as preliminary material the sensory experiences which we have considered in this chapter have to be used, wherever they occur as striking and evident in behavior as objective experience. With naïve confidence the physicist did likewise to a great extent, before he could develop the present methods of his science which restrict the use of sensory experience to certain special and very reliable cases. In our young science it will be convenient to follow his example with caution, as we proposed in the second chapter. But I wish to lay some stress upon the necessary caution. Only those should be allowed to apply this method in objective psychology who know how to discriminate between the apparent data of objective experience and the mere play of untrained and prejudiced imagination.

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VIII

Association

WITHOUT the observation of direct experience we should be in great danger of constructing an artificially simplified system of psychology, as is that of current behaviorism. On the other hand, it seems absolutely impossible to me to develop psychology as a science of direct experience or of "consciousness." For the development of that science the field of actual experience alone is inadequate, especially when compared with the totality of those processes in the nervous system, a few of which are at each moment accompanied by experience, but *all* of which seem to be more or less interdependent. How can one pretend to construct an adequate theory of psychological events using experience alone, if the processes underlying experience are merely a dynamical province of a much larger functional whole? It is almost impossible to deny this fact nowadays. Therefore, we cannot hope to understand experience itself exclusively from its own aspect, any more than one could hope to learn and understand the game of chess through watching only the moves in one corner of the board the whole time.

In this latter case the observer of the game would soon become aware of the fact that something important was going on beyond the narrow field of his observation, that evidently the moves in it referred to something beyond, since they even seemed to come from there and to disappear again into the unseen. Exactly the same is true of experience. When reading or talking about things which are not present, we do not usually develop adequate "images" of them. Sometimes scarcely anything at all seems to be experienced of those absent things. When asked about my profession I answer that I am a psychologist. But all the *actual* experience connected with this word is a certain feeling of familiarity, of being sure about it, and of a certain direction in which I might start at once in order to furnish more concrete and detailed data. This "readiness for transition" in a definite direction, the mark of which is not given explicitly in actual experience, has been described excellently by William James. It is one of the most frequent and common things to be found in experience. And its most conspicuous character consists in its being *experienced as* pointing beyond the actual field of experience towards something particular and ready outside. So experience tells us about its own functional incompleteness. Nor should we be too much astonished by the fact; nothing else can be expected if, of a larger functional whole, only a restricted field has experience as a correlate; to the dynamical dependence of this field upon definite physiological states beyond itself corresponds the felt definite direction in experience. Though not experienced actually, those physiological states "beyond" seem to be definite enough; for in general our reading and talking go the right way, sufficiently determined by those "states beyond."

Perhaps the simplest example of this is "successive comparison," in its different forms. After a few years of traveling I meet a friend, and my first idea is "How old he looks!" That does not at all mean that he looks particularly old on an absolute scale. I see older-looking men every day. Nor does it mean that I reproduce an image of him, as I knew him before, and now compare the two. Nevertheless, what is meant refers to the past and represents an extreme form of what occurs in most cases of successive comparison. If five seconds after a first noise I hear a second one, similar but sufficiently louder, I am able to judge about the relation of their intensities without any difficulty, though I do *not* reproduce the first at the moment of the second. (Personally, I am not able to experience even a somewhat inadequate "image" of one noise when I hear a second one which is slightly different.) In this point all observers at present seem to agree.

But how do we judge about the relation, if only one of the noises is actually experienced? We may answer that we do not experience the second noise as something isolated and discrete, but that, when it occurs, it seems to come with a definite direction "from something in the past."[1] Even if we take this for granted, however, our problem is not yet solved. Our judgments are usually rather accurate. Hence, what is left of the past, i.e., of the first sound, which now determines the direction with which the second noise appears, must be a fairly definite representative of the first noise. On the other hand, this *trace* of the first sound is a process not comparable to that process which, five seconds before, was accompanied by our experience of the first sound. If it were still of the same kind, it should be accompanied by a similar experience throughout, which it certainly is not. Hence, the only possibility seems to be that a physiological product of that first process remains after that process itself has ceased, that such a product represents sufficiently well that process by which it was produced, and that the definite direction with which the second noise appears depends upon the "level" of that product in its relation to the intensity of the second noise.

The concentration of those molecules which have taken part in the first process may be considered as a product of that process, left behind as its trace, with corresponding properties. There are other hypotheses. But no theory will be competent which does

[1] We may say, then, that the second noise has a definite rôle or property which belongs to it as to the second member of a pair (cf. Chapter VI).

not assume some trace corresponding more or less to the actual properties of the first process. Some years ago I developed a more detailed theory of successive comparison, primarily concerned with the way in which the trace of a first sound determines the direction with which the second sound appears.[1] I concluded that by experimentation on successive comparison we are able to find out what happens to the trace of the first process after this process itself has ceased. The final result of these considerations was that such a trace is preserved for a long time and that, probably, it is identical with what has to be assumed as the physiological basis of enduring memory and of reproduction.

In some degree all sound theories of memory, of habit, and so forth, will be hypotheses about physiological traces. Furthermore, each of these theories will have to assume that the properties of the traces correspond more or less to those of the processes by which they were produced. Otherwise we should not be able to explain the accuracy of reproduction which in a great many cases is very high. In *gestalt* psychology we state at once the assumption that, as far as the original process (and the experience which may have accompanied it) has been organized in a definite manner, its trace must have similar properties of organization, in general. Take the examples given in Chapter VI where the concept of "real form" was

[1] *Psychologische Forschung*, 4, 1923.

considered. If certain segregated wholes with definite forms are experienced in our field of vision, the aspect of one of them later on may reproduce other experiences which had been present at the same time. But if, for some reason, instead of that particular organization, grouping and form, we experience other wholes with their real forms, no reproduction will occur, though the same, or almost the same, stimuli may be given us. So the number 4 will reproduce its name when written in a certain environment (cf. p. 209), but when Fig. 10 is shown to an unprejudiced subject, this name will not occur at all. On the other hand, if the subject has once found the 4 in that figure, which means that 4 has now become a segregated whole, he will consequently find it again easily, and reproduce the name. From this it follows that the traces of past experiences are neither an indifferent continuum nor a mosaic of independent points; rather they must be pictures of past organization. As such they take part in processes of reproduction.

This same property of the traces may be deduced from recognition. When Rubin induced his subjects at the first presentation to see certain pictures in a definite distribution of figure and ground, they recognized them very well if, afterwards, the same organization was favored by the conditions of the experiment. If, however, in the second presentation what had been figure before became ground, and vice versa, the subjects found unknown forms in the field, i.e., failed to recognize, though the constellation of stimuli was the same throughout. Here again the traces are shown to correspond entirely to past organization, and not to the aggregate of past stimuli. We may go a little further: In most cases of reproduction the reproduced material itself appears as organized. This is true not only of "images," but also of motor "melodies" with which we are well acquainted, as Michotte and van der Veldt have shown most vividly in a recent book.[1] Readers who have visual images will easily find that their image of a definite individual tree is detached from the dim environment, as a single figure possessing a real form. Of course, in "free imagination" and in dreaming the field given us may be very different from any sensory experience we have ever had before. Nevertheless, even the oddest creations of dreaming will remain figures, groups or events, which, whether very definite in shape or not, exhibit all the essential properties of organization.

So decisive will organization be in a tremendous number of cases that we may change the stimuli in the most radical manner and still get the same recognition or the same reproduction, if only the essential traits of the organization in question remain the same. A melody is recognized in a key which may not contain a single tone of its first presentation. Or

[1] *L'Apprentissage du mouvement et l'automatisme*, Louvain, Paris, 1928.

again, some days after we have heard it the first time we find ourselves humming it spontaneously in a changed key. Here all the factors excepting organization seem to be indifferent for reproduction. I hardly need mention the fact that an unknown figure seen to-day in red color somewhat to the left of the point of fixation and in a definite size, will be recognized without difficulty if to-morrow it appears as green or yellow, somewhat to the right and in quite another size.[1] Evidently, then, recognition and reproduction deal with the organization of past events as much or rather more than with local effects of stimulation, which are assumed as the parts of past events in mosaic theory. We shall come back to this point later on.

From this viewpoint some facts can be explained which would remain perplexing if we did not recognize the reality of organization. When we experiment on delayed reaction in animals we find that some of them are able to choose the right object, let us say among three equal ones before them, after a delay of many seconds or even some minutes; though at the time when the delay is over and we release the animal, the right object does not exhibit any peculiar traits, as it did before the delay. If, during the delay, the animal remains directed towards the right object, his correct choice is not very astonishing. A real problem arises in the case in which the animal

[1] Becher, *Gehirn und Seele*, Heidelberg, 1911.

moves freely in his cage during the delay and still assumes the right direction after we release it. It has been said that in such a case the animal's reaction depends upon some internal cue. This is true insofar as there would be no reason at all for the right reaction after the delay without some after-effect of previous stimulation (for instance, the aspect of food) which at that time made one object a particular thing. That after-effect is undoubtedly an internal cue. But when, after the delay and after accidental movements of the animal, the internal cue works the right way, there must be some characteristic of the right object by which it can be discriminated. This something cannot be discovered when we examine each object by itself, because in exact experimentation they all have the same properties. Still there is some difference between them, and this difference is the rôle which each object plays in the group. One is the left end, one the right end, and one the middle or interior part of the group. If, with that after-effect as an internal cue, the animal reacts after the delay, the only property (of the right object) which can make the animal refer its cue to the right object is *the place of that object in the group of three objects.*[1] The particular stimulus (the food, for instance, or a light, as in Hunter's famous experiments) which was given at the beginning of

[1] Or in the organization of a larger whole, as O. L. Tinklepaugh has shown (*The Journal of Compar. Psychol., 8,* 1928).

the delay has operated as a distinguishing mark of one definite and well characterized member of the group (let us say, of the first to the right in a single case). Therefore, after the delay, the group, and that definite rôle which just one member of it plays in sensory process, is able to work upon the cue and to produce the right reaction. In this sense delayed reaction to one among several objects, having identical properties as isolated objects, involves the principles of organization and cannot be understood without them. This becomes even more obvious, perhaps, when we consider the multiple choice method which Yerkes has used with so much success. There it is evidently the definite rôle of one object in a group which has become connected with a definite reaction and consequently will reproduce that reaction whatever may be the position of the group.

The main interest of experimental psychology has not been directed toward the nature of the traces which are left in the nervous system after the original processes have faded away. When we say that the traces of organized processes must be organized themselves, we do not seem to have mentioned the most important feature of memory, habit and so forth, namely, that those traces are *connected* in the manner which is generally called their "association" and which leads to the reproduction of the second when the first is activated by a repetition of its process. That there are traces is one basic factor of habit, etc. But if two processes, A and B, have occurred frequently together, reactivation of the trace of A is said to reactivate B in consequence of a curious bond between them created by those past events. This is the second basic factor of habit, and almost all the classical research on memory has dealt with the formation of that bond and its career in the course of time. There is almost no other phase of our science of which we are prouder than this, because here methods have been developed and laws have been found which may almost be compared with those of the natural sciences.

In some respects this pride seems fully justified to me, though at the present time we have begun to realize that we have used those excellent methods to investigate inconsiderately a special type of memory and habit and that we should not apply the results obtained to everyday memory and habit. There is one problem, however, which was not made an object of those experiments because it did not seem at first to be a problem. Indeed, it was presupposed as a fact that the mere repetition of two processes, A and B, having occurred frequently together, would create that curious bond, the association, between them. This concept of association shall be discussed in the following paragraphs.

The law of association just mentioned has been considered particularly satisfactory, due to the purely mechanical character of its content, which does not in the least imply mystical forces or anything else foreign to the ideals of natural science. I must confess, however, that just from the viewpoint of natural science the law of contiguity appears rather strange to me. A and B happen to occur together and, whatever the nature of A and B may be, the bond is produced between them! I do not know a single law or rule in physics or chemistry which might be compared in this respect with the concept of association. I repeat what was said in Chapter IV: Wherever an A and B have anything to do with each other in physics, the effect is found to depend upon the properties of A and B in their relations to each other. This is the case in astronomy where acceleration depends upon the masses of A and B. It is the case in electrostatics where the occurrence of attraction or repulsion depends upon the nature of electric charges. In chemistry we find the violent reaction or indifference of atoms determined by the relation of their properties in the given case. But there is not a single example of an effect produced by the interaction of two things or processes quite independently of their properties. Nevertheless this is the character of the classical law of association, as we find it stated in most textbooks.

At this point we return to that trait of mosaic theory which we have found so characteristic of its treatment of sensory processes. If the distribution and direction of events in the central nervous system are determined dynamically by interaction in the field, they must depend upon "the relative properties" of those processes which exert influences upon each other. This is excluded in principle by the mosaic theory of the sensory field. Sensation processes are assumed to be indifferent to each other in general, so that, with regard to them, any pattern may exist equally well in the field. Exactly the same assumption of discrete bits of process, altogether indifferent to each other, underlies the classical concept of association for, without even mentioning the actual properties of A and B in their relations to each other, it states that *any* A and *any* B may acquire that neutral bond, if it is supposed only that they occur together a number of times.

It seems to me that, even at the present time and before any experimental research has been specially directed upon this question, we may say confidently that, in this point at least, the concept of association is opposed to a large number of well-known facts. In order to corroborate this claim, we have only to gather statements scattered in papers, the authors of which have worked with the classical methods and the classical nonsense syllables, which their subjects had to commit to their memories.

We see at once that in a series of these syllables the A and B (i.e., two syllables following each other) cannot remain altogether indifferent to each other, since by a simple experiment we can prove that A and

B are not even indifferent to F and G and H and so forth (i.e., to members remote in the series). When a subject is told to write down six syllables which we read to him quickly, he is usually able to comply. But if, instead of six, we give him a series of twelve syllables, the result is that on the average less than six are written down. This argues a mutual disturbance which indicates that even syllables rather distant in time are not indifferent to each other. If after some repetitions we test the subject, giving him single syllables and instructing him to name the following one in each case, our procedure is based upon the assumption that there are a number of independent associations and reproductions, the results admitting a statistical treatment. This does not seem to be altogether correct insofar as it neglects a more general interdependence. It may be neglected whenever this interdependence is the same statistically in all the series and when the problems under investigation are the usual ones. However, as soon as the nature of association itself becomes a problem, we must be more careful.

In a more striking manner the same warning may be deduced from what happens to a series of syllables during learning. The subjects read them in a definite and rhythmical distribution which, usually, consists of larger wholes, containing sub-units. At the same time the reading will assume the character of a melody, the pitch of the voice going up and down, following that grouping exactly.[1] Evidently, a process of organization occurs during learning and most of all during the first readings of the series. If that be so, the syllables ought to acquire a definite "flavor" which belongs to them insofar as they have a particular place in the larger whole. This consequence is verified perfectly in those cases in which, after learning a series as a whole, the subjects are shown the same syllables in another sequence. In the new order *they look or sound new or strange*. A similar implication is to be found in the highly important concept of "association with place," which means that a syllable is learnt as having a definite place, not absolutely but with respect to the whole series in which it occurs. Objectively, the rôle of organization is most convincingly proved if, after learning the whole series completely so that it is repeated as a whole without hesitation, the subjects are shown single syllables in order to reproduce the following one in each case. It has been found by Nagel that scarcely one-third of the reproductions was achieved under these circumstances.[2] When given alone, a syllable is obviously not the same thing as it is in the stream of the organized series.

Nonsense syllables were chosen by Ebbinghaus and

[1] Cf. the schemes given by Frings, *Arch. f. d. ges. Psychol.*, *30*, p. 430 f., 1914.

[2] *Arch. f. d. ges. Psychol.*, *23*, p. 156 f., 1912.

his successors as the best material for investigating associations, because they wished to keep their experiments free from the older, already-formed associations which, if "meaningful" material were employed, would influence the results in an uncontrolled manner. Furthermore, nonsense syllables seemed to be a more uniform material than any other. It would be altogether unjust if we should deny the value of the great impulse which psychology received from that method. It seems, however, that the early investigators used this method in a somewhat one-sided manner. The most valuable observations, perhaps, were made after the one-sidedness of the original point of view was discovered.

