VII

"CHANCE" AND "IMITATION"

THE experiments described heretofore proceed in general very simply and somewhat in the same manner. As those in the next chapter are of a rather different nature, it seems to the point to mention beforehand certain considerations which will ward off some facile objections against the real meaning and value of the facts. This would not be necessary in the case of results of a highly-developed experimental science like physics, in which the meaning of groups of observations cannot long remain altogether a matter of controversy. A system of knowledge which cannot be destroyed stands there firm and clear; and new discoveries *must connect up* with it in one way or another. No one can deny that we are far from such a happy state of affairs in the higher psychology. Instead of sure and fruitful knowledge, we have, so far, developed for the most part nothing but theories of a very general form, but so indefinite that even their supporters would not find it easy to apply them strictly and in detail to any specific case. The more energetic does the claim become that any one or other of these opinions contains the principle that will explain a great number and variety of phenomena; and their loose connexion with actual experience, together with the indefiniteness of the assertions, make it the more difficult to decide a conflict by observation and experiments, which is still almost in the region of a battle of faith. At the same time, it is inevitable that such actual observations shall lose in value. They are all too peculiar, too individual to attract the attention

already give to any general principles. And the indefinite nature of these principles on the one hand, added to the difficulty of really reliable observation on the other, make it possible for nearly anyone to explain anything. Thus, if, at the outset, there is greater interest in general principles than in facts, the facts must finally seem valueless; they can be explained as one wills.

In this book, no theory of intelligent behaviour is to be developed. Since, however, we have to decide whether chimpanzees ever behave with insight, we must at least discuss certain interpretations which cannot be accepted without the observations at the same time losing all their value in regard to this question. This will at least prevent any quite arbitrary treatment of the facts, and the direct meaning of the experiments will appear with more force and certainty. Perhaps finally it will be possible to make this meaning rest on its own merits, instead of allowing it to disappear in the solvent of general and indefinite principles.

An interpretation mentioned above runs: The animal solves its task in the general form of "roundabout behaviour," and since it is not born with a ready reaction for each case, it must *develop* a new complex attitude for these cases. The only possible origin for complex action is a great many fractions or parts of the whole achievement, which *separately* are quite natural to the animal; such "natural" impulses occur in great variety, and a certain number of them, which in the play of chance, may happen to follow each other, form, when put in a series, the whole actual course of the experiment. As the actual success or the corresponding pleasant feeling has the effect, in a manner not yet known, of making the preceding movements reproducible in later cases of a similar nature, with the origin of such achievements, their repeatability is also explicable. Like most of these general theories, this one has a certain value for some of the cases

arising in animal psychology. Where experience might teach one to doubt, two supplementary principals are usually called in to help. According to the first, the application of a theory up till now so well confirmed must be preferred to the recognition of conflicting facts and the development of corresponding new ideas, and this for the sake of scientific economy. According to the second principle, such behaviour directly arising out of the situation as a whole would be a miracle, which must be excluded *a limine* as contradictory to the foundations of scientific knowledge.

Further discussion of these auxiliary principles must be omitted here. The second declares the insolubility of a scientific problem, whose solution no one has yet properly attempted: why this lack of courage? The first expresses a misconception (at present widely current) of a correct epistemological proposition, according to which a scientific system, which is near to completion (i.e. half-ideal), tends to acquire the most precise form and strictest simplicity. Neither principle has the right of control over experience, and, where there arises a conflict between these principles and observations, the *former* must give way, not the latter. Evidently the epistemological proposition neither states nor implies that a science which has existed for only a few decades should at any price get along with the minimum of points of view that it has discovered in this early youth; and one does not get nearer to the ideal state any faster by trying to force the shortest cut to the goal of strictest unity, by proclaiming meagre beginnings as final principles, and by economizing on facts what one does not wish to spend on theories.[1]

The chief thing to be done now is to present the theory of which we have been speaking in such a way that its relation to the intelligence tests described will be easily seen. Let

[1] Cf. *Nachweis einfacher Strukturfunktionen usw*, *Abh. d. Preuss. Ak d Wiss.* 1918, No 2, p 40, seqq

those parts of the "solution" which the animal in theory achieves "naturally" and by chance be: a, b, c, d, e; generally besides these, and amongst them (without them, too), any others: F, Y, K, R. D, etc., may appear in any order.

The first question is whether a is accomplished with any reference to the fact that b, c, d or e should follow so that they all together produce a curve of behaviour which is adequate to the objective structure of the situation. Not at all; for when a occurs, it has as little to do with b, c, d, and e as with F, Y. K, etc., which might follow on a, just as well, and generally will, in whatever permutation; the succession is, after all, as accidental as the winning numbers in roulette. What is true of a is equally true of all the other "natural" fractions; to use a phrase which is beyond a simple analogy and which brings the whole question into connexion with the second principle of thermo-dynamics—they are all *completely incoherent*, representing in a somewhat magnified form a case of "molecular disorder." If the least thing is altered about this, the whole meaning of the theory is destroyed.

The second question is whether, after the animal has developed the sequence a, b. c, d, e, it *then* begins with a, goes on with b after a, and so on, because these fractions in this order correspond, as a whole, to the objective structure of the situation? Doubtlessly not. It proceeds from a to b, and so on, only because the after effects of its former life force it to make b follow a, c follow b, and so on.

Thus the only way in which, according to this theory, the objective conditions of the situation, its structure, can have any effect in the development of the new behaviour, is by a completely external meeting of the objective circumstances and the chance movements of the animal's body; the situation works, roughly speaking, like a sieve, which lets through only part of the things which are thrown into it. Apart from this result of the objective situation, which is not of great interest

for our investigation, we find that *nothing in the behaviour of the animal arises directly out of the relation of the several constituent parts of the situation to each other; the structure of the situation in itself has no power whatever directly to determine conduct appropriate to it.*

[According to the theory one has to recognize *one* limitation of the natural reactions caused directly by the situation; a tendency to keep approximately in the direction of the objective[1]. Round this fundamental motive, generally characterized as a manifestation of instinct, play the actual separate movements, whose spatial range thereby becomes narrower but which in this narrower range remain accidental as before. Since taking the straight direction towards the objective does not achieve much, in the experiments here performed, and is often even inadequate to the situation, I need not dwell longer upon this point, while there is no question of the formulation of a positive theory. The tendency is important enough, however, because it indicates something quite foreign to the principle of chance, but hidden under the harmless phrase "instinctive impulse". (Cf. remarks on this primary direction of action, p. 88 seqq.)]

Now I mention from the very beginning, how in the case of a roundabout-way experiment a practical "result" consisting of single and separate fractions, put together by chance, is sharply distinguishable from "genuine solutions". In these, the smooth, continuous course, sharply divided by an abrupt break from the preceding behaviour, is usually extremely characteristic. At the same time this process as a whole corresponds to the structure of the situation, to the relation of its parts to one another. Thus, for example: the objective is visible behind an obstacle of a certain form, on free ground —and suddenly the smooth and unchecked movement occurs along the corresponding curve of solution. We are forced to

[1] In the case of animals directed by their sense of smell: in the direction of the strongest intensification of the smell.

the impression that this curve appears as an adequate whole from the beginning, the product of a complete survey of the whole situation. (Chimpanzees, whose behaviour is incomparably more expressive than that of hens, show by their careful looking around that they really begin with something very like an inventory of the situation. And this survey then gives rise to the behaviour required for the solution.)

