VOL. 39, NO. 2 March, 1932

THE PSYCHOLOGICAL REVIEW

THE REPLY OF A PHYSIOLOGIST TO PSYCHOLOGISTS

BY I. P. PAVLOV

Leningrad

I

The article by Edwin R. Guthrie, 'Conditioning as a principle of learning,'[1] provides, it seems to me, special interest from its cardinal tendency, altogether justified to my mind, of ascribing phenomena designated as psychological activity to physiological facts, *i.e.*, of uniting or identifying the physiological with the psychological, the subjective with the objective, which I am convinced comprises the most important present-day scientific undertaking. The author treats the principle of learning in a general way, giving the characteristics of that process in an enumeration of its fundamental features, while he makes use of the material of psychologists, or of our physiological facts obtained with the method of conditioned reflexes with animals, without distinction. So far psychologist and physiologist proceed side by side. But beyond this point sharp differences arise between us. The psychologist takes conditioning as the principle of learning, and accepting this principle as not subject to further analysis, not requiring ultimate investigation, he endeavors to apply it to everything and to explain all the individual features of learning as one and the same process. For this purpose he takes one physiological fact and in a decisive way gives it a specific meaning in the interpretation of certain concrete facts of the learning process and does not seek an actual confirmation of that

[1] E. R. Guthrie, PSYCHOL. REV., 1930, 37, 412–428.

meaning. From this, the physiologist is inclined to think that the psychologist, recently split off from the philosopher, has not yet altogether renounced partiality toward the philosophical method of deduction from pure logical work, without verifying every step of thought through agreement with actual fact. The physiologist proceeds in quite the opposite way. At every phase of his investigation he endeavors to analyze the phenomena individually and in connection with facts, determining as much as is possible of the conditions for their existence, not trusting to mere deduction or to a single hypothesis. And this I shall prove in regard to several points, in which the author opposes me.

Conditioning, association by contiguity in time, conditioned reflexes, even if they serve as the factual point of departure of our investigations, are none the less subject to further analysis. We have before us an important question: What elementary properties of brain-mass form the basis of this fact? We have not yet reached a final solution of this question, but significant data are afforded by the following experiments. With our experimental animal, the dog, if the external agent which we wish to use as conditioned stimulus is applied after the beginning of the unconditioned stimulus, the conditioned reflex occurs (according to the latest and most exact experiments possible of Dr. N. V. Vinogradov), but is insignificant and temporary, disappearing in every case if the period of procedure be continued. A durable and lasting conditioned reflex, as we have long known, is obtained only when the external agent invariably precedes the unconditioned reflex. Thus the first procedure possesses a double effect: at first, temporarily, it assists in the formation of the conditioned reflex, and then destroys it. This latter action of the unconditioned stimulus comes out clearly in the following type of experiment. A conditioned stimulus which becomes effective by means of the second (the usual) procedure,—if afterwards it be systematically applied after the beginning of the unconditioned (or becomes submerged by the unconditioned, according to our usual laboratory terminology), particularly if it belong to the category of weak conditioned stimuli,—gradually loses all positive action and finally is even converted into an inhibiting stimulus. Evidently in this case the mechanism of negative induction (in our old terminology called *mechanism of external inhibition*) gradually prevails; *i.e.*, the cell excited by the conditioned stimulus is inhibited or comes to an inhibited state with repeated concentration on the part of the unconditioned stimulus—and the conditioned stimulus in this way meets in its cell a permanent state of inhibition. But this brings out the fact that the conditioning agent becomes inhibitory, that is, on being applied alone it now calls forth in its own cortical cell not an excitatory but an inhibitory process. Consequently during the usual process of formation of a stable conditioned reflex, the passage of a wave of excitation from the corresponding cortical cell to the center of concentration of the unconditioned stimulus is exactly the fundamental condition which fixes the path from one point to another,—more or less of a permanent joining together of the two nervous centers.

Let us pass now to other features of conditioned activity, where the author instead of our diversified analysis of facts offers his own monophasic interpretation of the phenomena which take place. The delayed or postponed conditioned effect, according to our experiments, is based on special inhibition of early phases of the conditioned stimulus, since they do not fit in closely with the time of appearance of the unconditioned stimulus. The author for some reason asserts that we attribute this to 'mysterious latencies' in the nervous system, and he gives his own explanation of the facts. He assumes that when for example the sound of a bell is presented as a conditioned stimulus, the animal responds with a reaction of 'getting set to listen,' a complex motor act, and the centripetal impulses in that act are, strictly, the true stimulators of the conditioned effect,—in our case of the conditioned food reflex, the salivary secretion.

According to the author, "when the salivary glands begin to secrete, the accompanying stimuli are not furnished by the bell but by these responses to the bell. The direct response to the bell is probably over in a small fraction of a second."

And further he says, "the apparent separation in time of a conditioning stimulus and its response is then quite possibly an illusion." The author even says that "Pavlov tends to forget," in his explanation of the delay, the existence of the above-mentioned centripetal impulses from the motor apparatus. On page 360 of my book, 'Conditioned Reflexes, an Investigation of the Physiological Activity of the Cerebral Cortex,' one can see that I not only take into account the centripetal impulses from skeletal musculature, but also consider as more than likely their existence in all tissues, to say nothing of individual organs. To my mind the whole organism with all its component parts can make itself known to the cerebral hemispheres. This shows that the point is not overlooked by me, but that actualities provide not the slightest ground for accepting the fact in the way it is explained by the author.

First of all, if we grant with the author that not the bell, but the centripetal flow of impulses from the motor act of listening is the true stimulus for the conditioned effect, why does that effect, in the case of delayed reflexes, nevertheless come out, not at once, but after an interval—and (furthermore) in accordance with the length of the interval between the beginning of the stimulus and the beginning of the unconditioned reflex? For, when the unconditioned stimulus is delayed for a very short time (only a few seconds) after the beginning of the conditioned, the effect, brought about according to the author by centripetal impulses from the motor act of listening, appears as soon as 2 or 3 seconds. Where then is the explanation of the length of the delay? and how, when the conditioned stimuli precede the unconditioned by several minutes, do the stimuli of the author, the centripetal impulses of motion, act after the lapse of minutes?

But as a matter of fact there is absolutely no ground for accepting a continuous action of the stimuli of which the author speaks. The listening response as a general orienting or investigating reflex, as I have termed it, appears with every new set of vibrations which habitually play upon the animal, and usually remains in existence only for the short period of the first application of the new recurring stimuli. Upon the formation of a conditioned reflex with a more or less short interval between conditioned and unconditioned stimuli, it is quickly replaced by the special motor reflex peculiar to the given unconditioned stimulus. And further, only the conditioned motor effect is permanently to be had, free from all trace of an orienting reaction. And thereupon the conditioned stimulus appears as a pure substitute for the unconditioned stimulus. In the case of a conditioned alimentary reflex the animal may lick the electric lamp, or appear to take the air into its mouth, or to eat the sound,—that is, licking his lips and making the noise of chewing with his teeth as though it were a matter of having the food itself. The same thing occurs in the delayed reflex which is worked out. The animal remains completely indifferent and quiet in the first period of action of the conditioned stimulus; or even (as is not seldom the case) immediately upon the beginning of that stimulus, he drops into a drowsy and sometimes abruptly into a sleeping state, with relaxation of the musculature and snoring. This, on entering the second period of the conditioned stimulus, just a little before the addition of the unconditioned stimulus, is replaced (sometimes with a start) by a clearly suitable conditioned motor reaction. In both cases it is only during the general somnolence of the animal in the course of the experiment and occasionally at the first moment of stimulation, that the orientation reaction shows itself.