Some psychologists have criticized the method of Ebbinghaus in that it does not at all investigate automatic associations, whereas the results of the method are formulated as though that were the case. Indeed, if the mere neighborhood of the syllables is supposed to produce their association, most experiments using this material are far from testing such a pure association, since the subject does not simply receive a succession of syllables, but rather *is asked to learn them.* If he follows this instruction, some factor takes part in what is built up during learning, and this factor is not even mentioned when the results are formulated in terms of automatic associations. Undoubtedly, here is a flaw in the scientific procedure. The mistake is a grave one for it has been shown that without that factor the learning of series of nonsense syllables would be almost impossible.[1]

But what are the subjects doing when they try to learn a series intentionally? No one is better authorized to give an answer to this question than G. E. Müller, who has spent a large part of his scientific life studying the rules of association and reproduction. His answer is: "A series of figures, consonants, syllables and so forth, is learned essentially by our combining the members of the series in an attitude of synthesis, so that they become solid groups."[2] We have seen in an earlier chapter that by a corresponding attitude one may favor definite forms of grouping, and that the products of this procedure may be just as real as any spontaneous organization is. Following the statement of Müller, we may say, therefore, that intentional learning essentially means intentional organizing.

Though such an attitude seems to be almost necessary in the case of nonsense material and, most of all, of syllables, it is not needed evidently where the material has other properties. Time and again we find ourselves reproducing events from our past experience, where the incentive to reproduction has certainly not been *intentionally* combined with what we are now able to reproduce. If that be so, we must draw the conclusion that the nonsense material used

[1] Kühn, *Zeitschrift für Psychologie, 68,* 1914. Also Poppelreuter, *Zeitschr. f. Psychol., 61,* 1912.
[2] G. E. Müller, *Abriss der Psychologie,* p. 25, Göttingen, 1924.

in classical investigations may be excellent in some respects from the viewpoint of exactness, but that it cannot teach us the whole truth about so-called associations. These are not formed in the same manner when, instead of the classical material, we have to deal with the more natural experiences of everyday life.

If now we ask ourselves, whether *all* our experiences outside the laboratory associate themselves spontaneously, so that the evocation of one may reproduce its neighbors, we must confess that this is not the case. We may hear a telephone number dozens of times together with a name and still remain unable to reproduce it when the name occurs later on. In this case conditions seem to be similar to those present in the case of nonsense syllables. The name and the number are as indifferent to each other as are those syllables usually. Thus the suspicion arises that we may have spontaneous association where organization is spontaneous, and that intentional association is needed where the material in itself is slightly organized.

This assumption is corroborated by the well-known fact that meaningful nouns form associations much more easily than nonsense material. In this case, of course, the nouns appear as imbued with their meanings by a process of learning accomplished in our childhood. When as adult subjects we learn a series of nouns we find these meanings ready-made, and obviously now it is these *meanings* which are so easily associated. But why should they be? At the present time most psychologists will answer that this is a consequence of their having been previously associated, so that the actual process of learning has only to strengthen some particular bond between the meanings of a pair of nouns which existed a long time before. But at this point the difference between *gestalt* psychology and associationism becomes striking. Let somebody read the following pairs of nouns a few times, with an attentive attitude: lake-sugar, boot-plate, girl-kangaroo, pencil-gasoline, palace-bicycle, railroad-elephant, book-toothpaste. Learning will be considerably easier here than if the same number of nonsense syllables were given. Can one really say that between lake and sugar, palace and bicycle, and so forth, there are old associations which just need some slight exercise and which thereupon facilitate the learning? It seems to me that we cannot, mainly because the same words have occurred thousands of times in other much more regular connections, which ought absolutely to inhibit the *assumed* associations in this particular case. But there is another explanation. When I read those words I may imagine as a (very strange) picture, how a lump of sugar might dissolve in a lake, how a boot might rest on a plate, how a girl might feed a kangaroo, and so forth. If this happens during reading I shall have experienced some well organized though quite unusual wholes, and it may be that here learning is so easy, because *organizing* is so much easier in this case than in that of indifferent nonsense syllables. In order to exclude the possibility of frequent similar combinations in the past, I have chosen strange pairs of nouns, the meanings of which may be organized into larger wholes (pictures), but do not do so quite spontaneously. It seems to me that those combinations and sequences which become even more easily associated in everyday life are simply cases of strong spontaneous organization.

We may say, then, that in one respect nonsense syllables constitute the worst material which might be chosen for discovering essentially what association is. As they do not spontaneously form well-characterized organizations, the nature of spontaneous association will not become apparent to the psychologist who uses them. Furthermore, as the series of syllables are built up in a haphazard manner, one cannot even examine the way in which learning depends upon what may be called the structure of a series. Even though it be composed of nonsense material throughout, a series may be constructed in a great many different ways. Syllables may be chosen which fit together phonetically in a very characteristic way, or the contrary may be done. Some pairs may be constructed according to one principle, some according to another. The whole series may exhibit an extraordinarily striking principle of structure or it may be an indifferent series, as are the usual ones. All these variations of material ought to be examined in order to see whether or not organization is the essential fact underlying so-called association. From the trend of the foregoing discussion, we should be inclined to say that it is.

As a last argument in favor of this thesis we may mention the fact that, sometime after learning a series of members by combining them into pairs, for instance, the subjects will be able to reproduce the second members of these pairs rather easily, if the first members are given them as stimuli to reproduction, whereas, on the other hand, the number of right reproductions will be small if we present the second members of pairs and ask for the following serial items which are the first members of the next successive pairs or groups. If we suppose that during learning the members of the whole series objectively formed a uniform sequence as to spatial and temporal intervals, this result makes it evident that the old rule of association is superficial and that the conditions of association are ambiguous as long as we do not take into account the organization of the material. We get strong association only in those parts of a series where organization is also strong. It is true that neighborhood or proximity in space and time is a factor of great importance in association. But it does not seem to operate directly. In an earlier chapter we saw that the factor of neighborhood is one of the conditions upon which depend grouping and the segregation of wholes. From what we have just stated it would follow, then, *that neighborhood in space and time influences asssociation only insofar as it determines organization.* Since this condition is just one among a great many others determining organization, and since organization seems to be the really decisive condition of association, the rule of association has to be reformulated accordingly.

I repeat. Where organization is naturally strong we have spontaneous association; where there is practically no organization association does not occur until some organization is created intentionally. Also, after being associated, the members of a series have definite properties which depend upon their position in the surrounding whole, as tones do in an organized melody. And finally, if in a series we determine those sequences which are the *solid* groups in the total series, and if, then, we also determine where association is strongest we shall find that both coincide.

After these preliminaries we can discuss the nature of that particular bond which is said to originate between the traces of two processes when these processes are associated. The prevailing opinion is that during learning the nerve fibers uniting the place of one process with that of the other acquire an increased conductivity, because with each repetition some particular current of nervous energy is assumed to pass from one to the other. In consequence of this change of conductivity, whenever the process corresponding to the first trace activates this trace the current spreads along those fibers to the second trace, and reactivates it also. With this hypothesis one may understand, perhaps, why after some repetitions the current should flow in that particular direction and thereby increase its conductivity; but we do not at all see why it should flow in just that direction on the *first* occasion. And this is a serious difficulty, particularly in those cases where after one single experience we find the association well established. One can scarcely avoid the inference that in this case the processes underlying the experience *as such* contain all that is implied in the concept of association, at once and originally, no special bond being necessary for it.

We do not know what happens in reproduction. The only thing we seem compelled to assume is some physiological communication between the traces of two processes, A and B, so that an event activated in A will spread to B and not to other traces, with the processes of which it had nothing whatever to do in the past. Now, two hypotheses are possible: If we believe A and B to be two processes indifferent to each other, which accidentally occur near to each other in space and time, then some special bond, some change of conductivity in fibers between them may be needed as a basis of the association. In full contrast to this older view, however, we may make quite a different assumption: Whenever A and B are associated, they are not experienced as two indifferent things, but as members of one organized group. This certainly may be taken for granted, after the foregoing analysis. In this case the process underlying the experience cannot consist of two indifferent parts, one belonging to A and one to B. It must be one functional whole in which A and B possess only a *relative* independence. If that be so, there will not be two separate traces left behind after the process itself has faded, but *one* trace of the functional whole, with the after-effects of A and B in it as local regions of that single trace. In this way A and B, or their after-effects, would be as well connected in the single trace of the whole organized process as they might ever be by means of the special bond which united them, according to the older point of view. If such a bond determined "the spreading of nervous activity" from A to B, the segregated trace of one process-in-extension might well do precisely the same. Unfortunately, in neither case do we know as yet what are the functional properties of that "spreading" and what sort of nervous communication would be capable of determining its course in the observed manner of reproduction.

It will be convenient to give our assumption a somewhat radical formulation so that it will be easier to distinguish it from the usual one. From our viewpoint, association is given up as a special and independent theoretical concept. It is not more than a name for the fact that organized processes leave a trace picturing their organization and that in consequence of it reproductions are possible. I do not deny that repetition will make the association stronger, but as long as the process remains the same, repetition does not involve a change in a special bond; it means that the whole trace of the organized process becomes more enduring and stable. Nor shall I deny that sometimes, as in the case of nonsense material, some special attitude is needed for producing an association. But, as we have seen before, such an attitude consists in favoring definite organizations. When these are experienced, the processes corresponding to them will also be organized, and so will their traces. The only problem which is new in such a case is that of "intentional" organization. But this problem shall be treated quite apart from association.

Some psychologists will be inclined to say that it does not matter very much whether we accept one theory of association or the other, since we are not able to look into the brain to decide which one is right. This indicates a complete misunderstanding, however, of the value of a hypothesis. If there is anything concrete in a hypothesis, it must be possible to deduce concrete consequences from it, and to examine them in further experimentation. It is easy enough to do that in this case.

In the old rule about association nothing was said about the properties of the A and B which are associated because, in principle, association was believed to be an indifferent bond between processes indifferent to each other and to the bond. Organization, however, is not at all an aggregation of indifferent material. On the contrary, in sensory experience we have found organization to depend most decidedly upon "the relative properties" of stimulation. Therefore, if association is a consequence of organization, it must also depend upon the mutually relative properties of what is or shall be associated. In some degree this is verified by what we have reported above. But much more has to be done in the way of varying the nature of the material according to the principles of *gestalt* theory, so that we may see more definitely whether, other circumstances remaining the same, an association will be the stronger, the better organized is the group or the whole, according to those principles. As yet we do not seem to have any experiments planned specially and deliberately in order to answer this question. As we know a great deal about the conditions upon which sensory organization depends, the path is laid and it will be the more interesting to follow in this direction since we should be able to deduce from all the rules known in the field of sensory organization, corresponding rules for association. Of course, we cannot hope to find all the properties of association in this manner, because the rules of sensory experience are in no way instructive with respect to the formation of *traces* as such; nor can we from those rules deduce the career of a trace in the course of time, and under the influence of other processes and traces which develop after it in the same region of the nervous system. On the other hand, when once the identification of association with the traces of functional wholes is verified sufficiently, further discoveries with regard to association may give us hints about properties of organization itself which, for some reason, we may fail to detect directly in sensory experience.

A second consequence of our hypothesis, which ought to be examined experimentally for the sake of that hypothesis, may have practical value as well. This consequence belongs to animal psychology. We have seen that nonsense syllables do not usually associate spontaneously because they do not form well-organized groups unless we combine them. In some cases series of them have been read passively hundreds of times without an appreciable effect in learning. But, as we have seen, the same subjects "have a good memory" in everyday life and can reproduce thousands of events which they have never intended to commit to their memories. This reminds one of a strange contrast which all animal psychologists have observed between animal learning, as it occurs during experimentation in the laboratory, and habit formation as it develops in the same animals outside the laboratory. I do not think that the concrete reason for this difference is contained in the reference to the "natural" circumstances present in the latter situation but not in the former. What does the term "natural" mean here? I suggest that it may mean "favorable" with respect to association in a very particular way, i.e., with respect to organization.

Indeed, it would seem in this connection that a great many experiments, most of all those on sensory discrimination, proceed in the wrong way, because, blinded by that old concept of association as a binding together of two indifferent things largely by many repetitions, we do not take account of organization at all when we are constructing the experimental situation. For instance, on the rear walls of two alleys we fix two objects which we wish the animal to learn to discriminate. On the floor, in no connection whatever with those objects, we put the wires for punishment in the case of a wrong choice, in such a way that an electric shock applied at this point will be more foreign to the wrong object even, perhaps, than one syllable is to the next in experimentation on human association. On the other hand, the animal will be fed after a right choice somewhere behind the scene, i.e., in a situation just as separate from the right object as the shock is from the wrong one. A young behaviorist once asked me whether, apart from vague concepts, *gestalt* theory had anything new and concrete to offer science upon which it might work. It seems to me that if we had nothing to give behaviorism except our criticism of this method and some suggestions toward a better one, it would be quite sufficient. In man learning seems to depend upon organization. It is highly improbable that the same rule should not apply to habit formation in animals. Therefore, when we investigate sensory discrimination in animals, instead of separating those "stimuli" from each other, the coöperation of which most probably determines learning, we ought to make that coöperation as easy as we can. Some years ago I proposed the following procedure: If the wrong object made a sudden movement against the animal, whenever the latter made the wrong choice, we should certainly have a situation much more similar to the animal's learning in common life, and a more efficient one, because then the negative stimulus would immediately imbue itself with "negativity." [1] It is quite possible that a variation of procedure in this direction would save the experimenter much time. In addition to this practical reason, however, it seems to me a sound law of experimentation that conditions should be varied in *all* respects. If behaviorists are interested in finding the conditions and the nature of learning, they may have great success some day if they vary

[1] *The Ped. Seminary*, 32, p. 681, 1925.

the conditions of habit formation according to the principles of organization.

These remarks apply to the formation of so-called conditioned reflexes in the same way that they do to older methods used to study learning. From the viewpoint of machine theory it sounds a little nicer if we talk about conditioned reflexes instead of associations. I do not find, however, that the first concept is clearer than the second. One might even say that what we call a conditioned reflex now is just one special case of a certain type of association, because it is evident that the "stimulus," which is to be artificially connected with a reflex, cannot be made to produce that reflex except by first becoming "connected" with an *adequate* "stimulus" which naturally produces that reflex. This means an association of two sensory processes however. The association may become so strong that *via* the mere trace of the adequate sensory process the new one will eventually arouse the reflex. But since that association is the thing to be "learned," before a conditioned reflex can occur, and since the association of two processes is a direct effect of their organization into one whole, the same consequences have to be deduced here as in human association and in the old form of animal discrimination experiments. As yet no one seems to have examined the question whether a change in the presentation of the "artificial stimulus," in its relation to the natural one, exerts an influence upon the building up of the conditioned reflex. A bell rings and food is shown or given, but no attention is paid to those experimental conditions upon which organization would depend. That is not at all astonishing since the old concept of "connecting two separate processes by learning" still prevails. On the other hand, animal psychology has an opportunity here to test the value of two assumptions at the same time, namely, that conditioning is practically the same thing as associating two sensory processes, and that associating depends upon organization.

When seen from the viewpoint we have reached in this chapter, certain earlier discussions will assume a somewhat surprising aspect. Our conclusion is that association depends upon organization because association is just an after-effect of an organized process. Now, when we first introduced the concept of organization, we were hampered at every step by those empiristic hypotheses which sought to explain by the influence of meaning or other affiliated factors whatever experiences did not fit into the scheme of mosaic or machine theory. In many of those cases I hope that I have shown that, fundamentally, the experiences in question cannot be derived from previous learning in any form; and that, therefore, organization has to be accepted as an original aspect of our experiences. Now we may go further and make the statement that, on the contrary, wherever the influences of past experience can really be discovered in actual experience, this secondary effect itself is a consequence of past organization, because, generally, meaning will accompany an actual experience or be contained in it by a process of reproduction, and this reproduction will be based upon the trace of a larger organized whole. Consequently, if we try to reduce organization in principle to associated meanings, we must explain actual organization by the traces of previous organization in a larger area. If our theory is right, the whole procedure would mean a vicious circle, for organization would be the more fundamental functional concept, and association or meaning would be ideas depending upon it. This refers to a question of principle. I repeat, however, that very often our actual experiences *are* really and thoroughly imbued with meaning. In these cases it should be acknowledged as a fact that the after-effects of previous organization determine the properties of actual experience. We may talk about an association under these circumstances if we keep in mind that even here it is the concept of organization which remains decisive functionally.