We can, in our own experience, distinguish sharply between the kind of behaviour which from the very beginning arises out of a consideration of the structure of a situation, and one that does not. Only in the former case do we speak of insight, and only that behaviour of animals definitely appears to us intelligent which takes account from the beginning of the lay of the land, and proceeds to deal with it in a single, continuous, and definite course. Hence follows this criterion of insight: *the appearance of a complete solution with reference to the whole lay-out of the field.* The contrast to the above theory (parts put together by chance) is absolute: if there the "natural fractions" were neither coherent with the structure of the situation, nor among themselves, then here a coherence[1] of the "curve of solution" in itself, and with the optical situation, is absolutely required.

[To anyone who is inclined to regard the above explanations as detailed trivialities, I would suggest a glance through the psychological literature of man and animal. These trivialities should be thoroughly emphasized; in the first place, they are not always clearly understood, but are seen only through a veil of general principles[2], and secondly, the last part, about

[1] The physicists have no word that fits exactly We use the term "Coherence" from the theory of radiation, as being the least inappropriate

[2] E Wasmann, e g. *Die psychischen Fahigkeiten der Ameisen*, 2nd ed, 1909, p 108, seqq. has sharply defined this contrast But he absolutely denies intelligence in animals, and further points to a logical theory of intelligent conduct (intelligence) in the case of man, which I cannot accept O Selz, *Die Gesetze des geordneten Denkverlaufs, I.*, 1913, treats of reproductive thought in man from a point of view somewhat related to mine.

insight, appears to some students not at all obvious, but rather as a sort of belief in miracles. No such superstition is meant or prepared here, and nothing that has been said involves it in the slightest.]

How one is to explain that the field as a whole, the relations of the parts of the situation to one another, etc., determine the solution, belong to the theory. Here we have only to exclude the idea that the behaviour of the animals is to be explained by the assumption according to which the solution will be accomplished without regard to the structure of the situation, as a sequence of chance parts, that is to say, without intelligence.

In the description of these experiments it should have been apparent enough that what is lacking for this explanation is that most necessary thing, a composition of the solutions out of chance parts. It is certainly not a characteristic of the chimpanzee, when he is brought into an experimental situation, to make any chance movements, out of which, among other things, a non-genuine solution could arise. He is very seldom seen to attempt anything which would have to be considered accidental in relation to the situation (excepting, of course, if his interest is turned away from the objective to other things). As long as his efforts are directed to the objective, all distinguishable stages of his behaviour (as with human beings in similar situations), tend to appear as complete attempts at solutions, *none* of which appears as the product of accidentally arrayed parts. This is true, most of all, of the solution which is finally successful. Certainly, it often follows upon a period of perplexity or quiet (often a period of survey), but in real and convincing cases, the solution never appears in a disorder of blind impulses. It is one continuous smooth action, which can be resolved into parts *only by abstract thinking* by the onlooker; in *reality* they do *not* appear independently. But that in so many "genuine" cases as have been described,

these solutions *as wholes* should have arisen from mere chance, is an entirely inadmissible supposition, which the theory cannot allow without renouncing what is considered its chief merit.

[I have noticed from myself and others, that what is particularly enlightening as to the ape's behaviour are the pauses mentioned above. A local colleague convinced, like most students, of the general value of the chance theory for animal psychology, came to see the anthropoids. I chose Sultan for the demonstration. He made one attempt at solution, then a second, and a third; but nothing made so great an impression on the visitor as the pause after that, during which Sultan slowly scratched his head and moved nothing but his eyes and his head gently, while he most carefully eyed the whole situation.]

This sort of question can best be answered by actually observing the facts with which this theory asserts it can explain *all* our experiments, thus making oneself, by observation, more capable of judgment. Behaviour fit for such an examination occurred in the building-with-boxes tests. Here, in the solution which, taken *en bloc*, "higher box up," was quite clear, the final result was achieved only by chance, and after an almost entirely unintelligent muddling around. This happened so often, and so uniformly with all the animals experimented on, that I can claim to know exactly the procedure asserted by that theory to be general. *It should, therefore, be the more emphasized that the most striking difference exists between this conduct, obviously ruled by chance, and the behaviour described as "genuine" in clear solutions.* In addition, the descriptions of these experiments have shown how unwillingly the chimpanzee embarks on procedure, of which the general outline will come to him as a genuine solution, but whose more detailed execution he must attempt as mere trying, that is, leaving it to chance. The animals

would never have hit on this kind of trying, if an attempt *genuine* in outline had not put them in a position, with the special conditions of which they were not able to deal. The fact that the animals on such an occasion do make blind movements does not in any way contradict the assertion that, as a rule, and in reasonable testing conditions[1], disorder of impulses is not observed.

In these experiments, whenever chance may have effected or favoured a solution, the fact is always mentioned. In complicated experimental conditions (see the next chapter, such cases are more frequent, but it must be said from the beginning that even then the course of the experiment does not entirely agree with that theoretical interpretation. In the first place, it may happen that the animal will attempt a solution which, while it may not result in success, yet has some meaning in regard to the situation. "Trying around" then consists in attempts at solution in the *half-understood* situation; and the real solution may easily arise by some chance outcome of it, i.e. it will not arise from chance impulses, but from actions, which, because they are *au fond* sensible, are great aids to chance Secondly, a lucky accident may occur in some action, which has nothing to do with the objective. Here again, there is no question of a meaningless impulse—the chimpanzee only gives way to these, as already remarked, when driven to it—but of some kind of intelligent activity, even if with no reference to the objective. This is what probably occurs, when Sultan discovers the way to combine two sticks; only a Philistine would call his playing with these sticks "meaningless impulses," because it follows no practical purpose. That an accident helped him is not the most important fact in either case; the important thing

[1] Arrangements are usually made—in accordance with the requirements of the problem—so that it is not easy for accidental solutions to take place.

is how the experiment then proceeds. For we know from Man that even an accident may lead to *intelligent* further work (or intelligent repetition), especially in scientific discoveries (compare Oerstedt: *Current and Magnet.* Thus Sultan's behaviour, when he has once carried out his usual play, "put stick in hole," with both the bamboo rods, is exactly the same as if he had discovered the new procedure in a genuine solution. After this there is no doubt that he makes use of the double-stick technique intelligently, and the accident seems merely to have acted as an aid—fairly strong it is true—which led at once to "insight."

If one does not watch attentively, the crude stupidities of the animals, already referred to several times, might be taken as proofs that the chimpanzee does, after all, perform senseless actions, a sequence of which may, by chance, give rise to apparent solutions.

The chimpanzee commits three kinds of errors :—

1. "*Good errors,*" of which more will be said later. In these, the animal does not make a stupid, but rather an almost favourable impression, if only the observer can get right away from preoccupation with human achievements, and concentrate only on the nature of the behaviour observed.

2. *Errors caused by complete lack of comprehension of the conditions of the task.* This can be seen when the animals, in putting a box higher up, will take it from a statically good position and put it into a bad one. The impression one gets in such cases is that of a certain innocent limitation.

