der usual conditions the effect of the corresponding stimulus disappears, it is still possible to re-establish it in a very weak form in the course of the following experiment. At first the corresponding stimulus is applied several times, being reinforced each time almost immediately after the beginning of its action (in one or two seconds); then, when there is a considerable delay (twenty to thirty seconds), a positive effect is observed immediately after the beginning of the stimulation, which, however, declines rapidly, falling even to zero by the end of the isolated action of the stimulus. This is an obvious manifestation of the weakness of the excitatory process. Finally, the third case which is simply a struggle of opposite processes; the isolated action of the conditioned stimuli leads first to the development of inhibition, since each of our conditioned reflexes is a delayed reflex, i.e., one in which the excitatory process, being premature, must, for a longer or shorter period, be preceded by an inhibitory process and temporarily eliminated.

An absolute, and not relative, determination of the strength of the inhibitory process can be effected, above all, by testing its duration, i.e., by finding out how long the nerve cell can endure a state of continuous inhibition. As mentioned above, the main principle underlying this distinction is as follows. The strong, but unequilibrated, animals, as well as the weak ones, cannot endure a protracted inhibition, with the result that the entire system of conditioned reflexes is temporarily disturbed, or a chronic nervous disorder—neurosis—sets in. The strong animals cannot endure this, since they possess a very strong excitatory process to which the inhibitory process, being sufficient in itself, does not correspond as far as intensity is concerned; this is a case of relative weakness of the inhibitory process. In weak animals both the excitatory and inhibitory processes may be weak—this would be a case of absolute weakness. When the inhibitory process is strong (specially differentiated) its instantaneous or chronic prolongation to a period of five to ten minutes may not evoke any disturbance at all, or cause only a very slight one. But when the inhibitory process is weak, its chronic prolongation, for example, to thirty seconds instead of fifteen, often cannot be effected without causing serious consequences; a prolongation to five minutes, even if effected once, is sufficient to cause a failure of the entire conditioned reflex activity in the form of a persistent neurosis.

The second essential index of the strength of the inhibitory process is its ability rapidly and exactly to concentrate. Usually when an inhibitory process begins to develop at a definite point, it invariably first irradiates and produces a prolonged, successive inhibition. But as soon as the animal possesses a strong inhibition, the latter inevitably begins to concentrate to an ever-increasing degree and, finally, the successive inhibition wholly or almost wholly disappears. When the inhibition is weak, it may remain forever in a more or less pronounced form. The concentration of a strong inhibition entails an acute positive induction, i.e., one which appears immediately or after a short period of time; it is manifested in heightened excitability both in relation to the stimulus closest in time, and to the positive stimulus at the point of inhibition (on its termination).

Another index of the strength or weakness of the inhibitory process is the duration of the development of the inhibitory conditioned reflexes; the delay in elaborating an inhibitory reflex may be due to the very great strength of the excitatory process and, consequently, to the relative weakness of the inhibitory process, as well as to an absolute weakness of inhibition. But the end of the elaboration is still more instructive. No matter how long the elaboration of an inhibitory process may last, it remains incomplete forever; more often this takes place when the excitatory process is strong, when there is a relative weakness of the inhibitory process. In some cases the inhibitory process is obviously insufficient and reveals constant fluctuations, even to the extent of complete disappearance; this usually occurs in weak animals with an absolutely weak inhibitory process.

The weakness of the inhibitory process is also expressed in the fact that an almost complete inhibitory conditioned reflex can be obtained only when in the course of the experiment it is evoked first, before any of the positive conditioned reflexes; but if it is evoked in between the latter, it becomes considerably or almost completely disinhibited.

Finally, the absolute weakness of the inhibitory process may also be seen from the animal's attitude towards bromide. In weak dogs only very small daily doses of bromide, not more than a few centigrammes, or even milligrammes, and at most amounting to several decigrammes, prove to be efficient and useful, i.e., maintain a considerable conditioned reflex activity. This fact is explained as follows: since bromide undoubtedly bears a relation to the inhibitory process, in the sense that it strengthens it, only a slight intensification of this process under the influence of bromide can be endured when there is an inborn weakness of the inhibitory process.

Probably the following phenomenon, too, should be taken into consideration when determining the strength or weakness of the inhibitory process. When a differentiation is elaborated along with a positive stimulus, two contrary consequences are usually observed: either the effect of the positive stimulus increases, or, on the contrary, there is a decline of the effect compared with the level before the differentiation. What do these facts signify with regard to the strength of the nervous processes? It can be assumed that here it is a question of the strength or weakness precisely of the inhibitory process. In the first case a strong inhibitory process concentrates and causes a positive induction; in the second case, being weak, it irradiates and continuously reduces the effect of its positive stimulus. A comparison with other more precise indicators of the strength of the nervous processes may help to establish exactly the mechanism of this phenomenon.

As regards determining the mobility of the nervous processes, until recently we, as mentioned above, did not pay special attention to this particular property of the nervous processes: hence, we do not possess, or to be more exact, have not contemplated any special methods for determining it. Consequently, the job of elaborating them still remains, or the corresponding experimental forms must be selected from among those already at our disposal.

Perhaps a special and most precise method could be elaborated by means of trace conditioned reflexes. By changing, on the one hand, the duration of the indifferent stimulus, which must be turned into a special trace conditioned stimulus, and, on the other hand, the interval between the end of the indifferent agent and the beginning of the unconditioned stimulus that reinforces it, we shall be able directly to measure the degree of inertness or lability of the given nervous system. It can be anticipated, for instance, that, depending on the time needed for the disappearance of the trace of the stimulus which has ceased to act, the above indicated interval will be of essential importance for a quicker or slower elaboration of a trace conditioned reflex, or even for the possibility of its elaboration in general. The duration of the indifferent stimulus will likewise make itself felt. It is conceivable that in a particularly inert nervous system there will be specially and rapidly revealed for this stimulus the minimum duration under which it is still possible to elaborate a trace reflex.

Next come the methods already tried on two of our dogs which exhibited a striking contrast with regard to the mobility of their nervous processes and which have been cited above as examples. We shall now dwell on them in more detail, partly for the purpose of their further methodical examination and possible perfection, and partly, with the object of elucidating the mechanism of their action.

It might seem that the last method, applied to the inert dog and consisting in a regular rhythmic reinforcement or non-reinforcement of one and the same stimulus, which determined the elaboration of the respectively interchanging excitatory and inhibitory processes, is specially designed to reveal the mobility of these processes. However, this must be proved in a more precise way. By varying systematically in one and the same dog, as well as in dogs belonging to different types of nervous system, the duration of the interval between the reinforced and non-reinforced stimuli and by comparing the results, one can become fully convinced of the essential role played in this respect precisely by the mobility of the nervous processes. This has been just tested on the dog in question. After the summer recess last year the dog finally coped with the required rhythm at usual intervals of five minutes between the stimuli. When the intervals were reduced to three minutes, the rhythm became markedly disturbed. Consequently, the successful elaboration of a rhythm in different animals depends on the intervals, that is, on the degree of mobility of the nervous processes. The longer the required interval, the lower the mobility, and vice versa.

In order to elucidate the mechanism, I must speak in more detail about the complicated experiment (unsuccessfully performed on the same dog) consisting in an unusual elaboration of a conditioned stimulus from an external agent; this stimulus repeated several times in the course of the experiment among other elaborated conditioned stimuli was reinforced only when applied the fourth time. Successful solution of the problem could be attained subject to complete exclusion of the action of all other reflexes on the repeatedly applied agent. Only on this condition was it possible to establish a differentiation between the first repetitions of the agent and its last application. This probably occurs in the same way as the elaboration of a differentiation between different moments of a protractedly acting stimulus in the case of a considerably delayed conditioned reflex, when to the initial phases of the action of one and the same prolonged stimulus there develops an inhibitory reflex, and to the later phases—a positive reflex. Otherwise, i.e., under the action of other stimuli, the excitatory process evoked by the repeatedly applied agent would not show regular fluctuations depending exclusively on the repetition of the agent, but would fluctuate accidentally and irregularly, depending in each case on the diverse influences of previously applied changing stimuli; hence, no differentiation between different applications of the repeated agent could be elaborated. Consequently, only a high mobility of the nervous processes, i.e., a rapid development and discontinuance of the processes caused by all the other stimuli applied in the experiment, including, of course, the process of eating, could ensure the successful solution of the problem. It should be added that this difficult problem was, nevertheless, solved by another dog, although after a longer period of time and with much greater and more painful strain (experiments of Virzhikovsky). The effect produced by the first three applications of one and the same new external agent, varying its place in the system of other positive and negative conditioned stimuli, was inhibited; only the last, fourth, application became a constant, durable conditioned stimulus. Since in this dog the conditioned salivary reaction always preceded the addition of the unconditioned stimulus, our inert dog naturally could not make use of any extraneous signals, and consequently, the differentiation between separate applications of one and the same agent could take place only due to the distinction made by the peripheral receptor and the corresponding nerve cell between the last and the first three applications.

Hardly anything can be added to what has already been said about the methods and experimental forms testifying to the lability of the nervous processes in our first dog. The transformation of contrary conditioned stimuli into stimuli of opposite action is obviously determined, above all, by the mobility of the nervous processes which rapidly adapt themselves to the requirements of the new external conditions. This is generally proved by the greater or lesser difficulty with which this procedure is endured even by many strong equilibrated animals, to say nothing of weak and almost all castrated animals, which, as a rule, fall into a chronic morbid state. Similarly the other experimental form applied to this dog, namely, the rapid elaboration of a considerably delayed conditioned reflex among other short-delayed conditioned reflexes applied much earlier, of course, directly testifies to the high mobility of its nervous processes. The new excitatory process, despite the firmly established stereotype in the action of other stimuli, rapidly adapted itself to the requirements of the new condition, at first being replaced by a durable inhibitory process and then just as quickly reappearing after slight modification in the course of its development, a modification which more closely coincided with the application of the unconditioned stimulus.

Experiments with a direct transition from an inhibitory to an excitatory process and vice versa should likewise be included in the category of experimental forms ascertaining the mobility of the nervous processes. We know that in certain dogs this transition is accomplished easily and with exactitude. Sometimes, in particularly perfect types, the direct precedence of the inhibitory process, owing to its positive induction, determines even an increased effect of the positive stimulus; but in weak types this is usually accompanied by a breakdown, i.e., by a more or less serious nervous disorder.

The so-called reshaping of the stereotype,[82] that is, a certain change in the sequence of a repeatedly applied system of the same conditioned reflexes (for example, a fully inverted sequence) must also be related to this category of experimental forms. In some dogs this change does not exert even the slightest influence on the effects of the different stimuli; in others it is sometimes accompanied by complete disappearance of the conditioned salivary reaction for days (in the case of alimentary conditioned reflexes).

In old age it often happens that the systems of conditioned reflexes, previously reproduced in a regular and stereotype way, i.e., with precise effects of the stimuli, become irregular and chaotic; the precision and constancy of the effect can be re-established only by a simplification of the system—either by the exclusion of negative reflexes, or by a simultaneous reduction of the number of positive reflexes. It would be most natural to explain the mechanism of these facts by a decline, above all, in the mobility of the nervous processes, brought about by old age, as a result of which the inertness and duration of the processes, at previously established intervals, lead to a confusion and collision of the effects produced by the different stimuli.

Certain morbid disturbances observed in our dogs when they have to solve difficult nervous tasks, expressed in pathological states of definite cortical points, should be also ascribed to pathological changes in the mobility of the nervous processes; such are the inertness and explosiveness of the excitatory process. On the one hand, it was frequently observed that the excitatory process of an isolated point of the cortex became abnormally tenacious: the effect of the conditioned stimulus connected with it was not susceptible to inhibition by preceding inhibitory reflexes to such a degree as the effects of other stimuli; its extinction proceeded much more leisurely, and this stimulus did not lose its positive action, in spite of the fact that it was not reinforced systematically for weeks and months (Filaretov, Petrova). On the other hand, the previous stimulus, which had acted normally and whose moderate effect appeared after a certain delay, increasing after the addition of natural alimentary stimuli and ending in the normal act of eating upon presentation of food, now, under a pathological state of the corresponding point of the cortex, began to produce a tremendous (secretory and motor) effect, arising and ending abruptly. When food was offered, the dog violently and obstinately rejected it (experiments of Petrova). It is clear that an extreme lability of the excitatory process was in evidence and that the latter, especially due to its summation with natural alimentary stimuli, rapidly reached the limit of working capacity of the cortical cell and evoked a very strong transmarginal inhibition.

Thus, I repeat, the possible variations of the basic properties of the nervous system, as well as the possible combinations of these variations, determine the types of nervous system; as calculated, their number amounts at least to twenty-four. But life shows that the actual number is considerably smaller: we distinguish four types which are particularly distinct and strongly pronounced, and, what is most important, differ in their adaptability to the external environment and their resistibility to morbific agents.

We must admit a type of *weak* animals, characterized by a manifest weakness both of the excitatory and inhibitory processes; they never fully adapt themselves to the conditions of life, are easily broken, often and quickly become ill and neurotic as a result of difficult life situations, or, what is the same thing, of the difficult nervous tasks which we place before them. But of still greater importance is the fact that this type, as a rule, cannot be improved to any considerable degree by training and discipline; it becomes fit only under particularly favourable, deliberately created, conditions, or, as we usually say, in hot-house conditions.

The type is in contrast to the types of *strong* animals which in their turn markedly differ.

Among the latter, in the first place, is the *strong*, but *unequilibrated* type with a strong excitatory process, but with a weaker, and sometimes even a considerably weaker, inhibitory process, in view of which this type is also easily subject to pathological disturbances when inhibition is required. This, predominantly, is a fighting type, but not adapted to everyday life with all its fortuities and exigencies. Nevertheless, being strong, it is capable of disciplining itself to a considerable degree, improving thereby the originally insufficient inhibition. We term it the *excitable type*, but to avoid misunderstanding and confusion it would be better to use the adjective *impetuous*, which directly stresses its defect and at the same time obliges us to regard it as a strong type.

From this strong type one must single out the *strong* and *equilibrated* animals.

But these animals, in their turn, differ greatly, first of all in external behaviour, and this, as we already know, is precisely due to the mobility of the nervous processes. In order to designate these *strong* and *equilibrated* types we can correctly accord them the attributes *calm* and *lively*, in conformity with their mobility.

Such are the principal types which exactly correspond to the ancient classification of the so-called human temperaments—melancholic, choleric, phlegmatic and sanguine.

As for the less significant variations, they are most frequently met with, as already mentioned, in the weak type, but they have not yet been fully investigated and systematized by us.

In conclusion I wish to say a few words about the frequency of these types among the multitude of dogs of various breeds that have passed through our laboratories during our study of the conditioned reflexes. The *weak* type in all its variations and the *lively*, sanguine type are the most frequent; then comes the *impetuous*, choleric type; rarest is the *calm*, phlegmatic type.

