CHAPTER II

BEHAVIOUR AND ITS FIELD

The Task of Psychology

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THE STARTING POINT

We have developed a very ambitious programme and must now begin to carry it out. But where are we to begin, what is our starting point to be? Everybody knows the kind of facts in psychology which he wants to learn something about; there are all too many, and that makes it so difficult to choose one for a beginning. Why do we love our families, why can one person enjoy music while it is boring to another, why is it so difficult to understand mathematics, how does a great scientist hit upon his new ideas, why are some people extremely conservative, others extremely radical, how do children differ from adults, animals from humans? However, all of these questions presuppose a whole theoretical system which we have not yet developed. No such question can therefore stand at the beginning of a treatise on psychology. Should we then start by selecting fundamental facts? The difficulty remains the same, for which facts are fundamental, and how would the student to whom such an allegedly fundamental fact was presented know it to be fundamental? This is a very real difficulty which I remember all too well from my student days. When in the first lectures of my first course in psychology the professor talked about colour mixture, colour contrast, and the colour pyramid, I began to be very deeply disappointed with psychology, for I could not for the life of me see why these were fundamental psychological facts.

Before a fact can become a fundamental fact, a setting must have been prepared in which all facts take their more or less prominent places, be it on the ringside or in the gallery.

DEFINITIONS OF PSYCHOLOGY

Such a setting is usually given by a definition of psychology, what its subject matter is, what its methods are. Since the methods depend upon the subject matter we shall concentrate on a definition or, better, on a delineation of our science first. Three different definitions of our subject matter can be discriminated: Psychology as the science of consciousness, of mind, and of behaviour. Although psychology was reared as the science of consciousness or mind, we shall choose behaviour as our keystone. That does not mean that I regard the old definitions as completely wrong—it would be strange indeed if a science had developed on entirely wrong assumptions—but it means that if we start with behaviour it is easier to find a place for consciousness and mind than it is to find a place for behaviour if we start with mind or consciousness.

The swing from consciousness to behaviour is largely due to the work of American psychology, although, as far as I know, William McDougall was actually the first to define psychology in terms of behaviour. But what he meant by behaviour was something different from and much more inclusive than what is meant by the American school which takes its name from this term. Since their usage of the term is restricted and implies a *theory* of behaviour we must return to McDougall's usage, which is purely descriptive and therefore does not prejudge in favour of any theory.

MOLAR AND MOLECULAR BEHAVIOUR

The difference between McDougall's and the behaviourists' meaning of behaviour has been very appropriately described by Tolman as the difference between behaviour as a molar and a molecular phenomenon. Without going into a detailed exposition at this moment I will give a few examples to bring this difference home. A molar behaviour is: the student's attendance at class, the lecturer's delivery, the pilot's navigation, the excitement of the spectators at a football game, Mr. Babbitt's flirtation, Galileo's work which revolutionized science, the hunting of the hound and the running of the hare, the biting of the fish and the stalking of the tiger, in short, all those countless occurrences in our everyday world which the layman calls behaviour. Molecular behaviour, on the other hand, is something very different: the process which starts with an ex-

citation on the sensory surface of an animal, is conducted by nerve fibres to nerve centres, switched over to new, efferent nerves, and ends in a muscle contraction or a gland secretion. Now the ordinary man, probably more than 99% of the population of the earth, knows nothing about the latter, whereas everyone knows the former; on the other hand, those who know anything about physiology will have to admit that molar behaviour always implies muscle contractions which in their turn set our limbs into motion and are activated by nervous impulses. It is very easy to pass from a statement like this to another: molar behaviour is a secondary phenomenon; it is but the last outwardly observable result of a great number of physiological processes; these are the primary events; these form continuous causal sequences; and, therefore, these alone can form the subject matter of a science. Therefore, for behaviourism molar behaviour supplies no more than the problems, the solutions must always be given in terms of molecular behaviour, so that the finished system of psychology will contain only molecular data, the molar ones having been completely eliminated. We are not yet concerned with the particular mode in which behaviourism tried to carry through its programme, but we may emphasize two aspects of its doctrine: (1) It attributes reality to parts, denying it to the wholes which these parts compose: the molar has to be resolved into the molecular; (2) as a result of this, psychology would forever remain exposed to the criticism of the Moral Sciences which we have discussed at the end of the first chapter. Meaning and significance could have no possible place in such a molecular system; Caesar's crossing the Rubicon: certain stimulus-response situations; Luther at Worms: so many others; Shakespeare writing "Hamlet"; Beethoven composing the Ninth Symphony; an Egyptian sculptor carving the bust of Nephretete, would all be reduced to the stimulus-response schema. What then holds our interest in these occurrences? If they are nothing but combinations of *one* type of events, stimulus-response sequences, why do we not take as much interest in the sequence of numbers that come out as winners on the roulette table, why do we not pore over a list of all the bridge hands that have ever been dealt? The behaviourist will explain this by saying that the sequence of stimulus-response situations in most of us has been such that now we react positively to Shakespeare and Beethoven, and negatively to statistics of rouge et noir. At this the historian would throw up his hands in despair, and would continue his work confirmed in the conviction that psychology, whatever else it might be, is perfectly useless for his purposes, and the behaviourist would

let the historian continue writing his fiction, equally convinced that his was the only truth.

Clearly such a state of affairs is highly unsatisfactory to anyone who is not a sceptic by nature or profession. What can he do to satisfy the just claims of the two opposing factions, to prevent the disruption of knowledge into a number of incoherent sciences? If psychology is to be the science of behaviour, must it not have a real place for Caesar, Shakespeare, Beethoven, a place which gives to the behaviour of these men the same *outstanding* and *distinctive* position in his system which they enjoy in the estimation of the ordinary educated person and the historian? It is clear that such an aim cannot be achieved if psychology begins and ends with molecular behaviour. Let us try molar behaviour instead. Perhaps it will be possible to find a place for molecular behaviour in a system that begins and ends with molar.

MOLAR BEHAVIOUR AND ITS ENVIRONMENT

What is the most general statement we can make about molar behaviour? That it takes place in an environment, whereas molecular behaviour takes place within the organism and is only initiated by environmental factors, called the stimuli. Molar behaviour of the type we have chosen for our examples occurs in an external setting: the student's class performance occurs in the classroom in which the lecturer holds forth; conversely, the lecturer behaves in a room filled with students who at least understand his language, if nothing else; Mr. Babbitt flirts in a very definite *social* environment, to say nothing about the partner necessary for this accomplishment; the hound and the hare both run through the field, and for each of them the other is the outstanding object of the environment. All this sounds obvious and banal. But it is not quite as trivial as it appears at first sight. For in reality there are, in all of the cases just mentioned, two very different environments to be distinguished from each other, and the question has to be raised: In which of them has molar behaviour taken place? Let us illustrate our proposition by an example taken from a German legend.

The Geographical and the Behavioural Environment. On a winter evening amidst a driving snowstorm a man on horseback arrived at an inn, happy to have reached a shelter after hours of riding over the wind-swept plain on which the blanket of snow had covered all paths and landmarks. The landlord who came to the door viewed the stranger with surprise and asked him whence he came. The man pointed in the direction straight away from the

inn, whereupon the landlord, in a tone of awe and wonder, said: "Do you know that you have ridden across the Lake of Constance?" At which the rider dropped stone dead at his feet.

In what environment, then, did the behaviour of the stranger take place? The Lake of Constance. Certainly, because it is a true proposition that he rode across it. And yet, this is not the whole truth, for the fact that there was a frozen lake and not ordinary solid ground did not affect his behaviour in the slightest. It is interesting for the geographer that this behaviour took place in this particular locality, but not for the psychologist as the student of behaviour; because the behaviour would have been just the same had the man ridden across a barren plain. But the psychologist knows something more: since the man died from sheer fright after having learned what he had "really" done, the psychologist must conclude that had the stranger known before, his riding behaviour would have been very different from what it actually was. Therefore the psychologist will have to say: There is a second sense to the word environment according to which our horseman did not ride across the lake at all, but across an ordinary snow-swept plain. His behaviour was a riding-over-a-plain, but not a riding-over-a-lake.

What is true of the man who rode across the Lake of Constance is true of every behaviour. Does the rat run in the maze *the experimenter* has set up? According to the meaning of the word "in," yes and no. Let us therefore distinguish between a *geographical* and a *behavioural* environment. Do we all live in the same town? Yes, when we mean the geographical, no, when we mean the behavioural "in." [1]

In Which Environment Does Behaviour Take Place? After having distinguished two kinds of environment we have to discuss the question more fully in which of them behaviour occurs. It will help to elaborate this latter concept if we raise the question: How does behaviour occur in an environment, what are the general characteristic relations between behaviour and environment? Take the example of the hound and the hare: the hare starts from a bush and runs across an open field in a straight line; the hound will follow him; when he comes to a ditch, the dog will change its running movement into a jumping movement and clear the creek. Now the hare changes his direction; at once the dog will do the same. I need not continue; what I have said will suffice to draw the inference that the behaviour is *regulated* by the environment. *Which* of the two environments does the regulating, the geograph-

[1] This point is lucidly developed in the beginning of Eddington's beautiful book.

ical or the behavioural? From our last example one might be inclined to answer: The geographical. But suppose now that the ditch were covered by a thin layer of snow, sufficient to bear the weight of the hare but not that of the hound. What would happen? The dog would fall into the ditch, i.e., he would not jump when he came to the ditch but would continue to run.. He would, before his fall, behave in a ditchless environment. Since, however, the geographical environment contained the ditch, his behaviour must have taken place in another one, namely, the behavioural. But what is true of the few short moments in which the dog stepped on the treacherous layer of snow, must be true of his entire behaviour; he has been in that behavioural environment all along.

THE STIMULI AS A SUBSTITUTE FOR THE BEHAVIOURAL ENVIRONMENT. Against this argument one might raise the following objection. Nobody in his senses could expect that the dog would jump over a snow-covered ditch, or claim that any animal behaved with regard to the geographical environment per se. Obviously, two different geographical environments which are equal with regard to the manner in which they affect the animal's sense organs, are also equivalent for its behaviour. If, therefore, one substitutes the term stimuli for the term geographical environment, the whole difficulty disappears, and there is no need to distinguish a behavioural from a geographical environment.

Justified as this reasoning appears with regard to our example, it can easily be shown to be wrong. We choose a new type of behaviour. Two chimpanzees are separately brought into a cage from the ceiling of which an enticing banana is suspended. The cage is absolutely empty save for a box in a place ten feet removed from the spot over which the lure dangles. One of the animals will, after a longer or shorter delay, run to the box, carry it under the fruit, and using it as a stool, take possession of the banana. The other, less intelligent, after various unsuccessful jumps will resign himself and eventually ascend the box to sit there sunk in gloom. Both apes have behaved in a geographical environment that contained a box; for both, the stimulus situation was identical. And yet they have behaved differently, and the behaviour of each was regulated by the environment. The geographical environment, or the stimulus situation, cannot be the cause of the different behaviours. But this difference is explicable as soon as we consider the behavioural environments of our two animals. We could describe or explain the activities of either of them very well if we assumed that the behavioural environment of the one contained a "stool" and that of

the other a "seat," or in more general terms, the behavioural cage of the one contained an object functionally alive with regard to the ape's present trend of action; that of the other an object functionally dead.