3. *Crude stupidities arising from habit* in situations which the animal ought to be able to survey (e.g. dragging the box to the railings—Sultan). Such behaviour is extremely annoying—it almost makes one angry.

Here we are dealing with the third class, and it is easily seen that these mistakes are not at all liable to confirm the

chance theory. This kind of behaviour never arises unless a similar procedure often took place beforehand as a real and genuine solution. The stupidities are not accidental " natural " fractions, from which *primarily* apparent solutions can arise—I know of no case in which such an interpretation is even possible—they are the *after-effects* of former genuine solutions, which were often repeated, and so developed a tendency to appear *secondarily* in later experiments, without much consideration for the special situation. The preceding conditions for such mistakes seem to be drowsiness, exhaustion, colds, or even excitement. For instance, a chimpanzee, when he performs an experiment for the first time and cannot reach the objective lying outside the bars without an implement, will never have the " accidental impulse " to drag a box to the bars, and even get up on it. On the other hand, one may see that actually, after frequent repetition of a solution originally arrived at genuinely, and in the consequent mechanization of the proceeding, such stupidities are easily committed. Not infrequently have I demonstrated an experiment to interested observers, and, for the sake of simplicity, usually chose the opening of a door, in front of the hinge side of which the objective was hanging. After the animal had done this about twenty times since the first solution, and always at the same place, there began to appear a tendency to fetch down objectives hung high up with the help of a door, even when other methods were more obvious, and the use of a door had been made very difficult, in fact, almost impossible. And if attempts at other solutions developed, they were more or less under the influence, or magnetic power, of the door. Chica, for instance, made out of the jumping-stick method, which she had in its simple form completely mastered, a combination of this and the door-method; and quite unnecessarily, because it was by no means an improvement. Before the door had come into

intelligent use for the first time, the chimpanzees had paid no attention to it in any experiment, not even when the experiment took place opposite to it.

According to this, processes, originally very valuable, have a disagreeable tendency to sink to a lower rank with constant repetition. This *secondary self-training* is usually supposed to bring about a great saving, and it may be so, both in man and in anthropoid apes. But one must never forget what a startling resemblance there is between these crude stupidities of the chimpanzees arising from habit, and certain empty and meaningless repetitions of moral, political, and other principles in men. Once all these meant more, one cared about the " solution " in a predicament deeply felt or much thought about ; but later the situation does not matter so much, and the statement of the principle becomes a cliché.

It should now be clear enough that these meaningless reproductions of originally genuine and correct solutions have absolutely nothing to do with the accidental and confused production of " natural " impulses of the theory discussed above.

[For the rest, it will be best simply to give the whole list of these stupidities :

1. Sultan puts one box on top of another, where the objective was before, not where it is hanging now ; the animal is quite exhausted (8.2.1914).

2. Sultan drags a box to that spot at the bars, opposite which the objective is lying (outside), and turns first one side, then the other towards the bars quite stupidly ; fetches more boxes, and begins as if to build. The animal has been performing experiments with boxes for about four weeks continuously , the experimenter is partly to blame (19 2.1914).

3. In the same experiment Sultan draws the observer thither, and climbs on his back, as if the objective were hung

high up; he jumps down again at once, and then follows the solution described above, p. 148 (19.2.1914).

4. Sultan drags a box to the bars, where the objective is lying outside them (20.4.1914).

5. Grande commits the same stupidity (14.5.1914).

6. With the objective outside the bars and at some distance, Grande drags stones about in her cage, as an after-effect of repeated experiments in the same place, in which stones served as a footstool for her (19.6.1914).

7. Koko shoves the box in the direction of distant fruit, and for a moment does not use it as a stick, as on the day before, but as a stool. The animal is very excited (1.8.1914).

8. Koko does the same when in a fury (6.8.1914).

A similar thing is hinted at once when, with the objective hanging high up, Sultan goes towards the nearest door—a good three metres away—takes hold of it, but lets it go again after a look at the objective, and turns to other methods. In this case he is near a meaningless reproduction, but is prevented by a survey of the requirements of the situation (13.3.1916).

Rana's habit of beginning over and over again to jump with tiny sticks, is hardly to be included here. Rana's brain, as it were, runs away with her, and, of course, it would be fine if she could jump like that. The animal will indicate the same kind of behaviour, even when she sees clearly that its execution is impossible.

That is all; practically all the cases have already been mentioned in the reports of the experiments. No one will assert that they represent the main character of our observations.

The chief cause of these phenomena (mechanization) need not, necessarily, according to the above, lead to externally observable effects of the nature of crude stupidities. Every solution repeated often under the same circumstances, and

adequate to them, changes somewhat in nature, and perhaps finally will not be so intelligent even in this, its original *milieu*, though still adequate. I must say that I like the behaviour of the chimpanzees during their tenth or eleventh repetition of a solution less than that in the first or second. Something is spoilt in the chimpanzee even when many different experiments follow each other in quick succession, but particularly when the same ones are repeated. I may not always have sufficiently considered this possibility in my eagerness to make researches.

The facts we are speaking of, by the way, seem to represent almost a reversal of what the theory we have discussed regards as the effect of repetitions. According to it, procedure developed by accident becomes smoother through practice, and more like a genuine solution. This may be true, where the theory applies; the chimpanzee's *genuine* solutions, at any rate, do not become more valuable in themselves through constant repetition, even if they appear more quickly, etc.]

For one who has actually watched the experiments, discussions like the above have something comic about them. For instance, when one has seen for oneself, how in the first experiment of her life (cf. above, p. 62), it did not dawn on Tschego for hours to push the obstructing box out of the way, how she merely stretched out her arm uselessly, or else sat down quietly, but then, fearing the loss of her food, suddenly seized the obstacle, and pushed it to one side, thus solving the task in a second—when one has watched that, then to "secure these facts against misinterpretation" seems almost pedantic. But the living impression will not be reproduced, and many a question can be raised on the words of a report, which would not even occur to anyone after some observation. Nevertheless, it may be that after these discussions, the description of a further experiment carried out as a model will be particularly instructive; an experiment which is characterized

both by its simplicity and its unequivocal relation to several theories.

A heavy box is standing upright at some distance on the other side of the oft-mentioned bars; one end of a stout string is affixed to it, and the string itself is laid down obliquely so that its free end lies between the vertical bars of the railings. Half-way between the box and the bars fruit is tied to the string (cf. Fig. 14); it cannot be reached from the bars as it is, but only if the string is laid straight. (19.6.1914) First of all, Chica pulls in the direction in which the string is

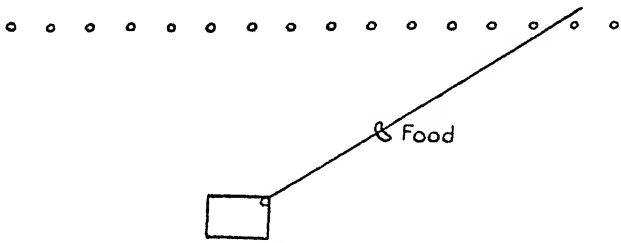

FIG 14.

lying, and so hard that the board of the box breaks, the string is freed, and the objective can be pulled to her. The box is then replaced by a heavy stone and the string tied round it. As the simple solution by pulling is no longer possible, Chica takes the string in one hand, passes it round the bar to her other, which she puts through the next space, and so on, passing it thus until the string is at right angles to the bars, and the objective can be seized.