Basing ourselves on the elementary physiological principles underlying the classification of the types of nervous system in animals, we must admit the same types in the mass of human beings—a classification already made by

Greek classical thought. Thus, Kretschmer's[83] classification of nervous types, which has obtained almost universal recognition, especially among psychiatrists, must be regarded as mistaken or inadequate. Kretschmer found his types in the clinic, among the ill. But are there not absolutely healthy individuals? And why must all human beings indispensably carry nervous and mental disorders in embryo?

Kretschmer's types represent only a part of all human types. His cyclothymics[84] are closest to our excitable, impetuous type, or to Hippocrates' cholerics, and his schizothymics[85]—to our weak type, or to Hippocrates' melancholics.

Since the first type lacks a proper abating and restorative process—the process of inhibition—its excitatory process often considerably exceeds the working capacity of the cortical cells. This causes a derangement of the proper interchange of normal work and rest, which manifests itself in extreme morbid phases of the excitatory and inhibitory states, both with regard to intensity and duration. Hence, the eventual development of a manic-depressive psychosis[86] under particularly difficult circumstances of life, or under certain unfavourable conditions of the organism.

In the second type both processes are weak, and because of this it cannot endure individual and social life with its severe crises, which mostly fall on a still young, not sufficiently adjusted and hardened organism. This may lead, and often does lead, to a complete destruction of the higher part of the central nervous system, unless some lucky chance in life, or, more often, the protective function of the inhibitory process, does not save it from disastrous overstrain during this difficult period. It can be rightfully assumed that for those representatives of the weak type who end up with schizophrenia[87] there are certain specific conditions, such as a particularly irregular course of development, or permanent auto-intoxication, causing extreme fragility of the nervous apparatus. Aloofness or reticence which, according to Kretschmer, is the main feature of schizothymics from childhood, does not present anything specific; in the case of a weak nervous system it is merely a general indication of the extreme complexity of the social environment; hence the natural withdrawal from it. Is it not a widely recognized and current fact that the mere transfer of a nervous person to a clinic or sanatorium, that is, the simple act of removing the patient from his everyday surroundings, affords relief and is even of curative importance?

It should be added that reticence or aloofness from society is by no means an exceptional feature of schizothymics, i.e., of weak individuals. Even strong persons may be reserved, but for quite different reasons. This type of person leads a strenuous but at the same time one-sided subjective life; he early becomes possessed by a certain inclination, concentrates on a single aim and is dominated and carried away by a single idea. Other people are not only undesirable; they even disturb him and distract him from the principal object of life.

Naturally, there are many great men also among cyclothymics (the strong type). But it is understandable, that, being unequilibrated, they possess a particularly fragile nervous system. Hence, the widespread and vividly discussed problem: genius or insanity?

And then comes, of course, the multitude of human beings more or less strong and even exceedingly so, and at the same time equilibrated, the phlegmatics and the sanguines, the people who make the history of mankind either by their systematic mundane but indispensable labour in all branches of life, or by the exploits of their mind, lofty emotions and iron will. Of course, as far as great men are concerned, no matter how strong they may be, they are also subject to breakdowns, since the scale of their activity is extraordinary, and there is a limit to any strength.

VIII

PROBLEMS OF SLEEP AND HYPNOSIS

SOME FACTS ABOUT THE PHYSIOLOGY OF SLEEP[88]

[JOINTLY WITH DR L. N. VOSKRESENSKY]

In our study of the so-called conditioned reflexes we often had to deal with the phenomena of sleep. Since these phenomena greatly complicated our experiments, disturbing them and deflecting them from their normal course, we were, naturally, compelled to devote special attention to them. In addition to accumulating isolated facts, two of our colleagues—N. A. Rozhansky and M. K. Petrova—elaborated this problem most systematically. N. A. Rozhansky investigated that form of sleep, or somnolent state which apparently results from the influence of monotonous, indifferent stimuli, for example, the isolated environment in which the experimental animal is placed. When the animal is enclosed in an isolated chamber and placed in the stand, it gradually becomes drowsy, and then goes into deep sleep. Sleep also occurs under the influence of definite, active stimuli from which strong conditioned stimuli have been elaborated. Under the influence of these stimuli there arises a sleeping hypnotic state in all dogs, and in some of them with particular ease. Recently Dr. L. N. Voskresensky had a case of this somnolent state which for us was somewhat unexpected, since numerous experiments had already been performed on this dog by Dr. A. M. Pavlova and no marked signs of sleep had been observed during those experiments. But now in the course of our research, sleep unexpectedly intervened and constantly disturbed our experiments with conditioned reflexes; as a result, the usual phenomena were sometimes entirely absent and sometimes distorted. How did this come about? At first we were not quite sure whether this state was really sleep, and attributed the disturbances to other causes. But thorough observation and various tests excluded all other suppositions. All that remained was to admit the development of a state of sleep in the dog. But what caused it? When we closely considered all the details of the recent experiments performed on the dog, it appeared that the sleep was due to the following reasons. Prior to this peculiar period the experiment was usually begun the moment the dog was placed in the stand—it was subjected to the action of special conditioned stimuli and food was given as an unconditioned stimulus. These conditions did not produce a sleeping state. Now, however, due to certain circumstances, the dog was left in the stand for a relatively long time, waiting for the beginning of the experiment. And it was the continuously acting, monotonous surroundings which caused the gradual onset of a state of sleep. This interpretation proved to be perfectly reasonable. As the particulars relating to the development of a state of sleep were of great interest to us, we decided to investigate the question with the utmost thoroughness.

First of all it appeared that from the quantitative point of view the environment acts with surprising precision, i.e., if immediately after the necessary preparations (fixing the different funnels, fastening the apparatus, etc.) you begin the experiment, the usual stimulations of the animal, there are no signs of the phenomena of sleep at all. But should a minute pass between the completion of the preparations and the beginning of the stimulation the first phase of sleep becomes manifest. If ten minutes pass you observe the next stage of sleep and so forth. Thus the sleep-producing influence of the environment can be truly dosed.

This made possible an easy study of the course of sleep, of the somnolent state which develops under these conditions. And here are the results of our observations. During the experiments we usually had before our eyes two reactions of the animal; on the one hand, there was a secretory reaction, a flow of saliva; on the other—a motor reaction—the dog seized the food offered to it. In other words, these were the motor and secretory reflexes. It turned out that the strictly law-governed development of the observed phenomena depends on the quantitative influence of the soporific environment; this is shown in the following table:

State of the dog	Phases of sleep	Reflexes* Secretory	Reflexes* Motor	Remarks
Awake		+	+	
	I	—	+	
	II	+	—	Deep sleep
Asleep	III	—	—	
	II	+	—	
	I	—	+	
Awake		+	+	

In the wakeful state both the secretory and the motor reflexes are present. Immediately after the conditioned stimulus begins to act a secretion of saliva appears, and the dog takes the food as soon as it is offered. Thus both reflexes are effective. Now we keep the dog under the influence of the surroundings at least for two minutes, i.e., when the preparations for the experiment are finished, we let two minutes pass, and then apply the conditioned stimulus. The *first phase of the state of sleep* is then observed. It is manifested thus: the secretory reflex disappears; the conditioned stimulus no longer acts; but when the dog is offered food it immediately seizes it, which shows that the motor reflex persists. Now you augment the influence of the surroundings, i.e., for example, you keep the dog

* The sign + signifies the presence and the sign — the absence of a reflex.

waiting ten minutes before the experiment begins. Then its sleep deepens, and another kind of reaction is observed, which, strange as it may seem, is of a reverse nature and represents the *second phase of the state of sleep*; the dog exhibits a secretion of saliva, but does not take the food, and even turns away from it. Thus the salivary reaction which is absent during the first phase of sleep, reappears in the second, while the motor reaction disappears or even passes into a negative reaction; the dog not only refuses food but even turns away from it. If the dog is left in the soporific surroundings for a period lasting from half an hour to one hour before the beginning of the experiment, a *complete, deep sleep* sets in, and both reflexes vanish. Now let us wake the dog from its deep sleep. This can be done at once, and the simplest method is to apply a strong sound stimulus. In our laboratory we use a very loud rattle, which wakens the dog instantaneously. The animal immediately returns to a normal alert state. However, a more delicate stimulus may be used.

One of the customary methods of gradually dispelling sleep is to feed the dog at definite intervals; feeding may be even begun with the forcible introduction of food into the mouth. Then you can observe the phases described above but in reverse order. After the deep sleep the secretory reflex is present but the dog does not take the food. Later on, the secretory reflex fails to appear; however, the dog eats. And finally, after frequent repetitions of the feeding, both reflexes reappear. Now I shall call your attention to some authentic figures. Take, for example, a dog that has just been fastened in the stand; we begin to apply certain conditioned stimuli and a secretion of saliva appears. Our scale shows 37 divisions, which indicates a normal salivary reaction. It should be added that the following precaution was observed by us in order to ensure strict precision in our investigation. The chamber itself had a hypnotizing effect on the dog; the moment the lively, mobile and responsive animal was brought into the experimental room, it changed entirely. It goes without saying that the state of sleep deepened when the dog was placed in the stand and prepared for the experiment. In order to determine exactly the moment of the passage from the wakeful to the sleeping state, we did everything to prevent sleep while the dog was being fastened and the apparatus attached to it; we called it by name, stroked it and patted it. When everything was ready, we would quickly leave the chamber and begin the experiment immediately. In this way we obtained the above-mentioned normal secretory reaction, equalling thirty-seven divisions of our scale; the motor reflex was also present. In the next experiment we allowed the surroundings to act on the animal for a space of two minutes, and the following was observed: the secretory reflex failed to appear at all, not a drop of saliva was secreted in response to our conditioned stimulus, but the dog took the food at once. Next time we let the surroundings act for four minutes; we obtained twenty divisions of saliva but the dog took the food only in forty-five seconds, and even then only when the food was brought into contact with its mouth. Finally, when we allowed the surroundings to act for half an hour to an hour, all the reflexes disappeared.

The procedures were, of course, varied by us, so that we were able to obtain both phases of sleep in one and the same experiment. For example, the dog remained in the chamber for seventy-five seconds; as a result, the secretory reflex was zero, but the food was taken at once. Then we let an hour pass, leaving the dog alone. The excitation produced by a single meal neutralized to some extent the soporific influence of the surroundings, and only the second phase was observed: the secretion of saliva equalled twenty-two divisions, and the dog ate the food only for about half a minute after the food had been brought into contact with the mouth. Here is another concrete instance of how sleep is dispelled. The dog is in a deep sleep and in order to arouse it we apply, among others, a weak stimulus: someone enters the chamber where the dog is kept in the stand. The noise produced by the person entering the chamber, and perhaps his odour, slightly disturb the animal's sleeping state. If we now apply the conditioned stimulus we obtain twenty-four divisions of saliva, but the dog takes the food fifty seconds later, not of its own accord, however, but only when it is put into its mouth. We then feed the dog once or twice, thereby stimulating it; we dispel the state of sleep and observe a transition to the following phase: the secretory effect is diminished, there are only ten divisions of saliva and the food is taken by the dog after twenty seconds. Whereas in the preceding case the dog ate the food after fifty seconds and from the experimenter's hand, now it takes the food of its own accord and only after twenty seconds. With a fresh stimulus applied after twenty minutes, the secretory reflex is zero, and the dog eats the food almost immediately. Finally, when the next conditioned stimulus is applied, thirty-five divisions of saliva are secreted, and the dog takes the food right away. This signifies the presence of a perfectly alert state. Hence, we must recognize as a thoroughly established fact that the processes of falling asleep and of awakening influence our two reflexes in a strictly definite way. We witnessed a very interesting fact which is, above all, of practical importance, since it enabled us to control the animal and to remove the influences which interfered with the experiment. It sufficed to feed the dog two or three times, or to prevent the surroundings from acting on it at the beginning, to make us masters of the situation; sleep did not disturb our experiments with the conditioned reflexes. Now the question arose: How to interpret this phenomenon? Certainly it is a complicated question and for the time being only an approximate answer can be given. Our colleagues N. A. Rozhansky and M. K. Petrova, on the basis of their experimental data, have come to the conclusion that both states of sleep observed by them represent an inhibitory process and that in one case it spread from several points of the cerebral hemispheres (case of Rozhansky), and in another case—from one definite point (Petrova). Our fact, it would seem, confirms this conclusion, since in our experiments there was actually in evidence a localization and even a movement of the somnolent state in the cerebral hemispheres. How can this movement of sleep inhibition be better traced in the cerebrum? A similar question arose and was successfully investigated in connection with another kind of inhibition, the so-called internal inhibition. A few months ago one of us addressed you here on this subject. This investigation gives us reason to hope that we may achieve the same with regard to sleep inhibition. The simplest way would be to trace the movement of this sleep inhibition in a definite part of the cerebral hemispheres, since, as shown by our experiments concerning the irradiation of, say, internal inhibition *over the entire hemisphere*, certain circumstances greatly complicating the picture are met with in this case (probably, the border-line layers between different parts of the hemispheres, various degrees of energy of stimulation, etc.). Experiments in this direction are now performed in our laboratory. It is more convenient to trace the movement of sleep inhibition in that part of the cerebral hemispheres which relates to the skin, being, as it were, its projection in the brain. Moreover, the conditioned stimulation of the skin provokes a sleeping state quite easily. If we assume that this sleeping state arises precisely at the point of stimulation, we see how this inhibitory movement spreads from this point over the entire cutaneous area of the brain; it will then be possible to determine how far and how quickly this process spreads. But, for the time being, this is only a hope.

CONCERNING THE SO-CALLED HYPNOTISM IN ANIMALS[89]

The so-called hypnotism of animals (the experimentum mirabile of Kircher[90]) consists in the fact that by means of energetic action, overcoming all resistance, the animal is brought to an unnatural posture (laid on its back) and kept thus for a brief space of time. Afterwards, when the hands are removed from the animal, the latter remains motionless for many minutes and even hours. Different authors, noting one or other detail of this phenomenon, have explained it in various ways. At present, thanks to the systematic study of the normal activity of the brain, I am in a position to indicate the biological significance of this phenomenon and to give an exact and full explanation of its physiological mechanism; thus I am able to combine all the separate facts of the different authors. The phenomenon represents a self-protecting reflex of an inhibitory character. Faced with an overwhelming power, from which there is no escape in struggle or in flight, the animal's only chance of salvation is to remain immobile in order not to be noticed, since moving objects attract particular attention, or not to provoke by fussy, restless movements an aggressive reaction on the part of this overwhelming force. Immobility is brought about in the following manner. Extraordinary external stimuli highly intense or very unusual in form, first of all cause a rapid reflex inhibition of the motor region of the cerebral cortex which controls the so-called voluntary movements. Depending on the intensity and duration of the stimulus, this inhibition is either confined to the motor region and does not pass to other regions of the cerebral hemispheres and to the mid-brain, or it irradiates over all these parts. In the first case there are present reflexes of the eye muscles (the animal follows the experimenter with its eyes), of the glands (when food is offered, there begins a secretion of saliva, although no skeletal movements in the direction of food are observed), and finally tonic reflexes from the mid-brain to the skeletal muscles in order to retain the position into which the animal has been brought (catalepsy). In the second case all the above-mentioned reflexes gradually disappear, and the animal passes into an absolutely passive, sleeping state accompanied by a general relaxation of the musculature. This course of the phenomena is further confirmation of the conclusion which I reached at a previous meeting of our section, namely, that the so-called inhibition is nothing more than sleep, but partial and localized. It is clear that the rigidity and stupor which seize us in cases of great fear is nothing else but the above-described reflex.