INDIVIDUAL DIFFERENCES. My discussion of this example will meet with no less fierce opposition than that of the first. Far from admitting the validity of my inference about the behavioural environment of the two chimpanzees, the critics will say that I try to reintroduce the old anthropomorphic explanations which fortunately psychology had discarded for good, and that in addition I had overlooked a much simpler explanation. If two animals behave differently under similar stimulus-conditions, then the explanation must be in the animals themselves; they are either by innate endowment or by their previous experience so different from each other that the one behaves in one way, the other in another. I shall not defend myself here against the first part of this attack and accept the proposition of the other. Certainly, if the geographical environment is the same for two animals and the animals behave differently in it, then the cause of this difference must be found in the ("geographical") animals. But I want to go beyond this conclusion, for it is incapable of explaining any actual example, just because it applies to any and every kind of behaviour. And clearly, when I view the *molar* behaviour of these two apes, I find that one *uses* the box as a stool; the other *uses* it as a seat. This description is as adequate as possible, for neither does the intelligent ape fumble about the box until after many vicissitudes he finds himself incidentally standing upon it, nor does the other behave similarly with the only difference that in the end the box is still in its old place and the ape drowsing upon it. No, their molar behaviour is truly described by saying that the one uses a stool, the other a seat. Certainly the two animals must be different animals, but we can now see that this difference must be such as to make of the geographical box two different *manipulanda,* to borrow another term from Tolman. What more do we say when we call these two manipulanda parts of the behavioural environments of our two apes? We started our whole discussion of molar behaviour with the proposition that it takes place in an environment. Since the geographical environment or the "stimulus-providing geographical environment" cannot be the immediate cause of the two behaviours, we must either deny our proposition and establish behaviour without environment—and then our manipulanda would have no place at all—or we must accept these manipulanda as realities, stick to our proposition, and then

retain the behavioural environment as that kind of reality which contains the manipulanda and possibly other things as well. In other words, we maintain that the relation between behaviour and the geographical environment must remain obscure without the mediation of the behavioural environment.

BEHAVIOUR AND GEOGRAPHICAL ENVIRONMENT. Let us summarize what we have gained so far: behaviour takes place in a behavioural environment by which it is regulated. The behavioural environment depends upon two sets of conditions, one inherent in the geographical environment, one in the organism. But it is also meaningful to say that behaviour takes place in a geographical environment. What does this signify? (1) Since the behavioural environment depends upon the geographical, our proposition connects behaviour with a remote instead of an immediate cause. This may be useful in itself, and it will help to set our problem, for (2) the results of the animal's behaviour depend not only on his behavioural but also on his geographical environment, quite apart from the dependence of the former upon the latter. The geographical environment, not only the behavioural, is changed through all behaviour: the fruit is eaten and thereby ceases to exist as a fruit; the snow bridge is broken and gives place to a hole; the box is actually transported when the ape uses his "stool." As a matter of fact, in all our examples, and in a great many others, the behavioural result depends upon a geographical result. The type of behaviour which we have so far exclusively considered cannot occur in a behavioural world alone, although there are other types where this is more or less the case, as, for instance, when a man in delirium tremens catches nonexistent fish in his tub and shows them with great pride to the attendants. We gather from this that the relation between the two environments will present us with a fundamental problem in our future theories.

Behaviour Defined. (3) One particular aspect of the second point may be given a special mention: certain properties of the geographical environment will produce movements of the organism which we have not yet considered. Think of a mountaineer who breaks through a snow bridge, and not being roped to a companion, falls hundreds of feet into the icy chasm. Here we have a movement of an organism that is exclusively determined by the geographical environment. Before the victim loses consciousness he may make frantic efforts to stop his fall. These movements are still behaviour occurring in a behavioural environment, but at the same time the body drops whether there exists a behavioural environment or not

and whether the man has retained or lost consciousness. This is again perfectly banal, and yet it gives us a means of defining behaviour: only such movements of organisms are to be called behaviour as occur in a behavioural environment. Movements which occur only in a geographical environment are not behaviour. It should be noticed that this definition does not claim that all behaviour is movement.

The Locus of Behavioural Environment. Let us now go one step further. So far the behavioural environment has been introduced as a mediating link between geographical environment and behaviour, between stimulus and response. These two terms denote objects which seem to have a very definite place in our system of knowledge; they both belong to the external world. But what is the locus of behavioural environment? To prepare for our answer we may discuss a new example, a series of experiments by Révész. Révész trained chicks to peck from the smaller of two simultaneously presented figures. Beginning with circles he then substituted rectangles, squares and triangles, taking good care that the relative positions of the two figures were continually changed; this is necessary, of course, in order to obviate the possibility that the animals, instead of learning to choose the smaller, might have learned to choose the "right" or the "left," the "upper" or the "lower." After this training was completed, he introduced as new figures two segments of a circle of different sizes presented in different positions; he then interspersed his critical experiments: two *equal* segments were presented so as to cause for us the well-known optical illusion called the Jastrow illusion. (See Fig. 1.) And in the vast majority of cases the hens pecked from the one which to *us* appears smaller. The whole course of this experiment is a demonstration of behavioural environment, for in terms of geographical environment it is meaningless to say that the birds learned to choose the *smaller* of two figures. "Of squares, parallelograms and triangles, the animal in the majority of cases chose the smaller figure *without any introductory training*" (p. 44). But for our present purposes the critical experiments are of particular interest. Why do the animals choose one of the two equal figures, when they have been trained to choose the smaller one? Described in these *geograph-*

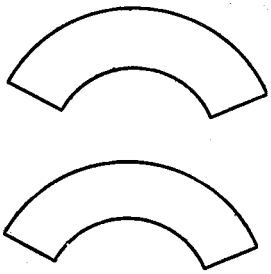

Fig. 1

ical terms their behaviour seems quite unintelligible, and neither stimulus properties nor experience can supply even the semblance of a satisfactory answer. But everything becomes perfectly plain and simple if we answer our question as every unbiassed layman would answer it, by saying: The animals chose one of the two equal figures because it *looked* to them smaller, just as it looks smaller to us. Or, in our terminology, the behavioural environment in the critical experiments was similar to the behavioural environment in the training experiments inasmuch as it too contained a larger and a smaller figure, although the critical geographical environment contained two figures of equal size. The behaviour of the hens cannot be explained in any way without the assumption that they were directed in their choice by a relation. Since this relation certainly does not obtain in the geographical environment, it must have been present somewhere else, and this somewhere else is what we call the behavioural environment. When now we remember what the layman had to say about this experiment, we see that our difference between the geographical and the behavioural environment coincides with the difference between things as they "really" are and things as they look to us, between reality and appearance. And we see also that appearances may deceive, that behaviour well adapted to the behavioural environment may be unsuited to the geographical. If, for instance, we were as naïve with regard to the Jastrow illusion as Révész's hens and happened to need two objects of equal shape and size we would not choose these two figures. I can illustrate the truth of this proposition by an experiment which I made in the summer of 1932 in a small village in Uzbekistan in Central Asia. I had shown the Jastrow illusion, using the pattern of the "Pseudoptics," to a young native who was the host of the tea house, the one meeting place of the male populace of the village. The man behaved as the hens did, apart from the fact that his reactions consisted not in pecking but in pointing to the larger of the two pieces of cardboard. I then put one on top of the other and gave them to him to handle. I wanted to see what explanation he would give to the curious discrepancy between their previous inequality and their now manifest equality. He said something like illusion, but without much conviction; and when I asked, "So you think they do not *really* change when you take them apart," he answered, "Oh, yes, I believe they *will* change a little."

Function of Behavioural Environment. Our argument, based on the Révész experiments, proved that the relation between the geographical environment, or the stimulus pattern, and behaviour

is tremendously simplified by the introduction of the behavioural environment as a mediating link. This relationship is thus broken up into two different relationships, that between the geographical and the behavioural environment and that between the latter and behaviour. That this second relationship is, at least in many cases, intelligible, appeared from our example; if the upper of the two geographically equal ring sectors was behaviourally smaller, then the fact that the animals trained to choose the smaller of two figures selected the upper one offered no new problem.

We might have demonstrated the same fact in precisely the opposite way. It happens again and again that *different* stimuli produce the same reaction: this becomes perfectly intelligible if we know that under the given conditions of the case these different stimulations produce the same behavioural object. We shall discuss such cases later (Chapter VI) when we treat perceptual constancies, like those of size and colour, from the point of view of the relation between the geographical and the behavioural environments. At the moment we will only point out that, e.g., two surfaces may both look black, although one may reflect a thousand times as much light as the other; or expressed in terms of behaviour: two stimuli as different as those which we have just mentioned may lead to the same behaviour, for instance, if the task is to pick up a black object. To account in terms of stimulus response for this uniformity of behaviour in the face of the tremendous diversity of stimulation is impossible, particularly if one remembers that under other conditions a difference in stimulation of only 2% will lead to different behaviour.

In terms of the behavioural environment the difficulty disappears; behaviour with regard to two stimuli is identical when they produce two identical behavioural objects; it is different when the two corresponding behavioural objects are different. The problem which remains is no longer that of the relation between stimulus and behaviour, but that of the relation between the geographical and the behavioural environment. This problem can be solved systematically, but the problem of the pure stimulus-response relation can find no systematic solution as proved by the facts of constancy —identical behaviour with respect to different stimuli—and those of the Révész experiment—different behaviour with respect to identical stimuli.[2]

[2] This criticism applies with equal force to Tolman's definition of discriminanda and discriminanda capacities. (Pp. 86 f. and 91 f. See also Koffka, 1933, p. 448.)

