CHAPTER XXI

THE STUDY OF THE HIGHER NERVOUS ACTIVITY

(Read at the International Congress of Physiologists in Gröningen, 1913.)

DISCOVERY OF THE MOTOR AREA IN 1870—PSYCHOLOGICAL CONCEPTIONS ARE USELESS FOR OBJECTIVE INVESTIGATION—THE NEW REFLEXES—NERVOUS ACTIVITY IS AN ANALYSIS OF THE INTERNAL AND EXTERNAL WORLDS—THE TWO MECHANISMS—DESTRUCTIVE (PAIN) STIMULUS ORDINARILY PROVOKING DEFENCE MAY BE MADE CONDITIONED STIMULUS FOR FOOD REACTION—EXCITATION FLOWS TOWARD THE STRONGEST CENTRE—CONDITIONED REFLEX MAY FORM ACCIDENTAL COMPONENTS OF THE CONDITIONED STIMULUS—THREE FORMS OF INHIBITION (SLEEP, EXTERNAL, AND INTERNAL)—FORMS OF INTERNAL INHIBITION (EXTINCTION, RETARDATION, CONDITIONED INHIBITION, DIFFERENTIATION, DIS-INHIBITION)—EXPERIMENT ILLUSTRATING CONCENTRATION AND IRRADIATION—HYPNOTISM AND SLEEP—TIME AS A STIMULUS—AFTER-EFFECT INHIBITION—EXPERIMENTS WITH SKIN ANALYSER—DESTRUCTION OF POSTERIOR HALF OF HEMISPHERES AND ANALYSERS—THE WATER REFLEX—FAILURE OF PSYCHOLOGY.

THROUGH the researches of Fritsch and Hitzig the year 1870 was made a famous epoch in the physiology of the central nervous system. The investigations of these authors became the starting point for a mass of important physiological experiments on the hemispheres of the brain. The results of this work have been applied in a striking manner to the diagnosis and therapy of diseases connected with cerebral affections. And why was this possible? It was, I think, because the facts are those of pure physiology—they fall within the limits of physiological conceptions. This circumstance must be emphasised, and it will be the standard for future enquiry into the physiology of the brain, which has only just begun.

Although investigation of the so-called "motor area" of the hemispheres is to be counted as a triumph of science, it represents only an episode in the physiology of the brain. The results of experiments on the sensory centres are not so exact. Undoubtedly the investigation of the cerebral hemispheres is a stupendous task lying before the physiologist. Sooner or later, we must comprehend and analyse completely this part of the central nervous system from the purely physiological point of view. With the exception of the facts discovered by Fritsch and Hitzig and some hints regarding the sensory centres, the functions of this system are even at present considered as "psychical," and psychical phenomena belong to a subject quite apart from the science of physiology. Probably this is the reason why the physiology of the higher nervous activity does not advance in proportion to what we

might expect from the intensely interesting and abundant material which it gives us.

It is one thing if physiology accepts the knowledge of sciences which are more exact than physiology itself; it is an entirely different matter to borrow notions from a discipline which, it must be admitted, has not reached the grade of an exact science, from a discipline whose representatives challenge one another to agree unanimously concerning its general postulates, its common problems, and its unquestionably fruitful methods. Thus the physiologist deciding to study the activity of the brain stands before a dilemma. Either he must wait until psychology decomposes its phenomena into elements and classifies them, *i.e.*, until it becomes an exact science? Not until that time can physiology use psychological data for the examination of the highly complicated functions of the brain. I can not understand how the present conceptions of psychology, which have *no relation to space,* can be fitted into a material structure such as the brain. Or—the other horn of the dilemma—the physiologist must try to follow a path entirely independent of psychology, and search for the fundamental mechanisms of the higher nervous activity of the animal and gradually systematise them; in short, he must remain a pure physiologist. It seems to me that there can not be much doubt in such a choice. If he accepts the psychological method it means that he must for an indefinite time reject the investigation of a highly interesting part of the animal organism. Consequently there remains to him only the second course. And I dare to think that there are serious and impelling reasons why he should take this course, and that it is not only promising but that its success is assured.

We all know what control of nervous phenomena, what an inexhaustible store of knowledge, physiology has derived from the first and fundamental conception of the function of the nervous system, the conception of the so-called reflex. Thanks to this point of view, there was acquired from a formerly mysterious region an enormous realm for scientific research. This conception established regulated relations between a great multitude of reactions of the animal organism and the phenomena of its own internal condition and those of its environment.

The time has come, gentlemen, to add something to this old notion of the reflexes, to admit that, parallel with this elementary function of the nervous system to repeat pre-formed reflexes, there exists another elementary function—the formation of new reflexes. If in machines made by human hand there are present certain conditions by which may arise new and appropriate combinations of the mechanical parts, why should one deny this elementary property to the nervous system—the most perfect of all regulators in the most complicated of all con-

structions? The reason is not because of lack of scientific facts or of a formula—both of these have been known for a long time—but what fails is the general acceptance and systematic application of this formula in the study of the higher parts of the nervous system. The phenomenon which we consider is perfectly clear: it is the property of living substance to adjust itself, or, as I prefer to say, to come continuously into equilibrium with the external world, *i.e.*, in the interest of the integrity and welfare of the given system of living substance, to enter into connections with new conditions—in other words, to respond to formerly indifferent agents by a definite activity.

This closing of new connections of the organism with certain external phenomena is clearly exhibited before us in the higher animals. Their life is the history of a constant and incessant formation and exercise of these new combinations. Details and parts of natural phenomena, which may have been without any significance for the activity of the organism, become transformed in a short time into powerful stimuli of the most vital functions. I and my collaborators have accompanied the feeding of the dog, or the introduction of acid into his mouth, with the action of various accessory agents, and have unfailingly obtained from the most diverse stimulators imaginable, a salivary secretion as a part of the general reaction of the animal to food or to acid. What is this? It is undoubtedly an answer to the phenomena of the external world, an answer effected through the nervous system; it is a reflex, not a stereotyped one, but a reflex which is formed before your eyes. If in the term "reflex" you include not only the reaction of the organism to a stimulation mediated through the nervous system, but the conception of a strict regulation of this reaction, you must only acknowledge—and it is obligatory for the biologist so to do—that the new connection (conditioned reflex) formed before your eyes is not accidental but a regular occurrence, and must recognise that the word "reflex" also fits this case.

What objection may there be for the introduction of a purely physiological formula for the newly arising reflex? It seems to me to be the following: In consequence of an unconscious or conscious analogy with our own internal world, we doubt the elementary character of this fact (of the new-formed reflex) and therefore do not accept a complete determinism in the formation of new reflexes. We judge from our subjective states, and fancy that in order to form the new connections, very complex processes and even quite peculiar forces are at work. But have we a right to do so?

In the lower as well as in the higher animals we have a mass of instances in which it is clear that the new conditioned stimuli of these reflexes act as directly as the old (unconditioned) ones. In our experi-

ments with these new reflexes formed to food or acid, using a stimulus of the eye analyser, we have seen that the same reflex was produced by the conditioned stimulus as by the food or the acid. In any event the alleged uncontrollable complexity of the new reflexes is not proven. But the converse is true. From the fact that under definite conditions these reflexes always appear, it is necessary to conclude that their formation is an elementary and comprehensible process. The relations of the newly formed reflex are another matter. These are, even in animals, highly intricate. A great mass of various stimuli act upon this reflex incessantly. Thus the complexity of the newly formed reflex consists not in the mechanism of its formation, as this is elementary, but in the extraordinary dependence of the reflex on the phenomena of the internal condition of the organism, as well as on the phenomena of the surrounding outer world.

I pass over now to the second fundamental mechanism of the highest parts of the central nervous system. Every living creature responds by its activity to certain phenomena of its external and internal worlds, decomposing them and choosing special components. The higher the animal stands on the zoological ladder, the more varied appear the units presented to it by the world, and the greater the number of separate phenomena which call out its general activity. A lower organism is wholly an analyser, and a relatively simple one. In the higher animals, an essential part of their well-developed nervous systems plays the rôle of specific analysers, performing functions comparable to our physical and chemical analysers in the laboratory. The finest analysis is a basic function of the most developed part of the nervous system. . . .

The activity of the analyser is in close relation to the mechanism of the formation of new reflexes, a mechanism which may bring into connection with the activity of the organism only that component which can be isolated by the analyser. And on the contrary, there is no doubt that every phenomenon, even the most insignificant, once it is isolated by the analyser of the given animal, can and must, sooner or later, under the corresponding conditions, become a special stimulator of this or that activity of this animal.

Thus the mechanism of the formation of the new reflexes gives a complete possibility of examining the activity of the analysers. This activity in higher animals is uninterruptedly at work, just as is the process of the formation of new reflexes. In the present imperfect knowledge of this activity we can hardly guess its far-reaching significance in the life of the animal, and what we consider as very complicated processes is only the most delicate and exact analysis. The real need is for a systematic study of the activity of the analyser. First of all, we must state what the analysers of the given animal isolate as units of

the external world. By this I mean all the qualities of the stimuli, all their intensities, their limits, and their combinations. Then we must study those basic rules which govern the analysis. Partial destruction of the peripheral or of the cerebral end of the analyser should make us gradually acquainted with the separate details of the analysers; and only from the combined activity of these parts will there finally be evident all the functions of the analysers as they are performed in the animal.

Our dozen years of persistent investigation have been devoted to the action of these *two mechanisms:* the mechanism of the formation of new reflexes and the mechanism of the analysers. Based on our recent results I shall now once more endeavour to systematise our facts. Obviously I can do this only in outline, and will devote a little time to the discussion of only the chief results.

First, I shall make two preliminary remarks. The newly originating reflexes I designate as *"conditioned,"* in contradistinction to the usual reflex which I call *"unconditioned,"* in order to emphasise an objective feature—their dependence upon a multitude of conditions. But the essence of the matter, of course, does not lie in the name. Other similar terms may be used—temporary and constant, acquired and inborn.

I and my collaborators investigate the conditioned reflex almost exclusively on the salivary gland, and, as mentioned before, this is because its activity is immediately directed toward the external world (in the form of food or other substances put into the mouth), it has relatively few inner connections, and it functions alone, independently, and not as every skeletal muscle which operates only in a complicated system.

Now for our system of facts. The essential condition for the formation of the conditioned salivary reflex is, as we have said, the combination of feeding of the dog or introduction of acid into his mouth with the action of some indifferent stimulus. After several such repetitions, the formerly indifferent stimulus applied alone calls out the flow of saliva. A new reflex has been formed. The previously indifferent stimulus has opened a path to a certain area of the central nervous system. There is a coupling, or linking, of the excitation process with a new point.

A conditioned reflex can be made not only from an indifferent stimulus, but from a stimulus which may be firmly connected with a certain centre, even though it is an inborn connection. A striking instance of this occurs with *destructive* stimuli, or, according to the psychological terminology, *pain* stimuli. Their usual result, their constant reflex is ***defence,*** the struggle of the muscular system against the stimulating

agent, its annihilation. Systematically repeating the feeding of the dog, *viz.*, the excitation of his food centre, combined with an electrical stimulation of the skin, even though it is injured, will lead without great difficulty to an entire cessation of the defence reaction and to its substitution by the food reflex, *i.e.*, the corresponding movements, secretion of saliva, etc. Now you may cut, burn or in any way destroy the skin, but instead of the defence reaction you see only the signs of the food reflex, or, subjectively speaking, of a strong appetite—the dog turns toward the experimenter, makes licking motions, and the saliva flows. This fact has been so often demonstrated in my laboratory that it may be accepted without question. What is its meaning?

How can this be otherwise represented than that a nervous excitation which formerly followed one path in the brain has now gone along another way and so reaches another region? Thus, the course of the stimulation has been switched from one track to another. Before us is an evident fact that in the highest parts of the central nervous system an incoming excitation is under certain conditions switched in one or another direction. This fact probably constitutes an essential property of the highest parts of the central nervous system.

This is apparently what takes place in the formation of conditioned reflexes from all indifferent agents. The presence of a definite circumstance (the simultaneous appearance of activity in the form of an unconditioned reflex or of another well elaborated conditioned reflex) causes the indifferent stimulation, which would have spread indefinitely over the brain mass, to be drawn to a certain point and to open a path thereto. Now arises the interesting question, what determines the *course* of the excitation along this or that path? Judging from our results, the deciding factor is the relative physiological strength of the given centres, or the degree of their irritability.

The following facts might be understood in this way. As mentioned above, there is no difficulty in making the destructive skin stimulus a conditioned exciter of the food reaction. However, notwithstanding our persistent trials we have never succeeded in forming a conditioned stimulus in undisputed form from the electrical stimulation of that part of the skin lying directly over bones, *i.e.*, from destructive stimulation applied close to bones. Likewise, we have not been able to make any destructive stimulus of the skin a conditioned stimulus for the reaction to acid (½% HCL put into the mouth). Generally speaking, it may be said that the centre for the destructive stimulation of the bone is relatively stronger than the centre for food stimulation, and that the centre for food stimulation is stronger than that for acid stimulation. If this is so, then *the stimulation is directed toward the stronger centre.*

Then there follows a number of other circumstances affecting the formation of conditioned reflexes. The most important of these is the fact that the conditioned stimulus must precede by at least two or three seconds the unconditioned stimulus by means of which it is formed. If you begin the experiment by feeding or by pouring acid into the mouth, and only after this apply the agent from which you intend to make a conditioned stimulus, although no more than three to five seconds have elapsed since the beginning of the feeding, you create by this order of procedure a great obstacle to the building up of the conditioned reflex. Why is this?

The following conception of the mechanism of this relation agrees completely, I think, with recognised properties of the central nervous system. The unconditioned stimulus produces a focus of strong excitation in a certain part of the cerebral hemispheres which leads to a considerable decrease of irritability in the other parts. Therefore a new impulse arriving in these parts of the brain is below the threshold of stimulation, or meets a hindrance to its spreading over the hemispheres. Only in a free and indifferent state of the hemispheres, so to speak, can the new stimulus prove effective and have the possibility of forming connections with successively and strongly excited places in the hemispheres.

The strict isolation of those stimuli with which the conditioned reflex is to be formed is, obviously, of prime importance. If, together with your chosen agent, there occurs some apparently insignificant stimulus, even unnoticed by you but perhaps absolutely and relatively of greater physiological strength than your agent, then the conditioned reflex is formed not with your agent, but with that accessory stimulus which you have not taken into consideration. With many experimenters it happens that at the start or even during the entire research, the conditioned reflex is formed only upon the experimenter himself, upon his movements or the noises which he makes and which precede the feeding or the introduction of acid. That is the reason why in my old laboratory all observations from some collaborators were made from the outside of the experimental chamber. In my new laboratory there is not only isolation of the animal from the experimenter, but from all vibrations, sights, and sounds of the outside.

I shall not discuss other less important circumstances which affect the speed of formation of the conditioned reflex, nor the different kinds of these reflexes and their properties, but shall proceed to the other great division of the physiology of the conditioned reflexes.

The elaborated conditioned reflexes, as mentioned previously, are highly sensitive, and therefore under ordinary conditions of life they incessantly fluctuate in degree and even decrease to zero. I cannot but

see here a convincing justification of our methods of investigation. As sensitive as the extent of the conditioned reflex is, the experimenter at present still has control over it. The changes in size of the conditioned reflex occurs in both directions. We have made a special and thorough study of the negative (decreasing) variation of the conditioned reflexes, which we consider as phenomena belonging to the generally accepted physiological conception of inhibition. The facts compel us to recognise three separate forms of *inhibition—sleep, external,* and *internal.*[1]

First there is the weakening and entire vanishing of all conditioned reflexes when the animal becomes drowsy or sleeps. Concerning this there are many interesting details which I shall not discuss.

The second kind of inhibition we call external. It is completely analogous to what we have known for a long time in the physiology of the spinal cord. It results from various stimuli, either from the outer world or from the internal *milieu* of the organism, calling out other conditioned reflexes or other nervous activity.[2]

The third and particularly interesting form is internal inhibition. This inhibition develops in consequence of special relations between the conditioned stimulus and the unconditioned stimulus by means of which the conditioned reflex is elaborated. Always when a previously well developed and active conditioned stimulus is temporarily or constantly (constantly, however, only under certain conditions) not followed by its unconditioned stimulus, inhibition develops. We have investigated several sorts of this inhibition: *extinction,* when the conditioned reflex is repeated several times at short intervals without being accompanied, or, as we say, reinforced, by its unconditioned stimulus; *retardation,* when during the elaboration of the conditioned reflex, there is a lapse of some time (one to three minutes) between the beginning of the action of the conditioned stimulus and the beginning of the unconditioned stimulus; *conditioned inhibition,* when the elaborated conditioned stimulus in combination with another indifferent agent is systematically not followed by the unconditioned stimulus; and finally *differentiated inhibition,* whereby we mean that agents, which are akin to the conditioned stimulus and which formerly produced an effect similar to that of the conditioned stimulus, become inactive when they are repeated without the unconditioned stimulus (for example, feeding), the conditioned stimulus itself being always accompanied by the unconditioned stimulus. That there is really an inhibitory process created in all these cases is proved by this, that it is always possible to remove immediately the inhibition and to obtain a more or less full effect of the conditioned stimulus. This possibility is given by any additional agent

[1] See footnote 1, chapter xx.—*Translator.*

[2] See previous chapter.—*Translator.*

of average strength which provokes the orienting reaction (looking, listening, etc.) of the animal, and also by some other stimuli. This peculiar phenomenon,—an easily reproducible fact—we call *dis-inhibition* of the conditioned reflexes.