Grande seems at first not to see the string, which is grey and lying on a grey ground. She drags stones about senselessly (compare p. 197)—an after-effect of earlier experiments—tries to detach an iron rod from the wall, which she presumably wants to use as a stick, and at last sees the string. After this the experiment runs as with Chica, a solution without any hesitations.

Rana first pulls twice in the direction of the string, then suddenly changes the direction completely, while trying to pull the string to a spot just opposite the one at which it is tied (cf. Fig. 15); at the same time she stands opposite this point herself and keeps on looking at the objective and pulling the string parallel to the bars. This vain attempt is made twice in succession, in separate stages, and then is replaced by the proper solution, as in Grande's and Chica's cases. This experiment shows that the task consists of two parts: one, crude in its geometric and dynamic properties,

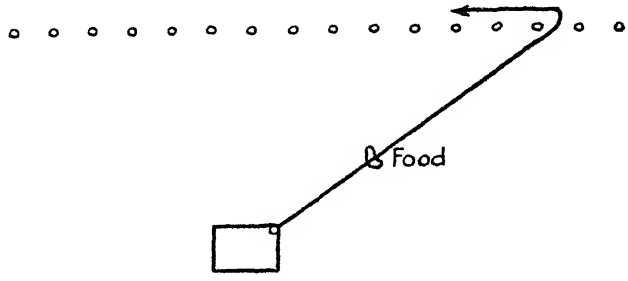

FIG 15.

" turn string at right angles to bars so that the objective comes nearer," and the more refined special problem, arising from the structure of the bars. Chica and Grande solve both parts at once; Rana solves the first one quickly, and the second one only later.

Sultan pulls for a moment like Rana (cf. Fig. 15) and immediately afterwards solves the problem completely, like the others. It becomes quite clear through this that the crude dynamic problem can be solved without any regard for the special one (the second problem), which in this case only seems to be noticed through non-success. Similar effects were encountered in the building with boxes.

Tercera cannot be cajoled into taking part. Tschego and Konsul show—in case it is not already realized—that

the solution is not obvious; for neither gets any further than pulling the thread in the direction in which it is lying.

(21.6) The experiment is repeated with Chica, but this time the thread lies on the floor *turned in the other direction*. The animal does not pull at all in the direction of the thread, but starts the hand-over-hand process straight away, in the *opposite* direction from the previous experiment, till the goal can be reached. After this I did not think it necessary to make the same experiment with the other animals.

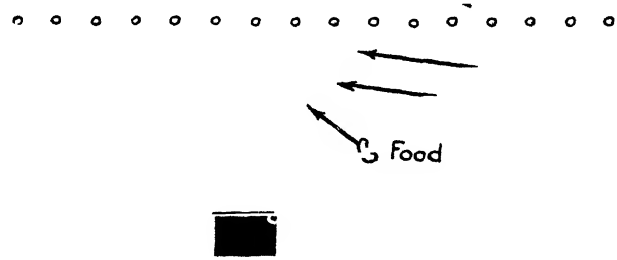

FIG. 16.

After the foregoing explanations, it need hardly be pointed out again that experiments like the one just described give *better* information about the chimpanzee than the usual animal tests with complicated locked doors, etc. Also it should be realized that an experiment so simple and clear as this contain the whole problem to be considered.

If anyone should still be of the opinion that such simple solutions are obvious and have nothing to do with intelligence, I can only invite him to show definitely and exactly the way in which the procedure comes into being. I am afraid no psychologist is able to accomplish this at present.

I separated the two parts of the problem, which, as we saw, are independent, and I shall now consider only the crude one and its solution. This can be characterized simply by the sketch (cf. Fig. 16), leaving out of consideration how the animal, at the first moment of the solution, actually performs

the arrow movement in detail (taking the bars into consideration or not).

Do the animals arrive at the solution in accordance with the theory we have discussed? If so, we should expect to find in all cases the appearance of a large number of impulses which might, in some of the chimpanzees, perhaps, accidentally contain the right "fragments" in the right succession. In reality Grande is the only animal that does anything senseless, and that in the form of a habit stupidity, when she had not yet thoroughly surveyed the possibilities of the problem; when she sees the string, a new stage of behaviour sets in, and immediately afterwards a perfectly clear solution is achieved. Altogether only two movements ("impulses" may occur in lizards, but rarely in chimpanzees) really take place with regard to the objective. These two movements are:—

1. *Pulling in the direction of the string*, i.e. a sensible proceeding, the practicability of which Chica once proves. No man, still less a chimpanzee, can otherwise find out if the string will not really come loose from box or stone.

2. Pulling at the string, or continuous passing of the rope hand-over-hand—in both cases in the *right direction for a solution* (see arrows in the diagram).

Not in a single animal was anything approaching a direction midway between these two observed, much less a third quite new one, etc. Where the more primitive tendency appeared first (in the direction of the string), the jump to the other one was yet made quite abruptly.

I should imagine that everybody must feel that we have here a very clear, though peculiar occurrence, and one which has nothing at all to do with the postulates of that theory. Are we to squeeze and force the facts to make them fit in with that theory, just to suit the so-called principle of scientific economy? In this case the observer is forced to the conclusion that attempts 1 and 2, appearing as wholes, yet each

on its own, are a direct result of a visual survey of the situation. A certain scientific attitude, which one might also formulate as a principle, the "principle of maximum scientific fertility," would lead one to begin the theoretical considerations with this character of the observations, and not to eliminate it at whatever cost as the theory of chance does.

There would be no need to discuss this theory further if the previous life of the chimpanzees we have tested were known to us in all details, from birth to the moment of testing. But unfortunately this is not the case; and even if the possibility that in the *experiment* the solutions arise by chance be excluded, yet the possibility remains that they were developed *before*, within the scope of the theory, by accident, that they were repeated and improved, and now seem to appear as genuine solutions.

It is always difficult to contend against arguments which are outside the domain of possible proof. In this case, however, not even the overstepping of the bounds of experience will be a weakness in the argument, for naturally the chimpanzees we tested had passed several years as lively animals, uncontrolled, in the jungle of the West Coast, and, while there, came into contact with several objects similar to those employed in some of our experiments. Thus it becomes necessary to consider whether this circumstance does not influence the significance and the factual value of the experiments.

But two points must be kept rigidly in mind, if the object of the discussion is not to be lost:—

1. The fact that the animals have had to deal with single objects or situations prior to the experiments, has not necessarily any direct connexion with out problem. It is only when, exactly according to the theory, during this previous period, meaningless but successful chains of actions, externally like the behaviour observed here, have been formed—acci-

dentally, and selected by success—that " previous experience " speaks against the value of these experiments. I am far from asserting that the animals tested in the second chapter have never had a stick, or anything like that, in their hands before the experiment. On the contrary, I take it for granted that every chimpanzee above a certain very low age has had some such experience ; he will have seized a branch in play, scratched on the ground with it, and so on. Exactly the same thing is very frequently observed in small children of less than a year, so that these, too, had their " experience " with sticks, before they used them as implements to pull things towards them that they could not otherwise reach. But just as this does not at all prove that they get accustomed to the use of implements in the mere play of chance and quite without insight, reproducing it again without insight at two, four, or twenty years of age, so also it does not follow for the chimpanzee, whose test-stick is not the first he has ever had in his hand.