P.S. It should be added that during the period when I did not have physiological literature at hand, which I managed to get only in the spring of 1922 in Helsingfors, a number of other authors came to this same conclusion concerning hypnosis in animals.

PHYSIOLOGY OF THE HYPNOTIC STATE OF THE DOG[91]

[JOINTLY WITH DR. M. K. PETROVA]

Besides the usual classical method of hypnotizing animals (laying the animal on its back and keeping it for some time in this unnatural position), which results in a hypnotic state manifesting itself in catalepsy,[92] our laboratories were able, in the course of their research into the normal activity of the higher parts of the brain, to study in more detail the diverse and very delicate manifestations of the hypnotic state. As already established by us, the basic condition required for the development of this state is a prolonged action of monotonous stimuli, which finally bring the corresponding cortical cells to a state of inhibition. This inhibition, on the one hand, is of different degrees of intensity, and on the other hand, spreads to a greater or lesser extent over the cerebral cortex and further down the brain. Corresponding facts were cited in a book published by one of us (I. P. Pavlov, *Lectures on the Work of the Cerebral Hemispheres)*.

But subsequent observations revealed a greater variety of symptoms of the hypnotic state, its more and more delicate gradations, which hardly differ from the wakeful state, and its ever-increasing mobility depending on the slightest changes in the surroundings, on insignificant modifications in the external stimuli acting upon the animal.

In the present article we shall deal with the phenomena observed by us in two dogs. Previously they were used by one of us (M. K. Petrova) for studying the various conditioned reflexes, but now they constantly fall into a hypnotic state the moment they are placed in our usual experimental conditions and respectively equipped.

Long ago in the works which originated in our laboratories it was repeatedly pointed out that in the case of conditioned alimentary reflexes there takes place a dissociation of the salivary secretion and the alimentary motor reaction when the dog falls into a sleeping state. It usually happened that our artificial conditioned stimuli or more often the natural stimulation (which, as has been proved, is also conditioned) produced by the sight and odour of food, evoked a profuse secretion of saliva, although the animal did not take the food. It was in this state of the animal that very diverse and highly interesting variations of the alimentary motor reaction were manifested in the course of our observations. These variations, which apparently represent different degrees of intensity of hypnosis, were now predominantly observed in one animal, now in another. One of the dogs, which was usually in a less profound hypnotic state, distinctly exhibited what in mental diseases is called negativism.[93] After a conditioned stimulation applied during a certain period of time we put food before the dog; the latter turns away from the food receptacle. But when we begin to move the receptacle away, the dog makes a movement in its direction. We present the receptacle anew; the dog again turns away from it. We move it away, and the dog turns towards it once more. We have termed the reaction of turning away from the food receptacle negative, or the first phase of negativism, and the movement towards the food receptacle—positive, or the second phase. This negativism may recur many times until the animal at last partakes of the food, which happens in most cases. The degree of hypnosis is expressed precisely by the number of repetitions of this procedure. At the beginning of the hypnotic state the food is taken and eaten by the dog after the second offering. When the hypnotic state becomes more profound, both phases of negativism recur more and more often. When hypnosis reaches the highest degree, the dog rejects the food, no matter how many times it is offered. But as soon as the hypnotic state is dissipated in this or that way—for example, by removing the apparatus attached to the dog for the purpose of collecting the saliva, or by loosening the chain which is attached to the dog and which during the experiment is fastened to the upper cross-bar of the stand, or by some other means—the dog immediately begins to devour the food.

In the other dog the alimentary motor reaction during hypnosis assumed an even more complicated form. In one of the more pronounced cases the phenomena developed in the following sequence. Under the action of our conditioned stimuli (usually to the end of their isolated action) the dog, if it was in a sitting posture, rose to its feet, if standing, it turned its body in the direction whence the food was usually presented to it. But when food was offered, it turned its head away from it, thus exhibiting the first phase of negativism. Then the food receptacle was moved away, and the animal, on the contrary, turned the head towards it and followed it with its eyes; thus the second phase appeared. After some manifestations of this negativism the dog at last brought its mouth close to the food, but was unable to take it. As if with great difficulty it began, little by little and repeatedly, to open and to close its mouth, but to no purpose—it did not take the food (abortive movements). Afterwards, it began to move its jaws with greater ease, took the food, at first in small portions, finally opening its mouth wide and swallowing rapidly without interruption. Thus, in this hypnotic phase we must distinguish three different states in three parts of the skeletal musculature relating to the process of eating: strong inhibition, immobility of the muscles directly participating in the process of eating (the masticatory and lingual muscles); considerable mobility, but of a periodic character, in the form of a negativism of the cervical musculature; and finally, normal activity of the remaining musculature of the body. The more profound the hypnotic state, the more immobile and inhibited is the direct musculature: the tongue is put out as if paralyzed, the jaws are absolutely motionless. In the cervical muscles only the first phase of negativism is manifested. Then the movements of the head cease completely, and only the trunk still turns under the influence of conditioned stimuli. Finally, when the hypnotic state becomes still more profound, this last motor reaction to the conditioned stimuli, as well as to food, also disappears. All these phenomena can be dissipated, abolished instantaneously by the same methods which have been described in the case of the first dog.

Concerning the alimentary motor reaction in our cases, the following should be added. Any slight change in the usual appearance of the food, and even in the manner of presentation leads to conversion of the negative motor reaction into a positive one; in other words, the dog eats the food which it has just rejected. For example, we offer the dog in an ordinary cup slightly damped and evenly spread powder of dried meat and bread. The dog refuses it. But if the same powder is presented partly in the form of a lump protruding from the cup, the dog seizes it greedily and then begins to eat the rest of the powder. A positive reaction can be also obtained if the powder is offered to the dog on a small plate or on a piece of paper. It takes the food also from the experimenter's hand instead of from the cup. Finally, sometimes, after the conditioned stimulation, it begins to lick up the powder spilled on the floor, although when offered in the cup, it refused it.

Along with these motor phenomena relating to the process of eating there were manifested in the course of our observations on the hypnotic state other specific motor reactions worth noting. Many dogs, after partaking of their small portion of food and being in an alert state, for a time lick their forepaws and breast. In the hypnotic state the licking usually assumes a protracted character; in the case of one of our dogs in question it soon passes into a peculiar form. The dog licks the forepaw and wets it with saliva, especially the flesh of the toes; then it brings the forepaw close to the apparatus attached to the salivary fistula and passes the toes over it—a gesture it repeats many times if not stopped. In the alert state the same dog did not do this. Some dogs in the alert state struggle against the apparatus only when it is first attached to them, afterwards they get used and pay no attention to it. We can rightfully suppose that our dog exhibited in the hypnotic state one of the specific defensive reflexes. When a dog has a wound on a part of the skin within reach of its tongue, it repeatedly cleans it with saliva, or, as we say, licks it (the auto-curative reflex). Apparently in this particular case the irritation evoked by the hardened cement, by means of which the apparatus is attached to the skin, is responsible for the manifestation of the reflex; and since the point of irritation is not accessible to the tongue, the latter is replaced by the toes of the forepaw.

Many of the above-described variations of the alimentary motor reaction usually take place during one and the same experiment and rapidly supersede one another. This variability, this mobility of the hypnotic state is also seen in other phenomena. We shall cite a few more cases illustrating the fluctuation of the hypnotic state and the modification of the effect of the conditioned stimulus, already described and reproduced by us, or noted for the first time in the course of our observations and experiments on dogs. These fluctuations and modifications are either due to causes still unknown or are related to definite conditions.

I repeat that, if the dog is susceptible to hypnotization under experimental conditions, the hypnotic state usually develops immediately after the dog is placed in the stand, and sometimes the very moment it crosses the threshold of the experimental chamber. With the progress of the experiment this state grows continuously and gradually, provided it is not dissipated by certain new conditions.

Let us consider first of all the dissociation of the secretory and motor reactions of the alimentary reflex. This dissociation often assumes the form of, so to speak, reciprocal antagonism. In some cases the stimulation evokes a secretion of saliva in the absence of any motor reaction, i.e., the dog, as mentioned above, does not take the food. In other cases, on the contrary, the dog rapidly seizes the food and eats it with avidity, but there is no salivary secretion in response to well-elaborated conditioned stimuli.

Here is the example of one of our dogs—"Bek." The experiment took the following course during two days in succession:

April 17, 1930

Conditioned stimulus	Secretion of saliva in drops during 30 secs.	Alimentary motor reaction
Rattle-box	15	Negativism, then takes food
Bell	15	Abortive movements; rejects food for a long time

April 18, 1930

Conditioned stimulus	Secretion of saliva in drops during 30 secs	Alimentary motor reaction
Rattle-box	1	Takes food at once, but eats inertly
Bell	0	Takes food at once and eats with relish

Sometimes these, as it were, antagonistic relations between the secretory and motor alimentary reactions rapidly interchange in the course of the experiment.

This can be illustrated by an experiment performed on another dog "John":

April 12, 1930

Beginning of the Experiment

Conditioned stimulus	Secretion of saliva in drops during 30 secs.	Alimentary motor reaction
Rattle-box	5	Negativism
Bell	0	Takes food at once

In the early works that originated in our laboratories it was frequently stated that a well-elaborated inhibitory stimulus, usually a differential one, can modify the hypnotic state in two opposite directions—either intensifying, or weakening it. The same thing was often observed by us in the above-mentioned animals in their state of hypnosis.

Finally, it should be pointed out that among our usual conditioned strong stimuli a particularly powerful conditioned stimulus often eliminates or weakens the hypnotic state whereas stimuli of usual strength either leave it unchanged or even reinforce it.

Here is an example connected with the experiment performed on the above-mentioned "Bek," the beginning of which has been described above. When the experiment was continued and a differentiation applied, the conditioned stimuli of medium strength—the rattle-box, the gurgle of water and the bell—did not produce any secretory effect, and the dog, while making abortive masticatory movements, did not take food for a long time. A strong crackling sound which is a very powerful conditioned stimulus, evoked a secretion of saliva, and after a short period of negativism the dog took the food.

April 17, 1930

Conditioned stimulus	Secretion of saliva in drops	Alimentary motor reaction
Rattle-box	0	Does not take food for a long time
Gurgling sound	0	Same
Strong crackling sound	5	Nagativism of short duration
Bell	0	Does not take food for a long time

How should the physiological mechanism of the above-mentioned phenomena be interpreted and understood? It is evident that at the present level of our knowledge in the field of the physiology of the higher parts of the brain it would be an extreme pretension, incompatible with the real state of affairs, to try to give a well-grounded and clear answer to all questions which may arise in this connection. However, we must constantly attempt to explain particular phenomena by the more general properties of the activity of the higher parts of the brain, to effect new variations of experiments that would ensure a closer approach to the comprehension of the extremely complex relations of reality which exist in the given case.

The difficulty which we meet when attempting to elucidate the mechanism of the above-mentioned phenomena observed in the hypnotic state, is that under stimulations, undoubtedly reaching the cerebral cells, we often do not know what in the ensuing nervous activity should be attributed to the cerebral hemispheres and what to the lower levels, the lower parts of the brain and even the spinal cord. In the course of the philogenic development of the central nervous system the nervous combinative systems, in the form of definite, so-called reflex centres, becoming more and more complex, steadily moved closer to the brain end; they effected an increasing analysis and synthesis of the stimulating agents due to the augmenting complexity of the organism and the growth of its relations with the external environment in ever-widening areas. Thus, along with a more or less stereotype nervous activity, and with ready complexes of physiological functions, called forth by a limited number of elementary stimulations, there gradually developed the higher nervous activity dealing with an ever-increasing number of conditions, of complex, and besides, variable, stimulations. Then a very complicated problem arises before the investigator, the problem of the connection and of the forms of this connection between different levels of the nervous system. As to our first problem concerning the dissociation of the secretory and motor reactions of our conditioned alimentary reflex, it is necessary to establish what in this reflex should be ascribed to the cortex and what to the adjacent subcortex, or, in ordinary terminology, what in this process is of a voluntary and what is of a reflex character. To be still more exact, it is necessary to know whether in the conditioned alimentary reflex the secretory and motor components equally depend on the cortex, or whether there is a difference between them in this respect. Does not the motor component predominantly depend on the cortex, and the secretory component on the subcortex?

Let us turn to the well-known facts.

Proceeding from the phenomena of human hypnosis we must admit that in the cerebral cortex along with a grandiose representation of the external world effected through the afferent fibres (an indispensable condition for the highest regulation of functions) there is also a vast representation of the organism's internal world, i.e., of the states and functioning of numerous organs, tissues and internal organic processes. In this respect particularly convincing are the facts pertaining to the so-called imaginary, self-suggested pregnancy. Numerous processes relating to the activity of passive tissues, such as the adipose one, arise and become intensified under the influence of the cerebral hemispheres. But it is clear that these two kinds of representation differ greatly in degree. Whereas the representation of the skeletal musculature is highly delicate and detailed, perhaps being equal in these respects to the representation of such external energies as sound and light, the representation of other internal processes lags considerably.[94] This is probably due to the slight practical significance of the representation. In any case, it is a constant physiological fact. And this, apparently, makes it possible to distinguish between the voluntary and involuntary functions of the organism, the former including only the activity of the skeletal musculature. This voluntariness signifies that the work of the skeletal musculature is, above all, determined by its cortical representation, by the motor region of the cortex (the motor analyser, in our terminology) which is directly connected with all the external analysers; in other words, in its orientations it is always determined by the analytical and synthetical work of these analysers.

Proceeding from these facts, we can present the mechanism responsible for the elaboration of our conditional alimentary reflex in the following way. On the one hand, this is a union between the cortical points of application of the conditioned stimuli and the reflex alimentary centre of the adjacent subcortex with all its particular functions; on the other hand, it is a closer connection of the same points with the corresponding parts of the motor analyser, i.e., those which participate in the process of eating. Then the dissociation of the secretory and motor components of the alimentary process taking place in the course of hypnotization might be interpreted as follows. The hypnotization evokes a state of the cortex when the motor analyser is inhibited, while all the other analysers are free. The latter evoke a reflex on the alimentary centre of the subcortex with all its functions, while the inhibition of the motor analyser, so to say, by direct communication, excludes the motor component from this reflex, thereby bringing the terminal points of movement, the cells of the anterior horns,[95] to a state of inactivity. Thus, in the alimentary process only the secretory reaction remains manifest.