Consciousness. In the beginning of this chapter I proposed to use behaviour as the primary subject matter of psychology. But in my distinction of geographical and behavioural environment, admittedly equivalent to that of reality and appearance, have I not smuggled in consciousness through a back door? I must deny this accusation. If we are forced to introduce the concept of consciousness, we have to accept it, whether we like it or not. But it is important to note that the *word* consciousness does not change the *meaning* of our own term behavioural environment. If anyone wants to speak of the animal's consciousness instead, he must apply this word to those objects which we call behavioural environment. Thus the dog's consciousness in chasing a hare would be "a hare running through a field," the ape's consciousness in trying to obtain the suspended fruit would be "a stool standing in that corner," and so forth. The field and the hare, the stool and the fruit, by being called conscious, or objects of consciousness, must not therefore be considered as something *within* the animal, if this has the meaning of a behavioural, or experienced, within. The behaviourists' aversion to consciousness seems to me to be largely founded on this misinterpretation. And their claim that they can write a psychology without consciousness can now be shown to be false. The animals they observe, the mazes and discrimination boxes they use in their experiments, the books in which they record their results, all these are first of all parts of their behavioural environments. Forgetting this fact, and believing that they are speaking of geographical environments only, they think that they can build a purely "geographical" theory without behavioural data. But every *datum* is a behavioural datum; physical reality is not a datum but a constructum. This confusion is obscured, and the general obscurity is increased by the use of the word stimulus, the vicissitudes of which we shall deal with later. Here I want merely to point out that it is easy to write a psychology without consciousness if one fails to recognize that one's own environment is a behavioural (conscious) and not a geographical (physical) one. I will add that there is some excuse for the behaviourists' error in the traditional treatment of consciousness about which we shall learn later. In consideration, however, of the possible misinterpretations, I shall use the term consciousness as little as possible. Our term behavioural environment, though it includes only a part of what is meant by consciousness, should escape misinterpretations; as fully equivalent with consciousness Köhler (1929) has used the term "direct experience" which we shall also adopt for occasional use. Our term has the ad-

vantage that it signifies the exact place which it has in the system, viz., the mediation between geographical environment and behaviour.

Behavioural Environment Only a Part of Direct Experience. But, as I said, it is incomplete; consciousness means more than behavioural environment. And it seems appropriate to indicate now at least the direction in which it is to be completed, although for a long time we shall be concerned only with the behavioural environment. This direction will be seen if we subject our term behaviour to the same analysis which we performed with regard to the term environment. We can, indeed, describe behaviour with reference to either of our two environments, and such descriptions may often be contradictory to each other. But whether they agree or not, behaviour itself must have a different meaning in these two descriptions: since behavioural and geographical environment belong to two different universes of discourse, the behaviours which occur within them must belong to the two different universes also. The man who rode across the Lake of Constance is a good example: his geographical environment was this big lake, his behavioural an ordinary snow-covered plain; accordingly, as we have pointed out before, although with regard to his geographical environment his behaviour was riding across the lake, his behaviour in regard to his behavioural environment was riding across a plain. Or in the terms of the layman: he *thought* he was riding over terra firma; he had no notion he was riding on thin ice.

Thus at first sight it seems as though the distinction between our two behaviours was completely analogous to that between our two types of environment: here the things as they look and as they really are, there the activity as the actor *thinks* it is and as it really is. But the similarity is not quite as great as it appears. Let us take another example: we observe three rats in the same maze, each starting at one end and finally emerging at the other. Then in a way we could say the three rats have run through the maze, a geographical statement. But our observation has convinced us that there were obvious differences in their behaviour: one ran for food, one to explore, the third for exercise or from general restlessness. These characteristics refer to the behaviour within the *behavioural* environment. A rat running for food does not do so only from the moment when it is near enough to see or smell it, but from the very beginning. Tolman's book gives ample experimental evidence for this statement. But the first part of the geographical maze does not contain the food, nor any stimulation emanating from the food.

If, nevertheless, the behaviour is directed towards the food, it must be so in its behavioural environment. The same is true of exploratory behaviour. We can explore directly only our behavioural environment, and indirectly merely, through the behavioural, the geographical one. And even in the last case, the behaviour for exercise or from restlessness is a behaviour in a behavioural environment since it is regulated by it. Now in all these cases it is no longer a true description of the two kinds of behaviour to say: behaviour in the geographical environment is the activity as it really is, in the behavioural as the animal thinks it is. For an excited behaviour is really an excited behaviour, an exploratory one really exploratory, and even a food-directed activity is really food-directed even if the experimenter has removed the food from the food box. In this last case, indeed, it is also true that the animal does not run towards the food, because geographically there is no food, and in some sense our distinction applies here as it did in the case of the Lake of Constance. But this is no longer a description of *behaviour*. I shall try to explain this by an example: a ball runs down an incline and finally falls into a hole. Now there may be water in the hole or not, and therefore I can say the ball falls into a hole with water or without water. But this difference does not affect the motions of the ball until it has reached that position in space where the water begins in the one case and not in the other. For the rest of the motion the presence or absence of water is wholly irrelevant; similarly, the statement that the rat does not run towards food when the experimenter has just removed it, is quite irrelevant to the run of the rat until it is near enough to notice the absence of food.

Behaviour and Accomplishment. If the description of behaviour with reference to the geographical environment is not truly a description of behaviour, what then is it? In order to simplify our terminology we shall from now on call behaviour with regard to geographical environment "accomplishment," and behaviour with regard to behavioural environment just behaviour. The name "accomplishment" indicates directly the reason for describing behaviour with reference to the geographical environment, because results of behaviour issue, as we have pointed out, in changes of the geographical environment. We are often interested in these results, which are the accomplishments of an animal. But we have learned just now that the knowledge of an animal's accomplishment is not knowledge of its behaviour. I will give a striking example where accomplishment and behaviour are in a sense opposed to each other. Suppose I see a person standing on a rock which I know is to be

blasted away at this very minute. He is too far away for me to explain his danger to him, so I shout as loud and urgently as I can: "Come here, quick!" The person, if sufficiently impressed by my command, will begin to run *towards me* behaviourally, but geographically, in running towards me, he runs away from the danger spot; geographically speaking, these two descriptions are absolutely equivalent. If I, however, afterwards relate this incident, I will say that he got away before the explosion occurred. I describe his accomplishment and not his behaviour; the latter was a motion towards something, the former a motion away from something. If the connection between behaviour and accomplishment were always of this kind, this world would be a strange place indeed, and it would certainly not be a world in which we would develop the concept of meaning. It might be a world of fairy tales; think of Aladdin who rubbed the lamp and accomplished thereby the appearance of the djinn. We shall see that experimenters have frequently put animals in situations where the behaviour and accomplishment were connected in a manner similar to the rubbing of the lamp and the appearance of the djinn. But even though as a rule behaviour and accomplishment do not hang together in this Wonderland way, the relation between accomplishment and behaviour is in one respect similar to that between the geographical and the behavioural environment: if we know one member of either pair we do not yet know the other. But whereas the first relation is one of the most important straightforward problems of psychology, the second has no such simple standing. As a general question, as may be deduced from our last examples, it does not, strictly speaking, enter psychology at all. Still it is a question of some interest which we shall take up again; furthermore, since the relation between accomplishment and behaviour is not, as a rule, of the fairy tale type, we may often be able to draw inferences from accomplishment to behaviour and *its* environment. The whole objective method makes use of this possibility; the time a rat takes to run a maze, the number of errors it commits, which blind alleys it will enter and which not, all such facts give us clues for an interpretation of behaviour and behavioural environment, but they are not in themselves statements about behaviour.

For we have seen that the only system of reference for describing behaviour proper is the behavioural environment. And thus we have so far failed to solve the problem that was set at the beginning of this long discussion, viz., to supplement our concept of behavioural environment so as to make it as comprehensive as the con-

cepts of direct experience or consciousness. We shall now return to this question.

OUR SOURCES OF KNOWLEDGE OF BEHAVIOUR! How do we acquire knowledge of behaviour? Behaviour of an animal is part of our behavioural environment, and we know it as such, together with all the other objects and events in our behavioural environment. The question, how we can know *real* behaviour, is, therefore, no different in principle from the question of how we know any non-behavioural reality. It will not occupy us now; we could not answer it before we had learned something of the general relation between our geographical and our behavioural environment. At the moment, two remarks must suffice: (1) That we must assume the existence of real behaviour just as we must assume the existence of real tables and books and houses and animals. (2) Since we have shown that behaviour is always behaviour in a behavioural environment, not ours but the animal's which behaves, we can now settle one of the objections formerly raised against our procedure, viz., that it is anthropomorphic. We observe an animal's behaviour in our behavioural environment. If we assumed, without further evidence, that our behavioural environment and that of the animal were identical, then we should certainly be open to the criticism of anthropomorphism. The assumption, on the other hand, that the animal behaves in *a* behavioural environment, viz., its own, is not anthropomorphic at all. How far this environment is identical with ours, in what characteristic aspects it differs, are very important questions indeed, and in their solution we must be careful to avoid anthropomorphism. But let us return to our main argument: on the ground of an animal's behaviour in our behavioural environment, and by more indirect methods, we infer the nature of the animal's real behaviour. But we are behaving ourselves. And of this behaviour also we have knowledge. We find it happening in our behavioural environment, but the word "in" has now a different meaning from the one it had when we spoke of another animal's behaviour occurring in our own behavioural environment. The animal is a *part* of our behavioural environment, we ourselves are the centre of our environment, although not "of it." The environment is always an environment of something, so my behavioural environment is the environment of me and my behaviour. Just as I know my behavioural environment, so I know myself and my behaviour in this environment. Only if we include this knowledge with the behavioural environment have we gained a complete equivalent of what Köhler calls direct experience, or what is called consciousness.

This knowledge includes, to enumerate a few items, my desires and intentions, my successes and disappointments, my joys and sorrows, loves and hatreds, but also my doing *this* rather than *that.* An example of the last: my friend asks me, "Who is the lady you raised your hat to?" I answer, "I did not raise my hat to any lady; I just raised it because it was too tight on my head."

REAL, PHENOMENAL, AND APPARENT BEHAVIOUR. We can now introduce a new terminology. We have seen that we must distinguish *two* classes of behaviour from real behaviour, my behaviour in somebody else's behavioural environment from my behaviour in my own behavioural environment; or, with an interchange of subjects, somebody else's behaviour in my behavioural environment from his behaviour in his own behavioural environment. We shall call the first of each pair *apparent* behaviour, the second *phenomenal* or *experienced* behaviour. The apparent behaviour may, as our hat-raising example shows, be misleading with regard to real behaviour, but it might also have been a true guide, e.g., if I had really bowed to a lady. The phenomenal behaviour, on the other hand, was a true index. No doubt phenomenal behaviour is a very valuable clue for our knowledge of real behaviour. Whereas the relation of apparent to real behaviour is of the same kind as that between behavioural and geographical environment, that between phenomenal and real behaviour is of a different nature. To some extent real behaviour reveals itself in phenomenal behaviour. But only to some extent. For phenomenal behaviour never reveals more than a fraction of real behaviour, and this fraction may not always be the most important one. We shall take up this point later. Now we draw the conclusion that it would be just as wrong to discard phenomenal behaviour for our knowledge of real behaviour as to use it exclusively and blindly.