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IX

Reproduction

PSYCHOLOGY knows three main topics which are to be investigated in the field of memory and habit: (1) learning and those processes the traces of which make reproduction and recognition possible, (2) the career of these traces in the time between their formation and recall, and (3) the processes of reproduction and recognition themselves. In some degree reproduction or recognition will be needed in the investigation of *all* these problems, to give us evidence of either the laws of learning, or those of retention, or those of recall itself. Nevertheless, in the first case we can keep conditions constant with respect to retention, and also with respect to the circumstances of reproduction, while the conditions of learning are varied; in the second case, the conditions of learning and those of reproduction will remain constant, those of the interval between learning and recall being varied; and in the last case, we vary only the circumstances of reproduction or recognition. So we are able practically to separate the three classes of problems. In this chapter our attention will be directed mainly to questions of retention and recall, though we shall also have to treat one highly important feature which may as well be regarded as belonging to the topic of learning, i.e., the formation of traces.

In the sixth chapter I mentioned certain experiments in which, after learning to choose one side of a pair, for instance the darker of two grays, animals had to react to a new pair, consisting of the "right" object of the learning period and a new object, which had more or less the same relation to the "right" object as this had had to the "wrong" object during learning. The result was that in the majority of the trials the animals chose that side which, in the new pair, played the same rôle which the right object had played during learning. This means that they chose the *new* object which, in our example, was now "the darker side of the pair."

This result is not quite general, however. It depends upon what interval of time elapses between the training with the old pair and the first trials with the new one. Occasionally, after the learning period, a chick was given the new pair in between choices on the old one; and this procedure was repeated until the single trials with the new pair, each of them distributed between trials with the old one, could be regarded as numerous enough. It was found that, under these circumstances, the animal chose the "right" object of the learning period quite as often as the new object, which would now be the right side *in the pair*. Obviously, this must be explained in the following manner: When the chick reacts to the group before him, the chick sees[1] it as a pair in which either gray plays a definite rôle as the dark or the bright side of it; but at the same time the first is seen as a more or less definite dark gray and the second as a bright one. Both ways of seeing them are altogether compatible with each other as long as the pair is not "transposed." If during learning the chick reacts to the right object as one side of the pair and also as a more or less definite gray, there must be two products of training which will become opposed to each other as soon as we introduce the new pair, because now the first product of learning will favor the choice of one object, and the second the choice of the other. Let us suppose, now, that these different effects of learning do not fade equally fast with time; an increase of the interval between trials with the old pair and trials with the new one will favor those reactions which depend upon the more enduring product of training. From our experiment it follows, then, that the habit depending upon the pair as a whole is more enduring than the habit depending upon the nuance *as such,* since the pair as a whole is relatively less decisive when the animal reacts to the new pair immediately after some trials with the old one. But the two habits are alike

[1] Here as always when I talk about animals I use terms like "seeing" for simplicity's sake. Whether or not the chick experiences a visual field or anything else, the terms have a definite functional meaning in which alone I am interested.

with the exception only that one of them depends upon the organization of a trace whereas the other depends upon the trace of a certain nuance of brightness. Hence, in the course of time, the traces of organization remain much more stable than do the traces of a more or less definite gray. This may be a more general fact. It has been remarked by several psychologists that we are frequently able to remember the general structure of something at a time when we cannot get the more particular content of it at all. It would be worth while to test this statement by appropriate experimentation, particularly since we might get some insight thereby into the nature of so-called "concepts." In the case of the chick it was very easy to examine the hypothesis by having the same animal react to the new pair once more, after some minutes had elapsed since his last trials with the old one. The result was a large number of "relative" reactions.

There is much to discover in this field. At the beginning of the last chapter we noticed one way of studying the career of traces by investigating successive comparison. Here we have a second way. A third one, somewhat similar to the second, is also to be found in certain observations on animals. I shall treat it as an example of what new problems *gestalt* psychology raises in a concrete case. When Yarbrough[1] investigated delayed reaction in cats he

[1] *The Journal of Animal Behavior*, 7, p. 87 ff., 1917.

found them able to react correctly after no more than four seconds if they had to choose between three objects, whereas in the case of two objects the delay could be increased to more than four times that amount. Why is the result so much greater in the second case? An explanation may be derived from the examination of a human subject in a similar, though more difficult, task. If I have 25 objects, all of which have the same properties, before me in a semicircle, they play very different rôles in the semicircle as a whole; the rôle of two of them is particularly well defined and characteristic, namely, that of the first to the left and that of the first to the right side, which are the "ends" of the whole; in some degree, but much less, a third one will be characterized as a special member of the group, i.e., the central one. The rest will be more or less like an indifferent filling. Suppose now that the experimenter points to one of the objects, and that, after a delay in which I do not fixate that object carefully, I am told to go toward it; my reaction will always be correct if one of those three objects had been pointed to. However, as long as I do not mark the right object in an indirect way by counting at the first presentation, in order to find it again by counting after the delay, wrong reactions will occur rather often when the designated object has an indifferent position. Thus I may choose the sixteenth instead of the seventeenth, or the eighth instead of the ninth, rather easily, and, if the delay is increased or my attitude is slightly dreamy at the first presentation, such errors may become rather frequent. Of course, this is the same dependence of delayed reaction upon the more or less specific position of a member in a group, which was demonstrated by Hertz in certain birds (cf. pp. 158-162), so that we may apply the same idea to the case of a cat which must choose between three objects after a delay. If the cue which the cat has acquired before the delay refers to the first object (for instance, at the left), the cue will be connected with a very definite place in the whole group; and the same will be true if the third object (the first to the right) becomes connected with the cue. If, however, the second object is connected with the cue before the delay, this cue will refer to a place which, for the cat, is much less definitely characterized in the group. At the time when that object is marked out (by a light, for instance) among the others, its rôle in the whole may be sufficiently clear for some moments; but chances are that after some time the cue will lose its rather unstable localization, the whole becoming just a stretch, only the ends of which remain well-characterized places. Consequently, the animal will react correctly less often when, instead of two objects, the experiment is made with three. The cat might begin to react more correctly again, perhaps, if the experimenter gave the three objects another distribution in space so that all of them would have well-characterized locations. The reader will be inclined to say that such an explanation appears somewhat artificial to him, since, with three objects, the probability of error is increased anyhow and the cue cannot be fixed so easily in a more complicated situation. This objection should be examined by introducing that change of conditions to which I have just alluded. From the viewpoint of organization, "complication" is not simply a matter of number, but also of distribution in space. If, therefore, the three objects are distributed in a way which characterizes each one equally well, we may be able to decide whether or not we should accept the explanation in terms of mere increase of number. There is one result in Yarbrough's experiments which makes me more inclined toward the other hypothesis: If mere number, as against lack of definite rôle in the group, were the decisive condition producing failure, the false reactions of the animals should be distributed in a haphazard manner among the three objects. But this is not the case. After a long delay (beyond 4 seconds) *some of the cats did not go to the second object at all.* All their reactions were directed toward the first or the third object! This is just what we should expect in terms of our explanation, whereas one cannot understand it at all without taking account of organization. We may say, then, with some confidence that in cats the traces of past events undergo a rapid transformation which destroys the minor or less stable characteristics of groups as wholes, so that subsequent behavior depends upon a more simplified organization than existed in the traces at first.

A somewhat similar observation was made by Mr. Tinklepaugh and me when, using another method, we performed some experiments on delayed reaction with a monkey. A very large square on the ground was covered with sand some inches high. Before the animal certain marks were made on the sand, as, for instance, a hill of the same material or, in another experiment, a straight line which we drew on the surface with a finger. After this preparation, food was buried in the sand in a place which, for the human subject, was characterized at once as having a rather definite position *near* the mark. We wished to see whether the animal would use the mark in fixing the place of the food because, without a mark in the homogeneous sand, his previous reactions to buried food had not been very clear. The monkey who had observed those preparations was not released from his place until some time had elapsed. When allowed to approach the sand he would go *to the mark* at once and search for the food *in it*. As far as I can remember now, he never searched *near*, i.e., around the mark. Though further observations would be desirable, one explanation seems probable at once: As in the last case where delayed reaction was investigated in cats, the reaction of the animal depends upon a trace and upon the actual organization of the field. This field is well organized, the hill or the line forming an outstanding feature in it. The localization of the place of the hidden food, however, is much less definite. Therefore, we may assume that a simplification occurs in this case similar to the one which determines the reactions of the cats in Yarbrough's experiments with three objects. The trace which remains in the monkey will be transformed during the delay to the disadvantage of its less defined regions and the result will be that the monkey's cue is absorbed by the outstanding mark just as, after some delay, the cats react only to the well-characterized parts of the situation. It may be that the method used by us in this preliminary way will be developed some day into a more accurate instrument for investigating the career of traces in animals.[1]

Traces are not rigid, then; there are definite dynamical tendencies in them; and in animals the traces may be transformed more easily than in man. That being the case, the study of delayed reaction in animals becomes highly important for general psychology because it may show us the working of those tendencies in a measure which could not be observed in man.

[1] Something of the kind has been done in the meantime by Mr. Tinklepaugh insofar as his "substitution method" is an excellent means for studying certain properties of the traces. (*The Journal of Comparative Psychology, 8*, p. 224 ff., 1928.) Cf. also *Psychol. Forsch., 1*, p. 10, 1921.

There can be no doubt that, in principle, we find similar changes in man also. Koffka and Wulf[1] have observed these changes by making their subjects draw figures which they had seen previously for a few seconds, first after a delay of some minutes and then after days or even weeks. The figures had not simply lost details at the time of graphic reproduction. The changes were much more interesting since they showed two opposite directions: In reproduction the figures were either more regular than the original ones or some traits which might be taken as characteristic of their organization were considerably exaggerated in the drawings. The authors give the explanation that, during the first presentation, the figures could be seen in somewhat different kinds of organization. If this organization was a rather simple and regular form, the change of the trace would be in the direction of increased regularity; if, however, some articulation, irregularity or lack of symmetry was seen as the main characteristic of the figure, the transformation would enhance this property. Therefore, in both cases the change seemed to produce an approach toward something like an extreme type or the ideal of the first organization.

In all these cases behavior depending upon the actual properties of the traces is used as an indicator of their changes. Still there are other cases where

[1] *Psychol. Forsch.*, *1*, p. 333 ff., 1922. Cf. also Werner *Ztschr. f. Psychol.*, *94*, 1924.

the traces can be shown to be practically unchanged, although reproduction is difficult or even impossible under the circumstances in question. In the last chapter we mentioned some examples of this kind. Now we have to treat others which will show us that the possibilities of reproduction are much more restricted than one might expect.

If association as a basis for reproduction is just another expression of the fact that the traces of functional wholes are themselves detached wholes, one might somewhat rashly deduce from this theory that, after a trace is formed, any group of stimuli which represents a considerable fraction of the original constellation of stimuli will reproduce those parts of the original process the stimuli for which are not actually given. However, this would mean a complete misunderstanding of *gestalt* theory, because from what we have seen in the sixth chapter, it follows that between the properties of an organized whole and the actual constellation of stimuli, let us say on the retina, there does not exist a correspondence as to parts, as though one region of the organized whole separately depended upon some definite fraction of that constellation, a second one upon another and so forth. On the contrary, upon a large group of stimuli and their "relative properties" depends the organized process as a whole in a manner which cannot be analyzed into independent local or partial effects of stimulation. Therefore, in general, if an organized process has left its trace in the nervous system, some fraction of the original group of stimuli will not determine a partial process which occurred in that functional whole, but something different and new, a picture of which is not contained in the trace of that whole. Consequently this new process will not be able to reproduce anything on the basis of that trace; just as some other group of stimuli which we might choose in an altogether arbitrary manner would determine a new process and conse-

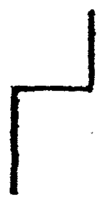

FIG. 20

FIG. 21

quently would be foreign to that trace. For instance, Fig. 20 will not reproduce the missing lines of an H normally, though, geometrically, that figure is the larger part of an H. Neither will Fig. 21 reproduce the missing lines of an R, and so forth, because, when we have seen an H or an R, we have never had the given figures as real experiences. There is nothing in the traces of an H and an R, which might correspond to the processes of Figs. 20 and 21, and reproduction does not occur. We shall conclude, then, that reproduction will be restricted to those cases where the process intended as the excitant of reproduction is sufficiently similar to a partial process of the original whole. This will be the case if the process intended as an excitant corresponds to a natural member or sub-whole of the original organization, because under these circumstances the trace of that sub-whole, though not identical with the exciting process, will be similar to it, at least. If this similarity is marked enough, reproduction will occur under favorable conditions. So U. S. will reproduce an A. rather easily, and the stars the rest of the American flag, because here we give as an excitant what was present in the original organization as a *relatively* independent region. This fulfills the chief requirement. If we draw a profile from the nose downwards to the chin, this line does not correspond to a complete sub-whole of a face. Nevertheless, since the influences exerted upon that outline by the organization of a complete profile are not so very strong, the process determined by that line will be sufficiently similar to the corresponding process in the whole profile and to the corresponding trace, so that reproduction can occur.

Even so, we find reproduction much more difficult than is supposed in current empiristic theories. It seems to be restricted to a rather narrow path between a Scylla and a Charybdis. Association is necessary for reproduction, and association means a sufficient degree of organization. Reproduction.

however, presupposes a certain degree of similarity between an actual process as an excitant and some region of the organized trace. The more a region is absorbed by a larger organization, the less will corresponding stimulation be able to effect reproduction. We have already described the narrow path that any possible reproduction must take between these limiting conditions.

To demonstrate the foregoing contentions I made

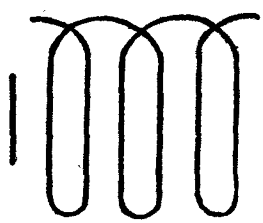

FIG. 22

FIG. 23

the following experiment: The subjects are shown simultaneously pairs of figures for a short time. After a while parts of those drawings are presented to them with the instruction to reproduce the missing lines. Now, in a case like that of Fig. 22, for instance, either the vertical at the left was presented or Fig. 23, which represents *geometrically* a much larger part of the original. With the first as an excitant, however, we get many more correct reproductions than with the second. Of course! The "larger part" is something which in the first exposure did not occur as a real form. Even the first vertical to the left has now lost its reproducing power, because in the original pair it had been segregated as something apart, whereas now it has become the left limiting end of a regular series of parallels.

If this last point proves that a trace does not respond by reproduction if the organization of the excitant becomes foreign by being embedded in a somewhat different environment, reproduction will be even much more restricted than we should have previously supposed. Evidently, in problems of memory and habit, not only the organization given at the time of first "association" will be decisive for reproduction, but organization existing in the actual field will be equally important in determining or preventing reproduction. When given in a certain environment a certain process may be an excellent incentive to reproduction on the basis of some trace existing in the nervous system. But it will not occur regularly again in just that environment which was given when the trace was formed. Hence the chances are that, quite apart from the rather crude obstacles considered above, even a slight change of the surrounding field may make an excitant unable to reproduce what has been "well connected" with it in the past. That this is true can be deduced from Nagel's experiments (cf. above p. 283). In a well-learned series of nonsense syllables each member, though embedded in the whole series, would certainly seem to be a thing by itself. But if one of them is given alone as an incentive to reproduction, this change of environment is sufficient in the majority of cases to make reproduction impossible.