Here is the reverse case. An artificial conditioned stimulus does not produce a secretion of saliva, but a motor reaction is in evidence—the dog takes the food at once. Now this can be easily explained. This must be a weak inhibition of the entire cortex, and an artificial stimulation alone is not sufficient to dissipate it; only with the presentation of food, when the artificial conditioned stimulus is supplemented by natural stimuli (the sight and odour of food, which in themselves are even stronger than artificial stimuli), does there arise a complete reflex with both components.

But there is one more phenomenon which was observed by us in the course of other experiments in our laboratories and which manifested itself outside the hypnotic state;

it would be opportune to analyse this phenomenon in the light of our present explanations. The dog eats the food, but no secretion of saliva is observed for ten or twenty seconds. This is undoubtedly due to the development of inhibition deliberately induced in the cortex by means of artificial conditioned stimuli for definite periods of time. How is this phenomenon to be interpreted? What mechanism is responsible for it? It must be assumed that an intense inhibition develops from the points of application of the artificial conditioned stimuli and spreads over the entire subcortical alimentary centre with both of its principal components—secretory and motor—as well as over the corresponding part of the cortical motor analyser. The moment food is presented, there arises at the points of application of the strongest natural conditioned stimuli, which have not participated in developing inhibition, an excitation rapidly affecting the alimentary region of the motor analyser; the latter is more labile in comparison with the subcortical centre, where the inhibition dissipates only if the motor effect of the unconditioned stimulus is more pronounced. One might draw a certain analogy between this phenomenon and the deliberate, volitional introduction of food into the mouth, its mastication and ingestion, in the absence of any trace of appetite.

However, it can, of course, be assumed (there are sufficient grounds for this) that the conditioned connection with the salivary secretion is likewise effected in the cortex through the cortical representation of the salivary glands, and if so, all the cases of dissociation of the secretory and motor reactions can be attributed to a different localization of inhibition at the onset of the hypnotic state and in the course of its development.

Another hypnotic phenomenon whose physiological mechanism must be elucidated by us is negativism. This, obviously, is a manifestation of inhibition, since it is a phasic phenomenon which gradually ends in sleep. Likewise, there is no doubt that it is a cortical localized inhibition because the salivary reaction accompanying it reveals a conditioned, i.e., cortical character. Consequently, it is natural to conclude that this is a motor inhibition related to the motor region of the cortex, to the motor analyser. But how is this form of inhibition to be explained? Why does the negative phase of the motor action appear first and the positive one next? What causes the change? It seems to us that this can be easily explained by more general, already known facts. When the hypnotic, inhibitory state sets in, the cortical cells become, as it were, weaker and less efficient—the maximum limit of their possible excitability diminishes. This is the so-called paradoxical phase, when a strong stimulus usually turns into a superpowerful one and may evoke not excitation, but inhibition, or it may strengthen the latter. We must also assume that a movement proceeding from the motor analyser, as is generally the case, consists of two opposite innervations—positive and negative, a movement towards the object and a movement from the object, which is similar to the relations of the flexors and extensors in the limbs. The negativism may be then explained in the following way. A conditioned stimulus, slightly inhibited or not inhibited at all, directs a stimulation from the cortex to a corresponding positive innervating point of the motor region which is in a paradoxical state due to a certain degree of hypnotization. That is why the stimulation does not excite the above-mentioned point, but intensifies its inhibition. Then this extraordinary local inhibition, in accordance with the law of reciprocal induction, excites the negative point which is closely associated with the positive one. Hence the first negative phase of negativism. When the stimulus is removed, the extraordinarily inhibited positive point, by virtue of internal reciprocal induction, immediately becomes excited itself; at the same time the negative point, excited by the induction, at once passes into a state of extraordinary inhibition and in its turn positively induces the positive point. Thus, after its first extraordinary inhibition the positive point undergoes, so to speak, a double excitation. In accordance with this, if the hypnotic state does not deepen, the positive phase usually takes the upper hand after a single or repeated presentation and removal of the food—the dog begins to take it. We observe, then, a highly labile state of the cellular activity which is one of the properties of the transitional phase. This is proved by the further course of developments. If the hypnotic state deepens, there remains only the negative phase; reverse induction becomes impossible, and no excitation of the motor innervating apparatus is observed at all.

Approximately in this period of the conditioned alimentary reaction under hypnosis there is manifested one of the conditions for a fragmentary localization of hypnogenous inhibition in the cortex. One of our dogs, as shown in the descriptive part of this article, exhibited a very interesting and peculiar phenomenon (already mentioned by one of us in a previous article[96]). This relates to a definite sequence of inhibition in the adjacent zones of the motor region. The sequence can be explained by the fact that the inhibition embraces first of all those regions whose activity was most intense before the onset of the hypnotic state. Since in the repeated process of eating the masticatory and lingual muscles worked most of all, then the cervical muscles, and finaly the muscles of the trunk, the inhibition manifested itself in the same sequence.

The interesting phenomenon of a positive excitatory influence exerted in the course of hypnotization by the slightest change in the appearance of the food and in the manner of its presentation, is likewise accounted for by the general property of the cortical activity already known to us. It was established in our laboratory long ago (by Dr. Y. V. Volborth) that there is a conditioned inhibition of the second order, just as there is a conditioned excitation of the second order. The phenomenon is as follows. If an indifferent stimulus repeatedly coincides in time with an elaborated inhibitory process (for example, in the course of a differentiation), then it soon becomes an inhibitory agent itself. It is then easily understood why everything acting on the cerebral hemispheres during the state of hypnosis (which in itself is a certain degree of inhibition) acquires an inhibitory character. Hence, it is sometimes sufficient to bring the dog into the experimental chamber to evoke in it a hypnotic state. Any new stimuli, even very insignificant ones, naturally do not produce this inhibitory effect, and consequently, evoke positive cortical activity.

The auto-curative reflex mentioned in the descriptive part of this article is simply one of the subcortical reflexes manifested in the state of hypnosis after short feeding. The process of eating with all its exciting components, acts on the more or less hypnotized cortex as a strong stimulus and entails an intensification of cortical inhibition. A positive induction then proceeds from the cortex to the subcortical centres, which are now under the action of the ultra-weak stimuli or traces of former strong stimuli. The animal begins to sneeze, to scratch itself, etc., which was not observed in the alert state. Of a similar nature was the experimental case with a dog whose state resembled a war-time neurosis; this case is described and analysed in the present volume of *Collected Papers*.[97]

As to the effect of differentiations, i.e., of conditioned inhibitory stimuli, we have long known that their influence on diffused inhibition is of a two-fold, contrasting character. In the case of a very feeble, diffused cortical inhibition, of a weak hypnotic intensity, the well-elaborated inhibitory stimulus concentrates the diffused inhibition to a greater or lesser degree and in doing so either fully abolishes the hypnotic state or weakens it. On the contrary, in the case of a strong inhibitory tonus of the cortex, the same stimulus intensifies the inhibition, as it were, by its summation with the existing inhibition. Consequently, the result is determined by the relations of intensity.

Let us, finally, consider the last experiment cited by us in the descriptive part of this article, when an extremely strong stimulus, contrary to stimuli of moderate strength and to weak stimuli, instead of intensifying the inhibition, often produced a positive action. The latter can be explained by the direct influence of the extremely strong stimulus on the subcortex; the intense subcortical excitation is communicated to the cortex, thus dissipating or weakening the inhibitory process in it. A special experimental method applied by us proves the correctness of this interpretation. When the monotonous experimental surroundings begin to have a hypnotizing effect on some of our animals, we, incidentally, counteract it by increasing their alimentary excitability by means of a certain diminution of their daily food ration. And naturally this increase of alimentary excitability must be located in the subcortical alimentary centre.

THE PROBLEM OF SLEEP[98]

Dear Comrades,

Although something extraordinary, one might say, even distressing, befell me yesterday, with the result that I am now, so to speak, not quite myself, I thought it necessary, nevertheless, to be present at the conference. Why? Because I believe that in a discussion of a scientific matter such as sleep, which is essential both from the practical and clinical points of view, my judgement will be not without interest, especially since I, jointly with my colleagues, have been studying the phenomena of sleep for thirty-five years in the course of our research into the higher nervous activity of dogs.

We came up against the phenomena of sleep at an early stage in our research; we were obliged to consider it, to subject it to special investigation, which now gives me the right to speak on this subject. That is why, despite my somewhat disturbed state, I decided to come here and to say a few words.

I

I should like first of all to make a general remark. The more perfect the nervous system of the animal organism, the more centralized it is, the more its higher part controls and regulates the entire activity of the organism, even though this is not clearly manifest. It might seem to us that in higher animals many functions are effected independently of the influence of the cerebral hemispheres, but this is not so in reality. The higher part controls all the phenomena which develop in the organism. This was established long ago in the phenomena of hypnotic suggestion and auto-suggestion. It is well known that during hypnotic sleep it is possible to influence many vegetative processes by means of suggestion. On the other hand, we know of cases of auto-suggestion, such as symptoms of imaginary pregnancy, accompanied by an active state of the lacteal glands and the accumulation of fat in the abdominal walls, simulating the pregnancy. All this originates from the head, from thoughts and words, from the cerebral hemispheres in order to influence such a peaceful and genuinely vegetative process as the growth of the adipose tissue.

If the cerebral hemispheres, as everybody knows, are concerned with the slightest details of our movements, bringing some into action and suppressing others, just as it takes place, for example, when one plays the piano, one can easily imagine the minuteness of the degree of inhibition: one movement of a certain intensity is effected, while another, neighbouring movement, even the smallest one, is suppressed and retained. Or take, for example, our speech movements. What a multitude of words we have for expressing our thoughts! Nevertheless, we are precise in conveying the sense; we never use unnecessary words, employing only those which are most suitable in the given case, etc. Consequently, if the cerebral hemispheres constantly interfere even with these minute every-day activities and regulate them, it would be strange to suppose that the division of our activity into wakeful and sleeping states does not depend on the cerebral hemispheres. It is clear that here, too, supreme power belongs to the cerebral hemispheres and all of us are well aware of this.

Now, at a certain time of the day we become drowsy, and, since we are tired, sleep sets in. But we can do without sleep a whole night, and even for two or three nights in succession. And it is our head, our cerebral hemispheres which, of course, control this phenomenon.

I shall now turn to the details.

It is clear, and everybody is aware of this now, since it has become a widespread and established physiological truth—that our entire nervous activity consists of two processes—excitatory and inhibitory—and that our whole life is a continuous interaction of these two processes.

When we began our objective study of the higher nervous activity by the method of conditioned reflexes, and began to elucidate the laws of the particular functions and tasks accomplished by the cerebral hemispheres, we, of course, immediately encountered the two processes. Every physiologist knows that these processes are inseparable, that they are always present not only in the nerve cell, but in each nerve fibre.

(I must make a certain reservation. If I begin to speak about conditioned reflexes this would take a lot of time, and I do not know when I would end. Since we have been working on conditioned reflexes for thirty-five years and have published the results of our work in special papers and books, allow me to assume that knowledge of conditioned reflexes is widespread and consequently, there is no need to treat this subject in an elementary way, i.e., to begin all over again.)

When we applied our conditioned stimuli and then carried out a detailed investigation of the activity evoked by them at every given moment, we constantly observed a spontaneous development of inhibition side by side with excitation. In other cases we produced the inhibition ourselves when we wanted to separate different phenomena.

Since you are acquainted to a degree with the conditioned reflexes, you undoubtedly know that we have, on the one hand, external stimuli which produce an excitatory process in the central nervous system, and, on the other hand, stimuli which produce an inhibitory process in the cerebral hemispheres. Right at the beginning of our research we observed that as soon as we applied the inhibitory stimulus, a somnolent state of the animal, in the form of drowsiness or sleep, immediately intervened. This was of a constant character. We had to conclude, therefore, that these phenomena are closely interconnected and that certain efforts and resources are necessary to get rid of this drowsiness or sleep in the course of experimentation. Thus, when an inhibitory process arises in the cerebral hemispheres, establishing in them a certain differentiation either between the stimuli or between different moments of stimulation, etc., a state of drowsiness inevitably develops.

You can see, as we have seen during the past thirty-five years, that every time a cortical inhibition sets in which analytically assigns its proper place to everything, giving free rein to one process and suppressing the other, a state of drowsiness or in its ultimate stage of development—a state of sleep—simultaneously and invariably appears. The view that drowsiness and sleep are phenomena related to the cerebral hemispheres and that they are the result of the action of definite stimuli, is strictly obligatory for us. Surely a phenomenon observed every day is beyond any doubt.

That, of course, leads to the next question. How does this come about? What has this to do with sleep when it is simply a matter of differentiation between stimuli? They appear to be different things having nothing in common.

But the matter is quite simple. If we admit that everything can be explained by a constant interaction between the excitatory and inhibitory processes, then we shall have no difficulty in understanding the phenomena. Every time you produce an inhibition, a physiological inhibition, i.e., when you want to separate the active state from the inactive, drowsiness, as I have already said, immediately begins to manifest itself. But you can always eliminate this drowsiness, suppress it, and, on the contrary, ensure the predominance of the excitatory process. This is within your power, within your experimental possibilities, and it is what we do. The moment a state of drowsiness develops in the dog during an experiment, i.e., the moment inhibition takes the upper hand, we apply a stimulation, thereby eliminating the drowsiness, limiting the inhibition and confining it within definite bounds.

How, then, is this matter to be further interpreted? It must be admitted that both excitation and inhibition are dynamic processes, which, on the one hand, may irradiate and spread, and, on the other, may be driven into definite narrow confines and concentrated there. This is the main point, the whole secret, and it is this that we use in all our physiological activity.

The basic property of both processes consists in the fact that on the one hand, when they arise, they tend to spread, to occupy an undue area; on the other hand, they can, given the corresponding conditions, concentrate in definite regions and remain there. When the inhibition is irradiated, diffused, you have the phenomenon of drowsiness or sleep.

Everybody knows, of course, that sleep does not set in instantaneously, that it is a gradual process. Similarly one does not awake all of a sudden; certain time is required before one gradually becomes active and, so to speak, completely throws off the fetters of sleep.

I advise everybody who values scientific truth, who does not want to reconcile himself to superficial knowledge, who is tormented by the thought "is this right or not?", to make a thorough study of two articles in my book *Twenty Years of Objective Study* which is the result of thirty-five years of intense reflections. One of the articles is entitled "Inhibition and Sleep" and the other, written jointly with M. K. Petrova,—"The Physiology of the Hypnotic State."

In any case, in order to give you a more or less clear illustration of this phenomenon, I shall cite one of our experiments.

I must tell you that when you observe the genesis of drowsiness and its first manifestations, you become convinced, and unshakably so, that hypnosis and sleep are, of course, one and the same process. In essence, hypnosis does not differ from sleep; it differs only in certain peculiarities. Hypnosis, for example, is sleep which develops very slowly, i.e., it is at first confined to a very small and restricted area and then begins to spread farther and farther until it finally descends from the cerebral hemispheres to the subcortex, leaving untouched only the centres of respiration, of the heart-beat, etc., though somewhat weakening these too.