In order to have under control the above-mentioned phenomena one must take into consideration the *latent after effects* of the stimulation. There arise numerous questions concerning the duration of these after effects. It is sufficient to say that in our experiments with various stimuli and under diverse conditions, however, with a certain definiteness in the order of the experiments, the after effect can last from some seconds to some days. It may be categorically asserted that such questions are subject to an exact investigation by our method of experimentation.

Now I return to the movement of the nervous processes in the mass of the cerebral hemispheres. Associated with the fact that the nervous excitation arriving in the hemispheres is led into one or another direction is the phenomenon that the nervous process spreads and floods, as it were, the hemispheres in all directions. I shall illustrate this by the following example. We have before us an animal which has probably been a watch-dog, as he attacks strangers, and besides he is nervous and excitable. If the person who habitually experiments with him sits in the room, he remains tranquil. In the presence of this experimenter conditioned reflexes and inhibition can be easily formed. If a stranger, however, comes into the experimental room the dog begins to bark, and if this person makes a threatening gesture or strikes the dog, the aggressive reaction of the animal reaches a high degree. . . .[3]

In connection with analogous experiments on the effect of different food reflexes upon one another, and on the interaction of cold and warmth reflexes, and with other observations, and finally in connection with the fact known since 1870 that under continuous electrical stimulation of separate points of the motor region of the hemispheres general epileptic convulsions ensue,—in connection with all these facts our experiments confirm the fact of the *dispersion* of the excitation from its original site as a fundamental phenomenon of the activity of the hemispheres. At the same time we see in our experiments quite the opposite phenomenon—the gathering, the *concentration* of the excitation about its original point, as the second phase of the entire process.

This relation manifests itself in a specially demonstrable and convincing form in that nervous process which we call *internal inhibition.* Although the fact has been described in a recent publication in French (*see* chapter xix), I shall review it briefly in order to bring it into our system. We arrange along the hind leg of a dog a series of apparatuses for the mechanical irritation of the skin and we make these irritations

[3] See chapters x and xxv for full description of this experiment.—*Translator.*

conditioned stimuli of the food reaction, but the lowest of these apparatuses we differentiate, we make it a negative conditioned stimulus, not accompanying its action with feeding; in such a case one can see how the inhibitory process arising from the action of the lower apparatus at first irradiates to all the higher apparatuses (*i.e.*, to the corresponding brain areas), and then gradually concentrates around the initial point.

During our investigation of the conditioned reflexes there has arisen the question of *hypnotism* and *sleep.* At first only in sporadic cases, but now more often, we can observe in all our dogs during the investigation of the conditioned reflexes, the following unexpected fact. If the conditioned stimulus always begins a half minute or several minutes (from one to three minutes) before the unconditioned is joined to it, then there develops, as previously mentioned, a retardation of the effect of the conditioned reflex, *i.e.,* the appearance of the conditioned reflex shifts farther and farther away from the time when the conditioned stimulus starts, toward the time when the unconditioned stimulus is applied. This period during which the conditioned stimulus has no effect, *i.e.,* the interval from the beginning of the conditioned stimulus to the beginning of its effect is charged, so to speak, with the process of internal inhibition.

But the matter does not end here. Gradually the effect of the conditioned stimulus, which was more and more delayed, disappears altogether, during the period of its isolated adaptation. It can, however, again be made manifest if the setting in of the unconditioned stimulus is delayed a little more; then you see the action of the conditioned stimulus during the last added seconds. But finally the conditioned stimulus becomes utterly ineffective. At the same time a kind of cataleptic state develops in the animal (he appears indifferent to external stimuli and becomes fixed in a certain active pose); or, and this occurs oftener, irresistible sleep follows with complete relaxation of the skeletal muscles. The speed of development and the intensity of the phenomenon depend upon certain conditions—upon the absolute strength of the conditioned stimulus, upon the interval of time between the beginning of the conditioned stimulus and the unconditioned stimulus, and the number of repetitions of the delayed conditioned reflex. The individuality of the animal has considerable influence. Sleep and the cataleptic state will disappear if the unconditioned stimulus closely follows the conditioned stimulus (three to five seconds). One can hardly fail to see that these phenomena are intimately connected with the nature of hypnotism and natural sleep. To these phenomena I shall refer afterwards when I shall speak of the experiments with extirpation of parts of the cerebral hemispheres.

To conclude this aspect of the conditioned reflexes, I should like to remind you that *time,* as such, proves to be also a stimulus; we may

make a conditioned stimulus of it, and investigate it with regard to differentiation, inhibition, and dis-inhibition.[4] I am convinced that directly along this path of exact experimentation lies the solution of the problem of time, which has occupied philosophers for countless generations.

I shall now briefly touch upon the data which we have accumulated in studying the activity of the analysers; for we have elaborated and added to our previously discovered facts. We are continuing to investigate further those properties and intensities of stimuli which can be isolated by the various analysers of the animal. Also we have collected more and more facts in order to confirm the generality of our basic law according to which analysis is carried out, *viz.*, that during the application of agents acting as conditioned stimuli, at first a larger and less special part of the analyser enters into the conditioned connection, and only later, with the repetition of the exact conditioned stimulus, always followed by the unconditioned stimulus does the conditioned stimulus become more and more specialised, *i.e.*, does it correspond to the smallest part of the analyser. In determining the degree of exactness and the limits of the work of the analysers, we have been handicapped by the imperfection of our instruments.

A specially detailed study has been made of that inhibitory process by which the differentiation of a given stimulus is brought about, *viz.*, by which the stimuli neighbouring and similar to the one chosen, having at first the same action as the chosen stimulus, gradually become ineffective. This process of the differential inhibition is easily accessible to the investigator in the form of *after-effect inhibition, i.e.*, as that inhibition which remains in the nervous system after the application of the differentiated ineffective stimuli. The higher the degree of differentiation, the stronger the succeeding inhibition. A new differentiation inhibits more strongly than a completely elaborated one. The better the differentiation is elaborated, the shorter the duration of the succeeding inhibition. If in the course of one and the same experiment the differentiated ineffective agent is repeated several times in succession, its inhibitory *after-*

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[4] These experiments were carried out in two forms. A dog was fed in the experimental room at regular intervals, say every 30 minutes. If now one feeding was delayed it could be noticed that at the 30th minute or sometimes a little later the food reaction began. Here the time *per se*, since the last feeding, is said to be the conditioned stimulus. In the other form the time is only a component in a sum of conditioned stimuli; for example, if a conditioned reflex is formed from a certain stimulus, say from a metronome, and exactly the same interval is used between all successive feedings, it can be seen that the new conditioned reflex when tested at this interval is always stronger than it is if tried at some other interval. Time is supposed to be here part of the sum from which the conditioned stimulus has been established. These phenomena give us the right to say that a period of time may also be considered as a conditioned stimulus.—*Translator.*

effect can be reinforced and summated. Dis-inhibition may be operative in the case of the differentiated stimulus as well as in its inhibitory *after-effect,* etc.

Now that we have become acquainted with the highest nervous functions as the work chiefly of two mechanisms—the mechanism of the conditioned reflexes and the mechanism of the analysers—we desire to know what is the effect on these functions of the partial or complete removal of that structure, which, as we have assumed, conditions the higher nervous activity. Owing to the lack of time, I shall dwell on only a few examples.

Especially marked were the results of our experiments with the skin analyser. If you have made conditioned stimuli out of the mechanical irritation of various spots on the surface of the skin—and this can be easily done, because at first every conditioned stimulus is generalised—and if you then extirpate certain parts of the frontal lobes of the cerebral hemispheres (gyri coronarius and ectosylvius), the conditioned reflexes from a definite section of the skin surface, within strictly limited borders, disappear; the conditioned reflexes from other parts of the skin remain normal. It is interesting that during mechanical stimulation of these ineffective skin areas there results a very strong inhibition of the conditioned reflexes from the irritation of the effective regions of the skin, and that in a formerly wide awake dog drowsiness and sleep quickly ensue. When with time the lost conditioned reflexes are restored, there is observed definite disturbances in the differentiation of the stimulations from these places; either a definite analysis is lacking, or the differentiation occurs with various peculiarities. The following relation deserves especial mention as one which has lasted and remained stable for some years. From such places the conditioned reflex can exist only as one which almost always coincides with the unconditioned stimulus. As soon as the conditioned stimulus systematically precedes by only a short time, ten to fifteen seconds, the unconditioned, the conditioned reflex begins quickly to disappear, and instead drowsiness sets in. From other neighbouring areas of the skin the conditioned reflex proceeds as usual. In this way the above-mentioned experimental phenomena, which I think are in close relation to hypnotism and sleep, can be even more clearly demonstrated after removal of the parts of the brain corresponding to these stimulated skin areas. I am confident that the skin analyser in consequence of its evident advantages will become the chief object of research in studying the activity of the cerebral hemispheres.

And further. Conditioned reflexes may be formed from stimuli which come from the skeletal muscles, for example, from bending the leg at some definite joint, when the movement reflex has been differentiated from the pure skin stimulus. The ultimate proof that this differentiation

can really be obtained is afforded by the extirpation, now of one, now of another, portion of the anterior lobes, and the consequent disappearance of the skin reflex and preservation of the movement reflex in one cases; and in the other case, the loss of the movement reflex and preservation of the skin reflex.

And even more! In one dog in which the posterior half of the hemispheres was completely removed, and which lived in good health for some years afterwards, the following experiments were carried out. The conditioned reflexes to various intensities of illumination were easily formed, but it was impossible to produce a conditioned reflex to any definite object. In the same dog one could readily establish conditioned sound reflexes, and even bring about differentiation between separate tones. But there was a marked divergence between the ear analyser of this dog and that of a normal animal. Though the ear of the latter can without difficulty differentiate one and the same series of tones used in ascending and descending order, this dog has not been able to do so; with the given injury of the analyser, that is apparently impossible.

From these facts it follows that the boundaries of the ear and the eye analysers in the brain must be considerably extended, and that partial destruction of the cerebral ends of these analysers is manifested by a definite limitation of the analysing ability. As an ideal in the investigation of the hemispheres, I suggest the situation in which we shall have such a multitude of differentiations that the smallest damage of the hemispheres will immediately be discovered by us in some perceptible defect in this differentiating system.

I will conclude with a fact that seems to me especially instructive for our study. We have before us a dog the front half of whose hemispheres has been removed. All the conditioned reflexes formerly elaborated have disappeared. In all vital respects he is completely helpless, he has lost all normal relations to the external world; he cannot take food which lies near him, he notices neither lifeless objects, men, nor animals; in walking he collides with objects and gets into most uncomfortable situations. And what do you think, gentlemen! In such an animal one can find a path to the discovery of entirely normal and complicated nervous activity. With the salivary glands of this dog may be formed the so-called "water reflex." When a normal dog drinks or has water put into his mouth, there is no flow of saliva or at most only one or two drops. If one has previously put some substance stimulating the salivary secretion, for example, acid solution, into the mouth, then the introduction of water also calls forth an abundant flow of saliva. Apparently the different stimulations which make up the entire act of introducing the liquid into the mouth and which accompany the reflex effect of the acid become a conditioned stimulus of the acid reaction: and

as such they are manifest in the introduction of the water. This salivary secretion has all the properties of a conditioned reflex. In the dog which I have described one can promptly form, with the help of the acid reflex, the conditioned reflex to water, having all the general properties of conditioned reflexes. This result was confirmed in another dog in which the frontal half of the hemispheres was removed but the olfactory lobe left intact. This dog was similar in all details to the preceding one, but he could form not only the water reflex but odour reflexes. As the autopsies showed, the posterior parts of both hemispheres in the two dogs were atrophied. Consequently, in the removal of the frontal parts there was a destruction of the conduction paths to the hinder parts. Speaking psychologically our animals became idiots, judged by their movements; but judged by the activity of the salivary gland, they were at the same time intelligent.

I shall call your attention to two conclusions from the last experiments. The advantage of using the salivary gland and not the reaction of skeletal muscles as an indicator of the higher nervous relations of the animal is obvious. Were we to judge by the muscular system the important fact that the complex nervous functions of the animal continue to exist after exclusion of the frontal half of the hemispheres would be completely hidden. The results of the above experiments deal a hard blow to the psychological classification of subjective phenomena; thus in our case, from the psychological standpoint there would be an insoluble contradiction and an incomprehensible concatenation of events. In an animal deprived entirely of the hemispheres, both with us and other workers, no conditioned reflexes have ever been formed.

Thus the cerebral hemispheres are the organ for the analysis of stimulations, and the organ for the creation of new reflexes, of new connections. They are the organ of the animal structure which is especially adapted to effect and maintain continuously an equilibration of the organism with the outer world, an organ for the appropriate and immediate reaction to the most diverse combinations and fluctuations of phenomena in the external world, and, to a certain degree, they are a special organ for the perpetual further development of the animal organism.

One may suppose that some of the conditioned temporary connections may be later transformed into unconditioned reflexes by heredity.

In conclusion I can testify with full objective justification that all our facts are very accommodating, and are easily reproducible. I and my collaborators, to whom I am sincerely grateful, have with complete success demonstrated these experiments in my two systematic courses on conditioned reflexes, as well as in the reports to scientific societies, and before many native and foreign colleagues in my laboratories.

During our many years of work we have never had an occasion to apply with any success psychological conceptions, or explanations based on such conceptions. I must confess that earlier, when seeking for actual causal relations I met with difficulties, I sometimes, partly out of habit, partly from a certain anxiety, resorted to those psychological explanations which for a long time have been considered as laws. But soon I understood that they were bad servants. For me there arose difficulties when I could see no natural relations between the phenomena. The succour of psychology was only in words (the animal has "remembered," the animal "wished," the animal "thought"); it was only the assistance of indeterminate thinking, without a basis in fact.

The methods for the examination of the higher nervous activity of the animal which psychology has originated—the learning of labyrinths, the opening of various contrivances, etc.—certainly leads to the collection of scientifically useful material, but material which consists of separate fragments, and which does not bring us nearer to the fundamentals, the elements of nervous phenomena because it must be itself analysed and explained. For the exact and systematic investigation of the functions of the higher parts of the central nervous system it is absolutely essential that the basis be laid on purely physiological conceptions. With the formulations which I have outlined one may work successfully. The results in the hands of other investigators will show how exact and how sufficient they really are.

I express my heartiest thanks to our honourable president for giving me the opportunity to speak before so numerous a gathering of my colleagues on a subject which has filled a whole third of my scientific life; and to you, gentlemen, I express my gratitude for your attention, which I have so long misused.

CHAPTER XXII

THE INSTABILITY (LABILITY) OF INTERNAL INHIBITION IN CONDITIONED REFLEXES

(From the Ehrlich Issue of the *Berliner Klinische Wochenschrift*, 1914.)

THE KINDS OF INTERNAL INHIBITION—EXPERIMENT.

THE study of conditioned reflexes can be divided into several branches, one of which has to do with inhibition. We recognise three kinds of inhibition: inhibition by *sleep, internal* inhibition, and *external* inhibition. The subject of the present report will be the general characteristics of internal inhibition.

Internal inhibition arises every time that an elaborated conditioned stimulus of physiological activity is sometimes or always (in the last case under definite conditions) repeated without being followed by the unconditioned stimulus with the help of which it was formed. This internal inhibition, as shown by our studies, is of different kinds: we distinguish between *extinction, retardation, conditioned inhibition,* and *differentiated inhibition.*

When we repeat several times a previously well elaborated conditioned stimulus without adding the unconditioned stimulus, it gradually loses its usual effect; not because it has been destroyed, but because it is temporarily inhibited. This phenomenon, which was one of the first we observed, we have called *extinction* of the conditioned reflex. If the unconditioned stimulus is added to a previously formed conditioned stimulus not immediately after the beginning of the latter, but 20 to 30 seconds or some minutes later, the action of the conditioned stimulus, *i.e.,* the conditioned reflex, sets in after a certain latent period, consisting of seconds or even of minutes; thus the effect of the conditioned stimulus is postponed until the time when the unconditioned stimulus is usually applied. This is also a phenomenon of inhibition, and is termed by us *retardation* of the conditioned reflex. When a well elaborated conditioned stimulus is combined with some indifferent agent and in the combination is not systematically accompanied by its unconditioned stimulus, the conditioned stimulus gradually loses its stimulating effect in this union, although it is active when applied alone, *i.e.,* without the extra agent. This results from internal inhibition, which we call in this case *conditioned inhibition.* When from any definite agent a conditioned stimulus is elaborated, then all similar

and related stimuli also have somewhat the same effect. But when the chosen stimulus is repeated many times these extraneous stimuli gradually become ineffective. This, too, is also the result of an inhibitory process, which we call *differential inhibition.*

All these forms of inhibition can be easily removed, can also be inhibited. This occurs under the influence of new stimuli arising in the surroundings and calling out the orienting or focusing reaction of the animal. The result is the restoration of the formerly inhibited reflex. We designate such a phenomenon as *dis-inhibition* of the conditioned reflex.