2. I am by no means trying in this work to prove that the chimpanzee is a marvel of intelligence[1] : on the contrary, the narrow limit of his powers (as compared to man's) has often been demonstrated. All that has to be decided is whether *any* of his actions have ever the characteristics of insight, and the answer to this question of principle is at present far more important than an exact determination of *degrees* of intelligence. On the other hand, the theory of chance, discussed here as a general principle of interpretation has no interest in the mere *diminution* of the *number* of intelligent acts in experiment, but, in order to be convincing, the theory must explain *all* tests, without exception, consistently with itself. And it fails when, even though some results observed are explained by it, others are not. In the latter

[1] One is obliged to-day to mention in a serious book that the chimpanzee has up till now shown no inclination or gift, for instance, for the study of fourth roots or elliptic functions

case, when the general application collapses, there will be less temptation to explain certain kinds of behaviour as products of accident, which, by their nature, do not invite such an interpretation, though they may be forced under this theory.

The past history of these animals, before the tests, is not altogether unknown. Since at least the beginning of the year 1913, they have been carefully watched, and for a further six months before that date, we can rest assured that any practice in a number of test-situations was impossible, because the animals were confined in the narrowest cages, with no " objects " in them (in Cameroon, on the voyage, in Tenerife). According to the information of my predecessor, E. Teuber, during the year of observation *before* these tests, Sultan and Rana did not get beyond using ordinary sticks (without any complications) for lengthening of the arm, and jumping—the others did not even achieve this much; occasional throwing of stones was observed, and in one case the fabrication of an implement as described above (pp. 101-2), when Sultan takes the shoe-cleaner to pieces.

In any case, the following circumstance is important: when it is a question of the principal decision, whether insight occurs or not, then for any explanation to be in accordance with the chance theory, *not the slightest trace of insight* must occur, not in the most hidden, or in the most innocent, disguise. Therefore since everything, to the smallest details, was to be put together out of chance combinations of elements, and rehearsed, until it could *seem* to appear as a single and intelligent action in the experiments, so we shall, in general, have to assume, not *one sole* former occasion in a similar situation, but a *series of repetitions* of such occasions. Only then somebody might say with conviction that this procedure or that, or rather *all* the lines of action here observed have had their origin and development, in accordance with the principles of the theory.

I remarked above that the general principles of higher psychology often had a tendency to hide rather than to clarify the things to be explained. For instance, when we say that the objectively-useful employment of a stick, as a means of reaching otherwise inaccessible objects, developed by accident and the selective working of success, it will sound very precise and satisfactory. When we look closer, however, our satisfaction with the general principle is soon diminished, if we are really serious in making the condition " without a trace of insight." Let us assume, for instance, that the animal seized a little stick *by accident* at a time when some food, otherwise unattainable, lay at some distance. As, for the ape, the stick and the objective have nothing to do with each other, we have to ascribe it to chance also, if, among a large number of other possibilities, the animal brings the stick into the vicinity of the object desired. For, of course, we must not assume that this action occurs all at once, as one. With one of its ends in the neighbourhood of the objective, the stick has still nothing at all to do with the objective, as far as the animal is concerned, he " does not know " that he has arrived objectively a little nearer to the attainment of the goal. The stick may be dropped, or pulled back, or pointed in all the directions of a sphere with the animal as centre; and chance will now have to work hard until from all the possibilities *one* emerges, namely that the end of the stick is put down behind the objective. But again, this position of the stick tells the unintelligent animal nothing; as before, the most various " impulses " may appear and chance might well have reached the limit of its capacity, if the animal now makes an accidental movement which brings the goal a little nearer to it. But this again the animal does not understand as an improvement of the situation; for it understands nothing at all, and poor, exhausted chance, which has to do all the work that the animal itself is unable to do directly, must now

prevent the stick from being dropped, drawn back, and so forth, and must bring it about that the animal keeps the right direction in further chance impulses. It may be said that there are very various sequences or combinations of impulses containing, for instance, as their last constituents "stick behind objective," and after that "the objectively fitting impulse." That is correct, and the possibilities open to chance, if it is to do this great work, become thereupon more numerous. And yet even now nothing is spared to it; for the majority of these combinations contain, of course, factors objectively quite meaningless, which only follow upon each other in such a way that the whole series *finally leads to the two elements mentioned above.* Therefore, if the first favourable combinations, of which these elements form the end, contain such objectively-meaningless components, chance must later complete the work by means of a large number of other favourable cases, until a perfectly smooth, and *seemingly intelligent,* procedure matures with the help of the (at first, probably extremely rare) successes; for as the use of the stick is observed here for the first time, it contains in no case a thoroughly false component, even if (as with Koko) weakness of the arm and clumsiness act as somewhat of a hindrance.

At this juncture it will probably be objected that the desire for the objective, the general *urge of the instinct* in its direction, is being left out of consideration. To this we reply: in the first place, to conform to the theory, we assume that this "instinct" is perfectly blind, that *the animal is not in any way aware that he is nearing its goal* by taking this direction—for otherwise the theory would be untrue to itself; secondly, according to the theory, this instinct exists *for the body of the animal,* and for the innervations of his limbs, *not for the stick* he happens to hold in his hand. I want to know therefore: if the animal, following that impulse, moves his arm in the direction of the objective in order to catch hold of it, why

should he keep the stick, of which his instinct knows nothing, in his hand, rather than open his hand to seize the objective, as at other times, and thus let go of the stick? For, all this time, the stick has, in the animal's eyes, *nothing to do with the objective*. Should he, however, contrary to this demand of the chance theory, continue holding the stick in his hand, that would, with his lack of any trace of insight, be possible in a variety of very different ways. It may be held right in the centre, so that the stick is parallel to his front and sideways, or it may be grasped at the extreme end, the other end pointing back towards the animal, upwards to the sky, or down to the ground, etc. For if nothing is assumed but the impulse of instinct in the direction of the objective, and accidental movements—*intelligence*, to the contrary, remaining wholly *excluded*, one way of holding the stick is as good as another and the different possibilities are limited only by the animal's muscular power; because success will have its selecting effect at the earliest after *one* favourable combination only. And so chance which has already, in opposition to the theory, left the stick in the animal's hand, has still plenty to do before it succeeds in obtaining the right manner of holding the stick, in eliminating the false elements by the help of chance *successes*, and obtaining a mode of procedure, superficially similar to intelligent behaviour.

[It might be further objected that it is not necessary that the first result be obtained *all at once* in an action of this kind: all kinds of *subordinate actions* might first develop accidentally, and be later more easily united in a combined action. But this reflection does not help us either, because the permutations corresponding to the said subordinate actions might occur once, but they cannot be co-ordinated into firmly connected partial actions, which alone would be of use. This is because *that "success" is lacking*, which, according to the theory, ensures the connexion of the individual impulses. What does

it matter if the animal is once in a way led by accident to putting one end of the stick behind the objective? That is no success, within the meaning of the theory, so long as, in accordance with the theory, the animal is without any trace of understanding. And so chance must either take one more step and continue its permutations to the end until objective and animal meet, or else it immediately thereafter strays into quite irrelevant impulses; and then, according to the theory, there is no tendency ever to repeat this combination, the last lap of which is—"point of stick behind objective."]