I shall now submit to you one of the numerous cases investigated by us in the course of thirty-five years. Let us take a dog which is falling into a state of drowsiness, sleep or hypnosis. What do we observe in this animal? Our experiments with conditioned alimentary reflexes show the following: at first the dog works and eats quite normally; then its tongue comes out of the mouth in a strange manner, and gradually begins to fall down. This is the first manifestation of a certain functional paralysis, of a diminution of activity, of inhibition of the minute centre in the motor region of the cortex which controls the movement of the tongue. This centre becomes inactive, as a result of which the tongue is paralyzed and falls out of the mouth.

A certain period of time passes, and you give the dog food. You see that its tongue functions very slowly and awkwardly; later, you also observe—not at once, but perhaps after the second or third offering of food—that the dog uses its jaws with difficulty, that its mastication is utterly impeded, since the mouth opens and closes very slowly. Thus you witness a weakening of the activity of the masticating musculature, its inhibition or sleep.

At the same time, however, you notice that when food is offered to the dog, which until then was standing with its head turned away or with its eyes fixed on the ceiling, it easily and quickly turns its head towards you and falls upon the food.

But as time goes on, you observe in the course of the experiment that although the dog turns towards you, it brings its head to the food with great difficulty. Consequently, the inhibition or sleep has already seized other points of the skeletal movement, namely, those which control the movement of the neck.

You then see that the dog is unable even to turn towards the food, that it does not move the neck and does not take the food. And finally, you observe the onset of a general passivity of the skeletal musculature: the dog hangs limply in the loops, it is in a state of sleep. Thus, inhibition gradually develops before your eyes in a very obvious and concrete manner; at first it affects the tongue, then it spreads to the cervical muscles, from there to the general skeletal musculature until, finally, sleep sets in.

When you observe this development you can hardly doubt that inhibition and sleep are one and the same process.

The articles to which I have just referred contain numerous similar facts. And anyone that makes a thorough study of them will be convinced that inhibition and sleep are one and the same phenomenon. The only difference is that when the most minute points of the cerebral hemispheres are inactive, it is inhibition and, at the same time, sleep of an isolated cell; but when this inhibition, duly or unduly, spreads under the influence of certain conditions, it embraces more and more new areas of cells and is manifested in a passive, inactive state of the numerous organs dependent on these regions.

It is a pity that cinematography appeared too late and could not be utilized by us and our physiological laboratories. Had it been as accessible then as it is now, all these phenomena could have been very easily comprehended. We could now demonstrate them to you in the space of fifteen minutes, and you would leave us with the deep conviction that inhibition and sleep are one and the same process. But while inhibition is a concentrated process, hypnosis and sleep represent an inhibition which spreads over more or less vast areas.

This displacement of inhibition is of great importance for the comprehension of numerous nervous phenomena.

The British mind, as far as I have been able to follow it, has fully realized and caught up this idea. Thus, Wilson, one of the outstanding British neurologists, now considers all cases of narcolepsy[99] and cataplexy[100] precisely from this point of view. And we, who have observed all these phenomena in dogs, fully agree with him. In our opinion, Wilson is undoubtedly of the right trail.

Such, in general outline, of course, is our understanding of the phenomena relating to alternating sleep in the cerebral hemispheres, as well as to the sleep of the entire brain, following the mobile inhibition.

2

I shall pass now to other facts which to a certain degree compete with the concept just developed by me.

First of all I draw your attention to an extremely important fact recently obtained in the Soviet Union by Prof. Galkin, in A. D. Speransky's laboratory. It should be pointed out that this fact had been observed long ago in the clinic, but only once. Of course, much consideration was given to it at the time, and it was even properly understood by some researchers; but a single fact is not sufficiently convincing. This fact concerns an observation made long ago by Strümpel on a patient, in whom most of the sense organs were damaged and who could communicate with the external world only through two openings which remained intact—one eye and one ear. When he covered these openings with his hands he inevitably fell asleep.

This phenomenon is now being reproduced in the laboratory, and in the following way. We destroy three distant receptors in the dog, namely, smell, hearing and sight; this means that we section the fili olfactorii,[101] sever the n. optici or extirpate the eyes and damage both cochleae. After this operation the dog sleeps twenty-three and a half hours a day. It wakens only when the elementary functions of the organism begin to annoy it—the necessity to eat, to evacuate the urinary bladder or the bowels, etc. But it is extremely difficult to awake the animal in the middle of the day. For this purpose it is not sufficient to stroke the dog, it is absolutely necessary to shake it; and then, before your eyes, it slowly awakens, stretches itself, yawns, and finally stands up. Such is the fact, and it is an exact fact. We repeated the experiment several times and the result was always the same.

The character of the operation performed on the dog excludes any supposition that its nervous system has been damaged. If the operation is done thoroughly, the dog comes through it more or less easily; the fact that two days after the operation it is able to eat shows best the ease with which it endures the loss of the above-mentioned receptors.

However, I must direct your attention to a minor detail. If you destroy the receptors gradually, i.e., at first one of them, the second two or three months later, and in another period of three months the third, then sleep does not set in. The dog, of course, is not as active as the animal which sees and hears normally; indeed, if it has lost the sense of smell and is unable to see, what can make it move? And it is perfectly understandable that for the most part it lies rolled up. But the moment you touch the intact receptor, for example, by stroking the dog, it immediately rises and begins to act.

When, however, you deprive the cerebral hemispheres of a large quantity of stimulations at once, the dog falls into a state of deep sleep. This indubitable fact, which must be reckoned with, naturally gives rise to the following question: how is this phenomenon to be interpreted? And in this connection there arises the problem of two kinds of sleep—the passive sleep caused by the abolition of a large quantity of stimulations usually reaching the cerebral hemispheres, and the active sleep which, in my understanding, is an inhibitory process, since the latter must be undoubtedly regarded as an active process and not as a state of inactivity.

Then the following question of principle arises: Does not the nervous system experience three different states—excitation, inhibition, and a certain indifferent state, when the first two are absent?

But proceeding from the general biological data we have grounds for doubting the existence of a neutral state. Life is a continuous interchange of destruction and restoration, in view of which a neutral state is simply inconceivable. On the whole, the problem can be reduced to the following: is not the passive sleep, which differs from the usual sleep developing under the above-mentioned conditions, also a result of active inhibition?

I think that certain considerations can be submitted which make it clear that the cases of sleep observed in dogs, operated upon in accordance with the method of Speransky and Galkin, could be also accounted for by inhibition; it is an active inhibition greatly favoured by the circumstances, since now there is no need for the inhibition to struggle against an extensive excitatory process and train itself, and as a result the stimulations falling upon the dog extremely facilitate the sleep. Why is this so? Because when the dog is mostly in a lying posture, certain points of its skin are continuously stimulated both mechanically and thermally. It is, therefore, conceivable that the passive sleep is evoked by a continuous and monotonous stimulation of the remaining receptors. And we know the fundamental rule according to which each cell, under the influence of continuous and monotonous stimulations, inevitably becomes inhibited. Consequently, it is possible to interpret this sleep as a result of inhibition proceeding from the remaining receptors subjected to a prolonged monotonous stimulation.

This is partially confirmed also by the following fact. When these dogs are transferred to new surroundings, they at first become more active, are wakened more easily, etc.; in other words, for a time they appear to be more lively.

It can be assumed, therefore, that here, too, due to a decline of the tonus, to the weakening of the excitatory process, the inhibition easily takes possession of the cerebral hemispheres and that weak, monotonous stimulations arise provoking an inhibitory process.

Then comes the following question: what happens to the dogs in which the cerebral hemispheres are extirpated? As a matter of fact, they, too, fall into a state of sleep. And this circumstance is often used as a serious objection to what I have just said, namely, to the statement that normally sleep originates in the cerebral hemispheres.

But I do not regard this objection as being physiologically grounded. It is clear that since sleep is a diffused inhibition, and the latter spreads over the nervous system up to the lower limit of the spinal cord, and since there is a central system and a nerve fibre, inhibition must indispensably take place. In cases when the cerebral hemispheres are absent, why should the inhibition not develop in the lower parts of the central nervous system, now in a concentrated, now in an irradiated form? This is all the more likely since dogs possess lower levels of distant receptors—corpora geniculata[102] (one relating to the ear, and the other—to the eye), and we know that a dog deprived of the cerebral hemispheres reacts to acoustic and visual stimuli. Consequently, the conditions remain the same as when the cerebral hemispheres are intact, and sleep in this case is not excluded—it must inevitably manifest itself. So long as there exists inhibition and there is a cell which, as a result of excitation, is bound to become fatigued and fall into a state of inhibition, all the conditions for the development of inhibition are present. But in the absence of the cortex sleep begins from the subcortical formations. Hence, there is no contradiction here as far as the fundamental facts are concerned, that is, the interchange of excitation and inhibition, their concentration and irradiation. If all these phenomena take place also in the lower part of the central nervous system, then why should sleep not develop there as well? Therefore, I regard these objections as being physiologically groundless; they cannot refute our statement about the initiative of the cerebral hemispheres in the development of sleep in normal conditions.

Next come more important facts. On the one hand, a clinical fact—the encephalitic sleep[103] or somnolence, and on the other, the physiological apparatus advanced by the Swiss physiologist Hess, which, as it were, rivals my concept about sleep originating in the cerebral hemispheres.

As for clinical sleep, the clinical concept of the centre of sleep is well known to clinicians; it is based on the fact that after an infection of the brain, the so-called encephalitis, which is accompanied by somnolence, considerable changes take place in the hypothalamus.[104] On the basis of this fact the simple conclusion is made that the centre of sleep must be located there.

However, I make bold to say that this reasoning, which is based on the fact that there is, on the one hand, sleep, and on the other, a destruction of the hypothalamus, is oversimplified. The above conclusion is, therefore, too hasty.

Firstly, all that we know about the work of the cerebral hemispheres makes the concept of the hypothalamus as the actual centre of sleep doubtful and incomprehensible. It is difficult to assume that an infectious process arising in the brain should in no way tell upon its most reactive part—the cerebral hemispheres. It is likewise difficult to assume that the toxins should remain exclusively in the subcortex, without spreading to the cerebral hemispheres. I fully realize, of course, that bacteria favour definite chemical media, and that there must be a very delicate difference betwen the above-mentioned parts of the brain in respect of their chemical composition. It is quite conceivable that this is true, that the process in question concentrates mainly in the hypothalamus and produces in the nerve cells changes which can be afterwards revealed microscopically. But it may be that in the cerebral hemispheres these changes have only a functional character and manifest themselves in the weakening of the excitability of the hemispheres; at the same time they may be inaccessible to microscopic investigation. It can be supposed that there is a certain gradation of the patho-anatomical changes—from visible phenomena to purely functional, and, finally, invisible ones.

On the basis of what we observe in the hypothalamus it is difficult to assert with confidence that these infections do not exert any influence on the cerebral hemispheres. I would regard such a conclusion as being too hasty.

Secondly, I do not contest the fact that encephalitis is accompanied by sleep, and that this phenomenon is related to the hypothalamus and complies with it. However, I am inclined to interpret this fact in the same way as I have done with regard to the fact established by Speransky and Galkin. Here is what I have to say in this connection. There is no doubt that the hypothalamus is a wide route with definite centres where the stimulations coming from the internal world, i.e., from all the internal organs, are accumulated; its destruction leads to the isolation of the cerebral hemispheres from the entire internal world, from the entire activity of the organs; in other words, it provokes a state analogous to that which arises when all three receptors are destroyed, i.e., when the cerebral hemispheres are deprived of external stimulations. The stimulations proceeding from the internal organs, although we are not conscious of them, constantly maintain a heightened tonus of the cerebral hemispheres. This is proved in the first place by the fact that, as I have already mentioned, dogs with extirpated cerebral hemispheres are in a continuous state of sleep. Further proof is provided by a pigeon deprived of the cerebral hemispheres and remaining constantly immobile and somnolent. But the moment there arises the necessity to eat or to evacuate the excretory organs, the pigeon awakes. Consequently, there is no doubt that these stimulations act on the cerebral hemispheres and bring it to a state of wakefulness.

On the other hand, we know very well that in certain, particular cases, we feel the heart-beat, the movements of the intestines, etc.

Another long-established fact shows that internal stimulations contribute to the maintenance of the cortex in an alert state, to its tonus. This fact was recently confirmed in America, in laboratory conditions, on a person in whom the ability to resist sleep for a long time was investigated. The following phenomenon was observed. A person who like yourself is interested in this particular investigation and who tries hard to keep awake as long as possible, despite a strong desire for sleep, successfully resists the state of somnolence only when he walks or when he is in sitting posture. The moment he lies down, i.e., relaxes the musculature, he immediately falls asleep.

Thus you clearly see that our internal stimulations greatly contribute to the maintenance of a certain tonus in the cortex.

In my view, encephalitic sleep is caused by the separation of all internal stimulations from the cerebral hemispheres due to an affection of the hypothalamus; it is, consequently, the same drastic decline of the tonus that is observed when the external receptors are destroyed.

There remains one more important fact which supports the reasoning of the clinicians concerning the centre of sleep. I have in mind the experiments of Hess, in the course of which sleep was evoked by electric stimulation of definite parts of the brain. I am not going to contest this fact either. I fully admit it and believe that it will be reproduced by other investigators; but I consider it necessary to say a few words about its proper interpretation and the objections which can be raised to the conclusion drawn by Hess.

The first thing which attracts attention is that the above fact does not fully accord with the clinical fact, since the points in the latter case do not coincide with those stimulated by Hess.

Hess himself emphasized this circumstance and stated that his experiments would disappoint the clinicians, since anatomically the points which produced sleep did not coincide.

Whereas the lesions caused by encephalitis are located in the region of the third ventricle, in its lateral walls, etc., Hess subjected to stimulation the lowest part of the brain, almost reaching the brain stem.

How is this fact to be interpreted? It must be pointed out that a phenomenon observed in the given organism under normal conditions, as in our case, is one thing, and a phenomenon observed under pathological conditions, especially when they are artificially produced in the laboratory, as for example, the stimulation of the brain, is another thing. They are, of course, absolutely different phenomena. While in the latter case maximum simplicity can be attained, in the normal state the phenomena become complicated. But in the given case even Hess, who obtained a definite state in dogs by stimulating certain points in the brain, stated that this could be an excitation not only of the cells of an imaginary, fantastic "centre of sleep," but of centrifugal or centripetal fibres; at the same time he drew attention to the fact that the points used by him for producing a state of sleep had been very limited.