The more experiments we make with conditioned reflexes, the more facts we accumulate, the more we have proofs showing that the process of internal inhibition is much more *labile* (unstable) than the process of conditioned excitation, *i.e.,* under the influence of extraneous stimuli the process of internal inhibition is suppressed more easily and more quickly than the process of conditioned excitation. This is a constantly recurring fact in the study of conditioned reflexes.

If I enter the room at the moment when my collaborator is working with conditioned reflexes, then the course of the conditioned inhibition is markedly distorted (extinction, retardation, etc.), but if the conditioned stimulation is being tried at this time, it does not suffer at all, or if so, only slightly. In the rooms of my old laboratory only seldom was it possible to observe a gradual and regularly occurring extinction of conditioned reflexes. Very often the extinction was interrupted by a return of a considerable effect of the extinguished stimulus, usually due to accessory agents, principally sounds, acting upon the animal.

The following unforeseen fact intruded itself in a remarkable way. I had decided to deliver two lectures concerning the main phenomena of the conditioned reflexes before a large audience, and to demonstrate my remarks by experiments. The first report concerned the mechanism of formation of conditioned reflexes, and those which had been elaborated upon many different agents were successfully exhibited. As the second meeting had to do with the analysing activity of the higher parts of the nervous system, we naturally desired to show also cases of differentiation. For this purpose we selected exact and well developed differentiations, but they could not be demonstrated. It turned out that those stimuli which had been differentiated (inhibited) and which were absolutely without effect (negative) in the laboratory, now had a full and positive effect. The new stimuli falling on the dog (due to his unusual surroundings in a crowded amphitheatre, etc.) were insufficient to inhibit the conditioned reflexes, but these same unusual stimuli of the new surroundings, strange people, etc., acting for the

second time and therefore decreased in strength, were sufficient to suppress completely the processes of internal inhibition on which was based the differentiation of neighbouring stimuli.

The process of internal inhibition in the form of retardation attains a high degree of sensitiveness in the experiments in which strong induction shocks applied to the skin are made a conditioned stimulus of the food reaction (experiments of Yerofeva). The feeding of the animal in these experiments always followed 30 seconds after the beginning of the stimulation with the electric current. For a long time after the formation of the resulting conditioned reflex, the conditioned effect, measured by the salivary flow during these 30 seconds, was of considerable size, and began quickly. But some time later the salivary secretion became less and less, and farther and farther displaced from the moment of beginning the conditioned stimulus toward the instant of eating, *i.e.*, retardation of conditioned reflexes set in. At this stage of the experiment may be observed the marked influence of all accessory stimuli, principally sounds, on the size of the conditioned reflex during the 30 seconds before the feeding; *i.e.*, through these stimuli the retardation of the conditioned reflex was abolished, and its original size was more or less fully restored. It would be interesting at this stage to make an uninterrupted phonographic record of all the sounds of the surrounding *milieu* in order to establish the parallelism between the vibrations of the sound phenomena and the phenomena of the disinhibition.

Such observations strengthen our conviction that we are gradually approaching the detailed registration of the uninterrupted total influence of the environment on the animal organism realised through the highest parts of the central nervous system, and that in this way we are coming nearer to the scientific determination of the complete activity of living creatures, including here with justification the higher functions of man himself.

CHAPTER XXIII

THE PURE PHYSIOLOGY OF THE BRAIN

(Prepared for the Congress of Psychiatrists, Neurologists and Psychologists scheduled to be held in Switzerland, August, 1914, but postponed on account of the outbreak of the War.)

UNCONDITIONED AND CONDITIONED REFLEXES—HIGHER AND LOWER ANALYSES—CONDITIONED STIMULI FROM DESTRUCTIVE STIMULI—EXTERNAL AND INTERNAL INHIBITION—LAW OF FORCE AND SPATIAL RELATIONS (MATHEMATICS)—IRRADIATION AND CONCENTRATION ILLUSTRATED BY EXPERIMENT—WHY PSYCHOLOGICAL CONCEPTIONS ARE CONVENTIONAL—INABILITY OF PSYCHOLOGISTS TO EXPLAIN THE GIVEN EXPERIMENT—THE FUTURE OF PHYSIOLOGY.

I HAVE received an invitation from the president of the organisation committee of our congress to read before the Section of Psychology a report on the activity of the brain based on the work of my laboratory.

Our highly esteemed president some years ago wrote the following: "When physiologists will have created a physiology of the brain independent of psychology—I mean a pure physiology, and not psychological fragments which appear under this name, a physiology capable of speaking for itself without the verbatim prompting of psychology as to what it must say, then we shall see whether there is any advantage in rejecting human psychology and consequently comparative psychology. But we have not yet attained to this." One cannot deny the justice of this criticism; and its defining of the question is highly serviceable.

Supported by facts acquired during many years in my own laboratories, where I have had more than a hundred collaborators, as well as in other laboratories, I dare to announce, with absolute conviction, that the physiology of the cerebral hemispheres (and indeed "pure physiology" in the sense of Prof. Claparède) already exists and is growing rapidly. This physiology uses in its examination of the normal and pathological activity of the hemispheres of higher animals only physiological conceptions, and has never found it necessary to borrow psychological words or ideas. Our investigations, like those of the other natural sciences, rest upon a solid basis of facts, and thanks to this circumstance, our science is collecting a mass of exact material and is opening for the experimenter an ever-widening horizon. Only in a few most general features can I trace here the fundamental conceptions and facts of this new physiology of the brain in order then to give them a more detailed description in some points which seem to me

to be of special interest and value for discussion in this our first meeting.

The basic activities of the higher parts of the central nervous system are: first, the coupling or linking of new temporary connections between certain external phenomena and the function of the different organs; and secondly, the decomposition of the whole complexity of the external world into its units—briefly the activity of a coupling or synthesising mechanism and of an analysing mechanism. Through these two activities there are established exact and fine adjustments of the animal organism to the outside world, or, in other words, a complete equilibration of the systems of energy and matter constituting the animal organism with the systems of energy and matter of the environment.

A constant connection between certain phenomena and the action of definite organs was long ago noted as a function of the lower parts of the central nervous system and was called by physiologists a reflex. The task of the higher parts of the central nervous system is the formation of temporary reflexes, and this means that the nervous system is not only a conducting apparatus but a coupling machine, which creates new connections. Thus before the modern physiologist are two kinds of reflexes, constant and temporary, inborn and acquired, the reflexes of the species, and those of the individual. For practical purposes of distinction we call the first reflex *unconditioned* and the second *conditioned*. It is highly probable that newly formed reflexes (individual) under the continuance of uniform conditions of life during several successive generations pass over into constant reflexes (generic). This must be one of the constantly acting mechanisms in the evolution of the animal organism.

Correspondingly, the primitive analyses fall to the lot of the lower parts of the central nervous system, and already this has been studied for a long time. When, for example, a decapitated frog differentiates between separate skin stimuli by their quality, location, etc., we see the work of the lower analysing apparatus. In the highest parts of the central nervous system there are the endings of the finest and most varied analysers by which the smallest elements of the external world are isolated and constantly brought into fresh connections with the organism, to form the conditioned reflexes. In the lower divisions of the central nervous system, on the contrary, relatively fewer and coarser agents of the external world come into connection with the organism through the constant reflexes.

The whole way along which the nervous impulse travels is, as is well known, called the reflex arc or path. In the lower central nervous system there are recognised three parts of this arc: the receptor (a receiving apparatus), conductor (conducting apparatus), and the effector

(the apparatus which exercises the special activity). Add to "receptor" the word "analyser" (decomposing), and to "conductor" the word "connector" (linking or coupling apparatus), and you have the expressions for the corresponding anatomical structure for the two fundamental activities which characterise the higher part of the central nervous system.

As has been shown by many investigators, the conditioned reflex is unfailingly formed in the presence of a small number of definite conditions, and hence there is no basis for considering its creation as especially complicated. Always when some indifferent stimulus synchronises with the action of some other stimulus which produces a definite reflex, then after one or many such coincidences, the formerly indifferent stimulus taken alone calls out the same reflex as the active stimulus which it previously accompanied.

In our experiments on dogs we have always used for the elaboration of conditioned reflexes one of two unconditioned reflexes—the reflex to food and the reflex to pouring acid into the mouth. The reaction we observed was the one that could be exactly measured. Motor reactions were noted only occasionally; a positive movement in the case of food and a negative one in the case of acid. In just this way can a new conditioned reflex be built up from an old conditioned reflex. A conditioned reflex may be formed even from a stimulus which is already firmly connected with a certain reflex act—indeed, such a stimulus may be made a conditioned stimulus for an entirely different kind of activity. We see such an event with destructive stimuli (in general parlance known as pain stimuli) in the following case. If the skin of a dog is stimulated with a certain strength of electric current, it naturally calls out the defensive reaction. By frequently uniting the feeding of the dog with these stimuli we can make the same current, or even a current of greater strength, or, in general, any destructive mechanical or thermal action (pricking, pinching, burning)—we can make all these destructive stimuli regularly produce the food reflex (*i.e.*, the dog turns toward the source of food and begins to secrete saliva) without any signs of defence.

A highly important detail in the formation of conditioned reflexes is that the stimulus must not coincide exactly with the unconditioned stimulus, but that it must precede the latter by some seconds.

I shall omit many details concerning the formation, systematisation, and general character of conditioned reflexes.

Concerning the activity of the analysers, the first fact to be observed is the following. All phenomena in the beginning link into the conditioned connection as entirely general stimuli, and not until later do they become specialised, *i.e.*, so that only definite phenomena provoke

the conditioned response. For example, if you make a conditioned stimulus out of some given tone, then at first not only the tone you have used but also other tones, and even noises, etc., provoke the same reflex. Afterwards, when your given tone has been repeated many times, the number of sounds which act becomes smaller and smaller, and finally only the selected tone produces the conditioned reflex. In this way the limits of the activity of the analyser are marked off, being in some animals capable of an almost inconceivable delicacy of differentiation, and presenting possibilities of wide development. I must omit a mass of particulars relating to these facts.

Conditioned reflexes as well as the process of analysis are subject to continuous fluctuation during the normal course of life. Besides chronic changes, which I shall not enter into, we may notice rapid variations in both directions, *i.e.*, both conditioned reflexes and analyses quickly become stronger, or weaker, or vanish. We have studied particularly the lessening of activity of the conditioned reflexes. We employ the general physiological word *inhibition* for this decreasing process, and we distinguish three kinds: the external, the internal, and the sleep inhibition.[1]

External inhibition is a complete analogue of that inhibition which was recognised long ago in the lower parts of the central nervous system when a newly arriving reflex inhibits one already present and active. It is evidently the expression of a ceaseless conflict among the different sorts of external and internal stimulations which determines which shall become at the given moment of predominant significance for the organism. External inhibition can in its turn be divided and subdivided.

Internal inhibition has its origin in the mutual interrelations between the new (conditioned) reflex and the old (unconditioned) reflex by means of which the conditioned reflex was formed. This type of inhibition always develops when the conditioned stimulus temporarily or constantly (but if constantly then only under definite and peculiar conditions) is not accompanied by the unconditioned stimulus with which it was elaborated. Now we are acquainted with four kinds of this inhibition. I ask your special attention to the kind which we call extinction. If an elaborated conditioned stimulus is repeated after short intervals (two, three, four or more minutes) without being attended by the old (unconditioned) stimulus by the help of which it was formed it is gradually weakened, and finally becomes inactive. This is not a destruction of the conditioned reflex, but only a suspension, a temporary inhibition; for after some time it is spontaneously restored

[1] For the new conception of sleep as irradiated inhibition, see chapter xxxii.—*Translator.*

completely. We shall refer to this again, as it bears on the most important point of to-day's report.

All kinds of internal inhibition as such can be disturbed in their course or suppressed, even themselves being inhibited. That is, the inhibited reflexes become freed from the inhibition—dis-inhibited—whereupon they appear in their full effect. This happens when external inhibiting agents of moderate strength act upon the animal. The study of the phenomena of internal inhibition, therefore, demands a specially fitted laboratory; if such does not exist, all accessory stimuli which are constantly falling upon the animal, chiefly sounds, frequently interrupt the course of our experiments.

Finally, the last kind of inhibition—*sleep*—which regulates the periodic chemical metabolism of the whole organism, and especially of the nervous system. It appears in the form of normal sleep or in the hypnotic state.

In describing nervous activity it is essential to take into consideration the absolute and the relative strength of the different stimulations, and the duration of the hidden aspects of the excitation—the latent stimulation traces, or after effects. The influence of both phenomena (strength of stimuli and duration of their latent traces) was clearly manifested in our experiments, and can be studied and measured without difficulty. And even more! We find here an astonishing and marvelous predominance of the laws of mass and energy, and involuntarily we arrive at the thought: it is not without reason that mathematics, the teaching of the relations of numbers, springs wholly and entirely from the human brain.

The individual characteristics of the nervous system of our various experimental animals is sharply manifested, and can be expressed in exact figures. An example of this will be given.

In the course of our examinations of the two chief cerebral functions there gradually unfold before us the properties of the brain mass. One of these properties is a peculiar movement of the nervous process in this mass. On the basis of our experiments I can present to you in striking form the fundamental law of the higher nervous activity. It is the law of *irradiation and concentration* of the nervous process. This law applies to the excitation as well as to the inhibition process. It has been frequently studied by us in the phenomenon of internal inhibition. Allow me to direct your attention to these experiments.

We have a dog in which, thanks to the unconditioned reflex (the effect of acid in the mouth), the mechanical irritation of more than 20 places on the skin has been made the conditioned stimulus of the acid reaction—*i.e.*, every time the mechanical stimulus is applied to one of these spots by means of an appropriate device, there begins a special motor

reaction and a secretion of saliva. The secretion obtained by stimulation of any of these places on the skin is equal, *i.e.*, all of the places have the same degree of effect. Now for the experiment itself. Let us take one of these points of the skin and apply the mechanical irritation for some time, say thirty seconds. We note a certain measureable salivary reflex which can be expressed in units. Now let us fail to add to the conditioned stimulus the introduction of acid into the dog's mouth—as we had previously combined them in forming the conditioned reflex—and repeat the mechanical stimulation of the skin every two minutes unsupported by the unconditioned stimulus (acid). The usual conditioned reflex (secretion of saliva) appears, but is decreased. We continue this repetition of the conditioned stimulus (mechanical irritation) without the unconditioned stimulus (acid in the mouth) until the conditioned reflex (secretion of saliva) fails to manifest itself. This is what we call the *extinction* of the conditioned reflex, a special sort of internal inhibition. We have evoked the process of internal inhibition at a certain point in the cerebral end of the skin analyser, which is in that part of the cerebral hemispheres connected with the skin. Let us trace the movement of this process. Just after we have produced a zero effect (primary extinction) by stimulation of a single point of the skin repeatedly without supporting it by the unconditioned stimulus, we immediately without any pause stimulate another one of the twenty points, 20 to 30 cm. distant from the first point (our dog is of average size). We get a normal reaction, 30 divisions on our graduated measuring tube. After one or two days we repeat the experiment in the following manner; we stimulate a new skin spot, not immediately after the extinction of the reflex from the first spot as in the last experiment, but five seconds after having obtained a zero effect from the extinguished conditioned stimulus. Now the salivary secretion instead of being normal as in the last experiment, is decreased to say 20 divisions (secondary extinction). At the next repetition of this experiment, but after a pause of fifteen seconds instead of five, the secretory action is reduced to 5 divisions instead of 20. If we use a pause of twenty seconds between the stimulation of the point for the extinguished reflex and the stimulation of the new point, we get a further reduction—not 5 divisions but 0.

Let us continue the experiment still further. After an interval of thirty seconds between the irritation of the two skin points we get not a zero effect but 3 to 5 divisions. At an interval of forty seconds, we have again 15 to 20 divisions; at an interval of fifty seconds, 20 to 25 divisions; and with an interval of sixty seconds we obtain the customary full effect of 30 divisions. During this whole period of sixty seconds, and even longer, the irritation of the site of the primary ex-

tinction remains zero. We may choose for comparison of the primary and secondary extinctions any two of our twenty skin points (on which equally strong conditioned reflexes have been elaborated) and we always get the same series of figures, provided the distance between the skin points is the same in the different cases. If the distance between the stimulated points is lessened, the difference consists in this—the decrease of the effect and the zero effect on the secondary extinguished point sets in sooner, the zero effect lasts longer, and the return to the normal effect is delayed. If certain precautions are observed, these experiments proceed with astonishing exactitude, as shown by our results from five dogs used by two workers during the course of a year. The repetition of these experiments yields such stereotyped results that I can say without exaggeration that for some time I could not believe my eyes.