Behaviour and Environment Summarized. We may, in concluding, schematize our discoveries about behaviour and environment. (See Fig. 2.) G is the geographical environment. It produces BE, the behavioural environment; in this and regulated by it, RB, real behaviour, takes place, and parts of it are revealed in PH B, phenomenal behaviour. In some sense BE, RB, and PH B occur within the real organism RO; but not in the phenomenal Ego, which belongs with PH B. RO is directly affected by G and acts back upon it through RB. Our schema does not indicate the dependence of BE and PH B upon the organism,

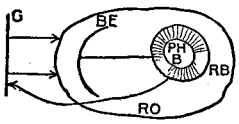

Fig. 2

neither does it contain the results of behaviour. But by RB affecting G two further changes occur: BE is changed and the phenomenal Ego is changed. When the ape has eaten the fruit, his behavioural environment has become "fruitless" and the animal itself "satisfied."

THE FIELD CONCEPT

So far we have clarified the concept of molar behaviour; we have seen that it takes place in a behavioural environment and that we have knowledge of it in two ways, the one revealing apparent molar behaviour, that of others, the other phenomenal molar behaviour, that of ourselves. Both types of knowledge are to be used for an understanding or explanation of real molar behaviour. Furthermore, we have gained some insight into the dynamical aspect of real molar behaviour. In this way we have laid the foundation for psychology as the science of molar behaviour. We must now elaborate this point. Which are to be the most fundamental concepts of our system? One of the postulates of our psychology was that it be *scientific*. Therefore let us try to discover one of the fundamental concepts of science which we can apply to our task. A short excursion into the history of science will lead us to our discovery. How did Newton explain the motion of bodies? According to him every change of motion is due to a force which arises either through impact—two billiard balls—or by an attraction exercised mutually by the bodies upon each other, according to his law of gravitation which gives a quantitative formula of this force. This force Newton assumed to act without time; it produced an action at a distance. There is the sun, here is the earth, nothing between them but empty space, nothing to mediate the attractive force of the sun upon the earth and vice versa. When, much later, the laws of magnetic and electric attraction and repulsion were discovered and proved to be quantitatively identical with Newton's law of gravitation, the same interpretation was given to them; they were interpreted as actions at a distance. This conception of a timeless action had been very uncongenial to Newton; he made it because he saw no other possibility, but by the time the first laws of electricity were discovered it had become a well-established concept and held a vested interest in the system of science. Therefore a young man whose brilliant experiments in the field of electricity and magnetism were duly recognized met with much contempt when he tried to explain his results in different terms, which excluded all action at a distance and explained electric attraction and repulsion of two bodies by processes occurring in the medium between them, the dielectric,

propagated in time from place to place. But Michael Faraday's ideas were taken up, elaborated, and given mathematical form by Clerk Maxwell, who introduced the more general terms: electric and magnetic field, as the carriers of the forces, and who was able to deduce the velocity of the propagation of electric and magnetic forces, which in empty space proved to be identical with the velocity of light. The believers in the action at a distance put up a strong fight but were driven from their positions in the fields of electricity and magnetism, and the attack came to a temporary halt. One fortress remained in the hands of the enemy, Newton's gravitation. And not until the beginning of this century was this citadel forced to surrender. In Einstein's theory of gravitation the actions at a distance disappeared just as they had disappeared before from electromagnetism, and the gravitational field took their place. Empty space as mere geometrical nothingness vanished from physics, being replaced by a definitely distributed system of strains and stressses, gravitational and electromagnetic, which determines the very geometry of space. And the distribution of strains and stresses in a given environment will determine what a body of a given constitution will do in that environment. Conversely, when we know the body and observe what it does in a certain environment we can deduce the properties of the field in that environment. Thus we discover the magnetic field of the earth by observing the behaviour of magnetic needles in different places, their declination and their inclination; similarly we find out the gravitational field of the earth by measuring the period of a pendulum of given length in different places.

Thus the field and the behaviour of a body are correlative. Because the field determines the behaviour of bodies, this behaviour can be used as an indicator of the field properties. Behaviour of the body, to complete the argument, means not only its motion with regard to the field, it refers equally to the changes which the body will undergo; e.g., a piece of iron will become magnetized in a magnetic field.

THE FIELD IN PSYCHOLOGY

Let us return to our own problem. Can we introduce the field concept into psychology, meaning by it a system of stresses and strains which will determine real behaviour? If we can, we have at once a general and scientific category for all our explanations and we should have the same two kinds of problems which the physicist encounters: viz., (1) what is the field at a given time, (2) what behaviour must result from a given field?

Behavioural Environment as the Psychological Field. But where shall we find a field that plays the rôle in psychology which the physical fields play in physics? That it must be a different field is evident from our previous discussion. The physical field is the field of the geographical environment, and we have shown that behaviour must be explained by behavioural environment. Is this, then, to be our psychological field? Let us try how this assumption works. It means that our behavioural environment, qua determinant and regulator of behaviour, must be endowed with forces. For we shall stick to the axiom: no change of movement without a force. Does this determination rule out the behavioural environment as our required field? By no means. When we describe our behavioural environment adequately we have to indicate not merely the objects which are in it but their dynamic properties as well. We shall discuss a number of examples. Think of yourselves as basking in the sun on a mountain meadow or on a beach, completely relaxed and at peace with the world. You are doing nothing, and your environment is not much more than a soft cloak that envelops you and gives you rest and shelter. And now suddenly you hear a scream, "Help! Help!" How different you feel and how different your environment becomes. Let us describe the two situations in field terms. At first your field was, to all intents and purposes, homogeneous, and you were in equilibrium with it. No action, no tension. As a matter of fact, in such a condition even the differentiation of the Ego and its environment tends to become blurred; I am part of the landscape, the landscape is part of me. And then, when the shrill and pregnant sound pierces the lulling stillness, everything is changed. Whereas all directions were dynamically equal before, now there is one direction that stands out, one direction into which you are being pulled. This direction is charged with force, the environment seems to contract, it is as though a groove had formed in a plane surface and you were being forced down that groove. At the same time there takes place a sharp differentiation between your Ego and the voice, and a high degree of tension arises in the whole field.

If we take from this example chiefly the description of fields with regard to their homogeneity or inhomogeneity, we see that the former are much rarer than the latter, particularly for us over-active human beings of Western civilization. For action presupposes inhomogeneous fields, fields with lines of force, with change of potential. An exceedingly good and instructive description of a field

with a very simple kind of inhomogeneity has been given by Lewin in his essay on the war landscape (1917). Here is a field which, apart from all details, has a polar structure in one direction: the enemy's land on the one side and home and safety on the other. This vectorial property is a primary characteristic and determines the entire field, no other characteristic being entirely free from it.

A number of other very instructive examples are contained in an article by H. G. Hartgenbusch on the psychology of sport. The author describes his own experience, or behavioural field, during several different sports. I select some instances from socker and one from weight lifting. "As they [the socker players] move towards the enemy goal, they will see the playing ground as a field of changing lines whose principal direction leads towards the goal." (1927, p. 50.) These lines are true lines of force in a behavioural field, changing continually with the changing configuration of the players and directing their actions. "All the motor performances of the players (as shiftings about on the field) are connected with the visual shifting. Certainly this is not a case of logical thinking, since thoughts, in the ordinary sense, are alien to a player. He knows nothing of them; in his tense state the visual situation produces the motor performance directly."

We must preface the next example with a more general observation. Our behavioural environments contain things and the holes between them. As a rule, the forces which regulate our behaviour originate in the former and not in the latter. Whether this is due to experience or not is a question we may leave open, although an affirmative answer seems to fit ill with the fact that the novice at cycling is attracted by all sorts of objects, although experience must have taught him the injurious effects of a collision. And yet, every prominent object in his behavioural environment will attract him, whether it be a woman pushing a perambulator or a heavy motor lorry. The mere fact that we speak of "prominent" objects in the environment indicates an inhomogeneity: where the object is, there is *more* than where the hole is. Of course the hole *may* become the most prominent part, and then there is *more* in the hole than in the objects around it, i.e., the hole is now the attracting mass. Another quotation from Hartgenbusch may elucidate this point: "The enemy's goal as seen by the attacking side was apparently walled-up except for a small hole at the left. From my position behind the threatened goal I saw how the attacking left halfback got hold of [3]

[3] Changed in conformity with the original. The English term would fit a rugger but not a socker game.

the ball, fixed his eyes on the hole, and with all his might kicked the ball through the one open spot. When I asked him afterwards what he had felt at that moment, the lucky player replied, 'I only saw a hole.' " However, football also gives us evidence for our first proposition that objects rather than holes are dominant points, centres of force. For the players have to *learn* to emphasize the hole and disregard the goal keeper: "When an expert . . . follows a football game attentively he will always notice that the goal keeper, standing before the comparatively large goal, is more often hit than can be accounted for by the mere adventitious kicking of the contestants" (p. 49), even when one takes account of the fact that the goal keeper whenever he can will try to intercept the ball. Our author then continues, "The goal keeper furnishes a prominent point in space which attracts the eyes of the opposing kickers. If the motor activity takes place while the kicker's eye is fixed on the goal keeper, then the ball will generally land near him. But when the kicker learns to reconstruct his field, to change the phenomenal 'centre of gravity' from the goal keeper to another point in space, the new 'centre of gravity' will have the same attraction as the goal keeper had before."

The next example from Hartgenbusch adds a new point, besides giving a very pretty illustration of the fact that behaviour takes place in a behavioural environment. It needs again a short introduction. If we exert muscular force, say, by lifting a weight, we must keep our body in balance; this presupposes a certain state of the general tonus of our musculature which will be determined by our task and the mechanical conditions under which it takes place. The neat point made by Hartgenbusch is that this poise, this fixation of our body on the ground, does not depend upon the geographical environment only, but also on the behavioural one and even on such aspects of it as have no direct mechanical or gravitational effect. He tells of a competition of "heavy athletes" where the performances, against all expectations, failed to reach even the earlier records. "One of the contestants found the solution of the puzzle. The place where the competition took place was a hall so brightly illuminated that there was no conspicuous fixation point on which the weight lifters could rivet their eyes. . . . The stability accompanying a fixed spatial orientation which is necessary for the lifting of heavy weights could not be attained under the conditions existing in the brightly lighted hall; the expected records did not appear" (p. 49). Thus we see that behavioural objects are dynamic, not only in the sense that they pull and push behaviour in various

directions, but also that they can give purchase, stability, equilibrium.

My examples should have demonstrated the meaning of the term behavioural field with its dynamic properties and the usefulness of this concept. There are many branches of psychology where explanation need not go beyond it, others where it will need only little supplementation. Thus the description of a mentality different from our own, be it that of children, be it that of primitive peoples, will be complete if the behavioural fields of these beings, together with the behaviour which these fields demand, are adequately described. Such work as that done by Lévy-Bruhl on primitives and Piaget on children is truly such description. The question whether the descriptions of Lévy-Bruhl and Piaget are right or not does not enter here, for even if, or inasmuch as, they are wrong, a true description would be a description of the same kind; it would be a field description of the behavioural environment and the Egos within it. And Lewin's theory of behaviour, of action and emotion, contains this behavioural field as a nucleus, even though he has to go beyond its limits. Finally, when we or the novelists or the historians describe behaviour, we do it in terms of forces in the behavioural environment, although we, as well as they, use an entirely different terminology.