And finally, on analysis the delayed reaction actually proves to be the result of interference of a special inhibition which is by itself well known to us, and is studied in detail in many cases of its appearance;—but this is not a 'mysterious latency.' The meaning of all this is clear. Although prolonged for a significant length of time, the conditioned stimulus remains one and the same; but for the central nervous system (and it is especially necessary to think of the cerebral hemispheres) it is distinctly different in different periods of its course. This comes out particularly clearly with olfactory stimuli, which we sense at first very keenly, and then quickly as weaker and weaker, even if they remain objectively constant. Obviously the state of the stimulated cortical cell under the influence of an external stimulus undergoes successive changes and in the case of delayed reflexes only the state of the cell near the time of the addition of the unconditioned reflex acts as a signal for the conditioned stimulus. This is exactly the case when, from different intensities of one and the same external stimulus, we can form different conditioned stimuli,—sometimes positive, sometimes negative, sometimes linked with different unconditioned stimuli. The analyzed fact of delay is an obviously interesting case of special adaptation, in order that the conditioned reflex might not occur prematurely, so that energy beyond the necessary measure is not uselessly expended. That this explanation corresponds to reality is proved by facts. First of all it is clear from the process of formation of the delayed reflex. If the conditioned reflex be formed first with the short interval of a few seconds between the beginning of the conditioned and of the unconditioned stimuli, and then suddenly that interval be increased to a few minutes,—then the conditioned effect, hitherto quick to come forth, will promptly go on to gradual but utter disappearance. And then, on continuing the experiment, there appears for a considerable lapse of time a period of absence of all conditioned effects. Only then does the conditioned reflex appear anew, at first just at the moment preceding the addition of the unconditioned stimulus. Thence it grows gradually and recedes somewhat from the time of appearance of the unconditioned stimulus.

That the first period of the delayed reflex is indeed a period of inhibition is proved by a series of facts. In the first place, inhibition of the delayed reflex can easily be summated. And again, from the delayed reflex one can observe successive inhibition. And finally, the drowsy and sleepy state which comes out in some animals in the first part of the delayed reflex is a striking expression of the state of inhibition.

The next phenomenon, extinction of the conditioned reflex, the author also discusses without paying any attention to the details of the facts of our investigation, having in view again the very same factor conjectured by him, but no more exactly defined than before. And along with it he now ascribes to me, besides the previously mentioned 'tendency to forget,' a concealing of something from myself.

First of all the author takes a stand against us by saying that it is not the brevity of the interval between repetitions of the non-reinforced conditioned reflexes that contributes to extinction of these reflexes, but the number of repetitions. But this is absolutely untrue. An unreinforced conditioned reflex without any repetitions, but simply prolonged from 3 to 6 minutes, ends in every case in extinction to an absolute zero —as we say, in an uninterrupted extinction, in contrast to an intermittent one. And furthermore, the author arbitrarily supposes that extinction is not a constant fact, but an exception to the rule of frequency. Again an absolutely incorrect statement. Extinction is one of the constant facts of the physiology of conditioned reflexes. Having accepted both these things in spite of reality, the author, so to speak, clears the field of action for himself and imagines some different kind of agents,—no better determined,—which in addition to the fundamental unconditioned stimulus take part in the formation of the conditioned effect. Probably here also movements of the animal are assumed, because mention is made of continuous and of various movements of the animal during the experiment. Thus, according to the author, the sum of the agents determining the conditioned reflex continually fluctuates, appearing now greater, now less. When these agents become fewer and the conditioned reflex is absent or becomes diminished, the rest of the agents, also unknown, become inhibitory as well, or, what is practically the same, they become stimulators of other responses.

The breaking up of extinction by extraneous stimuli the author explains by saying that those stimuli 'disorganize posture and orientation' which appear as inhibitors of the conditioned reflex at this stage of extinction and thus temporarily restore the reflex which was becoming extinguished.

The author does not deem it necessary to inform us, even hypothetically, just what sort of stimuli support the conditioned reflex together with the unconditioned, and what other sort here present serve as inhibitors of that effect. When the author in his own way explains the breaking up of extinction by extraneous stimuli, why does he not state in what way the extraneous stimuli, which sweep away the action of the agents that inhibit the conditioned effect, fail to remove also the action of those which facilitate the conditioned response? For they are different stimuli from these latter.

And so there is introduced by the author, without any factual confirmation of their actual meaning, a number of unknown stimulating agents utterly undetermined in a more exact way.

We must conclude that the author understands them to be just these same kinesthetic stimuli, but arising from different muscles. Of course there are many skeletal muscles, and from them arise during their action an almost countless number of combinations, and from all of them special centripetal impulses are constantly being sent to the central nervous system. But in the first place, the most important part of these impulses proceed to the lower divisions of the brain, and secondly, under usual circumstances they absolutely do not make themselves known to the cerebral hemispheres but serve only for the self-regulation and greater precision of movements, such as, for example, the continuously occurring cardiac and respiratory movements.

Under the conditions of our experiments only such movements are reckoned with as exert an influence over our conditioned reflexes,—only those movements which form special motor reflexes, the chief and almost the only one of which is the orientation reflex to the vibrations of the immediate environment, and sometimes also defense against some chance nocuous influence on the animal during his movements on the experimental stand,—a blow from something, some sort of pinch, etc.

If the centripetal impulses, such as the author assumes to arise from all movements which we execute, really proceeded to any considerable extent into the cerebral hemispheres, then by their very number they would provide a tremendous hindrance to a relationship of the cortex with the external world, almost excluding it from its principal rôle. Can it be that, when we talk, read, write, and in general think, our movements, which arise inevitably as we do so, disturb us to such an extent? Can it be that all this is ideally performed only during our periods of absolute immobility?

The constant fact of extinction is not due to the play of chance movements of the animal which are reflected in the work of the hemispheres, but it is the manifestation, according to law, of the most important properties of the cortical cells, as the most reactive of all cells of the organism, when they remain at work for a greater or less period of time—even if generally a short one—without a satisfying accompaniment for the fundamental innate reflexes; for, the chief physiological rôle of excitation of these cells is to serve as signals in place of the special stimuli of the latter reflexes. As the most reactive cells, they quickly become fatigued from work and go on not to an inactive state but to inhibition, which probably not only assists in their rest but also hastens their recovery. But when they are accompanied by unconditioned stimuli, then these stimuli—as we have seen at the beginning of the article—at once, and so to speak by way of protection, inhibit them and thus contribute to their recovery.

That extinction is actually inhibition, is proved as well by its successive inhibitive effect on other positive conditioned reflexes as by the transition to drowsy and sleeping states, which is without doubt inhibition.

As to the two other points, where the author offers merely his own view in place of our explanation, I can be more summary. In regard to the fact of gradual intensification of the conditioned effect during the process of its formation, it is necessary to state that in this case it is the gradual removal of extraneous stimuli which disturbs the formation of the reflex, and not the opposite, namely,—the author's view, which consists in attributing to these stimuli an ever-growing rôle in creating the conditions for the effect. During our first experiments often 50 to 100 or more repetitions of the procedure were required in order to develop a complete conditioned reflex, but now 10 to 20 times are sufficient, and often much fewer. Under the present technique of our experimentation, during the first application of a new indifferent agent (the future conditioned stimulus) there results only an orienting reflex, the detection of the motor component of which in the great majority of cases rapidly diminishes to complete disappearance,—so that there is here absolutely nothing out of which this ever-growing sum of determinations of the conditioned effect should form itself, as the author puts it. It is clear that the whole process consists in an ever-growing concentration of stimuli and then, perhaps, in gradually beating a path between connected centers in the central nervous system.