This theory of chance is preferred to many other attempts at explanation, because it is held to be specially exact, and in exceptionally good correspondence with the demands on scientific thought. For this reason many people would no doubt like to see not only the use of the stick, but *all* other performances explained by it as above described Little as there is to be said against the theory, in cases in which success can be easily achieved by chance (as, for instance, when an animal locked in a small box, tries blindly to get out, and in the course of its disordered movements, happens to push a lever which opens the door), there does not seem to be much value in it, just from the scientific point of view, when it is used to explain such experiments as the ones described here.

Those scientific ideas with which we here come into conflict are the ones which suggested to Boltzmann the (as yet) most comprehensive and important formulation ever made of the second law of thermo-dynamic. According to this law, neither physics nor theoretical chemistry allow of the fortuitous formation of a well-directed *total* movement in the course of the permutations of a large number of small chance movements, which are mutually independent, irregular and all of them equally possible.[1] For instance, in the case of Brown's molecu-

[1] In physics one speaks better and more exactly about "*Zustand-selemente*." It would lead too far to go into this more closely C f for example, Planck, *Acht Vorlesungen uber theoretische Physik*, 1910

lar movement, it is impossible for a suspended particle, pushed hither and thither fortuitously and irregularly, to be suddenly projected one decimetre in a straight direction. If such a thing did happen, without doubt a source of error would have entered, i.e. *an influence not following the laws of probability.* Now, whether it be a question of Brown's molecular movement, or of the so-called chance impulses of a chimpanzee, makes no essential difference here ; for the *bases* of the second law (according to Boltzmann) are of so general a nature and so obviously valid for more than thermo-dynamics (namely, for the whole domain of chance) that they are applicable also to our (alleged) subject-matter, the " impulses ". Anyone who reproaches us for playing with analogies must surely have misunderstood the fundamental thought of Boltzmann (and Planck). There does, however, exist a quantitative difference between the thermo-dynamic case and our own. In what degree the appearance of a special combination is improbable (up to practically quite impossible) depends on the number or size of the independent elements which are combined. One easily sees that in this respect the " impossibilities " of thermo-dynamics are not quite achieved by those of the animal tests (as above described in the use of the stick), as we have to deal here with *less* members (i.e. possible " impulses ") and these, compared with the total proceeding, are *relatively still great.* Of course, this alters nothing as regards the direction of our reflections and the fundamental doubtfulness of the assertions of the theory so long as one *does exclude non-accidental forces,* and thoroughly considers individual cases, with their immense demands, as was done in the stick experiment.

[Neither the general direction of the " instinct-impulse " towards the objective, nor the further development in " selection through success ", alter anything in the unfavourable condition of affairs ; the former does not, for the above-given

reason, the latter because from the first it presupposes the right succession of lucky hits, which is not likely to occur, but without which "success" will have no opportunity to work at all.]

Since similar considerations have been advanced on questions dealing with evolution by Bergson and E. v. Hartmann, and as they play a great part in vitalistic literature, the following observations seem relevant. E. v. Hartmann considers it impossible that the bird arrives at its nest by chance, and he concludes that "the unconscious" is the builder; Bergson considers accidental arranging of the elements of an eye altogether too improbable, and, therefore, makes the "*elan vital*" accomplish the miracle. The neo-vitalists and psycho-vitalists, too, are equally unsatisfied with Darwinian chance, and they consider that everywhere in the specifically animate realm "forces with a purpose," of the general type of human thinking, are required for explanation, though these forces are not actually experienced, as thinking is. *The only connexion* that this book has with such a line of thought, is that *here, too, a theory of chance is rejected.* But generally the transition from the rejection of this chance theory to the acceptance of one of those doctrines is regarded as almost obligatory, and I, therefore, wish to emphasize that the alternative is not at all between chance and factors outside experience. In that opposition lies the fundamental error, that all that happens in inanimate nature is to be considered as subject to the laws of probability, whereas, after all, great parts of physics have nothing to do with chance. As certain as it is that the study of physics does not consist solely of the laws of irregular heat movement, even so certain is it that one need not from a view like the above, which contradicts the theory of chance, jump to the assumption of agents outside experience. It seems particularly surprising from the standpoint of physics that one should continually insist on

speaking here of "either—or", when after all there are quite other possibilities[1].

I think I have shown that the theory of chance can in no way be considered exact in every case, and that in such performances as those described here, absurd demands are made of chance, whereas natural science in particular does not allow such blind confidence beyond certain limits. It is, therefore, advisable to throw another glance at the experiments themselves.

According to the chance theory, we never have before us a first occurrence, but—as the proceeding (compare use of stick) measured by "impulses" is relatively complex and yet perfectly smooth—always a case which is the product of frequent repetition. The objectively correct use of a box, etc., as a footstool, for example, could not be developed in less than, say, fifty repetitions. At least as often as that, an objective placed high up ought to have been unattainable by the animals, in any easier manner, and, at the same time, a box or similar object should have been at hand. One has only to remember how improbable it is even, that the animal in such a situation should seize the tool at all or move it, *as long as it has no insight whatever*. The more thoroughly one studies this case—as is much to be desired—the more will one consider far higher minimum numbers of repetitions necessary to chance in the development of the behaviour.

The same closer consideration of individual cases shows also that generally not even the simplest preliminary condition for such an expensive play of chance is present. How often, in any case, could a chimpanzee, under his normal conditions of life, be in a position to need, for example, a footstool to reach a high object—presumably the fruit on a tree? Over and above any use of implements, the solution for the chimpanzee

[1] Compare my book *Die Physischen Gestalten in Ruhe und im stationären Zustand*, Brunswick, 1920

is the roundabout way *in its literal sense*; every time the detour seems even a remote possibility to us (and beyond this human limit, too) these animals do not hesitate at all, and certainly show no other "impulses"; they *start on the roundabout way immediately*. At the beginning of the experiments it was my special task to make this easy proceeding impossible for them (compare above, p. 11 seqq.). If it is a question of trees—and where else in the Cameroon jungles could anything be hanging high up?—I maintain that there is scarcely anything that the chimpanzee cannot reach in some roundabout way or other. One must have seen for oneself how even a chimpanzee who is a bad gymnast (as, for instance, Sultan, whom I sometimes took out of doors) jumps from one tree to another, seems to fall, slips, and so forth; how he falls into the thin foliage of a tree which has no proper branches, but only leaves and the tiniest twigs, and yet, catching hold of it for the fraction of a second, lets it act as a sufficient brake to enable him to continue swinging himself along, jumping, and falling, until he comes to a standstill again in any firm spot he chooses. Thus I must emphatically deny (in the case of the use of boxes) that the animals had enough occasion to be forced, through the exclusion of the roundabout gymnastic methods natural to them, to combine any number of other impulses. Under natural conditions they arrive anywhere without stools, and only Man, making experiments, brings them into situations where such roundabout methods are excluded objectively, or through Man's prohibitions. The same may be said about the use of a stick for pulling objects to them which would otherwise be unattainable (in Tenerife such an action was not noticed in Tschego before the experiment, and Nueva and Koko were examined immediately on their arrival), about the pushing away of a box which is in the way of the railings (when at liberty the chimpanzee, of course, takes roundabout paths round stone blocks or thick tree-

trunks). As a number of farther experiments presuppose an appropriate employment of stick and box, they too lack—and for the same reason—the previous history necessary for adequate combination.