Then I am fully entitled to ask the following question: is not this simply a reflex sleep originating from the same cerebral hemispheres? Indeed, we know very well that a monotonous irritation of the skin, both in our laboratory experiments on dogs and in our experiments on human beings, produces a hypnotic state, a state of sleep. There is nothing surprising in the fact that certain stimulations of the nerve paths may provoke sleep. Consequently, these experiments do not prove that sleep is a stimulation of a definite centre. Along with hypnotization by means of passes, which, undoubtedly, is a reflex inhibition caused by monotonous stimulations, a hypnotic state can also be evoked with the help of the verbal method. The latter is addressed to the cerebral hemispheres. In our laboratory we produce a state of sleep in dogs by means of a weak electric stimulation of the skin; this sleep is so persistent that after several experiments the place where the electrodes were fixed becomes a conditioned hypnogenous stimulus: it suffices to touch this place or to cut the hair on it, and the dog immediately subsides into deep sleep. Such is the effect of peripheral stimulations.

What, then, is the value of Hess's proof, especially since he himself states that his sleep is produced with the help of a weak electric current, and besides, a special (faradic, and not direct) one? Consequently, this could be a very weak stimulation corresponding to that which we obtain in the laboratory by means of a weak electric current.

I find, therefore, that the Hess experiment, which was so highly convincing in the eyes of the author himself, and even more so in the eyes of the clinicians, can be rightfully contested and reduced to what I have already said, the existence of a special centre of sleep being out of the question. In my opinion, the crude idea that there is a special group of nerve cells which produce sleep, while another group produces the state of wakefulness, is, from the physiological point of view, contradictory. We observe the phenomenon of sleep in every cell; what reason have we, then, for asserting that there is a special group of hypnogenous cells? If a cell exists, it inevitably produces a state of inhibition, which irradiates and renders all the neighbouring cells inactive; and when the inhibition continues to spread, it produces sleep.

Such is my firm conviction.

DISCUSSION

Question: What is responsible for the absence of sleep in dogs whose distant receptors were extirpated at different times?

Answer: As you know, the inaction of one receptor always leads to an intense training of all others. It is a well-known fact, for example, that blind people have a highly sensitive touch. The same thing occurs in the given case with the reception of the external world when the olfactory receptor is removed; the activity of the latter is made up by the reinforced activity of the ear or the eye. It is, therefore, obvious that successive extirpation of the receptors makes possible such a training, while simultaneous extirpation excludes it.

It should be pointed out that there are indications which show that with the lapse of time, in the course of years, the dogs to a certain extent train themselves with the help of the remaining receptors (that is, of the oral and cutaneous receptors) and in the end become more active. In any case this fact was manifested in dogs which have been used for these operations.

Question: From the point of view of inhibition how do you explain a sleep accompanied by abundant dreams?

Answer: As I have already said, sleep is an inhibition which gradually and steadily spreads to the lower levels of the brain. It is clear, therefore, that when sleep and fatigue begin to set in, the highest part of the cerebral hemispheres, which controls verbal activity (I call it the second signalling system of reality), becomes inhibited first, since we constantly operate with words.

I can add now—for the sake of brevity I omitted it in my talk—that this inhibitory process has its external and internal stimuli.

Among the internal stimuli of inhibition is the humoral element, or consequently, certain cellular metabolites, which evoke this inhibition. On the other hand, the external inhibitory stimuli, as I have already mentioned, are monotonous and weak. Naturally, it is the highest part of our brain, the verbal part of our higher cortical activity which functions in the daytime. Fatigue calls forth inhibition, and this part becomes inactive. But along with the verbal function of the cerebral hemispheres there is a function which we share with animals and which is termed by me the first signalling system, i.e., the reception of impressions produced by all the stimuli acting on us.

It is quite clear that when we are in an alert state, the part of the cortex controlling our speech, inhibits the first signalling system; that is why in the alert state we (except the artistic type of man whose constitution is of a peculiar character), when speaking, never imagine the object which we designate by words. I close my eyes and think of the person sitting in front of me, but I do not see him in my thoughts. Why? Because the excitation of the higher part inhibits the lower part. That is why when sleep begins and embraces only the higher part of the hemispheres, the adjacent lower part bearing a direct relation to impressions prevails and is manifested in dreams. When there is no pressure from above, a certain degree of freedom sets in. And even here a new fact must be added, a fact encountered in physiology, namely, positive induction. When one point becomes inhibited, the other, on the contrary, becomes excited. And if we grant this, i.e., if we assume positive induction, the phenomenon of sleep becomes particularly clear.

Question: Judging by what you have said, there is no centre of sleep. How, then, are we to explain the fact that for such an important function as sleep there is no centre, while there are centres for other, even less important functions of metabolism, for example, a sugar centre, a water centre, etc.?

Answer: The explanation is quite simple. Inhibition and sleep exist for each cell. Consequently, they do not need any special cellular groups.

Question: How should the problem of fatigue be considered from this point of view?

Answer: I have already said that fatigue is one of the automatic internal stimuli of the inhibitory process.

Question: How do you explain the occurrence of fits during sleep?

Answer: There is nothing special in this, because we are aware of the resources of our nervous system, the cerebral hemispheres. The following phenomenon is often observed: inhibition spreads over the cerebral hemispheres and sleep sets in; nevertheless, certain points, which I call points on duty or on guard, may remain active. This is observed, for example, in the sleep of the miller who wakes up when the noise of the mill ceases, or in the sleep of the mother who wakes up at the faintest sound coming from her child, but who is not disturbed by much louder sounds. So that when the conditions for the excitation of a certain part arise, sleep does not prevent the development of the process.

Question: How can all the complicated reactions of a hypnotized person be explained, if we admit that in the state of hypnosis his entire nervous system is inhibited with the exception of the one point by means of which he communicates with the hypnotist?

Answer: I have pointed out that hypnosis is a kind of sleep which gradually spreads from a basic point.

Here is a fact which was observed in our laboratory. You have a dog which long ago was deprived of three receptors and which is in a constant state of sleep. Nevertheless, you can awaken it with the help of the remaining cutaneous receptors, bring it to the laboratory, place it in the stand and perform experiments on it. Then the following, extremely interesting phenomenon is observed, a phenomenon analogous to the hypnotic state: you can elaborate only one reflex in a dog of this kind; it is impossible to form in it, as can be done in a normal animal, two, three, or four reflexes simultaneously. This is explained by the fact that the cortical tonus, i.e., the excitatory process in the entire cortex, is very weak; hence, when it concentrates on one stimulus, there is nothing left for other stimuli and they remain inactive.

In this way I explain also hypnosis and rapport.[105] The cerebral hemispheres are not wholly embraced by inhibition, since certain points of excitation may be formed in them. Through such an excited point you evoke a response and suggest. And then the hypnotized person inevitably executes your order, for when you give it you have everything extremely restricted. Consequently, all the influence of the other parts of the cerebral hemispheres on that which is suggested by your words, on the stimulations which you produce, is fully isolated from all others. And when the hypnotized person wakes up after such suggestion, he is powerless to do anything with this isolated excitation, since it is detached from all others. Therefore, in hypnosis it is a question not of complete, but of partial sleep. That is the difference between hypnotic and natural sleep. Whereas natural sleep represents a general inhibition of the cerebral hemispheres, however, with the above-mentioned exception of the so-called points on duty and points on guard, hypnosis is a partial inhibition affecting only a definite point, all others remaining in an active state.

Question: How do you explain the regular interchange of sleep and wakefulness?

Answer: It is clear that our daytime activity is the sum total of the excitations which cause a certain amount of exhaustion; when this exhaustion reaches peak, it evokes automatically, in an internal humoral way, a state of inhibition accompanied by sleep.

XI

PHYSIOLOGY AND PSYCHOLOGY

PHYSIOLOGY AND PSYCHOLOGY IN THE STUDY OF THE HIGHER NERVOUS ACTIVITY OF ANIMALS[106]

First of all, I consider it my duty to thank the Philosophical Society for expressing readiness, through its chairman, to listen to what I have to say. I do not know to what extent my subject will be of interest to the members. As for myself, however, I have a special purpose which will be revealed at the end of my address.

I wish to inform you of the results of very extensive research carried out by me in the course of many years jointly with a dozen or so colleagues who constantly participated in it both with their heads and with their hands. Without their co-operation, this work would have been only one-tenth of what it is. So that when I use the word "I," you should take it not in the narrow sense of an author, but, so to speak, in the sense of a conductor. In the main I guided and co-ordinated the work.

Now for the essence of my subject.

Let us take any higher animal, for example, the dog. Although it is not at the top of the zoological ladder (the monkey occupies a higher place), it is closer to man than any other animal and has been his companion since prehistoric times. I heard the late zoologist Modest Bogdanov[107] state the following when reviewing prehistoric man and his companions, especially the dog: "Justice compels us to say that it is the dog that helped man to become what he is." Such is his appreciation of the dog. Consequently, the dog is not just an ordinary animal. Indeed, consider a watch-dog, a hunter, a domestic pet, etc.; before your eyes is their entire activity in all its higher manifestations, or, as the Americans are inclined to call it, their entire behaviour. If I wished to study this higher activity of the dog, that is, to systematize the phenomena of its life and to disclose the laws and rules which govern them, the following question would inevitably confront me: how shall I act and which way shall I choose? Generally speaking, there are two ways. One is the ordinary way taken by everybody. It consists in attributing the human inner world to the animal, that is, in assuming that the animal thinks, feels, desires, etc., in much the same way as we do. Consequently, this means guessing what takes place within the animal and interpreting its behaviour on the basis of these suppositions. The other way is entirely different; this is the way of natural science which considers the phenomena, the facts, from a purely external aspect, and which in the given case would concentrate only on the agents of the external world that act on the dog, as well as on the visible reactions of the dog to these agents.

The question, therefore, is this: which way is preferable, more expedient, the best way to tackle the problem? Allow me to answer this question, which is of great importance, by presenting the facts in chronological order. Several decades ago my laboratory made a study of the digestive process, and specially investigated the activity of the digestive glands producing the digestive juices by means of which the food is transformed, assimilated by the organism and enters into the vital chemical processes. Our job was to study all the conditions which determined the work of these glands. Much of our investigation was devoted to the first of these glands, the salivary gland. A detailed and systematic study of the latter demonstrated that its work is extremely delicate and highly adaptable to whatever substance enters the mouth, and that the quantity and quality of saliva show corresponding considerable fluctuations. When the ingested food is dry, there is an abundant secretion of saliva, since the food must be moistened; when the food is moist, the amount of saliva is smaller. If it is a matter of food which must pass into the stomach, the saliva is rich in mucus; it lubricates the mass of food and thereby facilitates its ingestion. But when there is a substance which must be ejected from the mouth, the saliva is watery and helps to rinse the mouth.

Thus, we see a series of delicate co-ordinations between the work of the salivary gland and the kind of substance upon which the saliva is secreted. Next comes the question: what underlies this delicate co-ordination, what is its mechanism? The physiologist—and that is my speciality—has a ready answer to this question. The properties of the food act on the nerve endings and stimulate them. These nervous impulses proceed to definite points of the central nervous system and thence to the nerves leading to the salivary gland. In this way there arises an obvious connection between the substance which enters the mouth and the work of the gland. The particulars of this connection are explained by the fact that the nerves from the oral cavity, where the substances act, are separately excited by acid, sweet, rough, soft, hard, hot, cold, etc.; thus the impulses are conducted now by one nerve, now by another. In the central nervous system these impulses are transmitted to the salivary gland along different nerves. Some of them evoke one kind of activity, the others—activity of a different kind. Consequently, the different properties of the food stimulate different nerves, and in the central nervous system there takes place a transfer of the impulses to corresponding nerves which evoke one or another activity.

Since we aimed at a complete investigation we had to consider all the concomitant conditions, apart from those I have just mentioned. The substances introduced into the mouth act on the salivary gland. But does the same thing occur when food is placed in front of the dog, i.e., is it effective at a distance? We know very well that when we are hungry, the sight of food evokes in us a flow of saliva. Hence, the expression "the mouth waters." It was, therefore, necessary to investigate this phenomenon as well. What does it mean, especially since in this case there is no contact with food substances at all? Concerning these facts physiology used to say that in addition to ordinary stimulation, there is psychical stimulation of the salivary gland. Very well. But what does this imply, how is it to be interpreted, how must we, physiologists, tackle the question? We could not ignore it, because it played a certain part. On what grounds could we discard it? Let us, first of all, consider the bare fact of psychical excitation. It turned out that psychical excitation, i.e., the action of a substance at a distance, is exactly the same as when it is in the mouth. It is absolutely the same in all respects. Depending on the kind of food shown to the dog, whether dry or liquid, edible or absolutely inedible, the salivary gland functions in exactly the same way as when these substances are introduced into the mouth. The psychical excitation reveals exactly the same relations, but on a somewhat smaller scale. How, then, is this to be studied? Naturally, when we see a dog eat rapidly, snatch the food and chew it for a long time, we think, willy-nilly, that the animal strongly desires to eat, and that it is this that makes it rush to the food and swallow it. It longs to eat. Another time the dog's movements are slow and languid, and so we say that it has no great desire to eat. When it eats, you see the work of the muscles alone, which is fully aimed at introducing the food into the mouth, at chewing and swallowing it. Judging by all this, one would say that the dog experiences pleasure in eating. On the contrary, when an inedible substance gets into the mouth, and the dog ejects it, forces it out with the help of the tongue and by shaking its head, we involuntarily say that this is unpleasant for the animal. Now, when we decided to elucidate and analyse this phenomenon, we at first adopted this trite point of view. We took into account the feelings, desires, imagination, etc., of our animal. And this resulted in a quite unexpected and extraordinary fact: one of my colleagues and I irreconcilably differed in opinion. We could not come to agreement, could not convince each other as to who was right; for decades prior to this, as well as afterwards, we always reached agreement on all questions, one way or another, but the given case ended in complete discord. This made us meditate on the matter. It seemed probable that we were not on the right track. And the more we thought about the matter, the more convinced we became that another course of investigation should be followed. Overcoming the difficulties which I experienced in the beginning, and taking the way of persistent thought and concentrated attention, I finally reached the ground of true objectivity. Such psychological expressions, as the dog guessed, wished, desired, etc., were wholly withdrawn from our use (in our laboratory a fine was even imposed on their use). Finally, all the phenomena with which we were concerned appeared to us in a new light. So what, then, is the point? What is that which the physiologists term psychical stimulation of the salivary gland? We, naturally, put ourselves the question: is it not a form of nervous activity, long ago established by physiology and well known to physiologists? Is it not a reflex? And what does this reflex of the physiologist represent? It consists of three chief elements. In the first place, there is an indispensable external agent producing the stimulation. In the second place, there is a definite nervous path by means of which the external impulse makes itself felt in the effector organ. This is the so-called reflex arc, a chain composed of an afferent nerve, a central part and a centrifugal or efferent nerve. And finally, in the third place, the law-governed, but not accidental or capricious, nature of the reaction. Given certain conditions the reaction always and invariably arises. Of course, this must not be understood in the sense of absolute constancy, in other words, as meaning that circumstances may never occur in which the agent does not act. It is obvious that there can be conditions in which the action remains disguised. According to the law of gravity all things must fall to the earth, but once you support them this does not occur.