Finally, in regard to the independent acquisition of a conditioned effect from the stimuli in the neighborhood, or near that to which the conditioned reflex is specifically formed, the author is again of a different mind from us. According to us, this is an irradiation of stimulation spreading over a definite part of the cortex. But the author, having taken for granted that for the conditioned stimulus there appears not merely a specific exciting agent but an orientation reflex accompanying it, now explains the matter by saying that all the neighboring agents receive their own activity thanks to one and the same orientation reflex. But this quite contradicts the facts. The neighboring agents in the majority of cases give the conditioned effect directly, without any trace of orientation. But when the orientation reflex exists besides, it is just then (on the contrary) that the conditioned effect either is completely absent or appears very weak; it comes forth and grows only in proportion to the disappearance of the orientation reflex.

And so throughout his article the author remains true to himself, to his own habits of deduction. Making incorrect use of one physiological principle—the fact of conditioning—all the details of conditioned nervous activity which he utilizes for his theme of learning he derives therefrom immediately and constantly, while a whole mass of concrete facts remain without the slightest attention on his part.

II

It seems to me that the second article, 'Basic neural mechanisms in behavior,'[2] to which I turn now, bears to a considerable extent the same tenacious character in the development of its theme as the first. This article presents a paper read by K. S. Lashley at the last International Congress of Psychology in the United States in 1929. Granting that its material is almost exclusively physiological, yet the author's method of treating it is quite that of the preceding article. The material is sacrificed to the fundamental preconceived tendency to demonstrate that the reflex theory 'is now becoming an obstacle rather than a help to progress' in the study of cerebral function, and that, of more strength and significance than the reflex theory, is for example the statement of C. Spearman, that "intelligence is a function of some undifferentiated nervous energy,"—an analogy to the tissue of sponges or hydroids, which, being crushed and sifted through bolting cloth, afterwards when settled out or centrifuged down, forms itself anew into a mature specimen with characteristic structure.

First of all I must state in a general and all-inclusive way, without going into detail for the present, that such a merciless judgment of the reflex theory divorces itself from actual facts absolutely. One may even say, it is somewhat strange that it does not desire to call attention to them. Is it possible that the author ventures to intimate that my 30 years' work, continuing still with success, with many collaborators, proceeding under the guiding influence of the conception of reflexes, presents only a drag on the interpretation of cerebral function? No; no one has the right to say that. We have established a series of important principles of the normal activity of the higher divisions of the brain, defined a series of conditions both of its waking and of its sleeping states; we have made clear the mechanism of normal sleep and hypnosis; we have produced experimentally pathological conditions of this neural level, and found means to bring it back to the normal. The activity of this level, as we have already learned,

[2] K. S. Lashley, Psychol. Rev., 1930, 37, 1–24.

found and is finding in itself quite a number of analogies with the phenomena of our subjective world, as is brought out in statements not infrequently made by neuropsychiatrists, educators, experimental psychologists, and in the assertions of academic psychologists.

Now, before the physiology of this neural level lies a vast horizon, with questions jutting out, absolutely definite problems for further experimentation, in place of very nearly a blind alley, in which this physiology unquestionably found itself a few decades back. And all this thanks to the use of experiments made on this part of the brain under the concept of reflexes.

Of what does the concept of a reflex consist?

The theory of reflex activity finds its support in three fundamental principles of exact scientific investigation: in the first place, the principle of *determinism*, *i.e.*, an impulse, appropriate conditions, or a cause for every given action or effect; secondly, the principle of *analysis and synthesis*, *i.e.*, the initial decomposition of the whole into its parts or units, and then the gradual reconstruction of the whole from these units or elements; finally, in the third place, the principle of *structure*, *i.e.*, the distribution of the activity of force in space, the adaptation of function to structure. Therefore it is impossible that the death sentence for the reflex theory be taken otherwise than as a misunderstanding or bias.

We have before us living organisms, man included, producing a series of activities, manifestations of force. And there is an immediate impression, hard to surmount, of some voluntary freedom of action, of some spontaneity. In the case of man, as an organism, this impression appeals to almost every one as obvious, and an assertion to the contrary seems absurd. Although Leukippus of Miletus [3] announced that there is no motion without cause and that everything arises out of necessity, is it not still being said, even of animal organisms besides man, that spontaneously active forces exist in the organism? And in regard to man, do we not hear even

[3] I take this information from Professor Kannabich's book on History of Psychiatry.

now of freedom of the will, and is there not rooted in the mass of intellects the conviction that we possess something which is not subject to determinism? I have met constantly and am still meeting not a few educated and intelligent people who are in no way able to understand how it could ever be possible to learn all about the behavior (for example) of a dog in a purely objective way,—*i.e.*, by merely comparing the stimuli acting upon the animal with the reactions to them, and therefore not taking into consideration their subjective world, which is supposed to exist analogous to our own. Of course we refer here not to the temporary, let alone immense, difficulty in experimentation, but to an absolute impossibility of complete determinism as a principle. It stands to reason that this same view is held, only with far greater conviction, in regard to man. It would not be a great error on my part if I held it probable that this conviction persists also among psychologists, masked by assertions of the *unique features of psychic phenomena*, under which, disguised by various scientifically decent synonyms, is felt all this dualism and animism immediately shared by a mass of thinking, not to say religiously minded people.

Now, just at its very first appearance, the theory of reflexes constantly increases without cessation the number of phenomena in the organism which are connected with the conditions that determine them; *i.e.*, this theory makes more and more clear the integrated activity in the organism. How can it possibly be an obstacle to the progress of studying the organism in general and the cerebral functions in particular?

Further, the organism consists of a great mass of separate parts and of billions of cellular elements, providing a corresponding quantity of separate phenomena, but closely interwoven among themselves and organized for the integrated work of the organism. The theory of reflexes divides this general activity of the organism into separate activities, connecting them with internal as well as external influences, and then unites them anew, one to another, which brings us to a more and more clear understanding of the total activity of the organism, as well as of the interaction of the organism with surrounding conditions. How has the reflex theory been and at the present time can it be superfluous or irrelevant, since there is still neither sufficient knowledge of the connections of the separate parts of the organism, nor a more complete interpretation of all the relationships of the organism with surrounding conditions? But all internal as well as external relationships in higher organisms are above all accomplished by means of the nervous system.

Finally, if a chemist, analyzing and synthesizing, for the ultimate understanding of the work of the molecule, has to use his own imagination about its invisible structure, if a physicist, similarly analyzing and synthesizing, for a clear idea of the work of the atom, also pictures to himself the structure of the atom,—how is it possible to repudiate the structure of visible masses and take for granted some kind of contradiction between structure and function? The function of the connections, of internal as well as external relationships in the organism, is realized in the nervous system, which represents a visible apparatus. In that apparatus of course spring up the dynamic phenomena, which must be timed exactly to the finest detail of the apparatus.