The same influence of actual organization upon reproduction has been demonstrated in a rather surprising form by Shepard and Fogelsonger.[1] These psychologists made their subjects learn pairs of syllables. Some of these pairs had identical second members. (Between the first occurrence of such a syllable and its repetition there was an interval of 25 minutes.) At the time of testing the product of learning, the first syllable of a pair was given as the excitant of reproduction; but where two syllables had been followed by the same second member at different times, both were given together as incentives to reproduction of their common partner. If it were not for organization one should expect that, both excitants working in the same direction, the syllable associated with them would be more easily reproduced than a syllable for which there was only one excitant. But the contrary was observed; it seemed as though some inhibition were in the way of reproduction when it was aroused by two excitants. The disturbance was particularly striking when both syllables were presented simultaneously, but it also existed when they were given in rapid succession. The explanation seems to be that during learning the subjects had always had a single first syllable together with its partner, and that when, in the critical cases

[1] *Psychol. Review, 20,* 1913.

of reproduction, two syllables appeared before them, both would look so foreign at first in this new grouping that neither could immediately reproduce the common partner. This explanation has been confirmed by observation. The subjects reported that reproduction became possible through an attitude of analysis which would sufficiently isolate one of the syllables. Furthermore, from this explanation it follows that any extraneous syllable, one which never appeared in the learning series, would have the same effect when presented together with the first member of a learned pair. The authors have found this to be the case, in fact. Thus the explanation seems to be quite verified. Our conclusion is that even a very slight alteration of circumstances, if only it influences the organization of the excitants, or incentives, will make reproduction difficult or impossible.

A similar result was obtained by Frings in his work on inhibitions,[1] though his problem refers to learning more than to reproduction. In the classical experiments it had been shown that, if a syllable A has become associated with a syllable B, the same A can not be as easily associated with a third syllable C as could any indifferent syllable. Also, after A has become associated with C as well as with B, when A is used as an excitant of reproduction, the competition of the two different tendencies to reproduction will

[1] *Arch. f. d. ges. Psychol.*, *30*, 1914. These experiments were planned by Bühler.

have an inhibiting effect. Frings was able to show that under certain circumstances these inhibitions may completely disappear. His subjects were made to learn series of syllables, the instruction being that the syllables should be read and memorized as groups of the anapest rhythm in which, after two less accentuated members, the third one follows with the main accent. In such a group the first two members will form a sub-whole. When after a while the product of learning is tested, these two members are given as an excitant, and the last one is to be reproduced. If now, in a given case, a group like (ac)d occurs in one series and a group like (bc)e in another series, we should expect the association between c and e to be inhibited, because c was first followed by d, and later followed by e. Similarly, *after* associating (bc)e as well as (ac)d, in spite of that inhibition, the subjects should have difficulty in reproducing e, if bc is given, or d, if ac is given. From the viewpoint of organization, however, we must realize that in (ac)d the syllable c is a member of the sub-whole (ac), whereas in (bc)e it is a member in a different sub-whole (bc), and that therefore c is not quite the same thing in both cases. We might also say that in the first case c has not been associated with d, but rather the sub-whole (ac), and again that in the other case (bc), not c, becomes or has become associated with e. Considering this, we should predict that there will be inhibition neither in learning nor in reproduction. Experimentation has confirmed this view. Wherever c figured as a member of two different sub-wholes, no inhibition occurred. It is particularly interesting, however, that inhibition occurred immediately if a subject had been very fatigued at the time of learning, and therefore had not been able to grasp the syllables in rhythmic complexes, according to the instructions.

Though the reader will have already noticed that, in consequence of varying organization, experimental findings may differ considerably from what ought to occur according to the classical laws of association and reproduction, the most radical restriction of these rules remains to be considered. We cannot proceed, however, without first discussing a certain more general topic.

In one of the preceding chapters we laid some stress on the point that our "self" occurs as an experienced whole in the same field which contains our experiences of surrounding objects and events. Consequently, that field of our brain, as a physical system which is the locus of the processes underlying objective experience, will also contain processes corresponding to the experience of the "self." In many respects, the processes belonging to this particular whole are different from those corresponding to "outside" experiences, though, with regard to dynamical interrelation, the processes corresponding to the "self" prove to be true members of the total field. Two examples will suffice to corroborate this point.

If a certain thing moves objectively in the visual field, I shall, under the usual conditions, see that thing as moving. But we know many instances in which, *objectively,* one thing being in motion and another at rest, the first *seems* to be at rest and the second to move. This is not an "accidental illusion," moreover. It depends upon definite circumstances and necessarily prevails whenever these are present. Every one has occasionally seen the moon in rapid motion when clouds were passing it in the opposite direction. When a point on the window of a moving railroad carriage is fixated, the objects outside at once begin to glide in the opposite direction. We may say, then, that any change of spatial relations in a part of the field may have dynamical effects upon those parts of the whole field which objectively are not moved. With regard to the spatial relation of the "external" environment and the "self," precisely the same observation is frequently made, as we all know. If our environment is, objectively, turned around us with appropriate speed, the result in experience will be that we feel ourselves turning around in the opposite direction. Michotte and Gatti have recently shown that the same effect may be produced by moving two handles toward one side, the subject's hands being turned passively with the handles; once more the body (as an experience) seems to turn in the opposite direction. We may draw the conclusion that under adequate conditions a change of spatial relations between the processes underlying our experience of external bodies and those corresponding to the experience of our own body, will have dynamical effects upon the latter processes, just as the processes corresponding to the experience of moving clouds exert a dynamical influence upon the processes underlying our experience of the moon. It becomes obvious, therefore, that we must include the self in the dynamical interrelation of the whole field.

As a second example, I refer to the fact that just as things or spots frequently appear before me as grouped in a definite manner, the self is very often experienced as a member of such a group. Of course, if I put my hands on a desk before me and another person does the same on the opposite side of the desk, I have two pairs of hands before me as visual groups. But the whole self may enter a group just as easily: If somebody accompanies me along a street and, ahead of *us* another pair is walking, I feel (and partially see) myself as a member of one of two groups.

Until now the reader may have restricted the concept of organization more or less to the region of external experiences. From the last examples it seems to follow that organization is an affair involving the whole field and that the "self" is included as the most interesting member of this larger organization. At first one may feel opposed to this treatment of the "self" because, in a great many respects, it remains something particular and apart. During all the radical changes which the outside field undergoes in the course of time the "self" under normal conditions does not lose its identity. At the same time, and notwithstanding that identity, inner experiences will generally be felt as much more lively and dynamical than the average objective experiences. Although these differences may exert an enormous influence upon the actual organization of the whole field, nevertheless they do not place the self beyond organization *as such*. Sometimes they confer upon the self a central position in the field. But for a great many persons not even this is permanently the case, especially since external experience contains other *persons* in social life, who may occasionally become the center of our total field instead of the "self."

With this remark, however, we have gone far beyond the discussions of the previous chapters. We have enlarged our view of dynamics as determining the field and its changes.

In external experience grouping may be "static" as, for instance, in the case of indifferent patches, a group of which would seem to correspond to a state of equilibrium in the nervous system. On the other hand, we have seen that nothing is more likely to determine grouping and organization in general than the direction of attitude and of visible behavior. Even a behaviorist like Watson describes the behavior of a child as being directed to or away from an object. In some situations a barking dog will be experienced by any observer as barking *at* something definite in his surroundings. If, therefore, we apply the term "dynamical" here in a more special meaning, we may say, perhaps, that the most compulsory organization which can occur in experience is a dynamical event or attitude, consisting of one member from which it issues, and another one toward which it is directed. Sometimes, as in the case of "avoiding" or "yielding," it would be a better description to say that one member exhibits an attitude directed *away* from the second member of such a whole. In both cases the type of organization remains the same, the main feature being the bipolar structure and the dynamical directedness of the whole.

This type of dynamical structure occurs not only in merely external experience but also between the experienced self and parts of the experienced environment. We may even say that, apart from drowsiness and similar states of low vitality, the organization of the total field will almost always have just that bipolar character, the self being directed to something else or away from it. The reader will remember our discussion of visual organization. Some diagrams were presented as examples of it. Organization was considered as a property of the visual field. This account, however, was not a complete description of the total field, since it included neither the self as a member of the field nor that particular attitude of so-called attention or interest which, as it were, was a direction in the field from the self to the diagrams. All psychologists know that, normally, this direction will coincide with that of fixation. It becomes particularly striking, however, if, while fixating a given point, we "direct ourselves" successively toward the other objects to be seen in the vicinity of that point.

Obviously, bipolar organization varies in more respects than in direction and in the type of external object. All the nuances of emotional attitude toward objects and events are phases of it. When we like or dislike, when we hate or admire something, that bipolarity *as such* remains the same. Sometimes, the direction is opposite: an explosion occurs in our neighborhood and we feel ourself frightened away; when confronted with a particularly imposing or overbearing man, we may also feel thrown back.

Bipolar organization reminds one of all those cases in physics in which either lines of force, or a process with a definite direction, develops between two parts of a field, depending upon the actual properties of those parts in their relation to each other. In *gestalt* theory the varieties of directed attitude are not considered as the operation of preexisting drives or instincts, but rather as the effects of actual situations. In this connection, however, the term "situation" has to be used with some care. Evidently it is not only the external situation which in a great many cases has to be considered, but the internal situation of the organism as well. This is so well known through recent studies on hunger, sexual behavior, and so forth, that I scarcely need mention the fact. Even after the adult has developed well-established preferences and forms of conduct in relation to these internal forces the corresponding attitudes will not appear unless the inner situation of the organism requires it. This does not mean that the intensity of those forces is independent of the external situation. But we may say, at least, that without any appropriate objects the internal situation will produce very strong expressive effects corresponding to it, whereas, on the other hand, when complete "saturation" is reached, even the most adequate object will not produce a corresponding appropriate attitude.

If we compare bipolar organization with a field of force or a stress existing between regions of different potential, these words may contain more than a superficial analogy. What we experience as our "self" depends first of all upon the inner situation of our organism as a physiological system. We may also say that the particular processes underlying the experience of our "self" are determined by the ever-changing activities of the organism. Perhaps it is not too bold an hypothesis to suggest that, according to the actual nature of those processes, in the first place, and the properties of processes underlying objective experience, in the second place, something like a field of force or stress originates between them. Since, in terms of our general principle, the organization of experience is a picture of underlying physiological organization, we can hardly find a more fitting assumption. In some cases, however, the directed attitude of the self toward external experience does not seem to depend upon the inner situation of the organism, as much as it does in the case of hunger and sex. After being alone for some weeks most persons will feel an all but insuperable "drive" toward social contact, even with strangers. It is difficult to understand, at the present time, how this directed attitude should depend upon the physiological situation of the organism as, for instance, hunger depends upon it. Nevertheless, for the most part, this attitude is quite similar to the need for food, and I do not hesitate to interpret it as a stress in the field between the self and those particular surrounding processes which are the physiological correlate of our experience of other persons.

The play of all these stresses, their origins, the strain exerted by them upon different parts of the total field, the changes which may be the consequence of that strain, and the cessation of stresses and strains which follows certain of those changes—this is the major study of psychology, as it is the major content of our life.[1] As yet experimental psychology has little information for us on this theme, and in these chapters we have not given it its due place, either. The reason for this neglect is my wish to introduce the concept of dynamical organization by applying it to simpler problems. After we become acquainted with the concept in the realm of sensory experience we may extend its use to the total field—and even beyond it. It is expedient, however, to return to sensory organization time and again so that a feeling of continuity and scientific prudence may accompany us on our way.

For instance, if the reader should feel a break between what has been discussed in previous chapters and our present statements, let him consider the following point with me. In the sensory field segregation and grouping have been regarded as products of dynamical intercourse. If directed attitude, as occurring between the self and the environment, is to be regarded as belonging to the same general class of physiological dynamics, we can expect it to have similar effects. This is the case, indeed, for, in the total field including the self, we find grouping dependent upon those directed attitudes. If in discussion two scientists argue against a third one, the field of each of them is organized according to this

[1] It will be the theme of the last chapter.

particular social situation and the direction of attitudes in it. They are not simply "three" men; they are a group of two, with one man external to, or rather opposed to, it. At the same time another grouping might be more natural from the viewpoint of purely *visual* organization, because, spatially, the third man may be nearer to one of his opponents than this opponent is to the second opponent. Nevertheless, if in the middle of the discussion one of the three happened to perceive this other, visual grouping, for some reason, he would realize that what he had previously experienced as the organization of his field was quite as real as now the new grouping is. Grouping based upon social forces is much more interesting than purely visual organization, however, because their members experience definitely directed attitudes between social groups which do not generally appear in the same lively manner in the situation in which a number of things appear as two groups solely for optical reasons. The reader will easily find similar examples in his daily life. Four people sitting symmetrically at a desk may experience themselves in several kinds of grouping. When they are playing bridge, however, two groups, consisting of two members sitting opposite in space, will immediately originate in the field of each of the four. A great many problems of social psychology, some of them much more important and serious than these, will acquire a new aspect as soon as we consider them in such a concrete manner. For the present our examples must suffice as a demonstration of the fact that directed attitudes influence organization and grouping quite as much as visual factors do. Therefore we are justified in applying the concept of organization to the total field including the self and the stresses occurring between it and its environment.

With the concept of the total field in mind we return to the discussion of association and reproduction.

First of all, we realize that it is incorrect to treat the problems of learning and memory as though they referred only to sensory experience. What is called an "association" has been found to be the trace of organized processes. If now we see that in the total field, including the self and its attitudes, we have as much organization as in sensory experience, one consequence will be that all the actual total fields which occur in the course of a biography may leave organized traces. Obviously we can remember our attitudes as well as those experiences toward which they were directed; and reproduction may proceed from an attitude toward its object, or vice versa, just as one objective experience may remind us of another.

But something more important seems to follow from our concept of the total field. The following experience is common: I have a task which, perhaps, I do not like, but which is urgent. In the course of the day I find myself occupied by a great many other things. I talk with friends, read a book, and so forth. But time and again something like a dark pressure appears somewhere in the field and, if I examine it, it will be found to issue from that task. Here we have a persistent tendency toward reproduction, or reappearance in the actual field, and this perseveration seems to depend upon the particular nature of the trace in question. It is in connection with this experience that we can understand certain important experiments made by Lewin and Zeigarnik.[1]

The subject is given a number of little tasks, one after the other, as, for instance, to copy some lines from a book; to continue an ornament, the principle of which is given in a sample; to solve a simple mathematical problem; to find twelve towns, the names of which begin with the letter L; and so forth. In some cases the subject is allowed to finish his work, in others the experimenter interrupts him before he has fully accomplished it. After a series of twenty-two tasks, one-half of which is finished, the subject is asked what tasks were given to him. In most cases the report is very characteristic. The first tasks which the subject recalls are those which were interrupted, and the total number of this class which is recalled is much larger than that of the other. When 32 subjects were examined in this way, 26 re-

[1] Cf. *Psychologische Forschung*, 9, 1927.

called more interrupted tasks than finished ones; in 16 subjects the superiority of the former was more than 60 per cent., and in the average of all subjects the superiority was 90 per cent.[1] Care was taken to eliminate the influence of particular properties of the different tasks; all tasks were interrupted exactly as often as they were finished in the case of different subjects. Of 22 tasks, 17 were more frequently recalled after interruption than after completion. When the same experiment was repeated with 47 students, the superiority of recall for interrupted work was again 90 per cent., in the average; in a third experiment with 45 children, it was 110 per cent.

The most plausible explanation points to the fact that, when solving a task, the subject is in a state of stress which usually will not disappear until the solution is accomplished. If the work is interrupted before the solution, the trace of this situation contains that stress. Moreover, just as during the work the stress may be considered as the force which keeps the work going, it seems to have a somewhat similar tendency in the trace. Since recall would be the first step toward finishing the task, we cannot be surprised by the result of this interesting investigation.[2] If this explanation is correct, several consequences may be

[1] This means that if the average person remembered n finished tasks he remembered 90 per cent. more unfinished ones; that is, $n + \frac{90}{100}$ n.