Inadequacy of Behavioural Environment as Psychological Field. However, there are imperative reasons why we cannot accept the behavioural environment as that psychological field which is to be our *fundamental* explanatory category. They derive from three sources: (1) the ontological status of the behavioural environment, (2) the relation of behavioural and geographical environment, (3) the insufficiency of the behavioural field. Let us discuss them one by one.

(1) Ontological Status of Behavioural Environment. I feel sure that in reading the description of the dynamic properties of the behavioural environment there will have been a certain reluctance felt with regard to accepting the behavioural environment as a truly explanatory concept. It may have been said that I was using a word with a well-defined meaning in a context where it cannot have this meaning. I am referring to the word force. "Force," it could be argued, "has a definite meaning in the physical world, but what can it mean in a behavioural environment? Force belongs definitely in the physical world, is a construct and not a datum; and yet has been treated as a property of the behavioural world also. It is introduced from its own universe of discourse into another where it has no place. Even if the descriptions are ade-

quate, even if it is admitted that one can speak of the attractive force exerted by a lure, the repulsive force exerted by a danger, this can be no more than a description; whereas force in physics is an explanatory term, the cause of change. But the explanatory meaning together with the descriptive meaning has been smuggled into the behavioural world. And a behavioural force has even been used in order to explain *real* behaviour, i.e., physical motion, whereas physical motion clearly can be produced by physical forces only. Furthermore, there has been no statement of *where* the behavioural world exists, what its ontological locus and status is. Are there two substances, a physical and a mental, the behavioural world consisting of the latter? If so, does this dualism imply an interactionism between mind and body; in this system, does a mental force interfere with the physical order of events? That the interactionism cannot be of the traditional kind where the soul as the Ego or the Self, a mental entity, controls the actions of the body, a physical entity, is clear; for in this system the body is also controlled by mental objects which are not the Self. But even though the interactionism would be of a new kind, it would still be a dualism, whereas in the introduction a system that contained separate realms of existence, like vitalism, was repudiated." I admit every word of this argument, although I must mention that there seems to be a possible way of escape from it which Lewin has indicated. One might argue that terms like force, field, and many others have a much wider significance than the one assigned to them in physics, that the latter is only one possible specification of the former. Simple examples will make this clear: if two containers are filled with water to different levels and then connected at the bottom, water will flow from the one vessel into the other because of a difference in pressure which gives rise to a force. This is a purely physical motion; but now consider the example: America had a great surplus of gold, Europe a great scarcity; what happened? Gold went across the ocean. Is not this example in its formal aspects quite similar to our hydrodynamic one? A motion takes place because of a difference in something which we call pressure in the physical case, a term which would fit the economic case equally well. Or another example: in Soviet Russia there has arisen an enormous new demand for all kinds of goods; the result is that factories are working day and night, and that more and more factories are being built; in the rest of the world supply exceeds demand, with the results that more and more factories decrease their output or shut down completely—this is not meant to be a description of our

economic crisis but merely a simplified example. Thus we might raise the question: What produces the goods? The machines in the factory; yes, but also the demand for the goods; that is, a force in a meaning different from that in physics and yet identical in its application. To summarize: just as we have introduced a behavioural field, we might introduce an economic field, and that field too would have its lines of force. And therefore no objection should be raised against forces in the behavioural environment and not even against their producing actual bodily movements. For the demand makes the wheels turn and the ships carry gold and goods from coast to coast. Economic forces, then, which produce economic results, achieve this by producing mediating physical motions. At the same time the economist does not assume a special substance, say Trade with a capital T; therefore the psychologist treating of behavioural fields need not introduce a special substance, the Mind.

This is all excellent argument which may lead to consequences of great bearing on the philosophy of science. But personally I do not feel satisfied with it because, as it stands, it leaves the relation between the one kind of effect, the physical, and the other, the behavioural or economic, totally in the dark. I want one and the same universe of discourse in which *all* events can take place, since action is defined within a universe of discourse and not from one to the other. The argumentation which I have borrowed from Lewin may lead to a definition of such a general universe of discourse and may thereby radically affect our conception of reality. But before the development of his argument into a consistent, epistemological and metaphysical system, I prefer to meet the argument which I suggested might be raised against my use of behavioural forces in a different manner.

As I said before, I admit the cogency of the argument, i.e., I admit that in our *ultimate* explanations, we can have but *one* universe of discourse and that it must be the one about which physics has taught us so much. Not only is the energy which is consumed in our behaviour of chemical origin, the forces which are responsible for each individual motion must be considered as physico-chemical forces also. The organism is a physico-chemical system by itself, although depending for its existence upon a geographical environment, and its actions must be *ultimately* explained in terms of processes within this system. If an action is reducible to a causal sequence of organic processes, it becomes intelligible because it is then reduced to one universe of discourse which is the same as that in which its actual movements take place.

It would be misunderstanding the trend of this argument if it were thought that it had excluded the use of the field concept. The opposite is true; if the locus of behaviour is the physical world, then the field concept which is so powerful a tool in physics must be applied to behaviour. Our argument denies merely that this field concept can be identical with the concept of the behavioural environment.

(2) The Relation of Behavioural and Geographical Environment. Our second reason against this identification is based on the relation between the behavioural and the geographical environment. That the former depends upon the latter is a truism, although the manner of this dependency is by no means simple or unambiguous. But whereas this problem will occupy us in the next chapter, one aspect of it is relevant in this connection: we assume that this connection is a causal one, the geographical environment being a cause of the behavioural. But then the difficulty appears again that both belong to different universes of discourse. For how can a cause in one universe of discourse produce an effect in another? All our causal laws refer to events within the same universe of discourse, and therefore, since the geographical environment belongs to the universe of physics, we require its effects to belong to it also. So we are again forced away from the behavioural environment; we are compelled to substitute for it some occurrences in the real physical organism. Of course this question does not always interest us. We may take its answer for granted or leave it in abeyance and deal with other problems. Science always works at different levels, and the work at higher levels may proceed for a long time without reference to the work at the lower ones. Thus chemistry became a very advanced science before it became connected with physics, and even today it is by no means possible to reduce all chemical action concretely to the action of protons and electrons, although every scientist is convinced that in principle such a reduction is possible.

Our present argument, therefore, means only that as a fundamental concept at the lowest level, the psychological field cannot be identical with behavioural environment, because as a fundamental concept the field cannot be taken for granted but must be causally connected with the geographical environment. At the same time we have pointed out that psychology works at different levels, and that on some of them the behavioural environment may be, if not the whole field, yet a part of it.

(3) The Insufficiency of the Behavioural Environment. The totality of our behaviour is not explainable in terms of the be-

havioural environment. There are at least three different types of behaviour for which no proper behavioural environment can be found. We shall discuss them one by one.

(a) SO-CALLED REFLEXES. At every moment of our life the tonus of our musculature is regulated. Were it not, we could neither sit nor stand nor walk. But all these adjustments take place without our knowing about them; there is no behavioural environment for them. What is true of the tonic reflexes holds also for the so-called phasic ones: I send a strong beam of light into a person's eyes, and his pupils contract; I remove the light, and they expand again. Now it might be said that here a behavioural environment exists inasmuch as the person will see the light coming and going. But even so, his behaviour is quite unknown to him; he is entirely ignorant of the movements of his pupil before he has learned about them, and even then he remains unaware of them. Thus even though a behavioural environment may be present in these cases, phenomenal behaviour would be missing. Moreover whether one has a behavioural environment or not does not make any difference. The pupils of a boxer knocked unconscious will still react.

It is clear then that if the field concept is to be applied to such reflexes, it cannot be the same as that of behavioural environment. One might, of course, be tempted to exclude the field concept from the explanation of reflexes; that is what has been done. The reflexes were the prototypes of pure stimulus-response connections; they seemed clear cases of behaviour in a purely geographical environment. We shall see later (in Chapter VIII) why it is impossible to accept such an interpretation. It would mean that there were two sharply distinguishable types of behaviour, such as are field-conditioned and such as are not, just as there are behaviours that depend upon a behavioural environment and those that do not. However, there is no such absolute break. An action may be more or less determined by a behavioural environment, and there is no sharp dividing line. Correspondingly, we must feel reluctant to accept behaviour which is not in some way field-conditioned. But then, its field cannot be the behavioural environment.

(b) FORCES THAT DETERMINE BEHAVIOUR OUTSIDE OF BEHAVIOURAL ENVIRONMENT. The forces which determine our behaviour may not always be those we believe to be the determinants. We may do something in order to please X as we think, when in reality we do it to spite Y, when Y need neither be present nor in our thoughts. Psychoanalysis in its various forms has brought to light many such facts, and perhaps its general tendency may be said to be the

proof that all our actions are of that type, reducible to a very few subterranean forces totally absent from our behavioural field. However far the psychoanalysts may overshoot the mark, it remains true that this type of action exists, that it cannot be explained in terms of behavioural environment, and that it is so similar to the rest of behaviour that it needs a common explanatory concept. Since the field concept is applicable to all behaviour, it appears again that the psychological field cannot be identical with the behavioural environment.

(c) MEMORY. There is memory. Now memory determines to a great extent our behavioural field, and in so far cannot serve as an argument against its universality. That I speak to A whom I met yesterday and not to B whom I never saw before is due to the fact that A is, in my behavioural environment, a familiar person, B a stranger. But there are other ways in which memory determines behaviour without the mediation of a behavioural field. The rapid and accurate activities of a trained typist are not explainable in terms of the actually present behavioural environment, as little as the playing of Kreisler or the tennis game of a Tilden or a Cochet. All their training goes into their present performances, but this training does not belong to the present behavioural environment. But skills are not the only memory effects that fall outside the scope of behavioural environment. I think of a person, a city, a mountain, but cannot recall its name. I want to very badly, but no effort seems to help. So I give up and do something else, when suddenly the name will appear. Again a type of behaviour which takes place without a behavioural environment but must, nevertheless, be the result of operative forces, a field process.

"UNCONSCIOUS." To call the facts adduced sub (b) and (c) unconscious or subconscious does not help us. Here we see the advantage of our terminology, for whereas the word conscious allows the formation of new words by the addition of prefixes like "un" and "sub," behavioural environment cannot become an "un"- or "sub"-behavioural environment without losing its meaning completely. And since we agreed that the word consciousness should be used only as an equivalent to direct experience, containing the behavioural environment and the phenomenal behaviour of the Ego, we must renounce the use of the terms un- and sub-conscious. However, there must be a reason why these words were coined and so widely accepted; why did not all psychologists simply distinguish between conscious and merely physiological processes? I believe the answer lies in the fact that the physiological processes were not treated as

field processes, whereas the processes called un- or sub-conscious had very definite properties which in our terminology we call field properties. If, then, we retain the field properties in the physiological processes, we shall no longer be tempted to speak of unconscious processes. And if we survey in review the facts presented under the heading "the insufficiency of the behavioural field" we seem again to be forced to turn to physiological facts.