The theory of reflexes began to investigate the activity of this apparatus with the definition of its special function, naturally of its more simple, grosser parts, and determined the general tendency of the dynamic phenomena arising in it. Here is the general and basic scheme of a reflex: receptor apparatus, afferent nerve, central station (centers), and efferent nerve with the tissue through which it operates. Then came and still comes a detailed elaboration of these parts. Of course a most complex and immense work has been in store and is yet in store for the central nervous stations, and in the parts of the central stations for its grey matter, and in the grey matter for the cortex of the cerebral hemispheres. This work concerns both the visible structure itself and the dynamic phenomena arising in it, while all the time of course the necessary tie between structure and function is not lost sight of. Owing to the difference of method in the study of structure and function, the investigation is naturally divided for the greater part between the histologist and the physiologist. There is not a histological neuropathologist of course who would venture to say that our knowledge of the structure of the nervous system and the special higher division of the central nervous system has been brought to an end; on the contrary he will admit that the structure of these parts still remains in a state of high confusion and darkness. Has not the cytoarchitectonics of the cortex of the hemispheres, though readily scrutinized, been shown only recently to be extremely complex and diverse? And has not all this manifold variety in the organization of the different parts of the cortex been hitherto without definite dynamic meaning? If it is possible for the histologist to analyze the structure as yet only to a small extent, how can the physiologist expect to trace fully the action of the functional phenomena along this inconceivable network? The physiologist, keeping to the reflex scheme, never imagined an investigation of the central stations worked out in detail even to a limited extent in the simplest structure of these centers, but he constantly held to and was guided by the fundamental representation of the fact of transmission, the transfer of a dynamic process from the afferent to the efferent path. As regards the higher centers, besides the possibility of adapting function to the details of structure, he concentrates his attention and his work, for the present from sheer necessity, chiefly on the dynamics, on the general functional properties of the brain. This has been done and is being done till very recently, mainly by the schools of Sherrington, Verworn, and Magnus, and by other individual authors, on the lower levels of the central nervous system; but on the highest levels it is being done for the most part and in the most systematic way right now by me and my co-workers under the guidance of the conditioned reflex variety of the general reflex theory.

In regard to the cortex of the hemispheres, at the beginning of the notable epoch of the 70's of the last century the first indisputable data were obtained about the detailed connections between its function and its structure. Though the existence of special motor tracts in the cortex was confirmed and reconfirmed by all further investigators, the very exact and limited localization of the organs of sense in the cortex, as originally described, soon met with objections on the part of physiologists as well as neurologists. This to some degree shook the doctrine of localization in the cortex. This uncertain state of affairs continued for a long time, owing to the fact that the physiologist did not have his own purely physiological characterization of the normal action of the cortex; and the treatment by psychological conceptions, at a time when psychology had not yet arrived at a natural and universally accepted systematization of its phenomena, was of course unable to assist in further experimentation on the question of localization. The situation changed radically when, thanks to the doctrine of conditioned reflexes, the physiologist at length received the means of viewing with his own eyes the special, though purely physiological, work of the hemispheres and thus was able to distinguish clearly the physiological action of the cortex from the action of adjacent subcortical parts and in general of the lower levels of the brain, in the form of conditioned and unconditioned reflexes. Then all the earlier facts, which had however been broken into, could be brought back into a distinct and strict order, and a fundamental principle of the structure of the hemispheres came out clearly. From the 70's on, the special tracts in the cortex which had been pointed out as centers for the chief external receptors remained the locations of higher synthesis and analysis of corresponding stimuli, but in addition to them representatives of these receptors were acknowledged to be scattered, perhaps throughout the whole cortex, certainly through the greater part of it, but available merely for the more simple and quite elemental syntheses and analyses. A dog without occipital lobes was unable to discriminate one object from another but did discriminate degrees of illumination and simplified forms. A dog without temporal lobes did not discriminate complex sounds such as his own name, etc., but did discriminate exactly separate sounds, for example one tone from another. What striking demonstrations of the fundamental significance of specialized structure!

Of interest as a more specific indication of the functional significance of the structural characteristics of special tracts is the following experiment of Dr. M. I. Elliason, which was reported in my book, 'Conditioned Reflexes, an Investigation of the Physiological Activity of the Cerebral Cortex.' From three tones of a harmonium, two extreme and one intermediate, covering a range of over 3½ octaves, given simultaneously, a complex conditioned food stimulus was provided, which yielded a definite amount of saliva as an index of the intensity of the food reflex. When further tested, the component tones of the complex also separately produced salivary responses, but less than the whole complex, and intermediate tones between these also produced salivary responses, but to a still slighter degree. Then the anterior parts of the temporal lobes (Gyr. Sylviaticus et Ectosylvius) on both sides, with the anterior part of the Gyr. Compositus posterior were excised. The following occurred. When all conditioned reflexes to stimuli from the various analyzers were restored after the operation,—such as the conditioned reflex to the chord (this even before some others),—the reflexes to the component tones of the chord were tested anew. The high tone, as well as the intermediate tone next to it, lost its action. But the middle and the low tones with their intermediate tones retained theirs; the low tone even increased its action, which now became equal to the effect of the whole chord. But when the high tone alone began to be accompanied by food, then it quickly (from the fourth trial on) became again a conditioned food stimulus and acquired a significant effect, not a lesser but a greater one than before. From this experiment one can draw certain exact conclusions: in the first place, that in different centers of the special auditory field of the cortex are represented individual elements of the receptive auditory apparatus; second, that complex stimuli use only this area; and thirdly, that representatives of the same elements of the auditory apparatus scattered through a great part of the brain have no positive rôle at all in these complex stimuli.

When it is seen, as I saw it with conditioned reflexes in hand, that a dog with most of the posterior part of both hemispheres removed orients himself with a high degree of exactness to skin and olfactory receptors, losing only complex visual and auditory relationships with his surroundings, *i.e.*, not differentiating complex visual and auditory stimuli; that a dog without the upper halves of both hemispheres, retaining fully a complex auditory relationship with his surroundings, loses only—with a striking isolation—the ability to orient himself with regard to hard objects met in the environment; and that, finally, a dog lacking almost all the anterior halves of both hemispheres seems to be completely incapacitated, *i.e.*, to be practically deprived of normal locomotion, of normal use of his skeletal movements,—but nevertheless by another indicator, namely, the salivary glands, there is evidence of complex nervous activity;—when all this is seen, is it possible not to be impressed with the paramount significance of the bare structure of the cerebral hemispheres in the fundamental problem of a proper orientation of the organism in its environment,—equilibrating with it? How then can we doubt the further significance of the more detailed features of the structure?

If one were to take the exact standpoint of our author, described further on in detail, he would have to bid the brain histologists throw away their work as unnecessary, useless. Who would not shrink from such a conclusion? Otherwise, all the details of structure which are revealed must sooner or later find their own functional significance. And therefore, along with further histological studies of cortical substance, carried on even more searchingly, it is necessary to pursue pure, rigorous physiological investigations of the activity of the hemispheres and of the adjacent parts of the brain, so that gradually one may be connected with the other, structure with function.

And this is what is accomplished by the theory of conditioned reflexes.

Long ago and firmly, physiology announced a constant connection of definite internal and external stimuli with the explicit activities of the organism in the form of reflexes. The theory of conditioned reflexes indisputably confirmed in physiology the fact of temporary connection between stimuli of all kinds, and not merely the definite ones, external as well as internal, with definite units of activity of the organism, *i.e.*, along with the conduction of nervous processes to the higher centers, it also stated exactly the phenomena of their connection and disconnection. By this addition, of course, no essential change in the conception of a reflex has come to pass. The connection of a definite stimulus with a unit of activity of the organism remains, but without exception under conditions that are exactly defined. That is why this class of reflexes has been given by us the designation of 'conditioned,' to distinguish them from the reflexes which exist inborn; and these older ones are called 'unconditioned.' Thanks to this the investigation of conditioned reflexes rests on the same three principles of the reflex theory: the principles of determinism, of gradual and successive analysis and synthesis, and of structure. For us, the effect is constantly linked with the cause, the whole is further and further divided into parts and then synthesized anew, and function remains connected with structure in so far of course as that is permitted by the data of modern anatomical investigation. Thus there is opened up, so to speak, an unlimited possibility of studying the functions of the higher divisions of the brain, *i.e.*, of the cerebral hemispheres, and of the adjacent subcortex with the most complicated fundamental unconditioned reflexes of the latter.