[2] In passing we may remark that, here at least, a relationship becomes discernible between a result of experimental psychology and Freudian theory.

deduced from it. I shall not mention more than one. Under normal conditions we do not expect the stress in a trace to be preserved indefinitely. It is altogether more probable that it should disappear with time. Indeed, when recall was examined after a delay of 24 hours, the superiority of the interrupted tasks had considerably decreased.

The experiments of Lewin and Zeigarnik refer to recall as depending upon the properties of the original situation. A much more difficult problem which we have to discuss is concerned with how far reproduction depends upon the actual total field in which something might operate as an excitant of reproduction. In this connection, almost all experiments on memory should be subjected to serious criticism. Just as we are not investigating automatic associations in experiments in which our subjects are instructed to *learn* nonsense material, we are not examining spontaneous reproduction in experiments in which we give our subjects one nonsense syllable out of the learned series, and then ask them to reproduce the next one. In this case, again, the procedure does not fit the meaning of the rule of association and reproduction. Were we to follow this rule more accurately, we should give the first syllable unexpectedly in some situation and wait for an automatic reproduction of the second. The usual experiment differs absolutely from such a procedure for obvious reasons. If a subject can be directed in several ways toward a part of his actual field, he can also assume very definite attitudes toward something which lies beyond that field, and primarily toward something in the past. We know this attitude very well from instances in which we try to remember the name of an author or of a place. We are certain that such an attitude has an influence upon reproduction. But although the law of association does not mention this condition, the classical investigation of that law quite generally and rather innocently introduces precisely this special condition. Therefore we are not justified in applying the results of that experimental work either to the concept of automatic recall or to reproduction as it may occur spontaneously under other conditions in the total field.

Some time ago almost all psychologists would have said that automatic reproduction in consequence of previous association is the prime motor of mental life and of behavior. Here caution seems to be highly advisable. As yet experimental evidence is strictly opposed to such a view, since it shows that reproduction will not occur unless very special conditions are fulfilled. The most important work in this field has been done by Lewin.[1] Among his experiments there is one in which the problem is examined directly. He made his subjects learn pairs

[1] *Psychologische Forschung, 1* and *2*, 1922. Somewhat similar experiments had been done before by Poppelreuter (*Zeitschrift für Psychologie, 61*, 1912), who was also the first to bring forward the criticism mentioned in the last paragraph.

of syllables either in the usual manner or by some new procedure, which I shall not describe here. After a large number of repetitions, distributed over several days, the subject was given single syllables with the instruction to read them and then to wait passively. If under these circumstances some of the learned syllables are presented among others, one should expect the second members of the learned pairs to be reproduced automatically. In general, however, this was not the case. Even when the instruction was changed to "Tell me the first thing occurring to you after reading each syllable!", the result remained negative, with a few exceptions. It is interesting to examine those cases where the reproduction of the associated syllable did occur. Obviously the subject's attitude is not well defined by the instruction to wait passively. After some "waiting" a particular attitude will almost always be assumed unintentionally. If a syllable appears as a familiar one to the subject, for instance, his attitude may become one of identifying and examining the learned material. However, as soon as he begins to be directed to the old context, in consequence of this attitude, reproduction will occur. Of course, such reproduction cannot be called automatic, since it depends upon a special attitude of the subject.

It does not suffice as a basis for reproduction for the subject to be directed *somehow* to the syllable itself. In these experiments he had to be directed to it as *to a member of a known pair or series;* otherwise, there was no reproduction. In everyday life we may observe the same fact whenever we like. Certainly, well-known objects are sufficiently associated with their names. Nevertheless when we walk along a street, we are successively directed to a great many things. But we are far from reproducing their names. If you say that objects are associated with a great many things besides their names, and that these diverse associations inhibit each other, you admit by this very argument that almost no associations lead normally to appropriate reproductions. When will they do so? If, as a psychologist, you have just stated that you did not reproduce the names of the car, the tower, the door, the window when you noticed them, in keeping with your normal attitude in walking along the street, this statement itself will very probably change your attitude. During the next minute you will probably assume the attitude of naming, and whatever you look at will at once reproduce its name. Therefore, the mutual inhibition of several associations does not seem to be the main factor that prevents reproduction. As soon as your attitude coincides with the direction of existing associations, reproduction occurs at once and without any appreciable effect of inhibition.

It may be a wholesome occupation for a psychologist to make more observations along this line. If he does, it will seem altogether strange to him that our science should have regarded automatic reproduction as the main motor of mental life and behavior for so long. We may congratulate ourselves on the fact that, in this case, unprejudiced experience is strictly opposed to theory. At any given moment in our normal lives we find ourselves occupied with some work, some problem, the subject-matter of some conversation, and so forth. Under these circumstances the total field is well organized; dynamical relationships between the self and its objects form one functional whole, the development of which we call our "working," our "solving" the problem, our "expressing" our own opinion, and so on. I admit that this is a somewhat optimistic description of life. If I return to this manuscript after an interruption of a week my "working" at first does not quite correspond to the description given. At one moment a faint noise will suffice to give me a direction irrelevant to the task, at another something else will affect me similarly. If, even then, I am occupied with a given thing at a given moment, a great many "deflections" of attitude will occur in the course of time until finally the work itself begins to be the development of one functional whole. What about reproduction during such a happy period of work? All the words and concepts occurring in the work itself are associated with other words, concepts and situations which have nothing whatever to do with the development of the work in question. These associations belong to very *different epochs and interests* in my life. If each of them automatically led to the *appropriate reproduction, the field* would become a chaos of incoherent stuff in a few moments, instead of one *identical whole transforming* itself in an orderly manner toward the solution of my one actual task. *Excluding states of insanity,* that evidently does not happen. Even if my attitude is deflected a few times before I become completely absorbed in the work, I am somehow so well directed toward my real task that "distractions" remain precisely what this term means, and instantly, in spite of them, my attitude is bent back to the main task. Automatic reproduction might account for some distractions and disturbances, just as do noises and other foreign experiences, but it cannot explain the normal continuity and persistency of our working, thinking and doing. On the contrary, if there are automatic and independent reproductions, they must be factors of little weight in comparison to the stresses and dynamical tendencies existing in an actual situation. Some of us, it is true, would be inclined to explain the order and continuity of actual work entirely by the force of associations. Originally, they would say, there was indeed a chaos of ever-changing attitudes and reproductions. But in the development from childhood to adult life, some of our attitudes, experiences, and so forth, have become so well associated that, now, continuous currents of them will predominate over a great many disturbances and reproductions of lesser importance. I cannot accept this theory, however. If my writing in English about a problem I have very seldom discussed even in my own language could be pictured as the reproduction of thousands of well-established associations, I might agree with the theory. Unfortunately, no such current of associations is ready-made in me to create what I have to write in these very pages. Nevertheless, I persist in writing about one subject-matter and, I hope, coherently. Therefore, when comparing the relative importance of automatic reproductions with the stresses of the actually developing organization of the field in such a case, the balance will undoubtedly be in favor of the latter. Habit has been enormously overrated in the theoretical treatment of life.

In some of Lewin's experiments this has been demonstrated quite convincingly.[1] I shall describe them in a simplified manner. To begin with, his subjects had to learn pairs of nonsense syllables. Afterwards they were shown certain other syllables serially, the instruction being that in each syllable the first letter should be put in the place of the last and vice versa; whereupon they had to pronounce the result. The time required for this operation was measured. The reader will notice that here we have the situation I was just describing. The subject is

[1] *Op. cit.*

working on a definite task. If, among the second series of syllables, the first one of a *learned* pair is given, automatic reproduction should either bring about a wrong reaction or, at least, inhibit the right one so that, in such a case, the time of reaction would be increased. On the other hand, if a syllable presented for reaction is the first member of a *learned* pair and if the syllable to be formed by the subject is identical with the second member of that learned pair, previous association should facilitate the reaction, and the time of reaction should be decreased. To the author's surprise nothing of the kind was observed. There were no wrong reactions caused by reproduction on the basis of previous association. Where reproduction should have inhibited the subject's response, the time of reaction was usually not raised above the average; where reproduction should have accelerated it, the time was the same as in the case of "neutral" or control syllables.[1] It is obvious then that, under these circumstances, the dynamical development of the total field will follow its own ways or laws, practically independent of the strong associations which some of its parts may have acquired in the past.

Recently Dr. Lewin told me that this fact might be explained, partially at least, in terms of a principle which I discussed at the beginning of this chapter.

[1] From these results it would follow that we are not able directly to measure conation by antagonistic association, as Ach has proposed.

If A has been associated with B, A will not reproduce B, if the properties of A are changed. We know that such a change will occur when, the stimuli corresponding to A being given, A becomes part of a new organization. If during learning a syllable A is read naturally, it is taken simply as a whole. However, if the subject later obeys the instruction to interchange the first and the last letters of the syllable A, he will view the syllable in terms of this task. Consequently, it will appear in a new organization, the first and the last letters being seen as outstanding members of the syllable, or whatever else may be changed by its entrance into a new total field. This might suffice to make A unable to reproduce B. The explanation seems to be corroborated by the observation that in most cases the subjects did not even notice the presentation of *known* syllables among the new ones. In any case, it is advisable to make similar experiments using other material, the properties of which would be more characteristic and less likely to be changed in a new total field. Neither Dr. Lewin nor I yet feel convinced that an adequate theory of the whole matter has been formulated. In some of his experiments the subject's attitude towards the syllables was practically the same during learning as it was during the subsequent testing, but it did not lead to reproduction either, so long as other circumstances remained as I have described them. If objects normally do not effect the automatic reproduction of their names (cf. above pp. 333-335), it would be a bold assumption to make in every case that reproduction did not occur because the properties of the objects had been altered too much by our actual attitude.

In certain very interesting experiments Lewin has finally succeeded in creating reproductions or inhibitions on the basis of previous associations, *against* the task given by the experimental instruction. This was achieved by arranging the whole situation in a special manner. If, in the case of certain syllables, reproduction will effect the same result as would the procedure strictly corresponding to the directions, and if, then, the subject is seduced into relying upon reproduction as the easier way, his attitude may unintentionally become completely one of reproducing. The total field having now acquired this direction temporarily, the next syllable will tend to reproduce its partner, though in this case *reproduction* may be absolutely wrong with respect to the experimental task. Both erroneous reproduction and inhibition of the correct performance were demonstrated by Lewin in these neat experiments. This would almost seem to show that reproductive tendencies cannot influence a given total field before the actual attitude has been transformed into one of reproducing.

Still I hesitate to adhere to such a radical assumption. I take it as definitely established that psychology has gone much too far in admitting the free play of independent and automatic reproductions. But shall we suppose that during our whole lifetime no reproduction will occur without our attitudinal direction being one of reproducing? It may be so. Until much more is known about the matter through further experimentation, however, we should hold our judgment *in suspenso*. Moreover, if actual attitude should be shown to be all but decisive for the reproduction issuing from a part of the field, another equally important problem will become that of the genesis and change of attitudes themselves. Certainly, attitudes do arise, they persist, they change and disappear, for many reasons. And we may safely say that the problem of reproduction is here again. We do not know very much about the reproduction of attitudes *as such*. But the possibility of this kind of reproduction, even in the simplest and most "classical" form, cannot be denied in our present state of knowledge.

In no case shall we be inclined to give our discussion of reproduction an exaggerated meaning. Current theories of habit, association and reproduction, it is true, do not realize how very special the conditions are which must be fulfilled before a well-organized situation is seriously influenced by "foreign" reproductions. It is also true that we are just beginning to see the most important problems in this field. Again it is fortunately true that reproduction will not occur in millions of cases in which we should have predicted it from the viewpoint of the superficial classical theories. But, after all, enough will remain of reproduction for it to be recognized. If, in writing these pages, I am not continually deflected from my task by a great many accidental reproductions, on the other hand, I could not write a single word if I had no reproductions at all. English words are continually though hesitantly arising from somewhere—by reproduction. In writing, my hand moves across the paper in forms much more easily reproduced. There is no question about the occurrence and the *value* of reproduction in almost every moment of our life. The problem is merely to explain why, excluding instances of error, of stupidly repeated habit, and so forth, reproduction should be restricted mainly to those cases *in which it has a value with respect to the actual total field and its dynamical development as a functional whole.*[1]

This is a chapter of criticism and of open problems. I cannot avoid adding one more question which has not yet found adequate attention in all quarters, though von Kries pointed it out thirty years ago.

Suppose A and B have become associated; how does a process A (or A^1, very similar to A), bring about the reproduction of B? In addition to the problems discussed in the preceding pages, here is a

[1] I have not yet been able to see McCarthy's book *The Measurement of Conation* (Chicago, 1926). From what I have heard about it, it would seem to be a highly valuable contribution to the problems discussed in this chapter.

question of great difficulty. In some textbooks, it is true, reproduction is explained easily enough on the basis of machine theory. The process A has been conducted along a definite path in the nervous system; similarly, and at practically the same moment, B has been conducted along another path. Between the central terminal nuclei of both paths, it is assumed that fibers become specially conductive, in consequence of those simultaneous processes in the central nuclei of paths A and B. From this view-

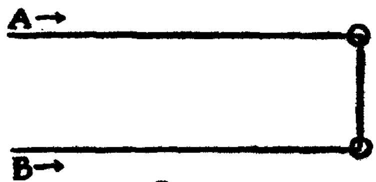

FIG. 24

point reproduction will occur later on when a new process A (or A [1]) is conducted along the same path to the same final station which, now, is especially well connected with the terminal nucleus of path B. Taking the line of least resistance, the process will spread to the place of B and reactivate it. Every one knows figures like this (Fig. 24) which, neglecting some minor complications and giving simple lines instead of neurones, contains the whole functional scheme involved in that idea. Obviously, this is purely a machine theory, in which reproduction does not depend upon the properties of associated processes. If A reproduces B, this is the case because, once and for all, the process A is conducted along one definite path to a nucleus of termination from which one set of conductive mechanisms—in the direction of B's nucleus—has previously been made more pervious and permeable than *any* others.

The nature of this scheme makes it unfit to explain real reproduction, for two reasons:

1) If a process X, altogether different from A, should happen to be conducted along A's path to its final nucleus, it would take the line of least resistance from there, spread to the nucleus of B and reproduce it, though X has never occurred together with B a single time. You object, claiming that this scheme was to be applied only to such processes as had occurred simultaneously before? But in the scheme itself such a restriction is not mentioned; in any case we cannot deduce from the scheme why it should be so. Therefore, the scheme will not explain one essential condition of reproduction.

2) If the process A should happen to start from quite another point of the receptor organ along another path, it will not be conducted to that cerebral area which has been especially connected with the cerebral nucleus of B. Consequently there is no reason why it should spread to B rather than to any other cells. This means that, in such a case, A will not reproduce B, though it is associated with it. If, under experimental conditions, however, a process A, starting from one region of the retina, has been

associated with another process B, a process A (or A [1]) projected upon quite another part of the retina, will reproduce B without great difficulty. So much has been shown by E. Becher.[1]

If you object that between the new path of A and the cerebral area of B there *may* also be some highly conductive path, you introduce chance conditions instead of an explanation by definite acquired machine arrangements, i.e., you give up your claim to precise explanation. Moreover, if coming from this new point of origin A should happen to find such readily conductive paths to B, the same might conceivably be true of all possible processes D, E, F and so forth, which, therefore, should "reproduce" B at any time without previous association.

The weakness of the whole scheme is, then, that it makes reproduction depend upon *the specific location of paths and nuclei* in each particular case, as though such simple location were a true representative of definite process. Only if this were so, might a theory of reproduction become a purely topographical matter. In the optic sector of the nervous system, at least, a given process may be conducted in one bundle of fibers at one time, and another process may be conducted in the same bundle shortly afterwards. Furthermore, the same process may be conducted in

[1] *Gehirn und Seele,* 1911. Lashley has done similar experiments with rats. I can confirm his findings: after learning certain visual discriminations with one eye blindfolded, a chick will react according to the new habit when, afterwards, the other eye is blindfolded. Becher's experiments, however, still seem to me to be more conclusive anatomically.

certain fibers now, and in other fibers later on. Therefore, the necessary requirement for making the machine theory of explanation possible is not fulfilled in the case of our most important sense organ. In the optical system the correlation between the different kinds of process and the different localities in which they may occur is very near to zero. This should destroy all hope of treating reproduction exclusively in terms of topographical conditions.