If we compare these facts with other similar ones and exclude extraneous suppositions, we arrive at the following conception as the most natural and simple. If we consider the skin as the projection of a certain region of the brain, we must assume that the process of internal inhibition arising at a definite point in this region first spreads and irradiates over the whole region, and then begins to condense, to concentrate about its point of origin. It is interesting to observe how slowly this process moves in each direction. It is especially noteworthy that this speed, so different for different dogs (we have seen a relation of 1:5 between our animals), remains uniform to a remarkable degree for any given individual; one might even say it is an invariable constant.

This law of *irradiation and concentration* of the nervous process is, as you can see, of unusual significance. It shows the relation between many otherwise incomprehensible phenomena; for example, the generalised character which every stimulus has the first time it manifests a conditioned effect and before it becomes a specialised conditioned stimulus; or the mechanism of external inhibition; indeed the formation of the conditioned reflex itself, which can be understood as a phenomenon of concentration of the stimulation. I shall not enter into detailed explanation of the importance of this law, but I shall use the foregoing experiment as an illustration of what I intend to show.

During the course of the thirteen years of our work on the conditioned reflexes, I have always had the impression that the conceptions and systematisations of subjective phenomena by the psychologists must differ fundamentally from the physiological representations and classifications of the higher nervous functions; that the reproduction of the nervous processes in the subjective world is unique, and is, as it were, a many times reflected image, so that the entire psychological idea of the nervous activity is extremely conventional and only approximate.

From this point of view the above experiment deserves especial attention. When we first established the law of extinction of the conditioned reflex, we were told that this was nothing unusual; that the explanation is simple; the dog notices that the signal does not correspond to reality, and therefore responds more and more weakly and finally reacts not at all. I think that many of you who are persuaded of the scientific validity of bio-psychology would say the same. Let this be as it may; but then, gentlemen, you are obligated to interpret psychologically the above experiment in all its stages and details. I have often proposed this to many intelligent people of various professions (biological, sociological, etc.), and the result was elucidating. Each gave his own interpretation, based on his individual conception of a chain of internal states of the animal, and it was impossible to reconcile the explanations one with the other. The bio-psychologists whom I questioned spoke of the ability of the animal to make distinctions, to remember, to draw conclusions, of his confusion and disappointment, and of such qualities in the most varied combinations. But actually only irradiation and the consecutive concentration occurred in the nervous mass, and the knowledge of this process made possible the exact prediction of the phenomena (in figures)!

What do you reply, gentlemen? I await your answer with great interest.

Herewith I conclude the part of my report dealing with facts. Allow me to make some additional remarks. All parts of the higher nervous activity of the animal are gradually brought within the framework of our examination of the conditioned reflexes, as one may see from a rough, approximate comparison between our observed facts and the psychological classification of subjective phenomena, such as consciousness, will, thought, effect, etc. The meaning of some of these facts was explained by us in our experiments on destruction of the hemispheres of dogs. And finally there are revealed more and more clearly the general conditions of the resting and of the active state of the brain.

The entire field of research which opens up before us is at present completely, though provisionally, comprised in our conception of the two chief activities of the cerebrum—the coupling, *combining,* or synthesising function, and the analysing or *decomposing* function—and in a few fundamental properties of the brain mass. The future must decide whether this explanation is adequate; for, of course, our general ideas of the functions and characteristics of the brain will be extended.

Thus you see that the horizon of the strictly objective study of the highest nervous activity continually widens. Why should physiology strive to delve into the hypothetical and fantastic internal world of the animal? During the thirteen years of our investigations I have not

once used with success any psychological conception. The physiology of the brain should not for a single moment leave the ground of natural science which every day proves its solidity and productiveness. One may rest assured that along this path, upon which the strict physiology of the brain has set out, astonishing and marvelous discoveries await us, and that there will result such power over the higher nervous functions as is in nowise inferior to the other achievements of natural science.

I recognise the mental effort in the work of both the old and modern psychologists, and I bow in deference before them. Yet it seems to me, and I believe this statement can hardly be doubted, that their investigations are carried out in an extremely inefficient manner; and I am fully persuaded that the pure physiology of the animal brain will not only lighten the Herculean task of those who have consecrated themselves to the study of the subjective states of man, but will crown their efforts with success.

CHAPTER XXIV

SOME FACTS ABOUT THE PHYSIOLOGY OF SLEEP

(Read before the St. Petersburg Biological Society, 1915, assisted by Prof. L. N. Voskresensky.)

OCCURRENCE OF SLEEP UNDER TWO SETS OF CONDITIONS—SOPORIFIC EFFECT OF SURROUNDINGS—EXPERIMENT—FALLING ASLEEP AND AWAKENING SHOW SAME PHASES IN REVERSE ORDER—SPREAD OF SLEEP OVER THE HEMISPHERES.

In our study of the conditioned reflexes we often met with the phenomenon of sleep. As our experiments were complicated, and were often interrupted and diverted from their normal course by this state, we were forced to give it our attention.

Besides isolated contributions, two of our collaborators have made systematic investigations of this subject—*viz.*, Dr. N. A. Rozhansky and Mme. Maria K. Petrova. Rozhansky studied that form of sleep which apparently results from the influence of uniform and indifferent stimuli, for example, when the experimental animal is removed to an isolated environment (such as an experimental room). When the animal is closed in such an isolated chamber and fastened on the stand, he gradually falls into a drowsy state and finally into deep sleep. Sleep also occurs under the effect of certain definitely acting stimuli from which strong conditioned stimuli have been elaborated. In the presence of such stimuli all dogs, and especially certain types, easily pass into a drowsy, hypnotic state.

Recently Dr. L. N. Voskresensky came upon a case of sleep which was entirely unexpected, for the dog had been previously experimented on for a long time by Dr. A. M. Pavlova, and had never shown marked signs of sleep. But now, during the researches of Voskresensky, sleep always crept in, and the experiments with conditioned reflexes were continually interrupted; sometimes the usual phenomena were entirely absent, sometimes they were only distorted. How did this come about? At first we were not sure that the condition was really sleep and we attributed the state to other causes, but close attention and repeated tests excluded all other suppositions. We were forced to conclude that a state of sleep had developed in this dog.

But whence could it come? As we scrutinised the details of the experiments, it appeared that sleep might arise in the following manner. In the former investigations of Dr. Pavlova, the experiment was begun as soon as the dog was brought into the room and put on the stand—

the effect of special conditioned stimuli was tried and followed by food (the unconditioned stimulus). Sleep did not appear under these conditions. Now, however, it happened that the dog was left for some time in the room, on the stand where he had to wait before the experiment was begun. The continuously acting, monotonous surroundings by and by began to call forth the state of sleep. Such an explanation of phenomena seemed perfectly reasonable.

We decided to study the condition in detail. In the first place it appeared that the total environment acts with surprising exactitude as a qualitative condition; for, if immediately after the necessary preparations (fixing on the dog's cheek the glass bulb for collection of saliva, etc.) you began at once the experiment with the usual stimuli, sleep did not appear. Let only a few minutes (one or two) pass between the end of the preparations for the experiment and the beginning of the stimulation, and the first stage of sleep becomes manifest. If now ten minutes elapse, the next phase of sleep sets in. Thus the sleep-producing surroundings may be administered in certain doses. Under the circumstances it became possible to study the progress of the state of sleep. And here are the results of our enquiry. In our experiments usually two reactions of the animal can be observed; one is the secretory reaction (the flow of saliva); and the other, the motor reaction (the dog seizes the food when it is offered); in other words these are the secretory and the motor reflexes. From the following table you can see that there is a certain law underlying the relation of the quantitative influence of the soporific environment and the observed phenomena.

TABLE I

State of the Dog	*Stage of Sleep*	*Reflexes*		*Remarks*
		Secretory	*Motor*	
Awake		+	+	
Asleep	I	—	+	
	II	+	—	Deep sleep
	III	—	—	
	II	+	—	
	I	—	+	
Awake		+	+	

In the waking state both the secretory and the motor reflexes are present (+). Immediately after the conditioned stimulus begins, saliva flows; and immediately after food is offered, the dog gulps it down. Thus both reflexes are effective. We do the experiment as follows: we keep the dog under the influence of the surroundings for two minutes at least, *i.e.*, two minutes elapse between the end of the preparations and the beginning of the conditioned stimulus. The *first phase of sleep*

sets in. It is manifested thus: the secretory reflex disappears (—). Your conditioned stimulus does not act on the secretory part of the reaction; but if you present the food to the dog, he takes it and eats, which means that the motor component of the reflex is present (+). Now you augment the effect of the environment by keeping the dog waiting ten minutes instead of two for the first stimulus; sleep deepens, and you have another sort of reaction, which, strange to say, is the earlier one reversed—the *second phase of the state of sleep;* the dog secretes saliva but does not take food, and even turns away from it. Thus the salivary reaction, which was absent during the first phase of sleep, is present in the second; and the motor reaction, which was present at first, disappears in the second stage, or even passes over into a negative effect; for the dog not only does not take the food but actively refuses it. Now if the dog is left in the sleep-producing surroundings for one-half to one hour before the beginning of the experiment, he falls into a complete and deep sleep in which both the motor and the secretory reflexes are lost (*third phase of sleep*).

Now let us gradually wake the dog out of the deep sleep and follow the course of events. This can be done at once simply by applying an intense sound stimulus, such as a loud clapping apparatus, etc. Immediately the normal waking state supervenes. A less violent stimulus may be used; one of our customary methods is to dispel the sleep gradually by repeatedly feeding the dog. Then you can observe the same phases which were described above, but in reversed order. After the deep sleep the secretory reflex is present, but the dog does not take the food. Later on the secretion fails to appear, but the dog eats, showing the motor reaction. After several repetitions of the feeding, both reflexes finally occur.

Now I shall call your attention to some actual figures. As soon as the dog is prepared on the stand some of our conditioned stimuli are applied, and we obtain a salivary secretion—37 divisions of our scale, which is the normal reaction. A certain precaution has to be observed in order to obtain exact results. The chamber itself has a hypnotising effect on the dog; for as soon as the animal, although wide awake and active, was brought under the spell of the room, he became entirely changed, even before he was put upon the stand; on the stand sleep became, of course, more marked and increased. In order to fix a certain moment for the passage from the waking to the sleeping state, as soon as the dog was put on the stand and preparations for the experiment began, we endeavoured to prevent sleep by calling him, stroking him, slapping him, etc. When everything was prepared we would go quickly from the room and begin the experiment immediately. In this way we were able to obtain the above-mentioned secretory reaction of 37, and also the

motor reflex. In the next experiment we allowed the surroundings to act two minutes after preparing the dog and before beginning the conditioned stimulation. When we used our conditioned stimulus the result was zero drops of saliva, but the dog ate at once. Next we let the surroundings act for four minutes; we obtained 20 divisions of saliva but the dog began to eat only forty-five seconds after the application of the conditioned stimulus, and then not unless the food was brought into contact with the mouth. If the surroundings were allowed to act for one-half to one hour, all the reflexes disappeared.

We tried to vary the procedures so as to see in one and the same experiment different phases of sleep. Thus the dog remained in the room for 75 seconds: the secretory reflex was zero and the food was taken at once. We then let an hour pass, leaving the dog alone. The stimulation produced by a single feeding neutralised to some extent the soporific effect of the surroundings, and only the second phase of sleep was manifested: the salivary secretion was 22, and the dog took the food, but only 20 to 30 seconds after it had been touched to the mouth. I shall give one instance more, of how sleep is dispelled. Our dog is slumbering, and in order to arouse and to awake him, we apply a weak stimulus —someone enters the room where the dog is on the stand. The noise of the entrance and perhaps the odour of the person tend to prevent the dog from sleeping. Now if we apply the conditioned stimulus we receive 24 divisions of saliva, but still the dog eats only 50 seconds later and then not spontaneously; this time it is necessary to put the food inside the mouth. Then we feed the dog once or twice, thus stimulating him by food, and we see the state of sleep dispelled and the transition to the following stage: the secretory effect is diminished, there are only 10 divisions of saliva, and the dog eats after 20 seconds. In the former case, it ate after 50 seconds and only from the hand, but now it takes the food of its own accord. With a new stimulus, tried after 20 minutes, the secretory reflex is zero, and the dog takes the food almost immediately. Finally, with the next conditioned stimulus 35 divisions of saliva are secreted, the dog takes the food without delay and we have an animal wide awake. We must thus recognise it as a thoroughly established fact that the processes of falling asleep and emerging from the state (awakening) have a characteristic influence on our two reflexes (motor and salivary).

We see an interesting fact of practical importance, which gave us the ability to control the animal and to remove the obstacle to our experiment. It was enough to feed the dog two or three times, or not to allow any lapse of time before beginning the experiment, and we became the masters of the situation; our experiments with the conditioned reflexes were not interrupted by sleep.

Now we must interpret this phenomenon. Certainly it is a difficult problem, and one that can be solved only provisionally. Our collaborators, Dr. Rozhansky and Mme. Petrova, conclude from their experiments that both states of sleep which they observed represent inhibition processes, and that this inhibition, in one case, spreads over the hemispheres from many different points (case of Rozhansky), and at another time, spreads only from one definite point (case of Mme. Petrova). Our facts described above confirm, we think, their conclusion—for we could actually see in our experiments a localisation and a wandering of the somnolent state in the mass of the hemispheres.

How can the movement of the sleep inhibition be traced in the cerebrum? We have offered a more or less acceptable answer to the same question applied to another kind of inhibition, internal inhibition. This gives us reason to hope that we may do the same regarding sleep inhibition. The simplest way is probably to trace the movement of sleep inhibition in a certain limited area of the hemispheres; for, as our experiments concerning the spread of internal inhibition over the whole hemispheres showed, very complicated conditions obtain in this case. The factors of this complexity are probably the frontiers of different regions of the hemispheres, various degrees of energy of stimulation, etc.

In our laboratory we are at present experimenting with the state of sleep. It is more convenient to follow the movement of the sleep inhibition in that part of the hemisphere which corresponds to the skin; its projection, as it were, in the brain. Besides, the conditioned stimulation of the skin produces the state of sleep very easily. If we assume that the state of sleep arises precisely at that point which is stimulated, then we may hope to see how the inhibition moves and spreads from this point over the whole skin projection (in the brain), and to be able to determine how far and how quickly this process travels. But this is now only a hope.

CHAPTER XXV

AN ANALYSIS OF SOME COMPLEX REFLEXES IN THE DOG; AND THE RELATIVE STRENGTH AND TENSION OF SEVERAL CENTRES

(From the volume dedicated to K. A. Timiryazev, 1916, assisted by Mme. M. K. Petrova.)

PRODUCTION OF THE GUARDING REACTION—REFLEX AND INSTINCT ARE FUNDAMENTALLY THE SAME—EXPERIMENT SHOWING INTERACTION OF THE FOOD REFLEX AND THE GUARDING REFLEX—RELATIVE STRENGTH OF CENTRES AS WELL AS THE TEMPORARY CHARGE ARE DETERMINING FACTORS—EXAMPLE OF A GLARING PSYCHOLOGICAL FALLACY TAKEN FROM LITERATURE.

Two of the many dogs which have served in our laboratory for experiments with the conditioned reflexes showed especial peculiarities. Although entering the experimental chamber where conditioned reflexes are being formed usually causes, besides a slight orienting movement, no special reaction with most dogs, in the two dogs mentioned above, such an act during the course of the experiments evoked aggression and hostility. Not only touching the dog, but even shaking hands with his experimenter, provoked a strong aggressive movement on the part of the animal. Evidently these dogs showed a special guarding reaction. In view of the peculiarity of this reaction and its distinctness and the disturbance it caused in the laboratory, we decided to make it the subject of more detailed investigation.

The complete *guarding reaction* was manifested in the following way: there was loud barking and an aggressive movement toward the stranger entering the experimental room, and the reinforcement of the aggression when the newcomer approached, and especially if he touched the experimenter. No one was exempt from this reaction of the dogs, not even the servants who brought them every day to the stand, nor the experimenters who had only a month or so ago completed a couple of years' work with one of these dogs. On the contrary an opposite behaviour was shown to the actual experimenter; the dog permitted him to do whatever he pleased, to attach the apparatus to the body and even in the mouth, and if necessary to scold or to strike.

First, it was necessary to determine the external conditions and the stimuli which produced and developed the guarding reflex. This task was not especially laborious. The chief stimuli of the reaction were unmistakable. The prime condition is the closed and isolated room with the customary experimenter. As soon as the animal leaves this room, he changes entirely his behaviour toward strangers and toward

his master. There is not a trace of the aggressive reaction; on the contrary the dog is friendly toward strangers. But at the same time he treats his master (the experimenter) with indifference and apathy, and now you may not only approach the master, but may strike him and the dog does not interfere.

The second condition is the limitation of freedom of the dog's movements by tying him in his harness. So long as the animal is free on the floor of the experimental room, he tolerates the intruder. But no sooner has he been put on the stand by the servant or by the master, and tied, than the aggressive reaction to every one but his master (the experimenter) begins.