Now let us return to our subject. What, then, is the psychical stimulation of the salivary gland? When food is placed in front of the animal, before its eyes, it certainly acts on the animal, on its eye, ear, nose. No essential difference between this action and that in the mouth is observed. There are reflexes from the eye and from the ear. Upon hearing a loud sound we start—a reflex action. Under the action of a strong light the pupils of our eyes contract. Hence, this does not interfere with our concept that what we call psychical stimulation is a reflex. The second element, the nervous path, is, obviously, also present here; for when the dog sees the food, the nervous path originates not from the nerves of the mouth, but from those of the eye, then proceeds to the central nervous system and from there puts the salivary gland into action. Again there is no essential difference here, and there is nothing to prevent us from representing this as a reflex. Let us now examine the third element—the law-governed nature of the reaction. In this respect the following must be pointed out: the given stimulation acts less regularly, less often than when the stimulant is in the mouth. However, it is possible to acquire such a degree of knowledge and mastery of the subject that all the conditions on which the action of the substance at a distance depends will be under our control. If this has been attained (which is now the case), then the law-governed nature of the reaction is in evidence.

But the "psychical" excitation has an additional feature. When we examine these phenomena more closely, it appears that among the agents acting at a distance there may be some which did not exist previously. Here is an example. Let us suppose that the attendant enters for the first time the chamber in which the dog is kept and brings in the food. The food begins to act the moment it is shown to the dog. And if the same attendant brings in the food several days in succession the upshot will be that the moment he opens the door and puts his head in, the action begins. Thus, a new stimulating agent has appeared. If this is continued long enough, then the sound of the attendant's steps alone is sufficient to evoke a secretion of saliva. Consequently, stimuli that did not exist before have now developed. The difference appears to be considerable and essential: while in the physiological stimulation the stimuli are constant, here they are variable. However, this point can be interpreted in the following way: should the new stimulus become effective under strictly definite conditions, which also can be determined by the experimenter, i.e., if the entire phenomenon obeys certain laws as well, then this cannot serve as an objection. Although the stimuli are new, they inevitably arise under definite conditions. Accident is ruled out. Here, too, the phenomena are related to definite laws. I can say that just as the first reflex was characterized by the presence of a stimulus which travelled along a definite path and, in certain circumstances, evoked our phenomenon, here, too, the phenomenon arises under strictly definite conditions. The essence, the composition of the concept of reflex has not changed in the least.

It has been proved that any agent of the external world can be made a stimulus of the salivary gland. Any sound, odour, etc., may become a stimulus that will excite the salivary gland exactly in the same way as it is excited by food at a distance. As to the precision of the fact, there is no difference whatever; it is only necessary to take into account the conditions in which the fact exists. What, then, are the conditions which can become stimuli of the salivary gland? The chief condition is coincidence in time. The experiment is performed as follows. We take, for example, a certain sound which has no relation to the salivary gland. This sound acts on the dog. Then we feed the dog or introduce acid into its mouth. After several repetitions of this the sound itself, without the addition of food or acid, begins to excite the salivary gland. There are altogether four or five, at most six conditions under which any stimulant, any agent of the external world, becomes a stimulus of the salivary gland in the dog. Once this is so, once it has become a stimulus under a definite series of conditions, it will always act with the same precision as food or as any rejected substance introduced into the mouth. If any external agent invariably becomes a stimulus of the salivary gland under definite conditions, and having become such, inevitably produces its action, then what grounds are there for saying that in essence this is anything other than a reflex? Actually this is a law-governed reaction of the organism to an external agent effected through the medium of a definite part of the nervous system.

As I have said, the ordinary reflex is formed like this: there is a definite nervous path along which the stimulation proceeding from the peripheral part is conducted to the effector organ, in the given case, the salivary gland. This conducting path is, so to speak, a live wire. But what happens in this case? Here it should be added that the nervous system is not only a conducting apparatus, as is generally regarded, but also a connecting one. And there is nothing paradoxical in this supposition. If in everyday life we widely use contactors, for example, in electric lighting, telephone communication, etc., then it would be strange indeed if in the most perfect machine on earth, there were no application of the principle of connection, but only of conduction. Hence, it is quite natural that along with conducting properties the nervous system should also possess a connecting apparatus. Analysis has shown that the constant form of stimulation of the salivary gland by food at a distance, which is an ordinary case known to everybody, is a similar formation of a new nervous path by means of connection. While working in Prof. Vartanov's laboratory, Dr. I. S. Tsitovich performed the following interesting experiment. He took a new-born puppy and fed it exclusively on milk for several months; the puppy had known no other food. Then he subjected it to an operation so that the work of the salivary gland could be observed; afterwards he showed the puppy foods other than milk. But not one of them, shown at a distance, produced any effect on the salivary gland. Consequently, when different foods act from a distance, this is a reflex newly formed as a result of individual life experience. The matter can be described thus: when a piece of meat is first placed before a puppy several months old neither its appearance nor its odour produce any action on the salivary gland. It is necessary that the food be taken into the puppy's mouth at least once, to evoke a simple, purely conducting reflex, and only then there develops a new reflex to the appearance and odour of the meat. And so, gentlemen, you see that we have to recognize the existence of two kinds of reflexes: one is ready from the time of birth, and is of a purely conducting character, and the other is continuously, incessantly being formed in the course of individual life, obeys exactly the same laws, but rests on the basis of another property of our nervous system—on connection. The first reflex can be termed inborn, and the other—acquired, or respectively—generic and individual. The inborn, generic, constant and stereotyped reflex we termed unconditioned; the other, which depends on a multitude of conditions and constantly fluctuates in conformity with the circumstances, we called conditioned; in this way we characterized the reflexes from the standpoint of practice, from the point of view of laboratory investigation. The conditioned reflex is also indispensable, and thus, like the unconditioned reflex, belongs entirely to the domain of physiology. With this formulation physiology, of course, comes into possession of an enormous mass of new material, since there is an infinite number of these conditioned reflexes. Our life consists of a multitude of inborn reflexes. When one says that there are three kinds of these reflexes, namely, the self-defensive, the alimentary and the sexual, this is undoubtedly only an academic scheme; actually, they are numerous, and they must be divided and subdivided. Consequently, there is a multitude even of these simple, inborn reflexes; as for conditioned reflexes their number is endless. And so, with the establishment of this new concept of conditioned reflex, physiology acquires a vast domain for investigation. This is the domain of higher activity connected with the higher centres of the nervous system, while the inborn reflexes are related to the lower part of the central nervous system. If you extirpate the cerebral hemispheres of an animal, the simple reflexes remain, but the new, connecting reflexes disappear. It is understandable that innumerable questions will arise in connection with these conditioned reflexes, if you constantly consider all the conditions under which they originate, exist, are disguised, temporarily weakened, etc. This is one half of the higher nervous activity, as the modern physiologist sees it. Now for the other half.

It is quite obvious that the nervous system of an animal consists of a set of analysers which decompose nature into its separate elements. We are familiar with physical analysers, for example, the prism which decomposes white light into its elementary colours, or resonators which split complex sounds into their component elements. The nervous system is a true collection of such analysers. The retina,[108] for instance, discerns in nature the oscillations of light; the acoustic part of the ear detects the oscillations of air, etc. Each of the analysers, in its turn, continues this endless division into separate elements. By means of our ear analysers we divide tones according to their wave lengths, wave amplitudes and forms. Thus the second function of the nervous system consists in analysing the external environment, in decomposing the different complexities of the world into their separate elements. This analysis is also effected by the lower parts of the central nervous system. If we decapitate an animal, with the result that its organism possesses only the spinal cord, the analysing function still remains. The moment you act mechanically, thermally or chemically upon such an animal, it reacts with a special movement to each stimulation. The most delicate analysis, of which both animal and man are capable, takes place in the higher parts of the nervous system, in the cerebral hemispheres. And this is also a purely physiological subject. As a physiologist, there is no need for me, when studying this subject, to resort to any notions and concepts alien to physiology. Investigation of the analysers which are situated in the cerebral hemispheres discloses very important facts. For example, when a new reflex is formed from a certain sound for the first time, the new stimulus usually appears in a general form, i.e., if you have elaborated a conditioned reflex from a definite tone, say of 1,000 oscillations, and then try for the first time some other tones, for example, of 5,000, 500 or 50 oscillations, you will obtain an action from each of them as well. In the beginning the analyser always enters into the reflex with its more general function. Only later, with the repetition of the reflex, specialization gradually takes place. This is a very important law. It is clear that we can investigate this fact, too, without having recourse to concepts alien to physiology. The limit of the analysing capacity is likewise accessible to investigation. It has been proved, for example, that the analyser of the dog can discern one-eighth of a tone. The sensitiveness of the auditory apparatus of the dog for tones is much greater than that of man. While we are able to distinguish a maximum of 50,000 oscillations per second, the auditory apparatus of the dog is excited by a frequency of even 100,000 oscillations. In this connection I wish to remind you of the following interesting fact. If we damage the cerebral hemispheres, where the corresponding ends of the visual, auditory and other analysers are situated, there take place, of course, definite disorders. For example, when the terminals of the eye analyser of a dog are damaged, the animal does not recognize its master, but at the same time it does not blunder into a chair, or run into its master. That is why it has been said that the dog sees, but does not understand. It must be admitted, however, that this phrase, when analysed, is in itself difficult to understand.

When, in the given case, it is said that the dog sees but fails to understand, this simply means that the analysing apparatus is demaged to a degree that reduces the analysing capacity to the minimum. The eye merely distinguishes light from shadow, and a space occupied by a body from a free space, but it is no longer able to distinguish the forms and colours of objects.

Thus, we recognize two aspects of the higher nervous activity in the higher animals: on the one hand, the formation of new connections with the external world, and on the other, the higher analysis of phenomena.

If you grant these two kinds of activity, you will see that they embrace a good deal, and it is difficult to assume that some things remain outside of them. Only a detailed study can establish this. All kinds of training, education, development of habits, orientation in the surrounding world, with its events, natural phenomena and people, represent either a formation of new connections, or the most delicate analysis. At least, very many manifestations are related to these two activities, and in any case limitless work awaits us in this field; but we, physiologists, in performing this work, do not make use of any alien concepts.

The study of the above-mentioned activities revealed that the first important property of the higher cerebral mass is a peculiar movement of the nervous processes in this mass. I shall not dwell on this now, since it will be the object of a separate experiment, of which I shall speak later and which I will describe in detail. Another extremely important property is that if in the higher part of the brain, in the cerebral hemispheres there is a certain functionally isolated element and if the latter is subjected to a constantly repeated stimulation proceeding from a definite agent, then, sooner or later, it inevitably falls into a state of inaction, a state of sleep or of hypnosis. The fundamental property of the higher nervous element is its extremely high reactivity; however, when it is temporarily isolated in such a way that the excitation does not spread, but concentrates in it for some time, i.e., if the excitation invariably acts on one point, then this element always passes over into a state of sleep. This relation of the higher nervous cells to the stimuli explains many things. It can be interpreted either as a protection of the precious substance of the cerebral hemispheres, a substance which must constantly respond to all the influences of the external world; or it may be understood in the biological sense, i.e., when it is variable, the stimulus determines a reaction in the form of a definite activity, and when it becomes monotonous and remains without further consequences, it ensures rest and the possibility of preparing for a new expenditure. I shall not go into the details.

I am coming to the end of my talk. I shall describe an experiment which partly illustrates the data I have mentioned. I am most anxious to have your views about this fact, about this experiment. If some of my descriptions are not clear, interrupt me right away and ask for additional explanations in order to understand the entire experiment as clearly as if you had been present yourselves.

Here is a diagram illustrating the animal used for the experiment. For the present you see two black spots, one on the foreleg and the other on the thigh. These are the places to which we attached our apparatus for mechanical stimulation of the skin. We used the apparatus in the following way. When we put it into action and the mechanical stimulation of the above-indicated places began, we introduced acid into the dog's mouth. The acid, of course, evoked the secretion of saliva through a simple inborn reflex. This was repeated several times in succession, from day to day. After some repetitions the flow of saliva could be obtained merely by mechanical stimulation of the skin, just as if the acid had been introduced into the dog's mouth, although no acid was used.

Now I pass to the discussion of this fact, physiologically and, at the same time, as far as I can, psychologically, from the standpoint of the zoopsychologists. I cannot guarantee that I shall reproduce their phraseology, since I have lost the habit of using their language, but I shall try to approximate to their expressions. The fact consists in the following. I apply a slight mechanical stimulation of the skin and then immediately pour acid into the dog's mouth. A simple reflex evokes the secretion of saliva. After this procedure has been repeated several times, a flow of saliva could be obtained by mechanical stimulation of the skin alone. Our interpretation of this phenomenon was that a new reflex had been formed, that a new nervous path between the skin and the salivary gland had been established. The zoopsychologist, who endeavours to penetrate into the dog's soul, says that the dog notices this and remembers that the moment the irritation of a certain place of the skin begins, acid is introduced into its mouth. Consequently, when only a stimulation of the skin is applied, it imagines that the acid is entering its mouth and reacts to it by a flow of saliva, etc. Let it be so. I proceed now to another experiment. A reflex elaborated by us, repeated itself every time with absolute precision. I put now into action the mechanical apparatus and obtain as usual a complete motor and secretory reaction; however, this time I do not introduce the acid. After an interval of one or two minutes, I repeat the experiment. Now the action is already less; the motor reaction is not so pronounced and the salivation not so abundant. Again I refrain from introducing the acid, and after two or three minutes repeat the mechanical stimulation. The reaction is now still less. After four or five repetitions there is no reaction at all; nor is there any movement or salivary secretion. This is a clear and absolutely exact fact. And here is how it is interpreted by the physiologist and the zoopsychologist. I say that the phenomenon of inhibition, already well known to us, sets in. I base this affirmation on the fact that if I interrupted the experiment and made an interval of, say, two hours, the mechanical stimulation would again produce its action on the salivary gland. As a physiologist I understand this quite clearly. It is an established fact that with the lapse of time all processes in the nervous system become effaced, if the cause which provokes them ceases to act. The zoopsychologist, in his turn, has no difficulty in providing an explanation; the dog, he says, notices that now, upon mechanical stimulation, no acid is introduced into its mouth, and therefore after four or five of these skin stimulations it ceases to react. There is no difference so far: you can agree with one as well as with the other. But let us introduce a new complication into our experiment. When the zoopsychologist and the physiologist contend with each other for the appropriateness, correctness of their explanations, it is necessary to formulate the requirements with which the explanations must comply. These requirements are well known. One of them is that each explanation must cover all that occurs physically. The facts must be explained from a single point of view. That is the first requirement, and the second is even more obligatory; it consists in being able, on the basis of the given interpretation, to predict the explained phenomena. He who is able to do so, is right compared with the one who cannot. And inability to predict signifies the bankruptcy of the latter.