I shall admit at once that, in this respect, we are as yet no better off with the hypothesis about association which was proposed in the last chapter. It is similar to the old view insofar as it assumes a physiological trace of (AB) and insofar as, in each case, this trace must have some *place* in the nervous system. If, then, the process A enters the nervous system from a different point of origin and along another path, how does it arrive at the specific locus of that trace? One feels inclined to give a dynamical explanation. The similarity between A and its trace might work *as though* A were attracted to the trace. I shall not venture an explanation here, however, for a very simple reason. The problem is formulated as if automatic reproduction were the simplest and most common event in the world. But we have seen that it probably is not. Perhaps, when we learn a little more about the concrete rules and conditions of reproduction *as such,* we shall be better able to treat its physiological basis.

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X

Insight

IF habit and reproduction are not to be regarded as the main motors of so-called mental life, what shall we say are the real ones? To this question there is one answer, not well formulated but implicitly accepted, which we may call the layman's belief. It is the layman's belief that, in general, he himself directly feels *why* at one time he has one attitude, and later on another; also that, for the most part, he knows and understands directly *why* he is inclined to do one thing in a certain particular situation and *why* a definitely different thing under subsequent different conditions. In his view, then, he is experiencing directly and truly much of that dynamical context, the development of which constitutes mental life. Opposed to this belief and altogether foreign to it, we have the view of most learned psychologists at the present time. From their viewpoint, one is inclined to do one thing now and then another, because, in the first instance, certain nerve paths are most available and, in the second instance, certain other paths are most open. Fortunate those people in whom the most permeable nerve paths in practise are usually the right and appropriate ones! From the psychologist's point of view it would be a pure mystery if a person's behavior should ever be determined, as the layman believes, directly by the concrete properties of the actual situation. The layman's belief is in full agreement with everyday experience; the learned view harmonizes with what we suppose to be the viewpoint of natural science. I shall confess at once that I choose the layman's belief whole-heartedly. In the treatment of sensory process *gestalt* psychology prefers the data of naïve description to the findings of prejudiced introspection. Following this line, the theory of the sensory field has begun to have a much more intimate contact with the natural sciences than it ever had when it imitated a self-made ideal of these disciplines. Similarly if, with regard to the total field, we adopt the view of common experience, what is *called* the viewpoint of natural science will soon be given up, in place of which we shall substitute the theory of dynamics. At the start of our present discussion, therefore, the obvious, almost the vulgar, will have to be said. It is not our fault that, to a deplorable degree, the obvious has disappeared from learned psychology, so that we have to rediscover it. Later in the course of this analysis the obvious aspects of common experience will assume the function of expressing certain fundamental properties of physical dynamics in the nervous system. This is exactly the contrary of the prevailing opinion.

We have attacked the atomistic theory of sensory experience. After things and the self, events and groups, have been recognized as natural parts of the field and its development, the worst mistake which might occur would consist in our falling back upon atomism at this higher level. It was once the psychologist's primary aim to look for all possible sensations, to collect and to order them in something like a museum. After they were thus gathered and arrayed as independent bits of experience, the sensory field had consequently to be regarded as a mosaic of indifferent spots. We would face the same danger in this present treatment if, after discussing a certain type of segregated whole in one chapter, and another type in a later chapter, the self in one chapter, and attitudes in another, we should also be tempted to open a museum to the public. The ways of real life do not coincide with those of classification, and if, by abstraction, we unite the members of one class, we very probably cut the live bonds of dynamical reality at the same time. Perhaps, the most interesting forms of dynamical context occur between members of altogether different classes. It is instructive to see hundreds of hearts together in a collection; but, in operation, a heart has much more to do with a lung than with another heart. We should not learn very much about their specific coöperation in a classifying museum. In a similar way, when we treat sensory wholes as one class, selves as a second and attitudes as a third, we might easily be misled into the view that out of some sensory objects, some self and some attitudes, actual total fields might be composed *ad libitum*. It is evident to any one that such an assumption would be altogether childish, there being certain *correlations* between the objects, the selves and their attitudes occurring in one common field. Even from the viewpoint of traditional psychology, it would be so. However, we cannot be satisfied with this statement because, generally, much more than mere correlation will occur between the most essential parts of a given total field. Just this surplus is the obvious aspect of every one's life to which we have alluded and it is forgotten in at least one-half of scientific psychology.

One attitude in which I sometimes find myself is admiration. But I am never simply "admiring." My admiration always is "of" something; it does not occur as something by itself and indifferently. Nor is there the slightest doubt about what its object is at a given moment. In the concert-hall, yesterday at 9 o'clock, it was that *alto* voice, singing calmly, confidently and seriously, toward which my admiration was directed,—not the nose of my neighbor, not the back of the conductor, none of the thousands of other objects and events before me. How is that? Admiration is a directed attitude; the voice is heard as singing at a definite place. Do I state that the direction of the first goes to the place in question and stops there, as a long stick might be fastened between me and that place, and end there? Do I notice something like that and say, then: "Oh, I guess this curious attitude of mine somehow has something to do with that singing!"? I certainly do not. As my attitude arises, it is experienced as being the natural outcome of what characterizes the singing voice. So long as the attitude persists, it is felt as being founded upon the properties of that performance. No indirect criteria, no coefficients of correlation are needed, then, to teach me about some probable connection here, because this actual attitude *is experienced as depending directly upon something definite.*

Some weeks ago I saw my little child smiling for the first time, and I was charmed. How did I know that my attitude was concerned with that smile? If the total field were a mosaic of states, events and things, some of them directed and some not, but all of them distributed indifferently in consequence of some hidden mechanism, I could not guess their mutual connection except by the indirect procedure of scientific induction: a change here would be followed by a change there; elimination of this part would be accompanied by disappearance of that other part. Empirical rules of concomitant variation, and so forth, would be all I could find out about the connections existing between the several parts. In my case, the frequent concomitance of the smile on the child's face with that other bit of experience, my being charmed, would have made me assume that probably there was some connection between the two. How far have we gone in psychology that it is possible, or even seems advisable, to carry on such discussion seriously! One side of my child's face is a little darker because of a shadow. Before the sufficient number of experiences have occurred, I might as well refer my being charmed to that shadow!

After a long walk, on a hot summer day, I drink a glass of fresh beer. There is the cool touch and a characteristic taste in my mouth; there is also great pleasure. Did I have to learn gradually that the second refers to the first? That it has nothing to do with the spider that I see on the wall, or the size of that chair? I did *not* learn it. I am no more sure of my enjoyment as such, and of touch and taste by themselves, than I am of enjoying just this touch and taste. Enjoyment is felt as the adequate attitude *belonging to* those actual experiences, or as their natural result. Between the attitude and its sensory basis we experience what in German is called *ihr sachlicher Zusammenhang*.[1]

The same is true in a great many cases which differ from the last ones only insofar as the attitude has a "negative" character.

[1] I have not been able to find an adequate translation of the German adjective *sachlich* in this connection. "Intrinsic" would come near to it, perhaps.

For two weeks I have been busy arranging a carefully adjusted set of instruments for experimentation. This morning I found them completely out of order. Whoever may be guilty, I am angry. If, now, I should say that here is the window, there the desk, in one corner the instruments out of order, in another a chair, and near the door myself in an attitude of anger, would that be an adequate description of the total situation? It would not. I am sure that the door has no connection whatever with my attitude; for I know that, on discovering the unfortunate change, I was angry *about the alteration.* Again, the attitude is felt as *founded upon* something definite, its character being the natural and direct consequence of that something's properties.

One beautiful night in Tenerife, when I was working calmly at my desk, I was suddenly frightened as I have never before been frightened. The house was rattling and shaking violently—my first experience of an earthquake! There was no doubt whatever about my being frightened *by* that sudden rattling and shaking. Once more the attitude—if fright may be called an attitude—*was felt as obviously and naturally produced by that new experience.* We do not gradually learn that unexpected events of a strongly dynamical type will be accompanied by fright, as though *a priori* any other experiences, a friendly face or the smell of a rose, might be accompanied by fright just as well. Fright is *experienced as* jumping at us right out of the very nature of certain definite events.

After sitting for half an hour in a restaurant, full of smoke and of talk all around me, I feel "nervous" and ready to go. My "nervousness" *refers to* those properties of my environment. I know this, not only because in past experience I may have discovered the rule that under such conditions I shall feel uneasy after a time. I experience myself directly as disturbed and confused *by* those surroundings. I might say also that I feel myself *necessarily growing "nervous" under the pressure of* sticky air and loud, disorderly noise. Actually these conditions can only produce that one effect upon me. That is how I feel them.

Last but not least, I was very much depressed two days ago. At the same time I could not hit upon an adequate presentation of what I conceived to be the subject-matter of this chapter. Were there two separate facts, then, the probable connection of which I might have stated by some indirect method? No need to! When I was trying to solve my problem, I felt my depression *as based upon* my failure directly. Just such a feeling had to grow out of such a situation because of its properties.

If, in all these examples, I feel how my own attitude is determined by the nature of something before me, in other instances something before me will be experienced quite as naturally as growing out of my attitude.

When I look at Fig. 1 without any special effort, I have two groups of dots before me. But if, for some reason, I become interested in oblique lines, the figure will be transformed into three oblique pairs of dots, for instance, each of which runs like an oblique line from a lower dot to the right to a dot higher up to the left. Suppose that the transformation really occurs; it grows out of my attitude quite as naturally as my anger develops out of a definite event. When I scratch my head or hum a tune simultaneously, I do not feel the slightest connection of these activities with the described transformation of Fig. 1. But the transformation is *felt as depending upon* that particular attitude directly.

What was the name of that town on the Santa Fé Railroad? There it comes! When I am searching for a name, searching does not occur as one thing apart; nor does the gap of the forgotten name occur as an isolated item among other things—a headache, the noise of the wind, the lamp before me, and so forth. Searching is experienced as directed toward that definite name, hidden as yet, but now, at last, yielding to the stress of my attitude. As it appears, its coming is felt as *being achieved by* just this little stress of mine. I ask again, Did I learn gradually through numerous experiences that in this particular situation such an attitude will be *followed* by a name so that now I may venture to guess about their mutual relation? Without previous training, might not that noise outdoors be connected with the given attitude as well?

I hold my arm horizontally for a while. Very soon the arm will not remain in that position unless I exert a special effort. In my total field there is, besides this effort, the blue sky, a lark's song, the aspect of the lifted arm, a smell of moist ground. There is also a curious feeling in the lifted arm, growing more intense as the minutes go by, and, with this feeling, something like a downward pull in the arm. Geometrically, as it were, or logically, my actual experiences allow a large number of different combinations into pairs. My effort may be combined with the lark's song, the smell, the color of the lifted hand, and the blue of the sky. But my effort is not an isolated something, equally indifferent to all parts of the field. It is experienced as keeping the arm horizontal *against that downward pull.* The nature of the pull is felt as requiring just such an effort in compensation, and the nature of the effort is experienced as compensating just this pull. If some one tries to describe the situation in terms of indifferent data, each of which has a place, some of which have a direction, independently of place, some occurring at one time, and some at another, and all of them having hundreds of mutual "relations" as to place, distance, succession or simultaneity, intensity and so on,—he will not even touch what psychologically is the main feature of the situation.

In discussing a somewhat similar case David Hume emphatically defended the opposite view. I do not know how my arm is lifted, he said, when I wish to raise it. There can be no more than mere succession in time, since I do not know the nature of the physiological mechanism that effects the raising. What a strange argument! And how much opposed to what Hume had promised to furnish as an analysis of pure experience. When we talk about "the arm" in this connection, we have to deal with an experienced thing, not with a physical object and its movements in physical space. Whatever the nature of peripheral innervation may be in Hume's case, we are dealing with a definite "wish" and a definite experienced movement of our arm. The problem is whether this particular wish is naturally experienced as being as foreign or indifferent to the arm's felt movement as it is to the seen color of that cloud or the brown of the skin. In haste the great philosopher seems to have skipped over a dangerous point here, unintentionally making use of a simple logical trick, by which, however, he saved his theory for the next two centuries.

In order to make the import of these paragraphs more circumscribed, I shall consider an objection.

One might say after all that those experiences of "depending upon," "being the natural outcome of," "being based upon" or of "sustaining against" do not prove that a *necessary* connection exists in the field. I greatly enjoyed the overture to *Tristan und Isolde* twenty years ago and, at that time, I might have given exactly the same description of my enjoyment as the direct and evident outgrowth of just that piece of music. What has become of my attitude toward the piece? I am tired of it. At present, I should be rather inclined to find aversion the only possible and natural outcome of hearing the overture. Nor would I doubt for a moment what this aversion refers to or is based upon. The answer to this objection will consist in our saying that all this is absolutely true, but that it does not really constitute an objection. With exactly the same physical situation, or sequence of sound-waves, the same person will feel "pleasure" *growing out of* his sensory experience to-day, and be altogether disgusted by it only a few days later on. A simple way to achieve that alteration experimentally would be to give our subject the same sequence of sounds three hundred times each day.[1] But we have no right to say that after more than a thousand repetitions the same melodies will be experienced as having quite the same properties they had to begin with. They have changed, no doubt; they have become a musical commonplace, sounding empty and stale. Keep away from these melodies for a few years! Then you may find something more

[1] Lewin and Karsten, *Psychol. Forschung*, *10*, 1927.

similar to their original nature again, and if, then, you enjoy them once more, your enjoyment will be felt to grow out of them as obviously as when you first listened to them. An attitude is felt as being the natural outcome of actual experience; the objection stated above misinterprets this definition, as though it meant a constant connection between a set of *stimuli* and the attitude.

For still another reason, music which I enjoyed twenty years ago may not please me to-day, even without the unfortunate influence of frequent repetition. What I call my self has changed its properties considerably in the meantime. Why, then, should the music produce the same effects upon this changed self? Once more I must point out that our feeling of something naturally depending upon something else does not refer to a correlation, or a highly constant "togetherness" *as such,* stated in terms of the external observation of a great many cases. It refers rather to an evident dynamical dependence as experienced *hic et nunc* in one actual case. As this flower before me is certainly red, though if I should become color-blind later on, it would be gray, so such a dependence in the total field is real now, independently of what the future will make of it.

From this consideration we may learn something more. The way in which a piece of music affects me now, in contrast to the attitude it produced twenty years ago, has been determined by some change of my self. I should hesitate very much to assert that *this* change of my self was experienced as the only possible outgrowth of definite conditions, quite as evidently and directly as, at a given time, my attitude toward the piece of music is felt to grow out of its actual properties with natural necessity. Therefore, I did not experience the *way* in which one attitude instead of another has become the natural outcome of my listening to that piece. Certain conditions must have gradually changed *outside* the field of my experience; the change in my attitude must somehow depend upon that change of conditions. But I have never felt the former change being brought about by the latter change.

It is an old rule in science that nothing makes certain positive statements more worthy of credit than their author's frank acknowledgment of negative cases and his attempt to emphasize the contrast himself. I shall be the last to deny that in hundreds of cases we are very far from experiencing how one state of affairs is brought about by others. Let a subject observe a continuous movement for a while and then fixate a face or a pattern. His surprise when he sees the after-image of that movement for the first time in his life will be sufficient as a proof here that the experience of the after-effect is necessarily brought about by certain conditions without the subject being aware of any natural bond between them.

Probably, here, the main conditions remain entirely outside the field of direct experience.

Again, after fixating the center of Fig. 8 for a while, many subjects will be greatly surprised when suddenly another, quite different, shape appears before them. We do not directly feel *why* just this transformation should occur after continuous fixation. We feel it so little that once a physicist, who had been shown such a figure consisting of bright lines in an otherwise dark room, afterwards asked me how, from my position twelve yards off, I had been able to put another figure in the place of the first, so quickly and surprisingly! He did not even suspect that something in his own organism was producing the transformation.