The Balance Sheet. What, then, is the balance of this discussion? We have gained and we have lost. Our gain consists in the establishment of a unitary universe of discourse. The physical field of the geographical environment acts on a physical object, the organism, and influences the physiological field within this organism; physiological field events take place which change the geographical field and thereby the physiological field. We have a pure problem of physics complicated by the relation of the two interacting fields, the physical and the physiological, and by the enormous complexity of the latter. But though complex, the problem is no longer obscure; we understand its terms, and as a matter of principle, we can follow each event from its beginning to its end over its whole course without jumping from one universe to another.

But our loss is equally obvious. We have, if we stop here, given up all the advantages which the behavioural environment brought to our system. We are no longer dealing with psychological facts at all but with pure physiology. As a matter of fact, this consequence will appear not as a loss but as a gain to many psychologists who will probably now be tempted to make the comment: "If you want to explain all behaviour in physiological terms, why did you ever introduce the behavioural environment?" We had set great hopes on our behavioural environment. With the help of this concept we thought we could build up a psychology which would be acceptable to the historian, the artist, and the philosopher as including motivation and beauty and rationality. And now we had to turn back and take refuge in mere physiology. Is that not equivalent to renouncing molar behaviour and putting molecular behaviour in its place? Are we not stultifying our own purpose? And lastly how can we hope to build a system of psychology in pure physiological terms when our knowledge of the central nervous system is almost a blank? Would not a new kind of speculative psychology supersede the experimental phase? Behavioural environment is something we know, but our physiological field is a totally unknown quantity.

So runs our balance sheet. And if we look at the assets and lia-

bilities which have appeared on it through our commitment to the physiological field we see that they are the reason for the war waged between the different schools of psychology. Those who regarded the assets as the items that counted became behaviourists, thinking as lightly of their liabilities as debtors are ready to do. Those, on the other hand, who were conscientious debtors on whom the weight of the liabilities rested like an insupportable load, thought nothing of the assets and became "understanding" psychologists. Between these extremes we find all sorts of compromises. But all compromises were unsatisfactory because they failed to find a way of using the assets to meet the liabilities. That is what we must do if we want to be honest and pursue our business with a plan that carries us over a long period and saves us from the everlasting threat of an imminent bankruptcy. Or, to choose another metaphor, we must know where we are going and be convinced that the road on which we tread leads to our goal. I remember an episode in my student days. A colleague of mine with whom I was going home asked me the question: "Have you any idea where the psychology we are learning is leading us?" I had no answer to that question, and my colleague, after taking his doctor's degree, gave up psychology as a profession and is today a well-known author. But I was less honest and less capable, and so I stuck to my job. But because his question never ceased to trouble me, I was ready to grasp the first chance that offered to find an answer.

Relation Between Behavioural and Physiological Field Crucial. Therefore, if I have not forgotten that rather casual conversation, another conversation with another colleague remains in my memory as one of the crucial moments of my life. It happened at Frankfort on the Main early in 1911. Wertheimer had just completed his experiments on the perception of motion in which Köhler and I had served as the chief observers. Now he proposed to tell me the purpose of his experiments, of which, as a good subject, I had been entirely ignorant. Of course I had had many discussions with those two men before. One could not live in constant contact with Wertheimer without learning some aspects of gestalt theory, even in those old times. But on that afternoon he said something which impressed me more than anything else, and that was his idea about the function of a physiological theory in psychology, the relation between consciousness and the underlying physiological processes, or in our new terminology, between the behavioural and the physiological field. To state it in these new terms, however, is not quite fair, because this very statement was only made possible by Wert-

heimer's idea; before, nobody thought of a physiological or, for that matter, of a behavioural *field*.

TRADITIONAL PHYSIOLOGICAL THEORIES OF BEHAVIOUR AND CONSCIOUSNESS. For what were the current physiological assumptions at that time? Nervous processes were pictured as events of *one kind* only, excitations, starting somewhere, travelling along a nerve, being transmitted to another nerve, from that to a third, until finally they gave rise to a muscle contraction or a gland secretion. The enormous complexity of behaviour was not explained by an equal complexity of the processes as such, but only by the combination of a host of separate processes, all of the same general kind but occurring in different places. The *locus* of an excitation became the most important aspect of it; diversity of process was introduced merely to account for the different sense modalities and qualities, the first coupled with a local difference, the second not. Sound stimuli would produce excitations of fibres in the acoustic nerve which would be transmitted to the temporal part of the cortex and excite the ganglion cells there to their specific forms of response, corresponding to the attributes of tone sensations; and light stimuli would similarly produce excitations which would be propagated to the occipital cortex and would there produce cell excitations, which, because of the different nature of these cells, would be different from the processes in the temporal cortex. But one and the same occipital cell must be capable of different kinds of excitation. Since there is, in this system of physiological hypotheses, a fixed connection between a cortical cell and a cell in the sense surface, e.g., between a cell in the visual cortex and a cone in the retina, the same cortical cell will always be excited when the same cone is excited. Now the same cone can be excited by light of different wave length with the result that the organism sees different colours. Consequently the same nerve fibers and ganglion cells from cone to cortex must be able to react in different ways.

This, however, was the only qualitative variety granted to nervous processes; apart from that all complexity was explained by the combination of differently localized cell-excitations. No wonder that the question of brain localization loomed so large on the psychological horizon.

I have said that this form of physiological theory was prevalent in 1911; I must add that ten years before the great physiologist J. von Kries had given ample proof that it was utterly wrong. But he had not been able to put an adequate theory in its place, and so the old theory survived as though nothing had happened; indeed this

theory has an iron constitution; it was still so vigorous in 1929 that Lashley, in his presidential address to the American Psychological Association, read before the 9th International Congress of Psychology at Yale, attempted to deal it a new death blow. The material against the theory had enormously accumulated since von Kries's famous speech; Lashley's onslaught looked deadly indeed, but the theory seems to have a charmed life; it seems to persist to this present day.

PHYSIOLOGICAL PROCESSES MOLECULAR, TOTALLY DIFFERENT FROM BEHAVIOURAL ONES. Therefore it is worth while to single out some of its salient aspects. In the first place it is what Tolman calls molecular. No molar characteristics can be found in the nerve excitations, the sum of which constitutes the nervous activity. In the second place, this theory of the physiological processes underlying behaviour with its behavioural environment, or, as it was formerly termed, underlying conscious phenomena, was constructed almost in complete independence of molar behaviour or conscious phenomena. The latter influenced it only by introducing the qualitative sensory differences we mentioned above. The facts of anatomy, interpreted in a particular way, seemed to reveal a number of separate structures, the neurones; and indeed anatomical facts are the main foundation of the theory. But not only is this theory independent of behavioural or psychological observation, it has exerted a decisive influence upon such observation. The description of behaviour as a combination of a multitude of reflexes, original or conditioned, and the description of the behavioural environment in terms of sensations as mental elements are both similar in form. When modern experimental psychology was created, the sensation theory was not created with it, but taken over from the older speculative systems. That it remained unquestioned for such a long time, that it became part and parcel of modern psychology, is without a doubt due to the physiological theory which originated in anatomical discoveries. Thus we see how facts are dependent on theories, how false, therefore, the claim is that a theory is nothing but a concise formulation of independent facts.

THEIR RELATION MERELY FACTUAL. In the third place, in this theory, as a consequence of the two characteristics just demonstrated, the relation between molar behaviour and behavioural environment on the one hand and the underlying physiological processes on the other, is merely factual. In essence they are totally different; did not Wundt emphasize the point that the sensation blue and the corresponding neural event had nothing, absolutely nothing, in

common? Or could anything be more emphatic than the assertion "Thought and feeling must be recognized, on any view, as fundamentally different from any material process, and the motion of the atoms and molecules of the brain as fundamentally different from thoughts and feelings" (Stout, 1913, p. 16). And does not Tolman write in his book published in 1932: "It will be contended by us .. that 'behaviour-acts,' though no doubt in complete one-to-one correspondence with the underlying molecular facts of physics and physiology, have as 'molar' wholes, certain emergent properties of their own?" (p. 7). If we interpret this statement to mean that qua molar, behaviour is fundamentally different from the underlying molecular physiological processes, we link up this third point with our first.

On all three counts the theory has to be condemned. The assumption of merely molecular physiological processes is erected on much too slender an empirical basis; it results either in a molecular interpretation of behaviour and consciousness, which is contradicted by the facts, or it severs completely the two series of processes, physiological and behavioural or conscious, whereas at the same time it establishes the closest possible relation between them by considering the one as the correlate of the other, the nature of this correlation being left entirely in the dark.

WERTHEIMER'S SOLUTION. ISOMORPHISM. And now the reader can understand Wertheimer's contribution; now he will see why his physiological hypothesis impressed me more than anything else. In two words, what he said amounted to this: let us think of the physiological processes not as molecular, but as molar phenomena. If we do that, all the difficulties of the old theory disappear. For if they are molar, their molar properties will be the same as those of the conscious processes which they are supposed to underlie. And if that is so, our two realms, instead of being separated by an impassable gulf, are brought as closely together as possible with the consequence that we can use our observations of the behavioural environment and of behaviour as data for the concrete elaboration of physiological hypotheses. Then, instead of having one kind of such processes only, we must deal with as many as there are different psychological processes, the variety of the two classes must be the same.

MOLAR PHYSIOLOGICAL PROCESSES. However, this theory may appear merely verbal as long as one does not know what molar physiological processes are. Are we not introducing new entities into physiology, and thereby into science, which are incompatible with the

principles of science? Is not physics a molecular science par excellence? Wertheimer saw that it is not; he knew the falsity of this objection. But it was left to Köhler (1920) to demonstrate the fallacy of this argument by showing that physics is a molar science. The name "atomic theory" seems to prove the opposite, but only to a superficial observer. Let us take the simplest example we can find: water is explained by the atomic theory as a compound of two elements, hydrogen and oxygen, in such a way that it consists of molecules, each of which is composed of three atoms, two of hydrogen and one of oxygen. Moreover, hydrogen occurs in nature in a form in which it is not composed of hydrogen atoms but of hydrogen molecules, each composed of two hydrogen atoms. Thus we have H, H_2, H_2O. This sounds like a straight molecular theory, but it is not anything of the kind. For H, H_2, and H_2O have all different properties which cannot be derived by *adding* properties of *H*'s and O's. And in accordance with that, physics endeavours to construct models of atoms and molecules which are just as different from each other as the actually observed substances. The simple hydrogen atom consists of one proton and one electron in *very definite* dynamic relationship, expressed in terms of the Rutherford-Bohr theory by the orbits in which the electron moves around the proton.[4] In H_2 two hydrogen atoms have been combined. But what has happened? A completely new system has been formed with two protons and two electrons. And the motions of this new system, the forces which are active at every moment, are totally different from the motions in the H system. In the simple water molecule, what a complexity and what a difference of structure from the H and the O atoms! It is wrong to say that this system consists of two hydrogen atoms and one oxygen atom. For where are they to be found in it? Looked at in this way, chemical analysis which dissolves water into hydrogen and oxygen means only that one kind of system has been transformed into other kinds of systems and that in this transformation certain characters like total mass have remained constant. But it does not mean that water is just hydrogen plus oxygen combined in a certain proportion.