We successively study the fundamental properties of the cortical substance, define the essential action of the hemispheres, and clear up the connections and interdependence of the hemispheres and the adjacent subcortex.

The fundamental processes of cortical activity are excitation and inhibition, their spread in the cortex in the form of irradiation and concentration, and their mutual induction. The special action of the hemispheres consists in an unceasing analysis and synthesis of stimuli entering (for the most part) from external surroundings as well as from within the organism, after which these impulses are directed to lower centers beginning with the adjacent subcortex and ending with the cells of the anterior horns of the spinal cord.

Thus all the action of the organism occurs under the influence of the cortex in a most exact and most delicate correlation or equilibration with the environment. On the other hand the adjacent subcortex sends a powerful stream of impulses from its centers to the cortex whereby the tonus of the latter is maintained. As the final result the center of gravity of the examination of the higher divisions of the brain is now being transferred to the investigation of the functional phenomena of the hemispheres and adjacent subcortex.

As stated above, the fundamental work of the cortex consists in analysis and synthesis of impulses flowing into the cortex. The variety and number of these stimuli are countless, even in an animal like a dog. The most suitable formulation for expressing this number and variety of stimuli would be to say that for individual stimuli there appear all gradations in the states both of individual cortical cells and of their various combinations. By means of the cortex it is possible to elaborate special stimuli from all stages and varieties of the process of excitation as well as of inhibition, in individual cells as well as in their various combinations. Stimulation from different intensities of one and the same stimulus, the relationship of stimuli, etc., may serve as an example of the first, and as an example of the second, different conditioned stimuli which produce hypnosis.

These countless states of the cells not only take form under the influence of a stimulus that is going on, not only exist at the time of action of the external stimulus, but remain also in the absence of these in the form of a system of different, fluctuating but more or less stable, degrees of excitation and inhibition. To give an illustration of this phenomenon. We apply daily for some time a series of positive conditioned stimuli of different intensity, and of negative stimuli, in one and the same sequence and with the same intervals between all of them,—and we obtain a system of corresponding effects. If, then, during an experiment we repeat after each interval only one of these positive stimuli, then it reproduces just the same fluctuations of the effect that all the successive stimuli together produced in the previous experiments; *i.e.*, the same system present in the state of excitation and inhibition of the cortex will repeat itself.

Of course, it is not permissible to carry out at once any far-reaching correlation between dynamic phenomena and details of structure; but this correlation is by all means admissible, for the structure of the cortex is so variegated throughout its whole extent, and there is the fact, which we already know certainly, that only certain phases of synthesis and analysis of stimuli are admitted to one portion of the cortex and to none other. And this same point is decidedly confirmed by a further finding of ours. From a series of different auditory stimuli (a tone, noise of escaping air, beats of a metronome, bubbling, etc.), or from the mechanical stimulation of different parts of the skin, developed into conditioned stimuli, we can lead a single stimulus to produce an abnormal or pathological effect and in the meantime the others will remain quite normal. We arrive at this result not by a mechanical process but by a functional one,—by bringing a given point of stimulation into a difficult position, or by means of excessive strength of stimulation, or by a severe conflict at that point between the processes of excitation and inhibition. But how can this be interpreted except that excessive functioning, brought about by us on the part of a given minute detail of the structure, had brought it to destruction, as though rough treatment with a very fine instrument had spoiled or destroyed it? How fine and highly specialized these details must be, if other auditory and mechanical stimuli remain completely preserved and untouched! Such isolated destruction could hardly be produced at any time by mechanical or chemical means. After this, one cannot doubt that, if at present we sometimes do not see changes in the behavior of the animal after mechanical destruction of the cortex, this is only due to the fact, which is self-evident, that we have not yet analyzed the behavior of the animal in all its elements, and that the number of these elements must be tremendous. And therefore, a dropping out of some of them naturally escapes our attention.

I have let myself linger so long on our data for the purpose, in the first place, of making further use of them in the criticism of the experiments and the conclusions drawn from them by Lashley, and, secondly, of showing once more how fruitful at the present time is the investigation of the cerebral hemispheres, based on the entire reflex theory with all its principles.

But what does Lashley bring up against the reflex theory? With what does he break it down? [4] First of all it is quite obvious that he conceives it in a peculiar way. Arbitrarily, not reckoning with physiology, he thinks of it all only in terms of structure, without a single word regarding its other principles. It is universally accepted that the notion of the reflex originated with Descartes. But what was known about the detailed structure of the central nervous system, especially in connection with its activity, in the time of Descartes? For the physiologico-anatomical distinction between sensory and motor nerves was not made until the beginning of the 19th century. It is evident that for Descartes the idea of determinism alone formed the essence of the notion of a reflex and from it issued Descartes' conception of the animal organism as a machine. In this sense all later physiologists interpreted the reflex, tying the individual actions of the organism up with the individual stimuli, at the same time gradually bringing to light the elements of nervous structure in the form of different afferent and efferent nerves and in the form of special paths and points (centers) of the central nervous system, until they finally gathered together the characteristic features of the functions of the latter system.

The chief actual grounds upon which Lashley's conclusion regarding the present harmfulness of the reflex theory is advocated and the new mode of conceiving the mechanism of the brain is recommended, are drawn by the author from his own experimental material. This material consists mainly of experiments on white rats which learn the shortest path to the food compartment in a more or less complicated maze. According to the author's experiments, it was shown that the

[4] Simultaneously with the above-mentioned paper read at the Congress of Psychology appeared the monograph entitled Brain mechanism and intelligence by K. S. Lashley, reporting more fully the author's own experimental material; I shall therefore, in my further statements, refer to the address and monograph without making any distinction between them, drawing on facts, conclusions, and discussions therein.

training is more difficult in almost the exact degree, the greater the destruction of the hemispheres in the first place; and aside from this, it is quite immaterial which parts of the hemispheres undergo destruction, *i.e.*, the result is determined solely by the mass of the hemispheres remaining intact. After some additional experiments, the author comes to the conclusion that "specific cortical areas, and association or projection tracts, seem unessential to the performance of such functions, which rather depend upon the total mass of normal tissue." Thus there is asserted to exist an original, but really quite inconceivable situation, that the more complex activities of the apparatus are performed without the mediation of the special parts and chief connections,—in other words, that the whole apparatus works somehow independently of its constituent parts.

And so the main question is: Why does the solution of the maze-problem, which is performed regularly more slowly, depend only on the *extent* of destruction of the hemispheres, regardless of the relationship of the *location* of destruction? And here one regrets that the authoi did not keep in mind the reflex theory with its first principle of determinism. Had he done so, the first question which he would have had to raise regarding the method of his experiments, would have been the following: By what means can the general maze-problem be solved by a rat? It surely cannot be solved without some directing stimulus, without some kind of cue. For if we accept the opposite view, notwithstanding its difficulties, then we would certainly have to show that the task can be actually carried out without any stimuli at all, *i.e.*, it would have been necessary to destroy all the rat's receptors at once. But who has done this and how can it be done? But if, as is natural to suppose, for the solution of the problem signals,—certain stimuli—are essential, then destruction of individual receptors or of some of their combinations is obviously insufficient. Perhaps all or almost all the receptors serve in the response, with a substitution of one for another separately or in some combinations. And for the rat, under the recognized conditions of its life, this is certainly the case. It is not difficult to picture to oneself that in solving the maze-problem the rat can make use of olfactory, and auditory, and visual, and tactile, and kinesthetic stimuli. And since the special centers of these receptors are situated in different places in the hemispheres, and representatives of their single elements very likely exist scattered throughout the whole mass of the hemispheres, there always remains the possibility of their solving the problem, however much of the mass of the hemispheres we have removed,—and the solution is naturally all the more difficult, the less there remains of unimpaired cortical tissue. But if one assume that in the case under consideration the rat uses only a single receptor, or only a few of them, then it is first necessary to demonstrate this by special experiments which leave no doubt, *i.e.*, by letting each sort act separately, or in some combinations, and excluding the others. But no such experiments have been made, either by the author or anyone else, so far as I know.