Finally, the third condition is the commanding, authoritative, and varied behaviour and movements of the experimenter, of both a positive and negative character, toward the dog in the given surroundings. One of the dogs was for two years handled by an experimenter who was especially reserved and restrained, particularly in his movements; in this dog the guarding reaction although present did not reach its highest development. The servant could bring the dog into the room and tie him on the stand. Strangers could come into the room and remain, as long as they did not make sudden or aggressive movements. When, however, this dog became the experimental property of another one of our collaborators, a significant change occurred in the third condition of the guarding reaction, due in part to the difference in the temperaments of the former and the new experimenter, and in part to an intentional effort to reinforce this element. Thus a considerable increase was brought about in the guarding reaction. The end result was that the dog had to be transferred to his master even before entering the experimental room. The appearance of a stranger, though only at the door, provoked a furious rage in the animal.

In conclusion, it must be noted that the process of feeding the dog, which was carried out many times in the experiments with conditioned reflexes, had not the slightest effect on the development of the guarding reaction; for this reaction remained exactly the same independently of whether the unconditioned stimulus was feeding or the pouring of acid into the mouth.

Three conditions thus take part in the development of the guarding reaction. When the reaction is still weak the presence of all these conditions is required to make it manifest. If the master leaves the experimental room, there is no aggressive reaction to the stranger although the dog is fastened on the stand. If the dog is released and put on the floor there is no aggressive reaction, although the master is present. If the guarding reaction has already been reinforced by the repetition of all of these three conditions, then only two of them are required to bring

it out. However, when the guarding reaction has reached its highest tension, then the sight and voice of the experimenter taken alone are insufficient for the manifestation of the reaction. In another room and out of the stand, the dog no longer guards his master.

Thus the described reaction of our dog, although highly complicated, is a constant and exact result of a definite sum of external stimuli.

Usually this reaction is called the *"guarding instinct"*. We prefer the word *reflex*. From the physiological point of view there is *no essential difference between* the two phenomena designated as *reflex and as instinct*. The intricacy of the actions can not be used as a distinction. Numerous reflexes are also extremely complicated, for example, the vomiting reflex and many locomotor reflexes, as is clear from recent investigations. The chain-like character of the process, the compounding of a complex effect from simple components, whereby the end of one action is the stimulus for the beginning of another, is a property of many instincts as well as of numerous reflexes. Many examples of these can be found in the vasomotor and locomotor innervations. That the instinct is dependent upon a certain state of the organism, especially upon its internal condition, is not a distinction between instincts and reflexes. The reflexes are also not always invariable in their repetitions, and are dependent upon many conditions, for example, on other simultaneously acting reflexes.[1]

When one considers that any given reflex, as a response to a certain external stimulus, is not only governed and regulated by other simultaneous reflex actions, but also by a multitude of internal reflexes as well as by the presence of many internal stimuli, *viz.*, chemical, thermal, etc., operating in different regions of the central nervous system or even directly in the executive elements (motor or secretory), then such a conception would include as reflexes the entire complexity of all responsive reactions, and nothing would remain to necessitate the forming of a special group of phenomena known as instincts.

Thus in the foregoing dogs we have to do with the guarding reflex. What kind of reflex it is—whether inborn (unconditioned) or acquired (conditioned)—we can not say with certainty because we had not observed the dog during his entire life. However the obstinate invariability of the reflex during several years of observation under laboratory conditions, its strength and violence, incline us toward the first view, especially as one of the animals was a typical watch-dog. The history of an inborn guarding reflex would not constitute any special obstacle to explaining all the peculiarities of this reaction. In order to fulfil his rôle as guard, the dog must be in a certain place; and furthermore, if he is a ferocious animal, only recently domesticated, he must be tied.

[1] See footnote 1, chapter xxvii, on instinct and reflex.—*Translator.*

An essential condition, evidently, was the power of a certain domineering person who caught the animal, subdued him, tied him, fed him and when necessary whipped him, forming in this way, based on the unconditioned reflexes, a positive reaction relative to himself, the master, and a negative one in regard to every one else. In the definite composition of the stimulus calling out the guarding reflex there is this third element which is as essential as the first two, for in reality they all three occur together.

In view of the great intensity and the complete stereotyped character of the guarding reflex in our dogs, we undertook a comparison of this reflex with the food reflex in order to explain certain pertinent questions. For this purpose one of us (M. K. P.) continued the experiments with the conditioned reflexes, thus simultaneously exercising and reinforcing the guarding reflex, while the other (I. P. P.) elaborated a complicated food reflex, using his own person as the conditioned stimulus. This preliminary period lasted for two months. In the main hall the dog was fed with sausage by my own hand (I. P. P.) while I repeated "Sausage, Usatch" (the name of our animal—a shepherd dog). The food was always given by hand in order to bring the scent of the person into the composite conditioned stimulus. I. P. P. often stood among other people in order that the dog might more exactly differentiate his form and appearance; often he went into another room of the laboratory and called in a voice of varying strength the usual words, "Sausage, Usatch," so as to increase the sound component of the conditioned stimulus. The pieces of sausage were usually carried in a glass case in the pocket. With the words, "Sausage, Usatch," the hand was put into the pocket, the container with the meat removed, and a morsel either given to the dog with the hand, or thrown to him on the floor.

With another dog, "Calm" (a house dog), the same procedure was repeated, but with this difference, that the dog, before receiving the sausage had to sit down and give his paw on the command, "Sit down; give your paw." In this way the food reflex was reinforced to such an extent that it finally gave I. P. P. apparently a great control over the animal. When it seemed that the complicated food reflex had reached its greatest strength, we applied our reflexes simultaneously. The author (I. P. P.) who had formed the food reflex to his own person, entered the room where the dog was with the other experimenter (M. K. P.). The effect was exactly the same as when a stranger entered, *viz.*, a fierce attack. We must confess that this result at first surprised and perplexed us. How could it happen that the powerful food reflex, of fundamental value for the organism, had been overcome by a reflex which in any event must be considered as secondary, a reflex artificially formed, and not directly essential for the animal?

The further course of our experiments satisfactorily solved our problem. From the beginning of the experiments we were struck by the contrast between the two dogs. Although "Calm" manifested a marked offensive reaction when I. P. P. appeared at the door, "Usatch" only looked intently upon I. P. P. and began to bark only on being approached. It may be assumed that the guarding reflex in "Usatch" had to some extent been inhibited. In the next experiment, to the form, sight, and probably the scent of I. P. P. were added the customary words, "Sit down; give your paw" for "Calm," and "Sausage, Usatch" for "Usatch". The effect was striking. "Calm" stopped barking, and "Usatch" allowed I. P. P. to approach. But for a closer approach the words alone were insufficient, and before I. P. P. could get close to the animals it was necessary to put the hand into the pocket as if to take out the sausage, in order to arrest the offensive reaction. Also the exhibition of the empty glass container made possible a further step toward the animal by I. P. P. But approaching and touching the other experimenter (M. K. P.) again provoked the aggressive reaction. The next time the experiment showed exactly the same results. But this time the sausage was put into a glass and I could then approach the other experimenter (M. K. P.) while showing the sausage; and by giving it with one hand to the dog, I could with the other hand make threatening gestures toward or even lightly strike the other experimenter without arousing the guarding reaction. Thus there was a complete victory of the food reflex over the guarding reflex. This was repeated many times with exactly uniform results.

In these experiments it was surprisingly clear that the reflexes can for a long time exactly balance each other, literally like weights on the two sides of a scale. You need only to increase the number of stimuli for one reflex, *i.e.*, to add weights in one pan of the scale and it sinks—one reflex suppresses the other. Depending upon the reflex to which you add the stimulus, the scale pan in which you place the additional weight, the one or the other predominates.

Thus in the case of the equilibration of reflexes, the elements of the complicated conditioned stimulus of the food reflex are the following: The form, sight, and scent of the experimenter (I. P. P.), the words, "Sausage, Usatch," etc., the movement of the hand for the glass, the sight of the latter, the appearance and odour of the sausage, and the sausage itself. In the case of the guarding reflex, the elements of the complex stimulus are: The gradual approach to the dog, to its experimenter (M. K. P.), and the touching of the latter. It is plain that while for "Calm" the form and the appearance of I. P. P. proved insufficient to inhibit the guarding reflex, this same stimulus, when of weak intensity, that is to say, when there was a great distance between the intruder

and the dog, inhibited to a certain extent the guarding reflex in "Usatch".

The fact of the influence of the sum of the stimuli on the preponderance of one reflex over another (as well as the highly important significance of the number and strength of the components) is frequently met with in the objective study of the higher nervous activity of the animal. There is no doubt that in time this fact of the summed influence of different stimuli, if there is some unit for measuring their strength and if they can be considered in all details, will form the basis of a strict scientific investigation of the activity of the brain.

How can we consider physiologically the above cited phenomena? We can do so and still remain within the limits of the earlier conceptions of the so-called centres of the central nervous system. We must add to the earlier exclusively anatomical conceptions the physiological point of view and admit the existence of a functional union of special parts of the central nervous system, thanks to certain well formed paths of connection. If we admit that the results of the above cited experiments can be thus formulated: In our dogs the relative strength of the two centres (guarding and food) is markedly different, the food centre being much more energetic. However, for the full manifestation of these strengths, and, consequently, for the orderly comparison of the intensities of the reflexes, the centres must be completely charged. Otherwise the most distorted relations may be observed. With a small charge of the strong centre and a heavy charge of the weak centre the latter will often preponderate.

When we observe such facts as are brought out in these experiments we are no longer astonished at people who in all seriousness speak of horses and dogs which think and reason.

It seems to me incomprehensible that a sober psychological journal (*Archive de Psychologie,* Geneva, Vol. XII, 1913, p. 312-375) can devote so large a place to the story of a dog who, while in the same room where children were being taught, learned arithmetic so well that he constantly helped the pupils with their more difficult exercises; and by his knowledge of religion the same dog astonished the clergy who visited him, etc. Is this not brilliant testimony of the lack of contemporary psychological knowledge, which is unable to offer a more or less satisfactory criterion for the distinction of sense from nonsense!!

We are happy through this modest contribution to express our sentiments of deep respect for Prof. K. A. Timiryazev as an energetic promoter of native science and a tireless champion of a real scientific analysis in the region of biology; for many investigators in this field have strayed along false paths.

CHAPTER XXVI

PHYSIOLOGY AND PSYCHOLOGY IN THE STUDY OF THE HIGHER NERVOUS ACTIVITY OF ANIMALS

(Read before the Philosophical Society, Petrograd, November 24, 1916.)

THE DOG'S PLACE IN HISTORY—CHOICE OF THE OBJECTIVE METHOD—BASIS OF THE CO-ORDINATION BETWEEN THE KIND OF FOOD AND THE RESULTING FLOW OF SALIVA—THE "PSYCHICAL" STIMULATION OF FOOD AT A DISTANCE INVOLVES THE SAME PHYSIOLOGICAL MECHANISM AS FOOD IN THE MOUTH—DIFFICULTIES IN THE BEGINNING OF THIS INVESTIGATION—THE THREE MAIN POINTS OF RESEMBLANCE BETWEEN THE "PSYCHICAL" AND THE REFLEX STIMULATION—FORMATION OF THE CONDITIONED STIMULUS EXPLAINED—THE TWO CHIEF PROPERTIES OF THE NERVOUS SYSTEM ARE THE ABILITY TO CONDUCT AND TO CONNECT—FORMATION OF THE NATURAL CONDITIONED FOOD REFLEX IN PUPPIES AND IN MAN—THE UNCONDITIONED AND THE CONDITIONED REFLEXES—THE CENTRAL NERVOUS SYSTEM OF AN ANIMAL IS A COLLECTION OF ANALYSERS—SPECIALISATION AND DIFFERENTIATION TAKES PLACE IN THE ANALYSERS—TWO SORTS OF HIGHER NERVOUS ACTIVITY (FORMATION OF NEW CONNECTIONS AND HIGHER ANALYSIS)—THE PRODUCTION OF THE INACTIVE STATE—DESCRIPTION OF SIMPLE EXPERIMENT ILLUSTRATING CONDITIONED REFLEXES—THE PSYCHOLOGICAL AND THE PHYSIOLOGICAL EXPLANATION OF THIS EXPERIMENT; THE FORMER INADEQUATE, VARYING WITH EACH PERSON, WHILE THE LATTER, INVOLVING SPATIAL RELATIONS AS IT DOES, IS STRICTLY SCIENTIFIC.

FIRST, I count it my duty to thank the Philosophical Society that it has given its consent, through its chairman, to listen to my report. To what degree my subject will be interesting to its members I cannot say. I, however, have a special purpose which will be evident at the end of my communication.

I want to tell you of the results of an extensive investigation, lasting over many years. This investigation has been made together with some scores of collaborators, who participated in it with their heads as well as with their hands. If it had not been for them, the results would not be one-tenth of what they are. When I use the word "I," you must understand it not in the narrow sense of an author, but as meaning, so to speak, a director. I have guided it for the most part, and verified it all.

Now I shall proceed with the discussion.

We take some higher animal, for example, the dog. Although he is not at the top of the zoological ladder (the monkey takes that position), he is the closest to man; for there is no other animal which has accompanied man from prehistoric times. I have heard the late Modest Bogdanov, the zoologist, in reviewing prehistoric man and his companions, chiefly the dog, use the following phrase: "Justice compels us to say that it was the dog who helped man to emerge from savagery." Thus he appraises

the dog! This animal is, therefore, no ordinary one. Consider the dog —a guard, a hunter, a domestic pet, a servant—and in all this higher manifestation of activity, you see what the Americans are wont to call *behaviour*. If I intend to investigate this higher activity of the dog, *i.e.*, to systematise the phenomena of his life and to discover the laws and conditions under which these phenomena arise, before me looms up the question: how must I find, how choose the way?

Generally speaking, there are two paths. First the ordinary path along which every one goes. Following this way, we must superimpose our inner world on the animal, thus assuming that he thinks, feels, wishes, etc., just about as we do. Consequently, we may guess what transpires within the dog, and thus try to understand his behaviour. Or there is a second and entirely different path. Along this way we observe from the standpoint of a naturalist who looks on the phenomena, on the facts, in a purely external way, concentrating his attention only upon these questions: what agents of the external world act, and what are the visible reactions of the dog to these agents; what does he do? The question consists in this: which path is preferable, which way brings us nearer to our aim and gives us more information?

Allow me to answer this question, which is one of immense importance, giving our facts in chronological order.

Several decades ago my laboratory made a study of digestion and investigated particularly the activity of the digestive glands and their elaborated juices by means of which the food is transformed so that it passes further into the depth of the organism and there enters into the vital chemical processes. Our problem was to study all the conditions under which the work of these glands was carried out. A large part of this investigation had to do with the first set of these glands, the salivary glands. The detailed systematic study of these organs showed that their work is extremely delicate, and very adaptable to whatever substance enters the mouth: the quantity of saliva and its quality vary in strict accordance to conditions. Dry food is taken, and much saliva flows, for the food must be moistened; with watery food, the amount of saliva is smaller. If there is food which must be passed into the stomach, the saliva secreted contains mucus, which lubricates the mass so that it is easily swallowed; if the substance is one which must be ejected, there is a thin, watery secretion to aid in rinsing the mouth. Here we see a number of delicate co-ordinations between the activity of these glands and the substance upon which the saliva is secreted.

Next rises the question, what is the basis of such a fine co-ordination, and what is its mechanism? For this the physiologist—and that is my specialty—has an answer ready. The properties of the food act on the nerve endings, stimulating them. These nervous impulses are con-

ducted into the central nervous system to special points, and there cross over to the fibres leading to the salivary glands. Thus there is evidently a connection between what enters the mouth and the work of the glands. The details of this union are explained as follows: the several nerves from the oral cavity, where the substances act, are separately excited by acid, sweet, rough, soft, hard, hot, cold, etc., and these impulses travel along different nerve fibres to the central nervous system. From there these impulses can reach the salivary glands along different nerves. The one calls out one kind of activity; the others, other kinds. Consequently, different properties of the food stimulate separate nerves, and in the central nervous system there is a transfer to the corresponding nerves calling out each its particular function.

When aiming at a complete investigation, it becomes necessary to consider all the possible conditions over and above those I have mentioned. Substances entering the mouth act on the salivary glands—but do they act in the same way when they happen to be in front of the animal, *i.e.,* are they effective when separated from the animal by a certain distance?

We know very well that when we are hungry and want to eat, saliva flows if we see food. Hence the expression "the mouth waters." The investigation should extend to this phenomenon. What does it mean? There is, however, no kind of contact here. Concerning these facts, physiology used to say that besides the ordinary stimuli, there is a *psychical stimulation* of the salivary glands. Very well. But what does this mean, how is it to be understood, how must we physiologists approach it? Neglect it we cannot, once it plays a part in the action of the glands. What cause have we to exclude this function? First, let us consider the bare fact of psychical stimulation. It appears that psychical stimulation, *i.e.,* the action of a *substance at a distance, is absolutely the same as when it is in the mouth.* In every way it is the same. Judging by what kind of food is placed before the dog, if it is dry or moist, edible or inedible, the salivary glands function identically, whether the substance is in the mouth or at a distance. In the psychical stimulation we observe exactly the same relations, though the reaction is smaller.