Molecular Theory and the Category of Substance. The fallacy contained in a statement like the last derives from a deep source. Man the builder assembles his bricks and erects his house. He knows that just as he made it he can destroy it, that he handled bricks

[4] Although the Rutherford-Bohr theory has been abandoned, the consequences which we draw from it are the same in the different forms of more modern theory. Therefore we use this simplest and most easily intelligible form of atomic theory in our text. See Eddington.

and that after all his house is just bricks. He forgets that he has piled these bricks in a gravitational field and that without this gravitational field he can build a house as little as without bricks. But the bricks are so much more palpable than gravitation that he thinks of them alone, and thus he models his concept of reality. Substance assumes for human thought the rôle of being the embodiment of the real. Molecular theory is nothing but an application of this idea. Fundamentally it derives from a selective principle applied to our appreciation of reality. In what does the reality of a house consist, or that of molar behaviour? The question becomes unanswerable when we attempt to solve it in terms of mere substance. Just so a molecule loses its reality if we describe it in terms of atoms only. We are left with the protons and electrons just as we were left with bricks in the case of the house and with our reflexes in the case of molar behaviour.

But this difficulty arises for the philosopher only, and not for the architect or the physicist. The physicist is far from such crude realism. As a matter of fact he finds it harder and harder to lay his hands on "substances." Organized fields of force assume for him the chief reality. The proposition: the world consists of protons and electrons, is as meaningless to him as the statement that Europe is inhabited by human beings is to the historian or politician. The second statement is incontestably true, but does it help to explain the history of Europe or the present political crisis? Europe is inhabited by British, French, Germans, and a vast number of other nations. Set a Frenchman on a deserted island, an Englishman on another, a German on a third, and so forth, then they will behave more or less alike; at least the fact that they are all human beings will be the main factor in the explanation of their behaviour. But the Frenchman in France, the Englishman in England, the German in Germany will be very different people. Why? Because not only human beings are realities, but also human societies with their institutions, forms of government, mores and customs, language and literature, art and music, social stratification and so forth. If we deny the reality of these we can neither be historians nor politicians, but as little can we be physicists if we deny the reality of the field forces in their distribution, or physiologists if we deny the reality of molar characters of physiological processes.

"Physiological Patterns." Perhaps one might object that nobody has done so, that the word "physiological pattern" is used in every book and treatise on the subject. True enough, but this word "pattern" obscures the issue. In what sense is this pattern considered

to be *real*? Only in what I shall call a geometrical or combinatorial sense, a sense in which it applies equally well to the throwing of dice. You shake six dice; each result can be called a pattern: 536224, 151434, 625251, etc., etc. Pattern means nothing here but combination of independent events. Such patterns may have very real results. I dial on my telephone the pattern 234 and the bell in the president's office rings; had I dialled 479 the psychology department would have been summoned, and so forth. This is the kind of reality which is attributed to the physiological patterns, quite different from the kind of reality I claimed for *molar aspects* of behaviour, physiological or physical events. An example which I have used in a previous discussion will contrast the two kinds of reality: "two insulated condensers of equal capacity are placed at a great distance from each other in a homogeneous dielectric. I convey to each of them the same amount of electricity E. Then they have an *equal* charge. But this equality is a purely logical equality. Nothing in the world compels me to compare just these two charges with each other. Physically, there is in this case no dynamic reality of equality. Indeed I can alter the amount of the charge in either of the condensers without thereby affecting the amount on the other. When, however, I join the two condensers by a piece of wire, the equality of their charges has become a physical, dynamical reality. Now this equality is no longer a relation which I can at will state or neglect, but has become a systematic property of the aggregate of conductors which can no longer be altered by changing the charge of one of the condensers" (1927 a, pp. 178 f.).

The equality in the second case is a true reality—but not in the first. "Physiological pattern," however, has been used in this first and not in the second sense, and therefore this term has nothing to do with the reality of molar properties.

Now we know what molar physiological processes are. They are not a sum or combination of independent local nerve processes, but nervous processes in extension such that each local process depends on all the other local processes within the molar distribution.

WERTHEIMER'S SOLUTION AND THE FACTS OF ANATOMY AND PHYSIOLOGY. The next criticism of Wertheimer's theory will challenge it with regard to its consistency with anatomical and physiological facts. These facts, at least, were duly preserved in the old physiological theory; do they not for that very reason invalidate the new one? Even the most cursory examination of these facts, however, will show this criticism to be nugatory. We might raise the question: What are the conditions under which a mere combination of local

events takes place and what are those in which processes in extension are formed? The answer must run like this: when and only when the processes are totally insulated from each other so that they can run their course in absolute independence, only then is the first case realized. Thus the different connections that are made in a telephone exchange are a pattern of purely local events. A talks to B, C to D, etc., etc., but the fact that A and B talk together has no influence on the second fact that C and D exchange compliments or that E and F make a theatre appointment.

On the other hand, where the local processes are not *completely* insulated they will no longer be completely independent, and therefore what happens in one place will depend upon what happens in all the others. The degree of insulation will determine the degree of interdependence, so that we are now dealing not with one case as opposed to another, but with an infinite variety of cases. The question which any physiological theory of nervous processes ought to raise is, therefore: Are the individual nervous structures which anatomy has revealed, completely insulated from each other or not? Only if the answer were affirmative would the traditional theory of a mere additive pattern be possible. As soon as the insulation is found to be incomplete, a theory of molar distribution must take its place. Therefore the anatomical evidence so far adduced is insufficient to support the old theory. What, then, is the added evidence? If we look for an answer in the writings of the founders and supporters of the old theory we search in vain. For they never saw the dilemma; they never chose consciously between the two alternatives, but seduced by the gross anatomical facts, jumped at the one horn without being aware of the other. Although this is no true scientific procedure, it might have been the right guess. But as a matter of mere fact it was not. It is true that the nerve fibres are insulated from each other over long distances, but there are innumerable cross connections which probably connect every nerve cell with every other, a fact of which the old theory has made full use in order to explain the enormous variety of possible "combinations." But if it is so, then the events in this network of nervous tissue can no longer form a merely geometrical pattern; if they are interconnected, then the processes that take place within them can no longer be independent and we must consider them as molar distributions with a degree of interdependence varying inversely with the actually operative resistances. Physiological processes in extension, then, have not been invented in order to support a particular theory. They are demanded by the anatomical facts

emselves. Two recent investigations from the psychological labratory of the University of Kansas give direct experimental support to this view. They show that the action currents of the dog's cortex which result from localized stimulation are not restricted to small areas of the cortex but form a pattern pervading the whole cortex with areas of highest activity varying with the kind of stimulation. Perkins (1933) used sound stimuli; Bartley pain, motor and visual stimuli. Moreover "the records lead to the conclusion that the so-called passive animal exhibits a pattern of cortical activity of essentially the same order as exhibited by the active animal. In other words, there seems to be a basic pattern operating under all conditions of behaviour and that any experimental stimulation of the animal under controlled conditions does no more than modify this pattern" (Bartley, p. 47). The same author concludes that "in accordance with the facts and suggestions that have preceded, a field theory of the nervous system is demanded if its activities are to become intelligible" (p. 54).

BEHAVIOURAL DATA FOR PHYSIOLOGICAL HYPOTHESES. There remains one point in Wertheimer's theory which will meet with scepticism. I claimed as an advantage of this theory that it would use psychological observation, i.e., observation of the behavioural field and of phenomenal behaviour, as material for a physiological theory, thereby greatly augmenting its empirical data. This will seem an unwarranted and highly speculative assumption. The data for a physiological theory must, so it seems, be physiological. Only data from the physical world can be used for a theory about the nature of a part of the physical world, viz., the physiological processes. But this objection overlooks a fact which Köhler (1929) has emphasized, namely, that all observation is observation of behavioural facts of direct experience. Through a careful selection of such facts it has become possible to develop the science of physics, although the relation between the behavioural and the geographical environment is an indirect one. Between these two worlds and mediating between them are the physiological processes within the organism. If, then, we can use the behavioural world in order to obtain insight into the geographical, why should it not be possible also to derive insight into the physiological processes from such study? The way is shorter in the latter case than in the former; in the former, we jump across the mediating link, in the latter we take only one step. Moreover the connection between the behavioural world and the physiological processes is much closer than that between the latter and the physical world; do we not speak of "underlying" physiological

processes or of the physiological "correlates" of conscious phenomena? Then, to quote from Köhler, "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" (1929, pp. 60 f.). Furthermore, if B stands for the behavioural world, G for the geographical, and P for the physiological processes, $\underset{\sim}{BP} \leftarrow G$ shows the relationship. Now P is in causal connection with G and in a more direct connection with B; the usual assumption, which we shall prove to be erroneous, was that P and G were in close geometrical correspondence, whereas B and P were totally different. Does not such an assumption make it totally unintelligible that B can give us information about G? For if B is totally unlike P, and P is very much like G, how can B lead to G? If, however, B and P are essentially alike, then it only depends upon the G-P relation when and how we can gain knowledge about G from P. And if it is so, then surely observation of B reveals to us properties of P. This theory, first pronounced by Wertheimer, was carefully elaborated by Köhler. In his book on the "Physische Gestalten" (1920) he has gone deeply into physics and physiology to prove the compatibility of the theory with physical and physiological facts; in his "Gestalt Psychology" he has formulated this theory of *isomorphism* in a number of special axioms. In his book (1920) he had formulated the general principle in these words: "Any actual consciousness is in every case not only blindly coupled to its corresponding psychophysical processes, but is akin to it in essential structural properties" (p. 193). Thus, isomorphism, a term implying equality of form, makes the bold assumption that the "motion of the atoms and molecules of the brain" are not "fundamentally different from thoughts and feelings" but in their molar aspects, considered as processes in extension, identical. Moreover the physiologist von Frey draws the following conclusion from his famous investigations on the sense of touch: "The progress achieved by the recent investigations lies, in my opinion, less in improved definition of concepts than in the conviction that the somatic processes which are co-ordinated to mental gestalten must have a structure similar to them" (p. 217).

OLDER FORMS OF ISOMORPHISM. That *some* isomorphism was necessary has been held by most psychologists since the times of Hering and Mach. Hering constructed his theory of colour vision in strict accordance with direct colour experience. The axioms underlying his system have been formulated as psychophysical axioms by G. E.