It appears indeed strange that the author pays no attention whatsoever to all these possibilities and does not put to himself the question: What then appears to be the basis for the rat's action in overcoming the mechanical obstacles; what stimuli, what cues serve for the corresponding movements? He limits himself merely to experiments which involve the destruction of individual receptors separately and in certain combinations, which do not wipe out the habit, and he ends his analysis of the fact of the habit with the statement: "The available evidence seems to justify the conclusion that the most important features of the maze habit are a generalization of direction from the specific turns of the maze and the development of some central organization by which the sense of general direction can be maintained in spite of great variations of posture and of specific direction in running." Indeed —one can say—some kind of bodiless reaction.

As additional experiments by the author relating to the reactions in the maze, various incisions into, under, and across, were made both in the hemispheres and in the spinal cord, for the purpose of excluding altogether the association and projection tracts in the hemispheres and the paths leading to the cord. But we must point out that all these, as physiologists well know, are only rough, approximate methods, and in no way decisive,—the more so, the more complicated the structure. This is much more true in respect to the gross and simple peripheral nervous system. Physiologists well know how difficult it is to completely isolate organs from the nervous connections with the whole body, and often only total excision of an organ from the body gives absolute assurance in this respect. Physiologists are quite familiar with the various crossings and loops, etc., in the peripheral nervous system. Let us recall for example the case of the antidromic sensory fibers (sensation) in the spinal roots and the innervation of a single muscle by fibers from different roots. Then how many times more diverse and more delicate must this, so to speak, mechanical immunity be in the central nervous system under the tremendous elaboration of its existing connections. It seems to me that up to the present, particularly in the physiology of the nervous system, this highly important principle has received insufficient recognition and has not even been formulated clearly and constantly. For the system of the organism developed itself in the midst of all its surrounding conditions: thermal, electrical, bacterial, etc., including also mechanical conditions; and it had to bring all these into equilibrium, to become adapted to them, possibly to anticipate or restrict their action when destructive to itself. In the nervous system and especially in its most complex central part, which rules the whole organism, and unites all the special activities of the organism, this principle of mechanical self-defense, the principle of mechanical immunity, had to arrive at absolute perfection, which it has actually done in the majority of cases. As we are unable at present to claim complete knowledge of all the connections in the central nervous system, all our experiments with incisions, sections, etc., practically appear in many cases to be merely negative, *i.e.*, we do not accomplish the final aim of severing them because the mechanism appears more complicated, so to speak more highly self-regulatory, than we had pictured it. And therefore to draw a decisive and far-reaching conclusion on the basis of such experiments is always risky.

In connection with our first question, I shall turn to the problem of the comparative complexity of habits, which the author was investigating. I shall do this chiefly for the sake of evaluating the methods which he uses. The author finds that the maze habit is more complicated than the habit of discriminating different intensities of illumination. But how is this demonstrated? In fact it is shown that, on the contrary, a habit in the most difficult of the mazes was formed in 19 trials, while the brightness habit was formed in 135 experiments, *i.e.*, seven times less easily. If a comparison be made with the simplest of the 3 mazes used by the author, then the difference in difficulty amounts to about 30 times. In spite of this the author comes to the conclusion that the maze habit is more complex. This is accomplished by means of various explanations, but, in order to carry conviction, he would somehow have to determine exactly, quantitatively, the significance of the several factors suggested in his explanation, showing why all of them taken together not only obscure the actual difference but even transform the result into its opposite.

With such a state of affairs, I would not venture to say what is complex and what simple. Let us come to the point. For the movements of the animal in the maze and in the box with different illumination we take into account only the turning to right or left, not every act of locomotion. In both problems cues or special stimuli are requisite for the turns. They exist in one and the other case. But beyond this a difference appears. In the maze there are several turns, in the box one. Therefore in this respect the maze is more difficult. But there is still another difference. In the maze the signs for turning are distinguished almost exclusively by their *quality*. For example, contact with the openings of the partition during turning occurs now on the right side, now on the left side of the body; in making the turns the muscles work alternately on the right and on the left. And this applies also to visual and auditory signals. In the box it is a matter of *quantitative* difference. These differences must somehow be evaluated. And, of course, the life habits of the rat must interfere, *i.e.*, the more or less early familiarity with one or another problem,—as the author rightly points out. But it is also impossible to disregard the fact that in a very complex maze the problem is greatly facilitated by definite rhythm, by a regular alternation of turns now to right, now to left. On the other hand, in the habit of discriminating intensities of illumination we must take into serious consideration the fact that the formation of this habit arises under the influence of two impulses: food and nocuous stimuli (pain), whereas in the maze only food fixes the habit. And this of course complicates the conditions of training. Still another question: Do two impulses favor or impede the formation of a habit? Moreover, we pointed out above that the formation of a system of effects is a very easy and persistent thing in nervous activity. Thus in both methods, in the maze and in the box, we have as data different existing conditions, so that an exact comparison of the difficulties of the problem becomes almost impossible. All this, together with the uncertainty of the cues in the maze, as we noted above, make the entire method of the author to a considerable extent problematical.

That our author is more inclined to theorize—to draw conclusions—than to make refinements in his own various experiments (which is a fundamental requirement in biological experimentation), can be seen from the following two investigations of his, in connection with these experiments.[5] In one of these papers he investigates the visual habit formed to a given intensity of illumination. Having destroyed in a rat the occipital third of the hemispheres, he finds that the formation of the visual habit does not lessen the speed in comparison with the normal animal. But if that habit be formed in normal animals and thereafter the visual part of the hemispheres be removed, then the habit drops out and has to be formed anew. From this he draws the rather daring conclusion, which is sufficiently hard to conceive, that the process of training in general is independent of the site of injury, while the mnemonic trace or engram has definite localization. But

[5] K. S. Lashley, The relation between cerebral mass, learning, and retention, *J. Comp. Neurol.*, 1926, 41, No. 1. The retention of motor habits after destruction of the so-called motor areas in primates, *Arch. Neur. & Psychiat.*, 1924, 12, 249–276.

the matter is far more simple. In the occipital lobes, as we know, lies the special visual area, to which first of all come the stimuli from the eyes and where they enter into functional connections with one another for the formation of complex visual excitations, and also immediately into conditioned connections with the various activities of the body.

But since the visual fibers extend much further than just within the occipital lobes, probably throughout the whole mass of the hemispheres, then outside these special lobes they serve for the formation of conditioned connections with the various activities of the body, in the form of more or less elementary visual stimuli only. And if Lashley should form a habit not to the intensity of light but to an individual object, then the habit would disappear after removal of the occipital lobes and would not be formed anew. And thus the difference between the place of formation of the habit and the place of the mnemonic path would not appear.