But how is this to be studied? Taking the dog when he eats rapidly, snatches something in his mouth, chews for a long time, it seems clear that at such a time the animal strongly desires to eat, and so he rushes to the food, seizes it, and falls to eating. He longs to eat. Another time the movements are slower, less avid, and therefore we say the dog does not want so strongly to eat. When he eats, you see the work of the muscles alone, striving in every way to seize the food in the mouth, to chew and to swallow it. From all this we can say that he derives pleasure from it. When on the contrary an inedible substance happens

to get into the mouth, and the dog ejects it, spews it out with the tongue, shakes his head, then we involuntarily want to say that this is unpleasant for the animal. Now when we proceeded to explain and analyse this, we readily adopted this trite point of view. We had to deal with the feelings, wishes, conceptions, etc., of our animal. The results were astounding, extraordinary; I and one of my collaborators came to irreconcilable opinions. We could not agree, could not prove to one another which was right. For some decades before, and also afterwards, we could settle all our questions, we were able to decide one way or another, and the dissension ended.[1]

After this we had to deliberate carefully. It seemed probable that we were not on the right track. The more we thought about the matter, the greater grew our conviction that it was necessary to choose another exit. The first steps were very difficult, but along the way of persistent, tense and concentrated thinking I finally reached the firm ground of pure objectivity. We absolutely prohibited ourselves (in the laboratory there was an actual fine imposed) the use of such psychological expressions as the dog guessed, wanted, wished, etc. Finally we came to look in another light upon all the phenomena with which we were concerned. What then is our view? What is that which the physiologist called the psychological stimulation of the salivary glands? Is not this a form of nervous activity which was established long ago by physiology, and to which the physiologists are accustomed? Is it not a *reflex?* What is this reflex of the physiologist? There are three chief elements. First is the essential external agent, producing the stimulation. Then comes a certain nervous path by means of which the external impulse makes itself felt in the executive organ. This is the so-called reflex arc, a chain composed of a receptor, a centripetal nerve, a central part and a centrifugal or efferent nerve. And, finally, the regularity of the reaction; it is not accidental or capricious, but law-obeying. Under certain given conditions the reaction always and inevitably follows. However, this is not to be understood in the sense of absoluteness, so that there never may occur circumstances under which the agent does not act. It is evident that there may be conditions which mask the action. According to the law of gravity all things should inevitably fall to the earth, but if they are supported this does not happen.

Now let us return to the work with which we are concerned. What, then, is the psychical stimulation of the salivary glands? When the food is placed in front of the animal, before his eyes, then it acts upon him, upon his eye, ear, nose. *There is here no essential difference from the action in the mouth.* They are reflexes from the eye and from the

[1] See preface to the first Russian edition for details of the dilemma which confronted Pavlov at the beginning of this work.—*Translator.*

ear. When there is a loud sound, we reflexly jump. Stimulation with a strong light causes the pupils to contract. Consequently this (action from a distance) is no reason why we should not call the psychical stimulation a reflex. The *second element,* the nerve path: here the similarity is obvious; for when the dog sees the food the nervous path starts not in the mouth, but from the eye, continues to the central nervous system, and from here calls out the activity of the salivary glands. Again there is no real difference here and nothing prevents our representing this as a reflex. Now we come to the *third element,* its regularity. Regarding this it is necessary to say the following: The stimulation acts less regularly, less often on the salivary glands than when the substance is in the mouth. However, it is possible to study the subject and to handle it so that ultimately all those conditions upon which depends the action of the object at a distance will be under your control. Having attained to this point (and this is now the actual state of affairs), we are able to see regularity. But the psychical stimulation has an additional characteristic. When we examine these phenomena more closely it is seen that the agents acting from a distance are distinguished by this—that among them there can appear some which formerly were without effect. Here is an example. Let us say that the servant enters the room where the dog is, and brings him food for the first time. The food began to act when the servant gave it to the dog. If the servant has brought the food for several days, then finally it is only necessary for the servant to open the door, and put his head in, and the action begins at once. Here a new agent has appeared. If it continues thus long enough, then only the sound of the steps of the servant will be sufficient to evoke the saliva. In this way is created a stimulus which did not exist before. Evidently here is a considerable and important difference: in the physiological stimulation the stimuli are constant, but here they are changeable. However, the question can be considered from the following point of view. If it be proved that this new agent begins to act under strictly definite conditions, which also can be determined, *i.e.,* if all the phenomena will be regular and obey certain laws, then no objection can be raised to our view. Now, although the stimuli are new, they arise inevitably under definite conditions. There is no accident about it. Moreover, these phenomena are related to a law. I can say that there (in physiology) the reflex was characterised by this—we had a stimulus travelling along a certain path and our phenomenon was dependent upon certain conditions, but here also the phenomenon arises under definite conditions. There is the definition, and the essential conception of the reflex has not changed.

It has been proved that *anything, whatever you will, from the external world, can be made a stimulus of the salivary glands.* Any sound what-

ever, odour, etc., may become a stimulus, and it will call out the activity of the salivary glands as definitely as does food at a distance. In regard to the exactness of the fact, there is no difference whatever, only we must make allowance for the circumstances under which the fact exists. What then are these conditions under which anything can become a stimulus of the salivary glands? The basic prerequisite is *coincidence in time*. The experiment proceeds in this way: We take, for example, a sound, no matter what, which has no relation to the salivary glands. The sound acts on the dog, and he at the same time is fed, or acid is put into his mouth. After several repetitions of such a procedure the sound itself without either food or acid will stimulate the salivary glands. There are altogether four or five, perhaps six conditions under which, in every dog, any stimulus, any agent of the external world inevitably becomes a stimulator of the salivary glands. Once this is so, once it has become such a stimulator under the definite series of circumstances, then it will always stimulate with the same accuracy as food or as some rejectable substance introduced into the mouth. If any agent of the external world inevitably becomes under certain circumstances a stimulator of the salivary glands, and having become such, inevitably acts, then what reason have we to say that this is anything other than a reflex? Here is a regular reaction of the organism to an external agent, brought about through the medium of a certain part of the nervous system.

The usual reflex, as I told you, proceeds in the following way: we have a definite nervous path along which the impulse originating from the peripheral parts is transferred to this path and reaches the executive organ, in the given case the salivary glands. This conducting path, we say, is, at it were, a living wire. What happens in the new case? Here it is only necessary to add that the nervous system is not, as generally considered, *only a conducting apparatus, but a connecting one too.* There is nothing paradoxical in this supposition. If in everyday life we use so many coupling or connecting apparatuses, in our lighting systems, telephones, etc., it would be strange indeed if in the most ideal machine which has yet arisen from the substance of the earth there were no application of the principle of connecting, but only of conducting. So it is quite natural that together with conducting properties, the nervous system should possess a connecting apparatus.

Analysis has shown that the constant form of stimulation of the salivary glands by food at a distance, in the usual case with which we are all familiar, is characterised also by the formation of a new nervous path resulting from setting up connections. Dr. Tsitovitch, in the laboratory of Prof. Vartanov, performed the following interesting experiment. He took a new-born puppy, and for several months fed it only on milk,

so that the animal had never experienced any other kind of food. After making a salivary fistula in order to observe the work of the glands, he showed the dog ordinary food, not milk. No kind of food at a distance[2] had any effect on the salivary glands. This means that when different foods act on you from a distance the reaction is a reflex which was first formed when, in childhood, you experienced these foods—saw them and then ate them. The matter is thus: when a piece of meat is placed before a puppy several months old, it has no action whatever on the salivary glands—neither its appearance, nor its odour. It is necessary for the meat to enter the mouth, where it evokes a pure, simple conducting reflex, and only then is there consequently formed a new reflex from the appearance and smell of the meat.

Thus, you see, that it is necessary to recognise the existence of two kinds of reflexes. One group of reflexes—ready from the time of birth—are purely conducting reflexes; but the other group—continually and without interruption being formed during the life of the individual, and just as regular as the first group—rest on the basis of another property of the nervous system, *viz.*, its ability to make connections. One reflex can be called inborn, the other acquired; the first generic, the second individual. The congenital, generic, constant, stereotyped one we term *unconditioned;* the other, because it depends upon a multitude of conditions and constantly fluctuates in correspondence with many circumstances, we called *conditioned,* showing in this way its characteristics as expressed from the point of view of the laboratory investigator. The conditioned reflex is also determined and therefore inevitable, and so it belongs, like the unconditioned reflex, entirely to the domain of physiology. By this formulation, physiology naturally comes into possession of an enormous mass of new material, because the number of these conditioned reflexes is legion. Life is made up of a mass of inborn reflexes. Obviously it is only an academic scheme to say that there are three kinds of reflexes—the self-preservative, the food, and the sexual. Their numbers are such that they must be divided and subdivided. Even of the congenital reflexes there are many, but the number of conditioned reflexes is infinite.[3] Consequently, with the establishing of this new definition of conditioned reflexes, physiology lays claim to an enormous territory for investigation. This is a territory of higher activity, connected with the higher centers of the nervous system, while the inborn reflexes are situated in the lower parts of the

[2] For a description of experiments with food at a distance, see chapters i and ii. —*Translator.*

[3] Herrick estimates the number of possible connections of two neurons in the human brain as $10^{2,783,000}$. This is so great that it makes the distance of the farthest star seem almost infinitesimal. (*Brains of Rats and Men,* C. Judson Herrick, 1926. Ch. I.)—*Translator.*

central nervous system. If you remove the cerebral hemispheres of an animal, the simple reflexes remain; but the new, connecting ones disappear. It is evident that if you take into account the conditions under which these conditioned reflexes originate, exist, are masked, are temporarily weakened, etc., then innumerable questions will arise. This is a half of the higher nervous activity, as it is conceived of by contemporary physiology. Now for the other half. It is at once obvious that the nervous system of an animal represents a set of analysers which decompose nature into its separate elements. We are acquainted with physical analysis. The prism splits a beam of white light into the different colours of the spectrum. A resonator divides complicated sounds into the component tones and overtones. Consider the retina, it decomposes the vibrations of light; take the acoustic part of the ear, it analyses the vibrations of air, etc.

Each one of these analysers, in its particular department, continues without limit this division into separate elements. With our ear analysers we classify tones according to their wave lengths, wave amplitudes, wave forms. Thus we have the second function of the nervous system—the analysis of the surrounding medium, the analysis of different complexities of the world into their separate parts. This analysis proceeds even in the lower sections of the central nervous system. If an animal is decapitated and only the spinal cord remains, analysis occurs just the same. Let a mechanical, thermal or chemical stimulus act on such an animal and by each a special movement is provoked. In the higher sections of the central nervous system, in the cerebral hemispheres, there takes place the finest analysis of which either animal or man is capable. This subject, moreover, is purely physiological. Being a physiologist, I never need in the study of this subject any kind of definition or conception foreign to physiology.

In the investigation of the analysers, situated in the cerebral hemispheres, very important facts are disclosed. For example, such a one: At first when a new reflex is formed from some sound, as a rule, the new stimulus appears in a general form, *i.e.*, if you have formed a conditioned reflex from a certain tone, say of 1,000 vibrations, and now if you try other tones, for example, of 5,000, 500, 50 vibrations, in the beginning you will get an action from each of them. Always at first the greater part of the analyser enters into the reflex. Only later with the repetition of the reflex does there occur *specialisation.* This is one of the important laws. It is clear that also this fact can be investigated without having recourse to any extraneous conception. It is well here to refer to our investigation of the limits of the analysing faculty. It has been proved, for example, that the analyser of the dog can recognise one-eighth of a tone. The sensitiveness of the auditory apparatus of the

dog for tones is comparatively much greater than ours. We distinguish sounds up to 50,000 vibrations, while the apparatus of the dog functions for sounds as high as 100,000. I shall remind you of the following interesting fact. Where there is an injury in the cerebral hemispheres at the site of the corresponding terminals of the visual, auditory, etc., analysers there is of course a break in the apparatus. A dog having a damaged eye analyser, for example, does not recognise his master. But he will avoid colliding with his master just as he would avoid striking against a chair. Regarding this, it has been said that the dog sees but does not understand. It must be admitted, however, that this phrase is difficult to comprehend if we consider it carefully.

When, in this case, it is said that the dog sees but does not understand, the trouble is that the analysing apparatus has been broken to such a degree that the analysing ability is reduced to the minimum. The eye distinguishes only light from shadow, a free space from that occupied by a body, but not the forms and colours of objects.

Thus we recognise in the higher animals two sorts of the higher nervous activity; first, the *Formation of New Connections* with the external world; and secondly, *Higher Analysis.*

If you consider carefully these two kinds of activity, you see that they embrace a good deal and it is difficult to represent what remains outside of them. Only a detailed study can determine this. All training, all education, habits, orientations in the surrounding world with all its events in nature and among people—all this is either a formation of new connections, or the finest analysis. Without doubt very much is contained in these two activities. At all events the work here is limitless, and still we physiologists do not employ any foreign conceptions.

In the study of the above kinds of activity it has been proved that the first important property of the higher cerebral mass is a peculiar movement of the nervous processes in this mass. I shall describe it later and in detail. Another exceedingly important property appeared, *viz.,* that if in the higher parts of the cerebral hemispheres, there is an element quite isolated functionally and if upon it beats a certain excitation proceeding from some agent, then before long there inevitably results the *Inactive State,* the state of sleep or of hypnosis. The fundamental property of the higher nervous mass is this extreme reactivity, but if there is temporary isolation, if the excitation instead of following off to the sides concentrates for a time, *i.e.,* if the excitation acts continuously at one point, then this element unfailingly passes over into the sleeping state. Many things are explained from such a relation of the higher nerve cells to the stimuli. This relation may be conceived of either as a sort of preservation of the precious substances of the cerebral hemispheres, of the substances which must constantly respond to all the

influences of the outer world; or it may be understood in the biological sense, *i.e.*, that if the stimulus changes every moment, you must react to it by a definite activity, but if it becomes monotonous, then without important consequences you may rest, recuperating for a new expenditure. I shall not enter into details.

Now I come to the end. I shall refer to an experiment which partly illustrates those facts I have mentioned. In particular I wish to hear your opinions about these facts, about this experiment. First I shall make the following request. Perhaps some of my descriptions will not be clear enough; then ask me at once, so that you may understand the whole experiment as easily as if you were present and saw it yourself.

Here you see a diagram of our animal. On it are two black spots, one on the front leg, one on the thigh of the hind leg. These are the places where we attached the apparatus for mechanical stimulation of the skin. We proceeded as follows. After we have started mechanical irritation of these places with the pricking apparatus, then acid is poured into the mouth of the dog. The secretion of saliva produced by the acid is, of course, a simple inborn reflex. This was repeated several times, yesterday, to-day, and day after day. . . . After a number of experiments a state of affairs results in which we get a flow of saliva when we begin only to irritate that spot of the skin; it is just as if we had poured acid into the dog's mouth, though in reality no acid is given.