Müller (1896), but this isomorphism was almost casual though demanded by the scientific problem; it concerned the geometrical, or systematic, order of sensation and not the real dynamic order of living experience. For that reason it remained an isolated part, unrecognized as a fundamental psychological principle. Mach (1865) indicated a more far-reaching isomorphism, one that looks identical with that of Wertheimer and Köhler. Yet it played no rôle in the development of our science; it was so little known that Köhler, who refers to Hering and Müller, fails to mention Mach in this connection. I found the passage in Mach to my great surprise by mere accident. Again we need not look far to find the reason for this apparent injustice of history. Mach was an excellent psychologist, who saw many of the most fundamental problems of psychology which, a whole generation later, many psychologists failed even to understand; at the same time he had a philosophy which made it impossible to give fruitful solutions to these problems. And so his dynamic isomorphism had no effect on psychology because of his interpretation of dynamics in general.

ISOMORPHISM AND OUR BALANCE SHEET. And now, with the tool of a thoroughgoing isomorphism in our hands, we return to our balance sheet which we drew up after stating the reasons why, when we come to fundamentals, we must choose a physiological field rather than the behavioural environment as our fundamental category. We find, then, that we have lost none of our assets, but have succeeded in turning them to such use that they will meet our liabilities. We are no longer losing the advantages gained by the introduction of the behavioural environment, for we construct our physiological field in accordance with, and directed by, the observed properties of it. Thus we have a good reason for introducing and keeping the behavioural environment, even though we look ultimately for physiological explanations. Therefore, all the hopes raised by the introduction of our behavioural environment survive in our new system. If physiological processes are processes in extension, if they are molar instead of being molecular, then we have escaped the danger of abandoning molar behaviour in favour of molecular behaviour. And lastly, we are not advocating pure speculation. The opposite is true; we want to use *more* facts for our physiological theory than the traditional theory did, not less. The brain processes are terra incognita, no doubt about it. Shall we as workers in a young science resign ourselves to this state of affairs or shall we not rather try our utmost to improve it? Physiological theory, as we envisage it, will indeed be much more difficult

than the old conception of telephone wires or railway tracks, b it will be just as much more interesting.

"Brain Mythology." In a very striking passage Köhler has d fended his hypotheses against the criticism that they were pur speculative, mere brain mythology. I translate only a short b incisive passage: "In the third place it has to be said that the arg ment betrays a strange misconception of the actual procedure empirical science. Natural sciences continually advance explanato hypotheses, which cannot be verified by direct observation at t time when they are formed nor for a long time thereafter. Of suc a kind were Ampère's theory of magnetism, the kinetic theory gases, the electronic theory, the hypothesis of atomic disintegrati in the theory of radioactivity. Some of these assumptions have sin been verified by direct observation, or have at least come close t such direct verification; others are still far removed from it. B physics and chemistry would have been condemned to a permane embryonic state had they abstained from such hypotheses; the development seems rather like a continuous effort steadily t shorten the rest of the way to the verification of hypotheses whic survive this process" (1923, pp. 140 f.).

ADDED ADVANTAGE OF ISOMORPHISM. Thus we have met point b point the arguments which appeared on the liability side of our ledger. But we can add three more items to our assets. (1) We hav gained an insight into the relation of molar and molecular facts When we saw that a psychology built upon molecular facts coul never hope to solve the most important psychological problems those of the historian or the artist, we suggested that a science buil on molar facts might find a place for the molecular ones. And our expectation has been fulfilled; for no real molecular fact disappear from our system; molecular facts merely cease to be independen events, the true elements of all facts. Instead they appear as loca events within and determined by larger field events.

(2) Granted, then, that our theory will be a molar theory, never theless it is a purely physiological theory, even though mental facts facts of direct experience, are used in its construction. Does that no reveal a materialistic bias, does it not imply a valuation with regard to reality in which the physical ranks higher than the mental? Is this theory not, after all, a posthumous child of materialism? Let me quote a very impressive paragraph from Wertheimer: "When one goes to the root of one's aversion to materialism and mechanism does one then find the *material properties* of the elements which these systems combine? Frankly speaking, there are psychological

ories and many psychological textbooks which treat consistently elements of *consciousness* and are nevertheless more materialis- barren, lacking in meaning and significance than a living tree hich possibly possesses nothing of consciousness. It cannot matter what material the particles of the universe consist, what mat- rs is the kind of whole, the significance of the whole" (1925, 20).

Thus the alleged materialistic bias of our theory disappears. A physiological theory which allows to physiological processes more than mere summative combination of excitations is less materialistic than a psychological theory which allows only sensations and blind associative bonds between them. But we can say even a little more. Is our theory really *purely* physiological? Would it not mean an abandonment of fact if it were? For the physiological processes which we construct as the correlates of consciousness are known to us in the first place through their conscious aspect. To treat them as though they were purely physiological, without this conscious aspect, would be to neglect one of their outstanding characteristics. True enough, this conscious side of the processes does not enter into our causal explanations, but it has to be recognized as a fact nevertheless. And that leads to the conclusion that it is of the warp and woof of certain events in nature that they "reveal themselves," that they are accompanied by consciousness. Why they are so, and what special characteristics a process must have in order to be so, these are questions that cannot now be answered, and perhaps may never be. But if we accept our conclusion, consciousness can no longer be regarded as a mere epiphenomenon, a mere luxury, which might just as well be absent. For in an aspect which we do not know, these processes would be different, were they not accompanied by consciousness.

(3) And this leads us to our last point. What about the consciousness of animals? That the behaviour of animals is molar and not molecular is a fact. Animal and human behaviour belong together; they are not totally different. On the other hand, we can never observe their behavioural environment, their consciousness. But the same is true with regard to any behavioural environment except our own. Directly, I can only know my own consciousness, you yours, yet nobody thinks of claiming a unique position for himself in the universe. Therefore the assumption of animal consciousness is nothing essentially new. However, if we do assume it, we are still faced with the problem, when shall we attribute consciousness to animals, when not? Is there, e.g., a definite point in the phylo-

genetic series where consciousness emerges? If so, where is it? Is an amoeba conscious? If not, a crab, a spider, a fish, a chick, a cat, a monkey, an anthropoid ape? Let us frankly admit that there is no answer to this question. Since we do not know what properties make a physiological process the correlate of a conscious one, we have absolutely no criterion by which we can decide with certainty whether any behaviour is conscious behaviour or not. All attempts to establish such criteria have begged the question by assuming a necessary relation between certain types of behaviour and consciousness.[5] But in our system this whole problem is of no importance. Have we not learned from Wertheimer that there are much more essential characteristics of behaviour than whether it is conscious or merely physiological? Molar behaviour will be a field process; by studying the behaviour we can draw conclusions with regard to the field in which it occurs; we can make molar physiological theories. Because of our isomorphism we can even go a step further; we can describe this field in behavioural rather than physiological terms. This is very useful, because we have a behavioural terminology for such field descriptions, but not a physiological one. When I said previously that a chimpanzee used a "stool," I employed behavioural terminology. How could I, at the present state of science, have used a physiological one? And yet I need not mean more by this terminology than a description of the physiological field, leaving it entirely outside the scope of science, whether a behavioural field corresponded to it or not. Thus we are even less anthropomorphic than we appeared in our last discussion of the problem. There we claimed that the assumption of a behavioural environment was not anthropomorphism; now we are willing to give up even the behavioural environment, substituting for it a physiological field, the properties of which can best be described in behavioural terms. Thus the issue between us and the behaviourists with regard to animal psychology is not conscious behaviour vs. purely physiological behaviour, but physiological behaviour of the *field* type vs. physiological behaviour of the mechanical connection type. This issue can and must be decided on the plane of pure science, and the decision cannot fail to affect the wider issues which distinguish gestalt theory and behaviourism.

One last remark in this connection: we said that physiological processes that are accompanied by consciousness must in some unknown aspect differ from physiological processes which have no such accompaniment. We must add that in other relevant aspects

[5] Compare my discussion of this problem (1928), pp. 13 f.

ery must be alike. For they are all field processes. Our whole solution of the mind-body problem would help us nought if we restricted the field concept to conscious physiological processes. But we do not. We view these as part events in a much wider field present and thereby avoid the argument against the behavioural field as a fundamental category which we have termed the insufficiency of the behavioural field. Let us introduce for future use the term 'psychophysical field," indicating by this term both its physiological nature and its relation to direct experience.

THE TASK OF OUR PSYCHOLOGY

And now we can formulate the task of our psychology: it is *the study of behaviour in its causal connection with the psychophysical field*. This general programme must be made more concrete. Anticipating, we can say that the psychophysical field is organized. First of all it shows the polarity of the Ego and the environment, and secondly each of these two polar parts has its own structure. Thus the environment is neither a mosaic of sensations nor a "blooming, buzzing confusion," nor a blurred and vague total unit; rather does it consist of a definite number of separate objects and events, which, as separate objects and events, are products of organization. Likewise, the Ego is neither a point nor a sum or mosaic of drives or instincts. To describe it adequately we shall have to introduce the concept of personality with all its enormous complexity. Therefore if we want to study behaviour as an event in the psychophysical field, we must take the following steps:

(1) We must study the organization of the environmental field, and that means (a) we must find out the forces which organize it into separate objects and events, (b) the forces which exist between these different objects and events; and (c) how these forces produce the environmental field as we know it in our behavioural environment.

(2) We must investigate how such forces can influence movements of the body.

(3) We must study the Ego as one of the main field parts.

(4) We must show that the forces which connect the Ego with the other field parts are of the same nature as those between different parts of the environmental field, and how they produce behaviour in all its forms.

(5) We must not forget that our psychophysical field exists within a real organism which in its turn exists in a geographical environ-

ment. In this way the questions of true cognition and adequate or adapted behaviour will also enter our programme.

Points (3) and (4) are the nucleus of a theory of behaviour; (1) and (2) are necessary for their solution. And therefore one cannot wonder that the two problems (3) and (4) have been much less studied than others; moreover, experimentation was started within the province of our first point, both in psychology in general and in gestalt psychology in particular. Therefore the reader must not be surprised when we devote more space to our first point than seems proportionate in consideration of its importance in the whole scheme. The value of theoretical concepts is tested by their application in actual research. The concepts which we have so far developed cannot be understood without a good knowledge of the concrete experimental research work in which they have played the leading rôle. But there is another point to remember. In our fifth item we have touched upon a fundamental philosophical problem. The studies in perception to which my last remarks referred will give us valuable clues for the solution of this philosophical problem. This must be kept in mind if perspective is not to be lost. There will be many experiments, which, though they appear neat and ingenious enough, will seem trivial when seen by themselves. Why such experiments? What can they contribute to a real knowledge of behaviour? The answer is that they serve as demonstrations of general principles; they are not meant to be of great significance in their own right.