In another research Lashley makes experiments on the motor tract in the cortex in monkeys. The motor habit does not disappear after removal of that tract. From this he draws the conclusion that the tract has no relation to that special habit. But in the first place, in his three experiments, he does not remove the tract entirely; perhaps the parts that remain are still sufficient for a mechanical habit of this given complexity. He sets this probability aside, not by experiment but only by argument. And again, besides the highly specialized motor area determined by electrical stimulation, there is perhaps a less specialized and more diffuse area. Accordingly, on these two grounds a more drastic complication of the mechanical problems is necessary. Finally, why has not the author blinded his animals?—for there is no doubt that in the manifestation of the habit vision has played a rôle, and stimulation in the lower motor apparatus might be effective through the visual cortical fibers as well. We meet striking examples of this in ataxic patients in cases of degeneration of the cord (tabes dorsalis). The ataxic subject can stand on one leg with his eyes open, but falls if the eyes are closed. Consequently in the first situation he replaces kinesthetic paths by visual.

Again a hiatus in the necessary further experimentation under the influence of his favorite negative attitude toward specific localization.

Let us turn now to other experiments and arguments of the author, aimed directly against the reflex theory. As regards the analysis of different adequate stimuli, the author says that there is surely no restriction to certain specific receptor cells which invariably take part in the formation of a habit and its reproduction, and that this is most evident in pattern vision. But in the first place we must see objects, *i.e.*, we receive definite combined visual stimuli with the help of every part of the retina, but not from the entire retina en bloc. And the effect is carried to the projection of the retina in the cortex. This is the reason why there is no definite connection between given receptor cells and a definite reaction. Only when we study an object in detail do we make temporary use of the fovea centralis; usually every part of the retina serves for a similar reaction to the given object. This principle applies also to the projection of the retina in the cortex. Second, as regards identity of response in the case of a geometrical white form on a black background and with the brightness relations reversed, with replacing of geometrical bodies by the corresponding contour outlines, and even with partial outlines,—on the one hand, what has just been mentioned covers this also, and on the other hand, this situation was long ago studied thoroughly, and it means that at first only the most general features of the stimuli act and only later, gradually, under the influence of the special conditions, a further analysis takes place and the more special components of the stimuli begin to act. In the given case, at first only combinations of white and black points without exact mutual relations and spacial distribution act as stimuli. This can be shown by the fact that with further special experiments a white figure on a black background can be differentiated with certainty from a black figure on a white background, *i.e.*, the mutual relationship of black and white will appear as a special stimulus. The same is true also in the replacing of a geometrical figure by a contour outline, etc. All these are but gradual stages of analysis, *i.e.*, only step by step do the more detailed elements of the stimulus become stimuli in themselves.

In the group of reactions, *i.e.*, in the motor mechanism, the author points out that the rat proceeds correctly in the maze notwithstanding that it sometimes runs quickly, or again moves slowly, or even makes circus movements, as in case of injury to the cerebellum. And this appears to him an objection against a definite connection of the stimulus with a definite response. However, the rat moves constantly forward and makes turns now to the left, now to the right, with invariably the same muscles in the cases just indicated, and everything else is an additional movement, conditioned by other additional stimuli. Furthermore in the case of the exclusion of muscles throughout the formation of habits during paralysis and their subsequent use on cure of the paralysis, it is necessary to know where the paralysis lies and why it arose. For we have a huge series of coördinating centers, extending from the end of the spinal cord up to the hemispheres, and impulses from the hemispheres may go to one and all. Further we know that with every thought of movement we actually produce it implicitly. And so a process of innervation can occur, although it does not take visible form. And again, if the stimulation cannot be made effective through the nearest path, by the principles of summation and irradiation it has to pass to the most available centers. Have we not long known of the case where a decapitated frog, in wiping off acid placed on the thigh of one side with the foot of that side, if it is unable to do so because of the amputation of that foot, after a few unsuccessful attempts with the disabled extremity, makes use of the foot on the other side?

The allusion to the absence of stereotyped movements in some forms of activity, for example in the building of nests by birds, is also based on a misapprehension. Individual adaptation exists throughout the whole extent of the animal world. And this, precisely, is the conditioned reflex, the conditioned reaction, which takes shape according to the principle of simultaneous action. Finally, his allusion to the monotony of grammatical forms agrees entirely with our previously adduced fact of the working out of systems in the nervous processes in the hemispheres in action. This is the combination or fusion of structure with function. Granted that we cannot now picture clearly how this comes about,—this is surely only because we do not yet know thoroughly either the structure or the mechanism of the dynamic processes.

I deem it superfluous to dwell further on the author's arguments against the significance of structure in the central nervous system. The features common to all this are the result of his failure to take into consideration at all the complexity of this structure, already known, with its further possibilities; whereas in a prejudiced way he constantly simplifies it to the bare scheme of a physiological textbook, which aims merely to point out an indispensable connection between stimulation and effect—and nothing more.

What then does our author offer in place of the reflex theory which he rejects? Nothing except more remote and altogether unjustified analogies. In seeking a solution of the problem of the higher brain mechanism, can one point to the tissue of fungi or hydroids, or to embryonal tissue, when in the higher part of the brain of higher animals including man we have the acme of differentiation of living matter? In any case, recognizing absolute freedom of hypothesis, we have the right to demand of the author at least a preliminary and elementary program of definite problems for immediate and fruitful experimentation upon this subject,—a program which can be profitably compared with the reflex theory, a program which would necessarily bring about an energetic advance in the problem of cerebral functions. But the author has practically no such program. A real and useful scientific theory must not only embody all existing material, but must also open up a wide possibility of further study and, one can say, of unlimited experimentation.

And such is at present the position of the reflex theory. Who will deny the extreme complexity,—scarcely imaginable by anyone,—of the structure of the central nervous system in its highest types, in the form of the brain of man, and the necessity for a more profound study of it by improved methods? On the other hand, on this very account, the human mind remains overwhelmed by the riddle of its own activity. The reflex theory strives to provide unmistakably a possible ground for both one and the other, and thus to interpret the striking rôle, so difficult to conceive, of this most extraordinary mechanism. The possibility of experimentation on the brain, and especially its higher parts, by means of the reflex theory with its requirements of constant determinism and unremitting analysis and synthesis of the underlying phenomena, is actually without limit. This I have felt and have seen throughout the past thirty years without intermission; and the further I have gone, the greater my conviction.

III

Now that I appear in the psychological literature for the first time, it seems to me a fitting opportunity on the one hand to consider a few tendencies of psychology, which in my opinion do not accord with the aims of successful investigation, and on the other, to emphasize more sharply my viewpoint on this our common field of work.

I am an empirical psychologist and I know psychological literature only through a few leading psychological texts and, compared with the available material, through an altogether inadequate number of psychological articles which I have read. But from the time when I became really conscious of life, I have been and am still a constant observer and analyzer of myself and others in the range of life that is accessible to me, counting in also the best literature and genre painting. I reject point blank and have a strong dislike for any theory which claims a complete inclusion of all that makes up our subjective world, but I am unable to give up analyzing it or interpreting it simply, in its individual points. And this interpretation must result in bringing into accord these individual phenomena and the data of our modern positive knowledge in the natural sciences. And for this it is necessary to endeavor constantly to apply these data to every individual phenomenon in the most elaborate way. And I am now convinced of this: that a purely physiological interpretation of much of what was formerly termed psychic activity has reached firm ground, and with the analysis of the behavior of the higher animals up to and including man, it has the right to make every effort to interpret the phenomena in a purely physiological way, on the basis of established physiological processes. In the meantime it is clear to me that many psychologists jealously, so to speak, guard the behavior of animals and man from such physiological explanations, constantly ignoring them and not attempting to apply any of them to any extent.

In confirmation of the statements just made, I take two very simple cases: one mine and the other Professor Köhler's. One could present many others, some much more complex.