[Once again let me repeat: a number of objects become familiar to the animals in some way or another before the experiments. But there is a big difference between touching a stick and using it "intelligently". If one now abandons the theory, and asks whether Nueva, for instance, has not sometimes *in intelligent play* pushed a stone about with the stick, the answer is doubtful, in such a changed aspect of the problem. For even with little insight many things, of course, become easy which could never occur by accident. I am very much inclined to answer the question in the affirmative, as every day, during the play of the animals, when they understand very well what they are doing, such things do happen.

Should any doubts remain about the foregoing, none can be entertained about my assertion that, in some cases, the situation either faces the animal *for the first time* when he performs the experiment, or else he may have experienced something similar, but only very rarely. The model test described above (p. 199) is an example. Who can seriously assert that any of the animals had been in a similar situation prior to this test, in which they are confined to a space behind railings, a string fastened outside lying obliquely on the ground, approximately in the middle of which a piece of fruit is tied, so that only a certain turn of the rope will make the object attainable? Even if the test is simple, the animals will not have experienced such a thing, and yet four of them, independently of each other, solve the principal task all of a sudden. Never, before the experiment, did an object hang in front of a door, and yet, in such an experimental situation, the door is suddenly looked at sharply, and at once opened, a clear solution. Leaving aside the question of how Sultan arrives at the use of a box at

all, there remains the other: what makes him, when the test is performed, take out the encumbering stones? Where did he ever get the chance of making blind combinations in a situation like this? And, further, there can only have been very few single cases, not by any means as many as required by the theory, of Nueva not being able to reach the objective with a stick too short and, by chance, finding a longer one close by, with which she could reach the first one, etc.—of course always through chance impulses.

It is indeed a great effort to argue so much against an explanation for which the observations give no grounds. Finally, I shall once more call attention to the character of these observations, which tell one more than any argument could, contrasting them with the requirements of the theory.

1. The animals are supposed to have accidentally got accustomed to such solutions in previous life; an *extremely familiar* action, the result of very much practice, was, it is presumed, observed, which, on account of its extreme familiarity, looks exactly like an intelligent solution. But the best and most obvious solutions which I observed, often occurred suddenly, after the animal had been quite helpless at the beginning of the experiment, and sometimes for hours after. Whoever considers Tschego's first experiment (when the box was in the way at the bars) or Koko's (use of box as a stool) to be the repetition of long-practised, mechanical and meaningless products of habit, does so certainly in opposition to the impression which observation of the procedure must make.

2. The animals are supposed to have so developed, strengthened, and perfected their performance, through the selection by success of "impulses", that they can now "easily" reproduce it *in this form*. No single experiment fulfils this requirement, as practically none is performed twice over in the same way; indeed, the movements by which one single one is performed vary greatly. The door is opened from the

ground, but also the animal sitting on top of it; when the box is standing in the way, it is pushed away by a corner from the bars, or thrown back over the bottom edge. If the box is to be brought underneath the objective, the same animal will drag, carry, or roll it along, just as the mood takes him. *The only limit is the sense of the proceeding.* For this reason no observer, even with the best of efforts, can say: "the animal contracts such or such a muscle, carries out this or that impulse". *This would be to accentuate an inessential side-issue, which may change from one case to another.* To give the essentials, it is necessary to use expressions in describing all this, which themselves involve meaningful actions; for instance, "*the animal removed from the bars the box which stood in the way*". Which muscles carry out which actions is entirely immaterial.

3. There are other variations not so unimportant, which likewise run counter to the theory, *but arise directly through unforeseen circumstances,* and represent the answer to these circumstances; these *cannot possibly all have been rehearsed.* The animal then does not continue carrying out a rehearsed programme meaninglessly, but answers to a disturbance by a corresponding variation. This is often the case when using the stick. It is easy to say, that the animal fetches an object with the stick, but in reality it does so each time in a different way, because on uneven ground each movement brings the object into a different position which requires special handling. When Sultan for the first time pulled one stick towards himself with another, the test went very smoothly on favourable ground. But the next time the stick encountered a pebble while he was drawing it to him, and so he could not get it any farther, as it was turned round and pointed straight towards him (lying lengthwise). The animal stopped *at once,* first pushed the stick, with careful little pokes of the second stick, crosswise again, and then pulled

here given do not support his explanation. The more he tries to advance more valuable data than the general scheme of his theory, and really think out and show how he would explain and interpret all the experiments in detail, the more will he realize that he is attempting something impossible. Only he must keep in view the condition that not even in the most innocent form or in the smallest detail is intelligence to be allowed to co-operate as insight into the structure of the situation.

Whoever is not sure from the very beginning (as a disciple of scientific economy) that this theory only and no other may be applied to animals, must be asked once again to look through the reports of some of the experiments. Even if that will give him but a faint idea of what direct observation of the actual occurrences teaches one—that cannot be adequately reproduced—he may perhaps feel that, besides the theory, such extended discussions about it are not suitable here; *to such an extent do observations, and the manner of explaining them, differ from each other.* Unfortunately one is forced, by the small value assigned to psychological observations compared to general principles, to such remote and amazing discussions, which the subject-matter itself does not at all require. Henceforth, I shall refer no more to the theory, and shall discuss the experiments only from the points of view which arise *directly out of them.*

I did not express my attitude towards the general theory of association when discussing the chance-theory, and, at the very beginning, it was pointed out that the question to be answered in this book might be affirmed or denied without thereby affirming anything regarding the relation of the experiments to the doctrine of association. For the time being, this will be assumed. If we accept the doctrine of chance, we shall also have to accept that animals have no insight whatever; this touches the very core of the investi-

gation. Association theorists know and recognize what one calls insight[1] in man, and contend that they can explain this by their principles just as well as the simplest association (or reproduction) by contiguity. The only thing that follows for animal behaviour is that, where it has an intelligent character, they will treat it in the same way; but not at all that the animal lacks that which is usually called insight in man. I can, therefore, dispense with any closer elaboration in this direction and will merely observe here that the first and essential condition of a satisfactory associative explanation of intelligent behaviour would be the following achievement of the theory of association, to wit: what the grasp of a *material, inner* relation of two things to each other means (more universally: the grasp of the structure of a situation) must *strictly* be derived from the principle of association; "relation" here meaning an *interconnexion based on the properties of these things themselves*, not a "frequent following each other" or "occurring together." This problem is the first that should be solved, because such "relations" represent the most elementary function participating in specifically intelligent behaviour, and there is no doubt at all that these relations, among other factors, continually determine the chimpanzee's behaviour[2]. They are not facts merely of the type "sensations" and the like, merely further associable elements, but it can quite definitely be proved (and quantitatively proved)[3] that they determine in a very marked degree the chimpanzee's behaviour, i.e. his inner processes, by their functional properties. Either the association theory is capable of clearly explaining the "smaller than," "farther

[1] The German word *Einsicht* is rendered by both "intelligence" and "insight," throughout this book The lack of an adjective derived from the noun "insight," apart from other considerations, makes this procedure necessary [Tr Note]

[2] As they determine memory in man too (Cf. Selz, *l. c*).