Eventually there cannot be a doubt that even certain emotional attitudes may occur without our feeling their *raison d'être* directly. As sometimes we suddenly feel that we have caught a cold without knowing where and when, thus some morning we may feel the way an ominous cloud looks, i.e., with a silent anger which does not seem to have an obvious basis in the present field of our experience. Such a cloud of anger, it is often true, will easily find something upon which to discharge itself, and, then, that external something will appear as an adequate object of the anger. Before that time, we cannot do more than *guess* about the hidden first cause of our angry mood. It may be some unknown climatological condition working upon our organism, or it may be disturbed digestion. However, we do not directly "feel" any of these causes. This example may be regarded as particularly interesting, since it seems to teach us two different lessons: It corroborates the view that, as well as instances in which dynamical context *as such* are experienced directly and actually, there are other instances in which effects are conspicuous in experience, although experience is blind and deaf as to their determination. The second lesson is that both sorts of determination may be united in one single event, because when, in the attitude of a threatening cloud, we discover something upon which to release the tension, that something will appear and probably *be* able to produce an angry reaction. But that we should react so vehemently will be the result of those hidden conditions which had transformed us into an ominous cloud before there was this opportunity for "justified" anger.

I do not see, however, that the fact of hidden determination as observed in some cases is an argument against experienced determination in others. In the case of cholera and plague, germs are found to be the decisive condition of disease. In diabetes this is not the case. Who would deduce an argument against bacteriology from such a "negative" case? Therefore we may accept the same duplicity in our case, too.

But is it not evident that, in spite of our examples, so-called "experienced determination," in a great many instances, may be a simple product of learning? When, in my mail, the address on one envelope is in a certain handwriting, I feel happy over the discovery; whereas with the finding of some other handwritings, just the contrary is true. We may neglect the graphological and esthetic sides of our reaction, since we know that it is chiefly our acquaintance with the writers which makes one handwriting very pleasant and the other disagreeable. In either case we seem to feel that our attitude is "the natural outcome" of the aspect of the handwriting. Nevertheless, our reaction seems to be one which we have had to learn. The same words and letters would not be felt as "being the natural basis of our reaction," if we had not become "conditioned" to them. What, then, about that mysterious feeling of natural determination? The answer is simple enough. Those words and letters have become imbued with all the friendly experiences we have had with one person or with all the animosity connected with another. As these persons themselves, when present, directly arouse the corresponding attitude, their handwritings do so now; not because this *attitude* has become gradually "connected" with the handwriting, but because, saturated with the specific character of those past experiences, the handwriting represents a *natural* basis for just this attitude, indeed. There is no reason why this determination of attitude by the now apparent properties of something before us, should not occur and be felt directly.

With respect to this question another remark will be to the point. When I am thirsty, I am inclined to think of refreshing drinks. What I am thus directed to is brought into my actual experience by reproduction. That does not matter very much in our present problem. However reproduction may have occurred, once the so-called "idea" of the drink is in the actual field, it is felt to be the natural basis of longing *for* it, and the craving is experienced as being determined directly by just that sort of "idea" in the present state of the self. The mere circumstance that something has come into our actual field by reproduction does not at all decide what kind of context it may be experienced in when it is there. It will be wise to remember that well, because the overwhelming importance of learning, of habit, of reproduction, has been so constantly emphasized in psychology, that we will almost stop thinking and considering any further problems with respect to a situation in which *some* influence or participation of past events and of reproduction has been shown. But even if all "parts" of a given situation should be found to owe their actual presence to reproduction, there would still be one problem before us: In what degree do they belong dynamically to each other here and now, in the manner we are discussing in this chapter?

We have come back to what has been called above the layman's belief. Without knowing anything about the fundamental weight of his conviction he takes it as the simplest matter in the world, that he has such a feeling now "because of" a certain aspect of his actual situation, and that he feels the "because," too; also, that he lifts his arm with an effort felt as "directed against" a heavy weight and "in spite" of it. I agree with the layman. In the organization of the total field and in thousands of cases, we do experience the live dynamical context determining one state this way and that change another way. Moreover, we feel where they come from and where they go to, in those instances. Above all, we may experience *why,* issuing from just such an event, thing or attitude, a given effect should be just the one we find growing out of it.

Surprisingly enough, in modern science and most of all in psychology, there is a tendency not only to neglect this simple truth, but even to persecute it, as though it could not be acknowledged from the scientific viewpoint. David Hume has been thought of as the father of all the energetic fight against it. But probably in this great figure of history we should rather see the exponent, the most eminent representative and the best enunciator of an intellectual current which took its origin in Greece or even before, thousands of years ago, arising from some deep need of mankind for clearness.[1]

There is one sort of clearness and simplicity into which our statements and the layman's belief do not fit very well. That ideal of clarity would be achieved, if the world could be conceived as an enormous number of equal and unequal pieces, related indifferently as neighbors in space and successors in time. That this is a true picture of *experience,* at least, is silently presupposed at the very outset in Hume's famous work, so that he does not prove subsequently more than he has already implicitly treated as a settled affair. Since we have noticed that, in a great many cases, we really do not at all feel *how* something is determined by something else, he had no difficulty in gathering examples appropriate to his point of view; and since he did not see the other cases, or disposed of them as quickly and incorrectly as I have described above, he seemed to have demonstrated the truth of his view beyond all argumentation.

Being neighbors and following each other, indifferent bits of experience will show all possible mutual relations as to space and time, which, however, do not prevent them from being an indifferent aggregate. In addition, there will be an indefinite number of other relations referring to quality, in-

[1] In some of Plato's discussions about the properties of the truly real world, the same tendency is quite obvious.

tensity, and so forth. These, however, also form for the most part an indifferent network throughout experience, merely visible to something like a logical eye, but inefficacious from the viewpoint of experience itself. Laws about natural coexistence and the sequence of events? No direct grasp of them is possible in such a world *à la Hume!* He is said to have been the greatest empiricist of them all. But if he constructs such a world, the plan of which is determined by his need for a special type of intellectual clarity, he might quite as justifiably be called a rationalist.

In his *radical empiricism* William James laid great stress upon the fact that "the relations between things, conjunctive as well as disjunctive, are just as much matters of direct particular experience, neither more so nor less so, than the things themselves."[1] This view is an obstacle in our way rather than an aid; neither do we receive any help from James, when he, though attacking atomism in the treatment of experience, clearly fails to recognize natural segregations in the sensory field (Chap. V). "Relations" might be found indifferently between all parts, fractions, or bits of the field, if we search for them. In this respect, and unless more is said, relations will be entirely unfit to make us understand why some definite and particular attitude is experienced as arising "because of" some definite and particular event or

[1] *The Meaning of Truth*, Preface.

thing in the field. The field is by no means experienced as being everywhere full of those dynamical bonds. There are *relations,* nevertheless, between that same attitude and all the rest of the field.

In some places, it is true, James attacks our problem from another side, as when he writes about our "sustaining a felt purpose against felt obstacles, and overcoming or being overcome";[1] also where in his description "the experiencer feels the tendency, the obstacle, the will, the strain, the triumph, or the passive giving up, just as he feels the time, the space, the swiftness or intensity, the movement, the weight and color, the pain and pleasure, the complexity, or whatever remaining characters the situation may involve."[2] This is something quite unlike an indifferent network of relations throughout the field. The stress is not placed precisely where I have tried to place it, largely because for James "activity" *as such* was of chief importance, whereas our description centers around the experience of a great many different attitudes as they arise naturally out of the properties of an event before us, or vice versa. Still there is some agreement, at least, about our experiencing directly certain determinations in the total field.[3]

A great deal of purely sensory organization may

[1] *Some Problems of Philosophy,* p. 213.
[2] *A Pluralistic Universe,* p. 376.
[3] Since James' time this view has been defended by several authors whose names will be found in the bibliography at the end of this chapter.

be called "silent" because, though we experience the *result* of it as segregated wholes with specific properties, we do not usually feel *how* this result is dynamically brought about and maintained. In this respect the total field is different; it tells us more about its innermost nature. Here, not only the result is experienced, but also very much of its "why" and "how" is felt in just the actual context. Wherever this is the case we apply the term "insight." When I used this term in my treatment of intelligent behavior in apes, I ran the risk of an unfortunate misinterpretation. Since that behavior comprised extraordinarily conspicuous accomplishments which we did not expect to find in animals, insight would be readily misinterpreted as some special and supranatural faculty producing admirable and otherwise inexplicable results. As I used and intended the term, nothing of that sort should be implied in it. To avoid a similar misunderstanding this time, I have introduced the concept here in application to very common and simple facts. It does not mean more than our experience of definite determination in a context, an event or a development of the total field; and in the actual cases there need be nothing like an invention, or a new intelligent achievement, or so forth. A total field would be experienced *without* insight, if all its several states, wholes, attitudes, etc., were simply given as a pattern, in which none was felt directly to depend upon any other and none to determine any other.

I have repeated several times that, from the viewpoint of common and unsophisticated experience, nothing can be more obvious than just that direct determination. So much so, that in natural life scarcely a single actual total field would be said to lack that characteristic. With respect to this conviction of the layman we have not added more than truisms, indeed. Among learned psychologists, however, there will only be a minority at present who, fully realizing the theoretical importance of this point, are quite ready to accept it. There is a second group, much larger than the first, comprising psychologists, who time and again will talk in a manner involving, presupposing and implying insight as a characteristic of their subjects' or their own experience. This is done, however, without any intention to do so on their part, and consequently one does not find trace of it in the list of their theoretical concepts. To participate in the naïve and practically sound procedure of the layman is not the same thing as to realize what one is doing from a theoretical viewpoint. The less so, since the well-established and socially acknowledged concepts of experimental psychology belong almost completely to the theoretical world of Hume. In a third and last group of psychologists all those scientists may be counted, to whom the creed of Hume or the scientific epoch of the nineteenth century seems to be *the* final stage of theoretical advance, any essential change or development thereof being forbidden. What has been described in the last paragraphs is mysticism in their eyes. They would probably find no difficulty in asserting that, riding in a bus, we might refer actual danger to the friendly face of a beautiful girl before us just as well as to the man who has at that moment chosen our feet as a basis for the considerable weight of his whole body. Or, more correctly, they would not acknowledge *any* experience of natural reference at all, since what "connections" there are, are all due to indirect learning or conditioning. Consequently, they never mean an actual "reference to"; they mean merely a "coming together" or a "following each other." There is no doubt that true behaviorists belong to this third class. But usually they decline to enlist in any of the parties; because our problem still seems to remain entirely in the field of direct experience, of which they refuse to take any account.

The problem can be readily transformed, however, into a matter of brain physiology. At the very outset (Chap. II) it was proposed to use direct experience as an indicator of those physiological processes which, occurring between the realm of outer conditions and the overt behavior of the organism, are not directly observable at the present time.

In that field of the brain the processes of which underlie our experienced field, the context of states and events was said to be a true picture of direct experience. To the actual order of the experienced field there corresponds the actual order of nervous processes, in each case. When one thing appears as a segregated whole in experience, a corresponding, detached whole exists in nervous process, and so forth. Mere continuity and consistency of theory has obliged us to acknowledge then, that, in some part of the same physiological field those processes must occur which, whether they be sensory or not, underlie the self, its actual states, properties and attitudes. As in experience I am surrounded by the things and events of my environment, so the processes corresponding to my self will be in the midst of a corresponding environment, consisting of sensory processes and so forth, in the brain field. If, now, there is experience not only of things and events, of my states and attitudes, all occurring indifferently by themselves, but also of *this actual attitude being the direct outcome of that feature of the environment,* or of *this state in my surroundings being determined by that actual attitude of mine,* there seems to be only one way to conceive the underlying physiological context. Where, in dynamical determination, the properties of one part of a field depend directly upon the actual nature of some other particular part, those properties do not exist indifferently as such; they originate and they are maintained by the stress of just those particular forces which issue from the determining part, according to its actual nature. Assuming that the same is true in the brain field which is true in a physical field, the properties of the actual physiological self[1] will change and may be kept changed a while, under the stress of some particular process in the same field, corresponding to an essential thing or event in the experienced environment. The changed state of the self does not exist independently; it is produced and maintained by something definite, the actual nature of which expresses itself in just this dynamical influence. If, therefore, this particular dynamical context is taken to be the physiological fact underlying our experience of direct determination of attitude, no mystery remains in our description of common experience.

To make this point more clear, the situation in one of our concrete examples could be thus stated. When, on a hot day I enjoy a cool drink, my enjoyment is felt to refer to, or to be based upon, the properties of the drink and my thirst, but not to the spider on the wall, nor to the size of a chair, nor to thousands of other things. In the brain-field, more particularly in that part of it which corresponds to the self, there is a special process B, felt as thirst in experience. Now when I begin to drink, another process A, corresponding to experienced coolness and taste, develops in the same region of the brain-field

[1] For brevity's sake the term "self" is applied here, though for more correct expression I should say: "the actual process corresponding to the self in actual experience."

(experienced interior of the mouth) where until now there was only the process of "thirst." In terms of the theory of insight and of direct physiological determination, A begins at once to exert an influence upon B, the influence depending upon the concrete properties of A and those of the actually existing state B. The change, which is produced, is felt as pleasure (and, concomitantly, decreasing thirst). And this change I assume to be determined by A no less directly than is the rising temperature of a black surface determined by the rays of light falling upon it or the "attitude" of a dwindling fire by a fresh supply of oxygen. From our viewpoint we do not have separate bits in experience where we have a functional context in the physiological total field, one part of which reacts directly to the definite and particular properties of another. *Therefore,* in our example, we cannot experience enjoyment as something apart, among a pattern of hundreds of other things. Enjoyment has to be felt as "referring to" and as referring to something definite, the process of which determines the process of enjoyment.

So far, instead of mystery, the most elementary concepts of physics are introduced into our view of the total physiological field. There is not the slightest reason why we should not experience determination *as such;* nor is there any reason why, in concrete cases, we should not feel just what our actual state or attitude is based upon; in one word, there is no reason why *insight* should not be an essential characteristic of much experience in the total field. Of course, what has been said about the determination of the self by parts of the environment, may be applied to the opposite case at once, in which an actual attitude of ours is felt directly as producing or as maintaining a change in our surroundings. Here, the influencing and the influenced have only changed places, both physiologically and in experience.

Although the layman's belief is thoroughly justified by such a view, there is strict contradiction between it and the opinion now prevailing in scientific psychology. The contrast between machine theory and dynamical theory becomes clear once more. Neither in the usual scheme of a reflex, nor in that of a conditioned reflex, nor even in any attempt to explain association (in the older form of that concept), will the properties of one particular process determine directly the nature of another. An influence always remains a matter of some intermediate "paths of optimal conduction," the particular properties of a first process A being something apart, transmission of "nervous energy" along those paths another separate event, and the effect upon a later process B, still a third thing, determined by the arrival of that indifferent "nervous energy." Therefore, if another path had been a better conductor, a different realm or process C, instead of B, would have been changed by the sudden impulse from without; in any case, the change of B into a B^1 or of C into a C^1 would have been independent of the concrete properties of A. In terms of such a theory A will produce B^1 only because there is that one-way street between them. By some change in nervous connections a thirsty man might be roused into a fit of anger at the experience of the coolness and the taste of a fresh drink!

The objection will perhaps be raised that, excluding certain radical behaviorists, no one has explicitly presented such an absurd theory of mental life. I shall not give much weight to this historical question, because one thing is much more important: Why do we not formulate the positive theory of direct dynamical determination in the total field? Why do we not expressly acknowledge insight as one of the most fundamental and common facts of mental life, and, therefore, as fundamental theoretically? Why do we discuss as many psychological problems as we possibly can *as though* the machine concepts were the only ones available or, at least, the best ones from the viewpoint of natural science? I do no more than derive quite frankly the consequences of such a scientific situation. In doing so, I have but one aim. We must advance from an implicit and almost unwitting presupposition of insight and direct dynamical determination, which, expressed in careless psychological terminology, cannot give rise to concrete consequences, to a clear realization of the essential point. This I believe to be the contrast between the indirect and indifferent connections of processes in machine theory and the directly adequate determination of one process by another in the dynamical theory of the brain field.