When we were working out a method of feeding an animal from a distance at the time of the experiment, we tried out many different methods. This among others: In front of the dog there was always an empty pan, to which a metal tube led down from a container above, which held the dried meat-powder and usually served to provide the food for our animals at the time of the experiment. At the junction of the container and the tube was a valve, which was opened at the proper moment by means of air transmission, so that a portion of the powder dropped down the tube and came out into the pan where it was eaten by the animal. The valve was not in good working order and if the pipe were shaken some of the powder from the container would drop into the pan. The dog quickly learned to make use of this, of his own accord shaking out the powder. And a shaking of the pipe took place almost continuously, when the dog was eating the portion of food which was given it and in doing so knocked up against the pipe. Of course this is exactly what takes place in training a dog to give one his paw. In our laboratory work, the conditions of life have in general done the teaching, but here, man forms part of the conditions. In the latter case the word 'paw,' 'give,' etc., the skin stimulation from the contact in lifting the paw, the kinesthetic stimulation accompanying the lifting of the paw, and finally the visual stimulation from the trainer, were accompanied by food, *i.e.*, were bound to the unconditioned stimulus for food. It is absolutely the same in the instance cited: the noise of the shaking pipe, the skin stimulation from contact with the pipe, kinesthetic stimulation in jostling against the pipe, and finally the sight of the pipe—all these became similarly connected with the act of eating, with excitation of the feeding center. This of course occurred through the principle of simultaneous association, presenting thereby a conditioned reflex. And here, moreover, two additional distinctly physiological facts appear· In the first place, the definite kinesthetic stimulation in this case is probably linked up by a conditioned setting (in the lower parts of the central nervous system by an unconditioned setting) with the execution of those movements or the activity which produced it,—this kinesthetic stimulation. And second, when two centers in the nervous system are connected or joined, nervous impulses are set in motion and pass from one to the other in both directions. If we accept the absolute law of one-way conduction of nervous impulses in all points of the nervous system, then in the case cited one must assume an additional connection in the opposite direction between these centers, *i.e.*, one must grant the existence of an additional neurone connecting them. When food is given on raising the paw, a stimulus undoubtedly runs from the kinesthetic center to the feeding center. But when the connection is established, and the dog, under the urge for food, gives his paw himself, obviously stimulation runs in the opposite direction. I can interpret this fact in no other way. Why this is merely simple association, as psychologists usually assume, and by no means not an act of intelligence, of ingenuity,—even if of elementary things,—remains unclear to me.

The other example I take from W. Köhler's book, 'Intelligenzprüfungen an Menschenaffen,' also referring to dogs. A dog is placed in a large cage situated in an open space. Two opposite walls of the cage are solid, through which nothing is seen. Of the other two walls one is a screen, through which clear open space can be seen; the other (opposite) has an open door. The dog stands in the cage behind the screen, and *at some distance* in front of the screen a piece of meat is placed. As soon as the dog sees it, he turns around and goes through the door, around the cage and takes the meat. But if the meat lies *close* in front of the screen, then the dog vainly pushes up against the screen, trying to get the meat through the screen, and does not use the door. What does this mean? Köhler does not attempt to decide that question. With conditioned reflexes at our disposal we understand the matter easily. Meat lying near at hand strongly stimulates the olfactory center of the dog and that center, by the principle of negative induction, strongly inhibits the rest of the analyzers, the other parts of the hemispheres, and thus the track to the door and the roundabout way remain under inhibition, *i.e.*, subjectively expressed, the dog has temporarily forgotten them. In the first case, in the absence of a strong olfactory stimulation, this trace remains under little or no inhibition and leads the dog more correctly to its goal. At all events, this explanation underlies the matter and harmonizes with further exact experimental proof. In confirmation, his experiment would reproduce the mechanism of revery, of strong concentrated thinking about something, when we do not see or hear what is going on around us, or, somewhat similarly, it reproduces the mechanism of what is termed blindness under the influence of passion.

I am certain that with persistent experimentation many other and more complicated instances in the behavior of animals and man would also prove interpretable from the point of view of many established rules of higher nervous activity.

The second point which I shall take up relates to the question of the significance of the aim and purpose of psychological investigation. It seems to me that on this point there invariably arises a confusion of different things. Before us is the sublime fact of the evolution of nature from the primordial state in the form of nebulae in space, all the way up to human beings on our planet,—in the form, to put it roughly, of phases: of the solar and planetary systems, of the inanimate and animate part of nature on the earth. In living matter we see especially strikingly the phases of evolution in the form of phylogeny and ontogeny. We still do not know, and probably will not know for a long time to come, either the general law of evolution, or all its successive phases. But seeing its manifestations, we anthropomorphically, subjectively, both in general, and in particular, replace the knowledge of the law with the words 'aim,' 'purpose,' *i.e.*, we merely repeat the fact, adding nothing to our present knowledge of it. But in seeking out the truth concerning the separate systems of which nature consists, up to and including man, it all boils down to a mere statement of the internal as well as the external conditions of existence for these systems,—in other words, to the study of their mechanism; and thrusting into this experimentation the idea of purpose in general simply results in a medley of different things and becomes a hindrance to lines of investigation that are accessible to us and that are immediately fruitful. The idea of a possible goal in each system can serve only as a help to our study or the use of scientific imagination for the sake of suggesting new questions and a variety of experiments,—just as when we seek to gain familiarity with a machine of which we are still ignorant, and which is the work of human hands;—but it is not the final aim.

With this is naturally connected the next question,—the question of freedom of the will.

This question is of course one of the greatest practical importance. But it seems to me there is a possibility of discussing it, both scientifically (on the basis of contemporary exact natural science), and at the same time not inconsistently with the feeling regarding it that is common to all men, and without involving confusion in its essential formulation.

Man is of course a system,—roughly speaking, a machine,—like every other system in nature subject to the unescapable and uniform laws of all nature; but the human system, in the horizon of our contemporary scientific view, is unique in being most highly self-regulatory. Among the products of man's hands, we are already familiar with machines which regulate themselves in various ways. From this standpoint the method of investigating the system of man is precisely the same as that of any other system; decomposition into parts, study of the significance of each part, study of the connections of the parts, study of the relations with the environment, and finally the interpretation on this basis of its general workings and administration, if this be within the capacity of man. But our system is self-regulatory in the highest degree,—self-maintaining, repairing, readjusting, and even improving. The chief, strongest, and ever-present impression received from the study of the higher nervous activity by our method, is the extreme plasticity of this activity, its immense possibilities: nothing remains stationary, unyielding; and everything could always be attained, all could be changed for the better, were only the appropriate conditions realized.

The system or machine—and man with his host of ideals, aspirations, and achievements—what a terrifying, discordant juxtaposition this seems at first glance. But is it really so? For according to the view of evolution, is not man at the summit of nature, the supreme personification of the resources of a nature which is without limit, the realization of its powerful but still unknown laws? Is not this sufficient to maintain the dignity of man, to fill him with highest satisfaction? And there still remains in life all that is also embraced in the idea of freedom of will with its personal, social, and civic responsibility; for me there remains this possibility, and hence also the obligation for me to know myself, and constantly, using this information, to maintain myself at the utmost height of my capabilities. Are not the social and civic duties and requirements, situations which present themselves to my system, and which must lead to appropriate reactions that will promote the integrity and perfection of the system? [6]

[6] I am greatly indebted to Dr. R. S. Lyman, who kindly assumed the difficult task of this translation; he has executed it with special care, both in regard to the subject matter of the article and to the particular style of the Russian text.

[MS. received September 14, 1931]