Now I come to the discussion of our fact, and will do it physiologically and then as far as I can possibly psychologically, as a zoö-psychologist would do it. I can not guarantee that I shall use the correct phrases, because I am out of practice in these expressions, but I shall approximate to those I have heard from others. The facts are these. I apply lightly the mechanical irritation of the skin and then give the acid. Saliva is secreted—the simple reflex. When this has been repeated several times, then only the mechanical irritation of the skin is necessary to call out the flow of saliva. Our explanation was that a new reflex was formed, a new nerve path was made between the skin and the salivary glands. The zoö-psychologist, who wants to penetrate into the dog's soul, says that the dog directed his attention and remembered that when he felt the irritation of the skin at a certain place he would receive the acid and, therefore, when there was only irritation of the skin, he imagined the acid was coming, and he reacted correspondingly —saliva flowed, etc. Let it be so. But let us proceed further. We shall perform another experiment. We had elaborated a reflex and every time it gave perfectly accurate results. Now I start the mechanical irritation and receive as formerly a complete motor and secretory reaction, but this time I do not give the acid. One or two minutes pass and I repeat the experiment. Now the action already is less, the

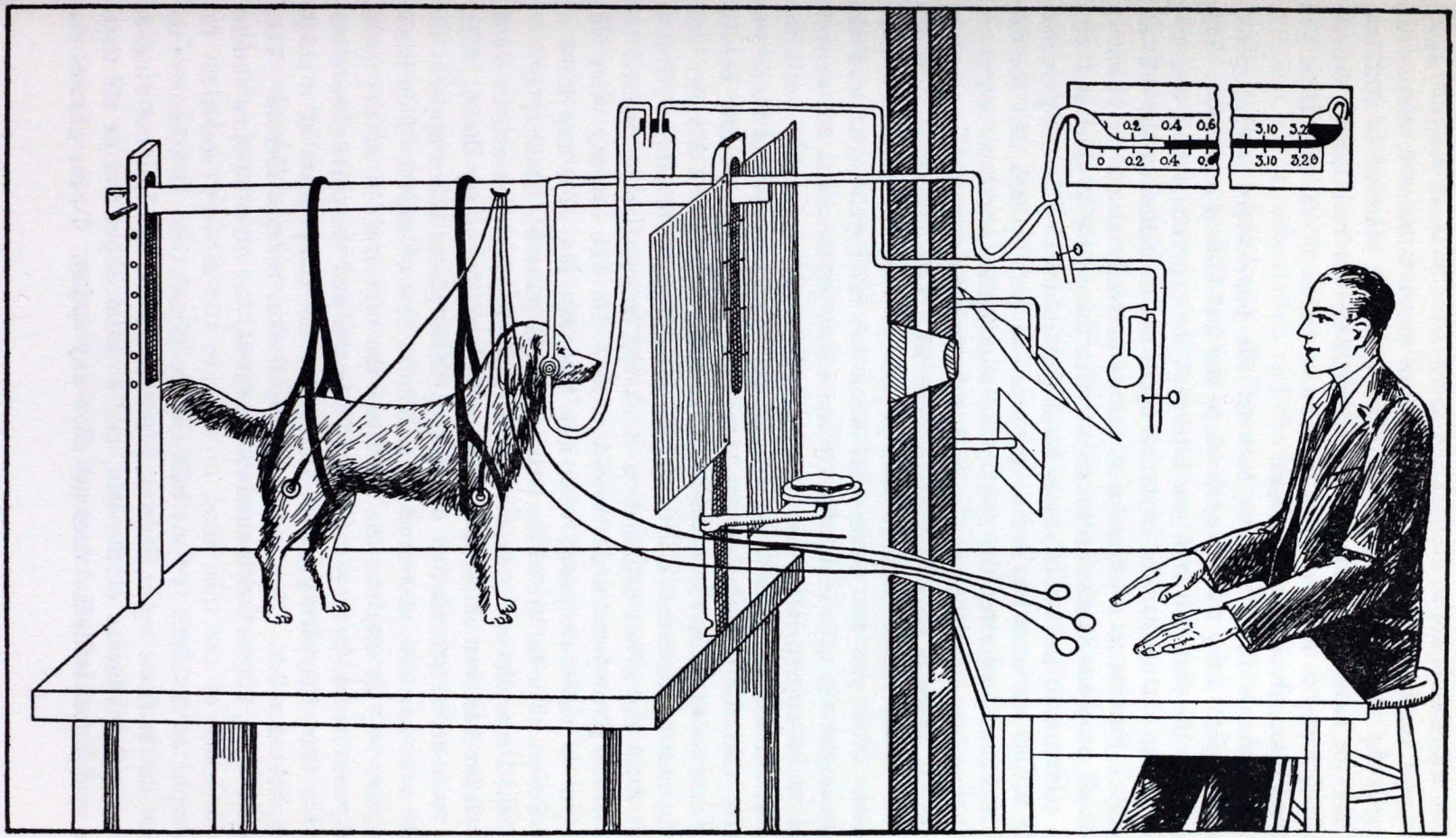

FIG. 7.—DIAGRAM ILLUSTRATING EXPERIMENTATION ON THE DOG

motor reaction is not so marked and there is not so much saliva. Again the acid is not given. We allow two or three minutes to elapse and repeat the mechanical irritation. The resulting reaction is still less. When we have done this four or five times, the reaction is entirely absent; there is no movement and no secretion of saliva. Here you have a clear, absolutely exact fact.

But here is the difference between the physiologist and the zoö-psychologist. I say that there develops our well-known inhibition. This I base on the fact that if I now interrupt the experiment and wait two hours, then the mechanical irritation again has its action on the salivary glands. For me as a physiologist this is perfectly clear. It is known that all processes in the nervous system in the course of time and with the cessation of the active causes become obliterated. The zoö-psychologist is also not at a loss for an explanation, and he says that the dog noticed that now after the mechanical stimulation acid was not given, and therefore after four or five such skin irritations he ceases to react.

So far there is no difference between us. You can agree with one as well as the other. But we shall proceed to more complicated experiments. Now you are aware that when the zoö-psychologist and the physiologist vie with each other to see whose explanations are correct, and more appropriate, then we must be well acquainted with the conditions which the facts are to explain. The prerequisite is, as you know, that the explanation should account for all that really occurs. The facts must all be explained without changing the point of view. This is the first requirement, and the second is even more obligatory. This is that from the given explanation it should be possible to foretell the explained phenomena under consideration. He who can say what will happen is right compared with him who can not give any kind of prediction. The failure of the latter here will mean his bankruptcy.

I shall complicate my experiment as follows. I have a dog in which our reflex has been elaborated at several places, let us say three. After the mechanical stimulation of each of these places there appears the same acid reaction, measured by a definite flow of saliva. This is the simplest way to measure the reaction; the measurement of the motor component would be more difficult. The motor and the salivary reactions go together, they are parallel. They are the components of a single complicated reflex. Now we have several skin reflexes formed. They are all equal, they act with absolute exactness, they give the same number of divisions of the tube used to measure the salivary secretion, for example, 30 divisions for one half-minute stimulation. I stimulate the place on the front leg in the way I have just said, *i.e.*, I do not combine it with the influence of the acid, and so after about five or six times the mechanical irritation does not show any action. To the physiologist

this means that I have obtained a complete inhibition of the reflex. When this has happened to the place on the front leg, I can stimulate another spot on the hind leg. And there developed such phenomena. If now I take the mechanical stimulation on the thigh—just as I did on the front leg, where I got zero—so that there is no interval between the end of that stimulation and the beginning of this, then at the new place I obtain a full action, 30 divisions on our tube, and the dog behaves as if this were the first application of the stimulus. Saliva flows freely, the motor reaction occurs, the dog acting as if he were rejecting acid from the mouth with the tongue, although there is no acid present—in short, the whole reaction appears. If in the next experiment I try the effect of the irritation on the front leg until again there is no secretion (by repeating the mechanical stimulation without giving acid), and then irritate the place on the hind leg, not after zero seconds but after five seconds, then I receive not 30 divisions from the new place but only 20. The reflex has become weaker. The next time I use an interval of fifteen seconds, and I get a slight action from the new place,—5 divisions. Finally if I stimulate after twenty seconds there is no action whatever. If I go further and employ a great interval, thirty seconds, then again I get an action from this place. With an interval of about fifty seconds, there is considerable secretion, 25 divisions, and with an interval of sixty seconds we see the full reaction. On the same place, on the shoulder, after we obtained zero result, if the irritation is repeated with an interval of five, ten, fifteen minutes, then we get zero (I do not know if I have made this clear to you). What does this mean?

I invite the zoö-psychologists to give their explanation of these data. More than once I have questioned intelligent people, having a scientific education—doctors, etc., about these same facts, and asked them for an explanation of the phenomena. The majority of the naïve zoö-psychologists gave explanations, but each one his own, and different from the others. In general the result was disastrous. They examined the facts as much as possible, but there was no way of making the various interpretations agree. Why is it that on the shoulder, when the experiment was so conducted that we got zero, the apparatus produced no further action, but here at the other place we obtain now a full action, now nothing, in a fine dependence upon different intervals of time between the stimuli?

I came here to get an answer to this question from the point of view of the zoö-psychologists.

Now I shall tell you what we think. Our explanation is purely physiological, purely objective, purely *spatial*. It is obvious that in our case the skin is a projection of the brain mass. The different points of the skin are a projection of the points of the brain. When at a

certain point of the brain, through the corresponding skin area on the shoulder, I evoke a definite nervous process, then it does not remain there, but makes a considerable excursion. It first *irradiates* over the brain mass, and then returns, *concentrating* at its point of origin. Both of these movements naturally require time. Having produced inhibition at the point of the brain corresponding to the shoulder, when I stimulated another place (the thigh) I found the inhibition had not yet spread this far. After twenty seconds it had gotten here; and in twenty seconds, though not before, complete inhibition occurred at this point. The concentration required forty seconds, and after sixty seconds from the end of the zero irritation on the shoulder, we already had a complete restoration of the reflex, on the second spot (the thigh). But on the primary place (the shoulder) the reflex was not yet restored even after five to ten or fifteen minutes.

This is my interpretation, the interpretation of a physiologist. I have had no difficulty in explaining these facts. For me it fits in perfectly with other facts in the physiology of the nervous process.

Now, gentlemen, we shall test the truth of this explanation. I have a means of verifying it. If actually we have a movement, then consequently in all the intervening points we should be able to predict the effect, judging by the fact that this movement occurs in two directions. I take only one intermediate point. What is to be expected at this place? In proportion to its proximity to that area where I produce the inhibition it will be inhibited. Consequently in it the zero effect appears sooner and lasts longer—while the inhibition passes further and then recedes. At this spot the return to the normal irritability occurs later. Thus it came to pass in the actual experiment. Here at the middle point after an interval of zero seconds, there were not 30 but 20 divisions. Then the zero effect appeared already after ten seconds, when the full inhibition had reached here, and this effect remained for a long time, both while the inhibition was spreading further, and also when it was contracting, and passing in the opposite direction. It is clear why on the shoulder the normal reactivity returned after one minute, but here only after two minutes.

This is one of the most astonishing facts that I have seen in the laboratory. In the depth of the brain mass there occurs a special process, and its movement can be mathematically foretold.

So here, gentlemen, is the complexity of our experiment, and its relation to the physiologist. I do not know how the zoö-psychologists will answer me, how they will consider these facts, but answer them they must. If, indeed, they refuse to give an explanation, then with full justice I can say that their point of view is in general unscientific, and unsuitable for accurate investigation.

CHAPTER XXVII

THE REFLEX OF PURPOSE

(Read before the Congress of Experimental Pedagogy in Petrograd, January 2, 1916.)

THE REFLEX (INSTINCT) OF PURPOSE—COLLECTING IN ITS BROAD SENSE IS A FORM OF THIS REFLEX—ITS RELATION TO OTHER REFLEXES (FOR EXAMPLE, THE INSTINCT OF LIFE, THE FOCUSING REFLEX, THE FOOD REFLEX), AND ITS ORIGIN FROM THE GRASPING REFLEX —VALUE OF RHYTHM AND PERIODICITY IN WORK, HABITS, DIETETICS—THE REFLEX OF PURPOSE GIVES INTEREST TO LIFE—ITS INTENSITY IN THE ANGLO-SAXON—ITS SUPPRESSION—THE RUSSIAN CHARACTER.

MANY years ago I with my collaborators began to make a physiological (*i.e.*, strictly objective) analysis of the higher nervous activity of the dog. One of our tasks was the establishment and systematisation of those simplest and most fundamental activities of the nervous system with which the animal is born, and upon which, during the life of the individual, there are built up by means of special processes the more complicated reactions. The inborn basic nervous activity is a group of constant, regular reactions of the organism to definite external or internal stimulations. These reactions are called reflexes and instincts. Most physiologists, not being able to find any essential difference between the so-called *reflex and the instinct,* prefer the general term, "reflex," because in it there is a clearer idea of determinism, a less doubtful relation of the stimulus to the response, of cause to effect. I also prefer to use the word reflex, allowing others, if they please, to substitute the word instinct.[1]

[1] Physiological evidence in favour of the fact that the so-called instincts are nothing but complex reflexes is given in chapter xxv, paragraph 9, and in chapter i of *Activity of the Cerebral Hemispheres.* Pavlov says:

It is necessary to add to the group of ordinary reflexes another group of inborn reflexes. They also take place in the nervous system, and they are inevitable reactions to perfectly definite stimuli. They involve the reaction of the organism as a whole, and make up the general behaviour of animals described as instinctive. . . . Between the simplest reflex and an instinct are numerous stages of transition, and among these it is difficult to find a line of demarcation. For example, consider the newly hatched chick. To any speck it sees it reacts by pecking, regardless of whether this be some object or only a spot on the ground. But how does this differ from turning the head, or closing the eyelids when something flashes by? This latter we call the reflex of defence; the first is termed the instinct of feeding, although pecking is only an inclination of the head and a movement of the beak.

Instincts are said to be more complex than reflexes. But there are very complicated reflexes which could never be mistaken for instincts, for example, vomiting. This is very complex, involving the co-ordination of many muscles (both skeletal and visceral) spread over a large area, and ordinarily used in entirely different functions of the organism. It also involves a secretory reaction of certain glands, whose activity generally serves another purpose.

The long time of action constituting certain instincts has been assumed to be a

An analysis of the activity of animals and of human beings leads me to the conclusion that among the reflexes there must exist a special one—the reflex of purpose—an aspiration to the attainment of a definite exciting object, using attainment and object in the broad sense of the words.

I take the liberty to present for your consideration a comparison of facts obtained in the laboratory with facts drawn from human life, which bear, it seems to me, on the reflex of purpose.

Human life consists in the attainment of every possible sort of purpose, high, low, important, unimportant, etc., to which is applied every degree of human energy. Attention is called to the fact that in this attainment there exists no constant relation between the amount of energy spent and the value of the object: now and then on absolutely trivial purposes there is expended an enormous amount of energy, and vice versa. This is often observed in individuals who work with the same ardour for great as well as for simple things. The conclusion follows that it is necessary to distinguish between the act of striving and the

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point of contrast, as the reflex is regarded as always being simple in its construction. As an example let us take the building of nests or of animal lairs. The events are linked up as a chain: the gathering of the material, its transportation to the chosen place, its arrangement and reinforcement. To look upon this as a reflex it is necessary to assume that one reflex initiates the following, *i.e.*, that it is a *chain* reflex. But this linking up of activities is not peculiar to instincts alone. There are numerous reflexes which form chains. If we stimulate some afferent nerve, for example, the sciatic, reflexly there is a rise of blood pressure; the high blood pressure in the left ventricle of the heart and the first part of the aorta is the effective stimulus of a second reflex, a depressor reflex, which tends to neutralise the first. Again, there are the chain reflexes described by Magnus. A cat thrown from a height will in most cases land on its feet, and even after the cerebral hemispheres have been removed. How is this effected? A change in the spatial relations of the otolithic organ evokes a definite reflex, a contraction of the neck muscles, restoring the animal's head to the normal position. This is the first reflex. With the righting of the head another reflex is called out, bringing the animal to the standing posture. This is the second reflex.

Then it has been argued that there is this difference; that instincts depend upon the internal state of the animal. A bird builds its nest, for example, only at mating time; or a simpler case—a glutted animal is no longer attracted by food, and stops feeding. The sexual impulse is subject to a like variation, depending on the age of the organism and the state of the reproductive glands, and to a considerable extent upon the presence of hormones—the products of the glands of internal secretion. But such a dependence is not a property of instincts alone. The intensity of every reflex, and even its very presence, varies with the irritability of the centres, which in turn depends upon the physical and chemical properties of the blood (automatic stimulation) and upon the interaction of other reflexes.

Finally, it is sometimes held that reflexes determine only the activities of tissues or of single organs, whereas instincts involve the activity of the organism as a whole. However, from the recent investigations of Magnus and de Kleijn we know that standing, walking, and the maintenance of equilibrium are nothing more than reflexes.

Thus we see that reflexes and instincts are both inevitable responses of the organism to internal and external stimuli, and therefore we do not need to use different terms for them. Reflex has the better claim of the two, because it has had from the beginning a purely scientific connotation.

The sum total of reflexes is the foundation of the nervous activities of man and or animals.—*Translator.*

meaning and value of the object, and that the essence of the matter consists in the striving—the thing striven for is of secondary consideration.

The most pure and typical of all the forms of the reflex of purpose, and therefore especially convenient for analysis, and also the most widespread, is the passion for *collecting*—the aspiration to gather the parts or units of a great whole or of an enormous classification, usually unattainable.

As is known, collecting is observed in animals. It is also seen especially in the age of childhood, when the basic nervous activities are manifested most clearly, because they are not yet submerged by the elaborations and conventions of life. If we consider collecting in all its variations, it is impossible not to be struck with the fact that on account of this passion there are accumulated often completely trivial and worthless things, which represent absolutely no value from any point of view other than the gratification of propensity to collect. Notwithstanding the worthlessness of the goal, every one is aware of the energy, the occasional unlimited self-sacrifice, with which the collector achieves his purpose. He may become a laughing-stock, a butt of ridicule, a criminal, he may suppress his fundamental needs, all for the sake of his collection. How often do we read in the newspapers of misers—collectors of money who in the midst of gold die alone, in dirt, cold, and hunger, hated and neglected by their fellows and kin. One must conclude that this is a dark, primitive, insuperable tendency—an instinct or reflex. Every collector hoarding his treasures, if he has not lost the ability to observe himself, is well aware that he will be directly attracted to the next article of his collection, just as he is attracted after a certain period of eating to another bit of food.

How does this reflex arise, and what is its relation to other reflexes?

The problem is difficult, as questions of origin generally are. I shall express a few considerations relative to the question, which have, it seems to me, significant weight.

All life is nothing other than the realisation of one purpose, *viz.*, the preservation of life itself, the tireless labour of which may be called the general *instinct of life*. This general instinct or reflex consists of a number of separate ones. The majority of these reflexes are positive *movement reflexes* toward the conditions favourable for life, reflexes whose object is to seize and appropriate such conditions for the given organism, grasping and catching reflexes. I shall dwell on two of them, as the most general and also the strongest, accompanying human life as well as the life of every animal, from his first day to his last. These are the food and the *focusing* (orienting, investigating) reflexes.