In the last pages it was my original intention to talk about insight as occurring only in an *established* total field, in which an actually existing attitude is felt to be the outcome of some definite fact in the environment, or vice versa. I have not been able, however, to restrict the discussion to such a settled case, despite my intentions. And nothing could be more characteristic of our problem than is this failure, because in a somewhat accurate description a certain feature, intrinsically contained in some of our concrete examples, must necessarily carry us beyond the actual state of the total field.

In more than one example it was not so much an existing state of the self which we felt to be determined directly by the properties of a given fact before us, but a *change* of the self as growing out of something particular, just appearing in the field. Sudden fright at an unexpected event of a certain sort is an instance of this sort of dynamical determination. Again, when in climbing a mountain we reach a dangerous place, fear may grow out of the observation of the lay of the land where we must quickly take the next three steps. After these steps have been successfully accomplished, a feeling of relief will develop out of the situation, no less directly.

Nevertheless there will sometimes be more of a *development* in such a dynamical state of the total field. When I described my feeling in a restaurant, full of smoke and full of talk, "nervousness," or uneasiness, was not the only thing I had to report about myself. I had to add that I felt very eager *to go.* This means that, beside my experiencing uneasiness as the natural effect directly determined by the environment, I felt a tendency in my self *to move away from it.* Again, this tendency was not experienced as something arising, *as such,* without reference to definite other parts of the total field; on the contrary it was felt to grow directly out of certain properties of the situation and my uneasiness. As a layman I would say that I wanted to go *because of* my uneasiness in this particular situation, and as a psychologist I shall admit that the layman's expression is absolutely correct, because it corresponds once more to the fact that there is insight into the direct determination of that tendency toward a definite sort of activity.

Our life is full of trivial instances of this kind and, occasionally, the most important and decisive impulses toward definite forms of conduct arise directly out of given conditions in the same way. And generally we experience the *how* and the *why* of such experience.

I am sitting in full sunlight quite contentedly.

After a while I feel too hot; at the same time a tendency to move away from where I am sitting, arises. A shadowy place near by seems pleasant; the impulse away from the sunlight becomes at once a tendency toward the shadow, and as at first the properties of one place made me inclined to move away from it, the properties of another now arouse an impulse of approach. In both cases there is insight, since we feel *how* in the first case one tendency grows directly out of the nature of one situation, and in the second *how* some other part of the field, due to its properties, quite as naturally determines another impulse. From an earlier remark the reader will remember that it does not at all matter for our present problem whether the relevant properties of the seen shadow are known to us by previous learning or some other way.[1]

The same description may usually be given of fright. When a sudden event is felt as producing fright in me, I shall have to add at once, in a correct description, that with the fright there arose a vehement impulse to move away from the locus of that event. Again, this tendency immediately to increase the distance between that place and myself is experi-

[1] I do not forget, of course, that subordinate parts of our organism will show reactions, externally similar to those described, though belonging to the reflex type of movement. Stimulated by a prick, the foot will be withdrawn by reflex. In no case would this fact create an objection against the theory of insight-reaction. In some of its activities the organism is doubtlessly similar to a very practical machine; at the same time in others, and most of all those in which it behaves as a whole, direct dynamics may decide what happens.

enced as being the direct outcome of the startling occurrence quite as much as the fright itself is felt as its direct result. We noticed before how, in Watson's description of a child's behavior, the overt behavior of withdrawing from the uncanny object may be regarded in the sensory experience of the onlooker, as a visible picture of the child's own total field. Does any one believe that the child feels a fear of the object, on the one hand, and an impulse to move away, on the other, as independent experiences? Or that the child might just as well feel a tendency toward embracing or swallowing the object, during his fear? As Watson has shown in this case, the object has become "dangerous" through previous learning. This does not prove, however, that between the danger itself, the fear and the withdrawal, there is not more than indirect connections.

As an impulse of withdrawal arises directly out of certain given situations, so the opposite tendency belongs as naturally to other situations. I have mentioned above the case of a shadow attracting, as it were, a person who has had more than enough of the sun. We all know that similar instances are more frequent in our life than trees in a wood. The child, observed objectively as reaching for an animal's head in Watson's experiment, feels attracted towards that interesting object, if we are willing to grant him any experience at all. From our own total field we are well acquainted with the fact that such a tendency appears to us as being a natural characteristic of exactly such a situation and of a great many similar ones. In Spain, after a particularly brilliant achievement of the toreador, the admiration of the crowd often drives them so intensely in their hero's direction, that, unable to enter the arena themselves, they hang over the balustrades, reaching toward him as far as possible; the stress will sometimes be so great that hats, handkerchiefs, and so on, are thrown down in the direction of the dominating impulse. Did these people learn gradually by some process of conditioning to connect an approach-tendency with admiration, as though a tendency toward frowning or shaking the left leg might have been, if conditioned, equally well "connected" with admiration? I cannot help thinking sometimes that whether we are introspectionists, behaviorists or whatever else, the chief dividing line among contemporary psychologists would separate those who acknowledge obvious, direct determination as it appears in common experience from those who can only admit indirect "connections."

As a last example I choose a simple, practical event: for some purpose I wish to break a wooden board. I press against it and, feeling my effort directed against the felt resistance of the board, I see and feel the board yielding in the same direction. Is it true that, as Hume would have us say, this yielding of the board in my experience does not develop more naturally out of the direction of my effort than would, for instance, a change of the board's color or a darkening of the sun, under the same circumstances? Personally, I feel the board yielding just in the direction of my effort and not otherwise, almost as naturally as I feel myself yielding in a definite direction, if a friend of mine and I measure our strength by pressing hard against each other's shoulders, and I lose the battle. However that may be, if once the board begins to yield to my effort, I immediately feel a tendency toward going on or, rather, increasing the pressure as a direct outcome of that change in the board's resistance. And, indeed, I really do increase my effort.

What is the common content of these examples? That there is more than mere succession in the development of our attitudes as they refer to actual objects in our field! Things *might* occur in human experience in this way: First, I feel uneasy near a hot radiator, for instance; then, as something apart and as an altogether separate bit of experience, I feel an impulse away from the radiator, and, eventually, as a third independent feeling, I find myself moving in some direction which, *geometrically,* would increase the distance between that object and me. It is almost impossible to talk about these three experiences in a manner which would exclude any natural context between them as radically as, in terms of the viewpoint I wish here to exhibit, it ought to be excluded. As we read the words, the context of insight is built up at once, spontaneously. What is meant will become more apparent if again I say that, from such a viewpoint, any event whatsoever might follow the first instead of the second, and similarly with the relation between the second and the third. In experience the real development is supposed to have no better "connection," originally, than any other sequence would have. The first experience does not *require* the second, nor the second the third!

Again we return to machine theory. Normally, one might say, the development will consist of just those three events, indeed. Feeling hot, feeling a tendency toward moving and moving in a direction which, geometrically, coincides with the direction of that tendency. Why do just that second and that third event follow the first? One possibility: because there is something like an original reflex, a well-established path in the nervous system conducting from the place of the first process to the place of the second, and a number of similar paths leading from this second place to the centers controlling the particular movement of our feet in the certain direction. In such a theory of the sequence, the result would be practical enough from the viewpoint of an observer, and even the subject himself might feel satisfied at the *end* of the series, but that would be all. If, in the same situation (heat), pinching his left thumb between the fingers of the right hand occurred, followed by his moving in a certain direction and, then, by his feeling well, the subject would understand the sequence quite as much or, better, quite as little as when he sees a natural context in the real development. Mere connections between parts of the nervous system enforce the sequence, the *properties* of A *as such* do not take part in the determination of B, and those of B are altogether innocent of C's succession. Such a production of the sequence cannot produce in experience more than a series of feelings indifferent to each other. There cannot be insight.

Suppose that no reflexes are provided in this case; what theoretical explanation can we give, then, of the right succession? The second possibility is learning or habit. Many different things originally happened in my experience when I felt very hot. Among them, occasionally, there were impulses toward moving. Again, among these now and then there was one accidental impulse, the direction of which was away from the radiator, *geometrically*. (Not for the subject, however, who might just as well define this direction as northwest, for instance!) After these few occasions another series of events followed in a haphazard fashion, all of them equally indifferent to the foregoing. But sometimes real motion in the stated direction, and the experience of it, would be the next events and, finally, an experience of feeling satisfied. However it came to pass in the first instance, this one sequence, after it has fortunately happened to occur frequently enough, becomes a well-established chain of events, in short, a habit. Thereafter it operates as soon as our subject feels hot enough. But its operation is no more immediate or natural than all those other possible combinations *as such* would have been. The subject feels satisfied at the end of the sequence. But if, after the feeling of heat, counting from one to twenty-six had brought about this same feeling of relief, and if this sequence had been well associated, he would not experience a less natural or satisfying development than he really does in the case of the habit of withdrawal. No one development is more natural than any other; without exception all sequences are *merely* sequences, and nothing else. It is only by some fortunate mechanism that those sequences are "stamped in" as a habit, the end of which is *practical success.* The subject cannot feel in experience more than an indifferent series of events since, in the associated series, the properties of heat do not have any influence upon the "right" tendency as the next event, and the experience of the "right" movement will be the third event, independently of the nature of that tendency. Synapses have become less resistant between the localities of those processes; the connecting fibers have become more readily conductive. But the connections in the nervous system have had nothing to do with the properties of the heat or with the direction of that tendency away from the radiator or with the direction of the movement itself. The neural factors were merely frequency of repetition, recency, and success in a purely external sense of the word, or any other principle independent of the concrete properties of heat, tendency and movement in their mutual relations with one another.

And instinct? I know only two ways to give this term a somewhat concrete meaning as an explanation of real development in experience and in behavior. Often the concept means the same thing as "a chain of reflexes," with or without some adornments in the guise of habit formation. In this case what has already been said about machine theory, about habit or conditioning as the building up of a secondary machine, applies immediately to instinct. Two machine theories of development, knit together in one application, can lead only to a more complicated theory of essentially the same type. Instinct is sometimes regarded as a supranatural process, accomplishing those practically adequate sequences of behavior which we are not able to explain otherwise in a sensible manner. In this form the concept is so little suited to my scientific taste that I should like to be excused from further discussion of it.

And then? Surprisingly enough, apart from these explanations of serial behavior, we can only account for merely "random" sequences in psychology.

Where neither an inherited nor an acquired machine determines the sequence of events, our theoretical formulations, not accustomed to dynamical considerations, will regard chance as ruling whatever may happen to occur. So much we have already seen in Chapter IV. But a third possibility of orderly determination was found there, which we shall now discuss in its application to the total field.

After what has been said about the self's states as depending upon the properties of its environment, felt uneasiness (felt as resulting from too much heat on one side of the body) will have a corresponding process in its underlying brain-field, which will be determined directly by that other process underlying the experience of laterally unequal heat. But the tendency to move away from the heat is felt as developing in this situation, just as directly as the uneasiness is. Consequently we must again draw the same physiological conclusion: if some process underlying a certain sensory experience directly causes another process underlying dislike and uneasiness in the self, a tendency toward increasing the distance between the place of the first and the place of the self must develop just as directly at the same time. As the tendency is felt to grow out of the nature of that sensory experience, and of the experience of uneasiness, the tendency must be physiologically determined by the concrete properties of the corresponding processes. In dynamics such a tendency toward increasing the distance between two things or events is called a field of force. We may say, then, that there is in the brain-field a field of force tending to increase the distance between the place of the radiator and the place of the self. If, now, the subject overtly moves in that direction, what happens in the brain-field? As the distance is enlarged objectively, exactly the same occurs in the brain-field,—the process corresponding to the self moving away from the processes of the radiator and of the heat. The subject will feel himself moving in precisely the same direction which was felt one moment before as the direction of a tendency to movement; and, physiologically, the increase of distance in the brain-field will correspond exactly to the stress which, as a field of force, was tending in that same direction. Therefore, physiologically, as well as in experience, what happens will be no more than the natural consequence, the real development, or the evolution of *something* which, in germ, was already implicitly contained in the tendency.

So we are led to a more complete application of the theory of direct dynamical determination. There is no mere sequence of indifferent events, connected indirectly. Each phase of what happens grows out of its predecessors, depending upon their concrete nature. And the subject, whose experiences are an expression of this one developing context in the brain-field. will experience the development, along with its "referring to," "depending upon," "away from" and so forth—that is, with *insight.*

One remark is needed here with regard to the overt movement of the organism in its relation to a field of force, existing between the self and some part of its environment, in the brain-field. We know that during the first months a child is not able to move his hands, in accordance with the interest which an object has aroused in his sensory field. This fact may be interpreted in two different ways: (1) Either there is no natural determination of overt movement by definite fields of force in the brain. In this case all "connections" between the two would have to be learned. (2) Or the hand does not yet move in the direction of actual interest for some other reason, whereas when that unknown obstacle is removed a little later by organic development, the general direction of the clumsy movements depends directly upon actual fields of force in the brain. The astonishingly early coördination of eye-movements would make me inclined to consider the second hypothesis no less seriously than the first. However, the theory of direct determination does not depend upon this choice. Even if learning should be the only way in which movements of the hand or of any other parts of the body become connected with the distribution of forces in the brain-field, the *right* movement will still be marked out by more than the final success it achieves. As soon as, partially or entirely, the direction of an overt movement coincides with the actually existing nervous field of force, there will be a relaxation of tension or stress. Therefore, any beginning in the right direction will be felt as being in the right line. Later on, when coördination of movement is complete, familiar overt movements will always go in the right direction; they will be felt as fitting into the dynamics of the situation.

In order to show how the fundamental motors of behavior can bring about more complicated conduct through direct determination, I shall conclude with the analysis of an observation made on an ape several years ago.

Outside the animal's cage a banana is placed on the ground, beyond the reach of his arm. In a similar situation some days before the ape had used a stick as an instrument for the first time. He finds a stick to-day, too, but it is a rough thing. A branch projects from the side of the stick, near the ape's hand. At first this branch does not operate as an obstacle because, when he reaches for the fruit, the ape holds his instrument accidentally in such a position that the branch is parallel to the bars of the cage and does not touch them. Presently however, in order to change his place with regard to the fruit, the ape pulls the stick back into the cage. When he reaches for the fruit again, the branch hits a bar and the stick does not move in the direction of the fruit.

The animal tries to overcome the difficulty by pushing hard and crudely a few times. Suddenly, however, and just after pressing forward with all his force, he pulls the stick back in the cage and bites the branch with the ferocity of anger. It does not matter for our purposes whether or not this new conduct was successful. But how did it come about at all? An explanation may be given in terms of brain-physiology or from the viewpoint of experience. Though I do not think that the experience of animals is a matter of great intrinsic interest for us, I shall treat the case in terms of experience, since it would be more difficult to understand the meaning concretely if I made the neurological analysis. When the ape is first directed completely toward the fruit, his reaching for it is determined directly in the manner we have described. To begin with, the obstacle in his way directly produces an increased effort in the same direction. But the ape can scarcely pull against the bar of his cage without localizing the place of the obstacle after a while by touch and, perhaps, by vision. He does not feel the obstacle in the direction of his main effort; he feels it in the locality of the branch. When an obstacle is experienced in our way we become angry by immediate determination, and our anger is an attitude directed toward or against the obstacle. Consequently, after the main object, the banana, has determined the primary direction, the felt obstacle will make the ape's direction bend toward the branch, and since the tendency directly determined by anger is attack or destruction, the ape will begin to destroy the obstacle.

I do not know whether this is a completely satisfactory description of the whole process. When he attacks the branch, the ape, though actually angry, is probably still determined by his *main* tendency at the same time. And this would be required. Otherwise the whole development would become a sequence of events, each occurring in direct determination, i.e., with insight, but not to be regarded as members of one single development occurring in one given situation. Here, however, we reach the field of intelligence proper, which I have excluded from the program of these chapters.

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