[3] Cf *Abhandl. d Preuss Akad d. Wiss*, 1918, Phys-Math. Section, No. 2

away than," " pointing straight towards," etc., according to their true meaning as mere associations from experience, and then all is well; or else the theory cannot be used as a complete explanation, because it cannot account for those factors primarily effective for the chimpanzees (as for man). In the latter case only a *participation* of the association-principle could be allowed, and at least that other class of processes, relations and *not* exterior connexions, should be recognized as an independent working principle as well.

The following explanation, which is often suggested by non-professionals, but which none who has had much experience with animals will take too seriously, can be dealt with much more shortly. Could the chimpanzees, perhaps, prior to the experiments, have seen similar methods of procedure carried out by human beings, and do they not simply imitate such proceedings?

This idea must first of all be brought into clear relation with the question dealt with in this book. It should only be brought forward in the form of an objection if " mere imitation " means imitation *without a trace of insight* into things that have been seen; for otherwise, instead of an objection, we should be dealing with a very special suggestion as to the interpretation of the intelligent action we have actually before us. I presume that even this slight explanation of the so-called objection will somewhat lessen the tendency to bring it forward. For any sudden introduction of relatively complex proceedings, seen *without a trace of insight*, but now performed just as if they were intelligent, would constitute a phenomenon which, as far as I am aware, has never yet been witnessed either in human or in animal psychology; it would have to be introduced here as a new hypothesis. It appears to me, therefore, that we are faced with the following mistake. For the adult human being nothing is easier, in general, than to " imitate " what he sees, or has seen others do; and particu-

larly such actions as the chimpanzees here carry out would be copied immediately by one human being from another, if occasion arose; in such cases we may certainly speak of "mere imitation." Now this fact, carelessly considered, might lead to the said objection; but when applying it to the chimpanzee, one leaves out of account that the human imitator has long been acquainted with the action, and, as long as the model does not become too complicated, will immediately *understand and intelligently grasp what the action of the other means*, and to what extent it is a "solution" of the situation in question. However, that it may be possible, even after a lapse of time (for the experimenter excludes all opportunity for imitation immediately before the experiment),[1] to achieve complex methods of behaviour in no wise and in no detail understood, as clear and complete actions, simply because they were witnessed once or several times before: I repeat: none of our experience has shown us this, and there is little prospect that it will in the future show us anything so remarkable. What is really important is that we consider carefully, and allow not the smallest trace of the insight type to be included in what we are here assuming under "imitation."

Even animal psychologists have not always paid sufficient attention to this fundamental difference between "simple" human imitation and the imitation we so lightly expect from animals, and so people were to a certain extent astonished when it was first shown experimentally that animals do not so easily imitate as expected. Less astonishment would perhaps have been felt if it had been realized that, after all, man has first to *understand*, in some degree, before it even occurs to him to imitate. Now we have to test whether animals also require a certain minimum of understanding of what they have seen, before they can imitate it. Recent

[1] Excepting in those cases in which "imitation" is to be investigated.

experiments by American investigators[1] have proved quite definitely, contrary to Thorndike's results, that some imitation, clumsy and laborious enough, occurs among the higher vertebrates. Their reports bear out the assumption that, in general, the animal must work *hard* to gain some understanding of the model, before it can imitate it. "Simple imitation"! I can only say to any who have not yet experimented with animals: when any animal suddenly does manage to imitate a performance enacted before him of which he knew nothing before, he inspires the greatest respect immediately. Unfortunately this is a very rare occurrence even among chimpanzees,[2] and when it does occur, the situation, as well as its solution, must lie just about within the bounds set for spontaneous solutions. It will now be seen how far removed from experience an objection of the "simple imitation" type is.

[Chimpanzees (and also other higher vertebrates) will "imitate" with ease as soon as the same conditions as those required in man are present, i.e. if they are already familiar with, and understand the action to be imitated. If, *in such circumstances*, there is any reason to watch the model (animal or man), and if his actions are of interest, then either the animal "takes part" or "tries the same solution," etc. Thus, in imitation, similar circumstances and qualitative conditions seem to exist in the higher animals as in man. It can easily be shown that humans do not "simply imitate" either, if they do not sufficiently understand an action, or a line of thought. I shall return to this subject when describing the imitation of chimpanzees.]

Anticipating later accounts, I will, for the present, mention

[1] Berry, *Journal of Comp. Neurol and Psychol*, 18, 1908; Haggerty, *ibid*, 19, 1909

[2] Compare Pfungst, *Bericht uber den 5 Kongress. f exper. Psychologie*, 1912, p 201. Pfungst, however, goes too far, even human beings are imitated by chimpanzees when necessity arises, *if they are understood.*

only briefly that four kinds of imitation occur in chimpanzees, but that none of the observations give the slightest ground for thinking that the animals could "simply" and *quite without insight* have "imitated" important parts of their performances. The chimpanzee cannot do this.

For the rest, the following remarks may be useful in tracing, for the present, the limits of what might be taken over in imitation of whatsoever form:—

1. The question whether the animals could ever have seen anything similar to their performances carried out by human beings is doubtless to be answered in the affirmative in some cases; or rather, the animals *must* have seen some of these acts before their tests, though it cannot be ascertained how much attention they paid. It is, for instance, almost impossible to keep a chimpanzee in captivity without someone in his presence doing something similar to his use of a stick. Even the cleaning of his cage with brooms, and so on, must, unless some very complicated system is to be introduced just on this account, lead to similar actions. Attempts to forbid the keeper to use things in this way are useless, as first, it would be too late, because the same things may have been done on board ship, and secondly, it is very difficult for non-experts to refrain from all such actions, because men use simple implements quite unconsciously. These things have to be taken into consideration. It is less probable that they have seen boxes, and such like, used as stools, but, on the other hand, they may quite likely have seen ladders used. How far such examples influence the ultimate action of the animals, when no *immediate occasion* or incentive to imitation arises, will be discussed in another connexion. I will now merely draw attention once more to the fact that where there is no trace of understanding, the presence of the chimpanzee in

cases where implements are being used, seems to have no effect whatever.[1]

2. In a number of cases any kind of imitation is, from the nature of the case, impossible: (*a*) because the task in question may never have been performed by man in the presence of the chimpanzee (remember the use of a door, the unburdening of the box filled with stones, the experiment described above with the string running obliquely to the bars, and others); (*b*) because no human being would ever hit on the solution attempted by the animals (for instance, the jumping-stick and the "good errors"). Who could at any time have given them an example of how to place a box high against a wall, or to hold two sticks together to make one longer one, a purely visual solution?

On the other hand, I must emphasize the following: It has been maintained that the chimpanzee *never* takes over a human method of procedure. That is not correct. Cases occur in which the greatest sceptic would have to admit that the chimpanzee does take over new performances, not only from his own kind, but from man also.

[1] I can state with absolute certainty that *no intentional instruction* of the animals ever took place, with the exception of those cases in which I personally tried my utmost to obtain some result by doing so