Every day we strive for certain substances necessary for us as a ma-

terial for completing our vital chemical processes; we introduce this material into our bodies, become quiet for the time being, and strive again some hours later or the next day to grasp a new portion of this material, *food.* Every new stimulus acting on us calls out, on our side, a corresponding movement in order better and more fully to inform ourselves regarding this stimulus. We attend to every figure that appears, listen to all sounds that arise, strongly sniff strange odours, and if a new object is close to us we endeavour to touch it; in general, we try to grasp or test every new phenomenon or object with the appropriate receptor surfaces, the corresponding sense organs. How strong and impelling are our propensities to touch an interesting object is evident from those obstacles, requests and prohibitions which are necessary to preserve articles on exhibition in shops, etc., from even the cultured public.

As a result of the daily and tireless practice of these *grasping* and many other similar reflexes, there originated, and was fixed by heredity, a common, generalised grasping reflex relating to every object as soon as it attracts the marked attention of the human being. This generalisation may occur in various ways. We may imagine two mechanisms of its origin. First, by irradiation, or the spreading of the excitation from one or another grasping reflex in case it is of great intensity. Adults as well as children, having a strong appetite, *i.e.,* in the presence of an intense food reflex, often take into the mouth and masticate inedible substances if there is no food, and the child during the first months of life puts into his mouth all kinds of objects. Further, in many cases, as a consequence of the time coincidence, there should occur an association of many objects with various grasping reflexes.

That the reflex of purpose and its typical form—the collecting reflex—stand in a certain relation to the *chief grasping reflex*—the food reflex—can be seen from their mutual properties. In both cases the most important feature, accompanied by definite symptoms, is the striving toward an object. On attaining it there begins a quickly developing calm and indifference. The other essential feature is the periodicity of both reflexes. Every one knows from his personal experience that the nervous system is capable of adapting itself to a certain order, *rhythm,* and time of activity. How difficult is it to change one's accustomed rhythm of walking, or of speaking, etc. In the laboratory, studying the complex nervous phenomena of animals, many gross mistakes can be made if one does not carefully reckon with this disposition. Therefore, the great strength of the reflex of purpose as expressed in collecting can be seen precisely in the coincidence of the obligatory periodicity of collecting with the periodicity of the food reflex.

Just as after a certain period of abstinence from food there will be

again the desire for more, so after acquiring certain things, for instance postage stamps, there will undoubtedly be a wish to obtain others. That *periodicity* in the reflex of purpose constitutes an important point is shown by the fact that people usually divide their incessant tasks and problems into parts, lessons, etc., *i.e.*, they desire conditions of periodicity. This is especially favourable to the conservation of energy, and facilitates the final attainment of the purpose.

The reflex of purpose is of great and vital importance, it is the fundamental form of the life energy of us all. Life is beautiful and strong only to him who during his whole existence strives toward the always desirable but ever inaccessible goal, or who passes from one purpose to another with equal ardour. All life, all its improvement and progress, all its culture are effected through the reflex of purpose, are realised only by those who strive to put into life a purpose. And indeed everything can be collected, the trivial as well as the important: the comforts of life (the aim of practical people), right laws (aspired to by statesmen), knowledge (the goal of educated people), discoveries (the treasure of scientists), virtues (the ideal of righteous people), etc.

And now the converse—life ceases to be attractive as soon as the purpose disappears. Do we not often read in the letters of suicides that they ended life because they found it purposeless? The purposes of human life are, of course, unlimited and inexhaustible. The tragedy of the suicide lies in the fact that he has an inhibition, as we physiologists would call it, of the reflex of purpose—most often a momentary and only rarely a continued inhibition.

The reflex of purpose is not immutable, but like everything else in the organism, it fluctuates and changes according to conditions, either by becoming stronger and developing, or by becoming weaker and almost completely disappearing. Here again the analogy with the food reflex is easily demonstrable. With a regular dietetic régime—a proper amount of food and a *periodicity* in taking it—there is always maintained a good appetite, a normal food reflex and normal nutrition. We can all recall ordinary cases like the following. The child can be easily excited by talking about food, and even more so by seeing it, with the result that the food reflex appears at an earlier period than necessary. The child looks for food, asks and even cries for it. And if the mother sentimentally but not wisely satisfies these impulses and wishes, it will end in the child's getting his food fitfully and inevitably before the period for eating, ruining his appetite, taking the main nourishment without relish, eating on the whole less than he should, and, if this disorder is often repeated, in the impairment of his digestion and nutrition. Finally his appetite, *i.e.*, the striving toward food, the food reflex, will weaken or entirely disappear.

You see that for the full, regular and successful manifestation of the reflex of purpose a *certain intensity* is necessary. The Anglo-Saxon, the highest personification of this reflex, knows this very well, and this is why to the question, "What is the chief condition for attaining a goal?" he gives the answer—unexpected and incredible for Russian ears and eyes —"The existence of obstacles". He seems to say, "Let my reflex of purpose be put to the test, let it be strained in overcoming obstacles, and then I will reach my goal no matter how difficult of attainment it may be". It is noteworthy that in the answer the impossibility of achieving the goal is completely ignored. How different is this from us, to whom "circumstances" excuse everything, justify everything, and are reconciled with everything. To what a great degree do we lack a practical conception of this most important factor of life, the reflex of purpose!! And this conception is so very necessary for all stages of life, beginning with such an important one as training.

The reflex of purpose may be weakened and even *suppressed* by a reverse mechanism. Let us turn again to the analogy of the food reflex. As is known, the appetite is strong and unbearable in only the first days of starvation; then it becomes much weaker. Exactly the same happens as a result of continued undernutrition—weakness of the organism, de crease of its strength, and together with this the failing of its fundamental and normal impulses, as we know from those who systematically fast. With continued limiting of the gratification of the fundamental impulses, with constant reduction of the activity of the chief reflexes, there fails even the instinct of living, the attachment to life itself. We know how indifferent the low and poorer classes are to dying. There exists in China, if I am not mistaken, the possibility of hiring some one to be executed for another.

When the negative features of the Russian character—laziness, lack of enterprise, and even slovenly relations to every vital work—provoke melancholy moods, I say to myself, No, these are not our real qualities, they are only the veneering, the damning inheritance of slavery. It made a parasite of the master, freeing him, through the unpaid work of others, from the practice of natural and normal striving to obtain his daily bread for himself and family, from the necessity of making his way in life; and it left the reflex of purpose without exercise in the fundamental habits of living. Of the slave it made a completely passive creature, without any vital perspective; for continually in the way of his most natural aspirations arose an insurmountable obstacle in the form of the powerful egoism and caprice of his master. But I venture further: A spoiled appetite and poor nutrition may be *restored* by a careful régime and special hygiene. The same can and should happen with the reflex of purpose which has been suppressed during Russia's

history. If every one of us will cherish within himself this reflex, as the most precious part of his being, if parents and instructors of all ranks will make their chief problem the strengthening and developing of this reflex in the plastic masses, if our society and state will provide a full opportunity for the exercise of this reflex, then we shall become that which we should and can be, judging from many episodes of our history and from some strokes of our creative strength.[2]

[2] This and the following chapter show that Pavlov, instead of allowing science to kill his aspirations and finer feelings, considers that it will prove to be the deliverer of the human race. In regard to the freedom of the will, he remarked to the translator that real freedom would come in proportion to our knowledge of the physiology of the brain, and that we would attain a victory then over our own natures as we have done over nature, through scientific knowledge.—*Translator.*

CHAPTER XXVIII

THE REFLEX OF FREEDOM

(Read before the Petrograd Biological Society, May, 1917.)

ACQUIRED REFLEXES ARE BUILT ON THE INBORN REFLEXES—NEED FOR A CLASSIFICATION OF REFLEXES—DISCOVERY OF THE REFLEX OF FREEDOM (A CONGENITAL REFLEX) IN THE DOG—ITS SUPPRESSION BY THE FOOD REFLEX—THE REFLEX OF SLAVERY AND ITS APPLICATION TO RUSSIA.

In analysing the normal nervous reactions one has the right to say that physiology at last has shown that the complicated nervous (psychical) activity is, like the lower, made up of reflex acts. Furthermore, it has succeeded in establishing in addition to the formerly scientifically proved, elementary, fundamental form of nervous activity—the inborn reflex—another, also fundamental, but more complicated form—the acquired reflex. The further study of the subject must be carried out along the following lines. On the one hand, it is necessary first to establish and systematise all the inborn reflexes as basic and unchanging fundamentals on which is built up the enormous structure of the acquired reflexes. The systematisation of the acquired reflexes must have as its basis a classification of the inborn reflexes. This forms, so to speak, the morphology of reflex action. On the other hand, there must be made a study of the laws and mechanism of the acquired as well as of the inborn reflex activity. The investigation of the latter was begun long ago and is still going on; the study of acquired activity is new and just beginning. It is destined to attract much attention; for it promises quick and abundant results.

To-day our report concerns the systematising of the reflexes, particularly the inborn. It is obvious that the existing, common classification of reflexes * into food, self-preserving, sexual, etc., is too general and inexact. In order to be precise it is necessary to speak of a preservative individual and a preservative generic reflex, as the food reflex is also preservative. But our division is likewise partly conditional, for the preservation of the species presupposes also a preservation of the individual. Consequently, there is no particular value in a general systematisation. Instead there is great need of a detailed systematisation, a careful description, and a full enumeration of all reflexes, because under every known general reflex there is a multitude of separate ones. Only

* On closer analysis reflexes and so-called instincts show no fundamental and essential differences.

the knowledge of all the separate reflexes will give the possibility of gradually clearing up this chaos of phenomena of higher animal life, which now at last is falling into the order of a scientific analysis.

Although we have not made a special study of this field, still we have not neglected opportunities to observe instances, if they were marked, which appeared incidentally during other investigations. We have examined such a case in the reflex of freedom.

One of our many dogs, used during the past year for the study of acquired, or conditioned, salivary reflexes exhibited especial characteristics. This animal when first used by us for experimentation gave, when placed on the stand, in distinction from all other dogs, a spontaneous and constant secretion of saliva during an entire month. This, of course, rendered it unsuitable for our experiments. This secretion of saliva is, as we know from previous observations, dependent upon a general excitation of the animal, and is usually accompanied by dyspnœa. Such excitation of the dog is evidently analogous to the state of excitation in the man, where it is manifested, however, by sweating instead of by salivation. A short period of such excitation is seen in many of our dogs during the first experiments with them, and especially among the untamed and wilder of them. But on the contrary, the dog in question was very tame and quickly became friendly with us all. That made it even more strange that for a month the excitation in the experimental stand did not diminish to any degree. Then we undertook to study more closely the peculiarity of this dog. During two weeks when we kept him in a separate experimental room while forming conditioned food reflexes, his nature did not change. The conditioned reflex formed slowly, remained weak, and always fluctuated. The spontaneous salivary secretion continued, and gradually increased with each experimental séance. Also the animal constantly moved, struggling in every possible way in the stand, scratching the floor, and pulling and biting at the frame, etc. This was accompanied by dyspnœa, always increasing towards the end of the experiment. At the beginning of the séance with the first conditioned stimuli, the dog immediately took the offered food, but later he either took it at a more or less considerable interval after the opening of the feeding box, or began to eat only after a preliminary forceful introduction of a portion into the mouth.

We endeavoured first to answer the question, what precisely calls forth this motor and secretory reaction, which of the surroundings excite the dog?

Standing upright on a table acts on many dogs as a stimulus. Removing the stand from the table to the floor is enough to quiet them. But this caused no change in our dog. Some dogs do not tolerate solitude. As long as the experimenter is sitting in the same room, the

dog is quiet; but if he leaves the room, the dog becomes immediately excited, struggling and whining. Again this had no influence upon our dog. Perhaps being a sensitive dog, he required movement: but when freed from the stand he would often immediately lie down at the experimenter's feet. Possibly his harness excited him by pressure or friction, etc.? The harness was loosened everywhere, but the state of the animal remained the same. But with freedom, even though the cord was about the neck, the dog was quiet. We varied the conditions in every possible way. One thing was evident—the dog did not tolerate being tied, nor having the freedom of his movements limited. Before us there was sharply emphasised and well isolated the physiological reaction of the dog—the *reflex of freedom.* In such a pure and constant form this reflex in dogs had been noted only once by one of us—although before us have passed many hundreds or even thousands of dogs—but then the fact was not appreciated because the conception of the reflex was lacking. In all probability the perseverance of the reflex in these two dogs was due to a rare chance that some of the preceding generations, both on the mother's and father's sides, were untrained curs, and never having been tied, were accustomed to full freedom.

The reflex of freedom is of course a common trait, a general reaction of animals, and one of the most important of inborn reflexes. Lacking this, every trivial obstacle which confronts an animal would entirely interrupt the course of his life. And this we well know; for all animals deprived of their usual freedom strive to liberate themselves, especially wild animals captured for the first time. This fact, which was so well known, had received no proper denotation, and had not been included in the classification of congenital reflexes.

In order to place more emphasis on the inborn, reflex character of our reaction, we proceeded further with its investigation. Although the conditioned reflex which was elaborated on this dog, was the food reflex, *i.e.,* the dog was starved for about 20 hours preceding the experiment, and was fed on the stand during each conditioned stimulus, even this was not sufficient to suppress and overcome the reflex of freedom. This was even more strange, because we in the laboratory were already acquainted with conditioned food reflexes to destructive stimuli,[1] when, for example, strong electrical destruction of the skin, usually provoking a marked defensive reaction, is always accompanied by feeding, there can be elaborated without especial difficulty the food reaction with the complete disappearance of the defensive. Is the food reflex weaker than

[1] As "destructive stimuli" Prof. Pavlov designates stimuli of such strength as to cause destruction at their point of application. Of course such stimuli are always accompanied by pain and are commonly known as pain stimuli.—*Translator.*

the reflex of freedom? Why will not the food reaction now conquer the reflex of freedom? But it was impossible in our experiments not to notice the difference between the conditioned destructive (pain) reflex and the present one: in the former the food and the destructive reflex occurred at almost exactly the same time; in the latter, the stimulation of the food in the oral cavity took place at great intervals and continued for only a short while, but the reflex of freedom acted during the whole time of the experiment, and became stronger the longer the dog remained on the stand.

Therefore, in the further continuance of the experiments with conditioned reflexes we decided to give the dog all his daily food only on the table. When fed on the stage, for the first ten days the dog ate little and became thin; but later he began to eat more and more, and finally took all the food that was given. Three months elapsed, notwithstanding, before the reflex of freedom failed to show itself during the experiments with conditioned reflexes. The separate parts of this reflex disappeared gradually. Small traces of it apparently were expressed by the conditioned reflexes, which had every other reason to be great and constant in this dog, but which remained weak and fluctuant, as if partially inhibited. This was evidently a remnant of the reflex of freedom. It is interesting that at the end of this period the dog began to jump voluntarily on the experimental table. We went further; we again abolished the daily feeding of the dog on the stand. After one and a half months of continued experimentation with conditioned reflexes, the reflex of freedom began to reappear, finally attaining the same strength as it had at the outset. Besides the indisputable confirmation of the enduring character of this reflex, pointing to its congenital nature, we believe that its recurrence annuls all other interpretations of the described reaction.

Only after keeping the dog for four and one-half months in a separate cage where he was fed, did the reflex of freedom become finally suppressed. Then it was possible to work as easily with this dog as with others.

We again insist on the necessity of describing and enumerating the elementary inborn reflexes, in order gradually to understand the whole conduct of the animal. Without such a classification we have only the usual empty conceptions and words: "the animal forms and breaks habits, remembers, forgets, etc.," but we never arrive at a scientific study of the complex activity of life. There is no doubt that a systematic study of the fund of inborn reactions of the animal will greatly favour an understanding of ourselves and the development in us of the ability of self-guidance. It is clear that together with the reflex of freedom there exists also an inborn reflex of slavish submission. It is a well

known fact that puppies and small dogs often fall on their backs in the presence of larger ones. This is the surrendering of self to the wishes of the strong, the analogy of the man's falling on his knees, or prone on his face—the reflex of slavery. This has, of course, a use in life. The intentional passive attitude of the weak leads to a natural decrease of the aggressive action of the strong, while even an ineffective resistance tends to increase the destructive ambition of the strong. How often and varied appears the reflex of slavery on Russian soil, and how useful to recognise it! Let us take an example from literature. In Kuprin's story, *River of Life,* there is described the suicide of a student who was tormented by his conscience after having betrayed his companions to the police. From a letter of the suicide it was evident that he was made a victim of the reflex of slavery inherited from his mother, who was a *prijivalka.*[2] If he had had an insight into his condition, he would first have understood his limitation, and secondly he might by systematic measures have developed control and successful suppression of this reflex.

[2] *Prijivalka* is the name of the higher servants formerly attached to rich Russian families. They lived as parasites whose duties were to cater to every caprice of the mistress.—*Translator.*