I complicate my experiment in this way. Our reflex has been elaborated at several points on the dog's skin, let us suppose, at three points. The mechanical stimulation of each of these places produces the same acid reaction measured by a definite amount of saliva. This measurement is the simplest, since measurement of the motor reaction would be more difficult. The motor and salivary reactions go together and are parallel. They are the components of a single complex reflex. So, we have formed a number of cutaneous reflexes. They are all equal, act with absolute precision and give the same number of divisions of the graduated tube used in measuring the secretion of saliva, for example, thirty divisions for one half-minute stimulation. I stimulate the point on the foreleg in the way I have just mentioned, i.e., without accompanying it by the introduction of acid, so that after the fifth or sixth application the mechanical stimulation does not produce any effect whatever. Thus, in the terminology of the physiologists, a complete inhibition of the reflex has taken place. When this has been obtained on the foreleg, I bring another mechanical apparatus into action and begin to stimulate a spot on the hind-leg. The following phenomena are then observed. If, immediately after discontinuing the foreleg stimulation which produced a zero effect, I bring the mechanical apparatus on the thigh into action, so that there is no interval between the end of the first stimulation and the beginning of the second, I obtain a complete effect at the new place equalling thirty divisions, and the dog behaves as if the stimulation were applied for the first time. The flow of saliva is abundant and there is a motor reaction—the dog tries to eject the non-existent acid from its mouth with the help of the tongue, in short, it acts accordingly. Then, when I bring the effect of stimulation on the foreleg again to zero (by repeating the mechanical stimulation without the acid), and when I stimulate the point on the hind-leg not after a zero interval but after five seconds, I obtain not thirty divisions from the new place, but only twenty. Thus the reflex has diminished. I then prolong the interval to fifteen seconds and obtain a slight effect from the new place, equalling only five divisions. Finally, if I stimulate after twenty seconds, there is no effect at all. Continuing, I extend the interval to thirty seconds; the effect at the new point reappears. With an interval of fifty seconds the effect becomes greater, reaching twenty-five divisions, and after an interval of sixty seconds it is again complete. If, after obtaining the zero result, we repeat the stimulation at the same point, on the shoulder, with intervals of five, ten, fifteen minutes, the effect will always be zero. I am not sure whether I have made this quite clear to you. What does this signify?

I invite the zoopsychologists to give their explanation of these facts. I must tell you that on more than one occasion I have gathered intelligent people, men with profound knowledge in the domain of natural sciences, doctors, etc., placed before them the facts to which I have just called your attention, and requested an explanation of the phenomena. Most of the naive zoopsychologists attempted to give their explanations, each in his own way but at variance with all others. In general, the result was deplorable. All possible and extremely diverse interpretations were examined, but they could in no way be reconciled. Why is it that on the shoulder, when a zero effect has been obtained, the stimulating apparatus no longer produces any action, while at the other point, we obtain now a complete effect, and now no effect at all, strictly depending on the intervals between the stimulations.

And I have come here in the hope of getting an answer to this question from the point of view of the zoopsychologist. I shall now tell you about our viewpoint. Our explanation is of a purely physiological, purely material and spatial character. It is obvious that in our case the skin represents a projection of the cerebral mass, and that different points of the skin are a projection of corresponding points of the brain. When at a definite point of the brain we evoke a certain nervous process by stimulating the corresponding part of the skin on the shoulder, this process does not remain in the same place, but shows a certain movement. First it irradiates over the cerebral mass, and then returns to the point of origin and concentrates there. Naturally, each movement requires a certain amount of time. When, after elaborating an inhibition at the point of the brain corresponding to the shoulder, I immediately try to stimulate another place (the thigh), the inhibition has not yet spread to that place. It takes twenty seconds to get there; that is why in twenty seconds, and not earlier, the thigh also becomes fully inhibited. The concentration requires forty seconds, and therefore one minute after the end of the zero stimulation on the shoulder, we already have a complete restoration of the reflex at the second place (the thigh); but in the initial place (the shoulder) the reflex is absent even after five, ten or fifteen minutes. Such is my interpretation, the interpretation of a physiologist. I have not had any difficulty in explaining this phenomenon. For me it fully coincides with other facts in the physiology of the movements of a nervous process. Now, gentlemen, let us verify the correctness of this explanation. I have the means for doing so. If we actually have a movement, then we can predict the intensity of the effect at all the intermediate points, on the basis of the fact that this movement occurs in two opposite directions. I shall take only one intermediate point. What is to be expected there? Being nearer to the point where I produce the inhibition, it will become inhibited earlier than the other points. Consequently, the zero effect will appear at this point sooner and persist longer, until the inhibition spreads and then returns. At this point the return to normal excitability will require more time. Precisely this occurred in the actual experiment. At the given intermediate point after a zero interval there were not thirty but twenty divisions. Then the zero effect appeared after ten seconds, when the complete inhibition reached this place; it persisted for a long time, while the inhibition was irradiating and then returning to the initial point. It is understandable that while the normal excitability on the thigh was re-established after one minute, here it reappeared only after two minutes. This is one of the most striking phenomena ever observed by me in the laboratory. A definite process is taking place in the depth of the cerebral mass, and its movement can be predicted with mathematical precision. So there you have, gentlemen, our complication of the experiment, and our attitude, the attitude of physiologists, in relation to it. I do not know what answer I shall get from the zoopsychologists, what attitude they will take in regard to these facts, but answer they must. If, however, they refuse an explanation, I shall be justified in saying that their point of view is in general unscientific and of no use for creative research.

REPLY OF A PHYSIOLOGIST TO PSYCHOLOGISTS[109]

1

The article by Edwin R. Guthrie "Conditioning as a Principle of Learning,"* it seems to me, is of special interest because of its fundamental tendency—in my opinion fully justified—of basing the phenomena of psychical activity on physiological facts, i.e., of uniting, identifying the physiological with the psychological, the subjective with the objective, which, I am convinced, is the most important scientific task of our time. The author analyses the problem of learning from the general aspect and characterizes this process by enumerating its fundamental features; in this he utilizes without distinction both the material of psychologists and the physiological facts obtained by us on animals by the method of conditioned reflexes. Thus the psychologist and the physiologist marched side by side. But beyond this point profound differences arose between them. The psychologist regards conditioning as the principle of learning; he considers that this principle is not subject to any further decomposition, i.e., does not require further investigation, and he endeavours, therefore,

* *Psychological Review*, Vol. 37, No. 5, 1930. (*Note by I. P. Pavlov.*)

to base everything on it, to represent all the separate sides of learning as one and the same process. For this purpose he takes a physiological fact and in a definite way attaches to it certain significance in interpreting particular facts relating to the process of learning, without seeking actual confirmation of this significance. Hence, the physiologist tends, willy-nilly, to think that the psychologist, who only recently departed from the philosopher, has not yet fully renounced his inclination for the philosophical method of deduction, for pure logical activity which does not verify every step of thought by agreement with reality. The way of the physiologist is the reverse of this. At every moment of his investigation he endeavours to analyse the phenomena separately and concretely, to determine as much as possible the conditions for their existence, without relying on mere deductions or mere hypotheses. This I shall try to prove on the basis of certain points in which the author opposes me.

Although conditioning, association by simultaneity, conditioned reflexes serve as the factual point of departure in our research, they are, nevertheless, subjected to further analysis by us. We have before us the following important question: what elementary properties of the brain mass underlie this fact? This question has not yet been finally solved by us, but certain data for its solution are afforded by the following experiments. With our experimental animal (the dog) it was observed that when the external agent, which we wish to use as a conditioned stimulus, is applied after the beginning of the unconditioned stimulus, we get a conditioned reflex (according to the latest and most precise experiments carried out by Dr. N. V. Vinogradov), but it is insignificant and temporary, and invariably disappears if the same procedure is prolonged. A stable and durable conditioned reflex, as we have long known, can be obtained only when the external agent constantly precedes the unconditioned stimulus. Thus the first procedure has a double effect: at first it contributes temporarily to the formation of the conditioned reflex, and then abolishes it. This latter effect of the unconditioned stimulus is clearly manifest in the following experiment. A conditioned stimulus, well elaborated by means of the second, usual procedure—if afterwards it is systematically applied following the onset of the unconditioned stimulus, or is covered by it, in our laboratory terminology—gradually loses its positive action (especially when it belongs to the category of weak, conditioned stimuli) and finally is even transformed into an inhibitory stimulus. Obviously in this case the mechanism of negative induction (according to our old terminology, the mechanism of external inhibition) gradually prevails, i.e., the corresponding cell of the conditioned stimulus is inhibited, reaches a state of inhibition under the influence of repeated concentration on the part of the unconditioned stimulus—and the conditioned stimulus thus meets in its cell a constant state of inhibition. And it is this which makes the conditioned agent inhibitory, i.e., when applied alone it now evokes in its cortical cell not an excitatory but an inhibitory process. Consequently, during the usual procedure of elaboration of a stable conditioned reflex, the passage of a wave of excitation from the corresponding cortical cell to the centre of concentration of the unconditioned stimulus represents precisely the principal condition for the fixation of the path from one point to another, for a more or less constant union of the two nervous centres.

Let us pass now to other particularities of the conditioned activity where the author proposes his own uniform interpretation of the phenomena instead of our diversified analysis of concrete facts. The delayed, retarded conditioned effect, according to our experiments, is based on special inhibition of early phases of the conditioned stimulus, which do not coincide closely with the time of the appearance of the unconditioned stimulus. The author alleges for some reason that we attribute this to "mysterious latencies" in the nervous system, and gives his own interpretation of the facts. He admits that when, for example, the sound of a bell plays the role of a conditioned stimulus, the animal responds with a reaction of strenuous listening, with a complex motor act, and the centripetal impulses of this act are, strictly speaking, the real stimulators of the conditioned effect, in our case of the conditioned alimentary reflex—the salivary secretion.

According to the author, "when the salivary glands begin to secrete, the accompanying stimuli are not furnished by the bell, but by these responses to the bell. The direct response to the bell is probably over in a small fraction of a second." And further he states: "The apparent separation in time of a conditioning stimulus and its response is then quite possibly an illusion." The author even says that "Pavlov tends to forget in his explanation of the delay" the existence of the above-mentioned centripetal impulses from the motor apparatus. On page 312 of my "Lectures on the Work of the Cerebral Hemispheres"* one can see that not only do I take into account the centripetal impulses for the skeletal musculature, but I regard it as being more than probable that they exist even for all the tissues, to say nothing of the separate organs. In my view, the entire organism with all of its components is able to report about itself to the cerebral hemispheres.[110] Consequently, this is not the matter of an omission on my part; the matter is that actually we have not the slightest grounds for interpreting the fact in the way the author does.

First of all, if we agree with him that it is not the bell, but the centripetal impulses from the motor act of strenuous listening that is the actual stimulus for the conditioned effect, then why does the effect not manifest itself at once, but is retarded (in the case of a delayed reflex) and, besides, in accordance with the length of the interval

* Second edition. *(Note by I. P. Pavlov.)*

between the beginning of the stimulus and the beginning of the unconditioned reflex? For, when the unconditioned stimulus is delayed for a shorter time—only a few seconds—after the beginning of the conditioned stimulus, the effect, which, according to the author is caused by the centripetal impulses from the motor act of listening, also manifests itself very quickly, namely, in two or three seconds. How, then, does he explain the duration of the delay? And why, when the unconditioned stimulus is separated from the beginning of the conditioned by an interval of a few minutes, do the stimuli admitted by the author (namely, the centripetal impulses of movement) act after a lapse of minutes?

Besides, there are no grounds whatever for admitting a constant action of the stimuli of which the author speaks. The listening reaction, like the general orienting or investigatory reflex, as I have termed it, evoked by any new fluctuation in the habitual surroundings of the animal, usually exists only during the first brief period of application of the new recurring stimuli; when a conditioned reflex is formed, with a more or less short interval between the conditioned and unconditioned stimuli, it is quickly superseded by a special motor reaction peculiar to the given unconditioned stimulus. Subsequently, only the conditioned motor effect is permanently manifest, and there is no trace of an orienting reaction. The conditioned stimulus now becomes a pure substitute for the unconditioned stimulus. In the case of a conditioned alimentary reflex the animal may lick the flashing electric bulb, or attempt to take the air into its mouth, or try to eat the sound itself; in doing this the animal licks its lips and grinds its teeth, as if dealing with real food. The same thing takes place in the case of an elaborated delayed reflex. The animal remains wholly indifferent and quiet during the first period of action of the conditioned stimulus; not infrequently, immediately after the beginning of the stimulus, it even falls into a state of drowsiness and sometimes into a state of profound sleep (accompanied by relaxation of the musculature and snorting). With the beginning of the second period of the conditioned stimulation, just a little before the addition of the unconditioned stimulus, this state is replaced, sometimes impetuously, by a clearly pronounced corresponding conditioned motor reaction. In both cases it is only during the general somnolence of the animal in the course of the experiment that the orienting reaction now and then reappears at the first moment of the action of the stimulus.

And finally, the retardation in question is actually not the result of a "mysterious latency," but of the interference of a special, induced inhibition, which is well known to us and has been studied in detail in its various manifestations. The matter is quite clear. Although the considerably prolonged conditioned external stimulus remains one and the same, for the central nervous system and especially, one must suppose, for the cerebral hemispheres, it is obviously different at different periods of its course. This is particularly manifest with olfactory stimuli, which are sensed by us at first keenly and then rapidly become weaker and weaker, although objectively they remain constant. Apparently the state of the stimulated cortical cell under the influence of an external stimulus undergoes successive changes, and in the case of a delayed reflex only that state of the cell which closely precedes the addition of the unconditioned reflex plays the role of a signal conditioned stimulus. Exactly the same thing occurs when from different intensities of one and the same external stimulus we elaborate different conditioned stimuli, now positive, now negative, and now related to different unconditioned stimuli. The fact of delay under consideration represents an obviously interesting case of special adaptation, to ensure that the conditioned reflex does not appear too early, and that energy should not be expended beyond the necessary measure. That this explanation conforms to reality is proved by facts. Above all, this is clear from the procedure of formation of a delayed reflex. If the conditioned reflex is first elaborated with an interval of a few seconds between the beginning of the conditioned and of the unconditioned stimuli and the interval is then suddenly increased to a few minutes, the conditioned effect, which earlier manifested itself rapidly, gradually diminishes and soon fully disappears. And then, when the experiment is considerably prolonged, there comes a period of absence of any conditioned effect. Only afterwards does the conditioned effect reappear, at first just before the moment of the addition of the unconditioned stimulus; then it grows gradually and begins to manifest itself somewhat earlier.

That the first period of the delayed reflex is actually a period of inhibition, is proved by a number of facts. In the first place, the inhibition of the delayed reflex can easily be summated. In the second place, a successive inhibition can be observed from this reflex. And finally, the drowsy and sleeping state which arises in some animals in the first part of the delayed reflex, is a striking manifestation of the inhibitory state.

The next phenomenon—the extinction of the conditioned reflex—is also discussed by the author without any consideration of the factual details of our investigation; he has in view again the factor which he himself conjectured but did not define more precisely, and at the same time he attributes to me, in addition to the previously mentioned "inclination to forget," also a tendency "to conceal something from myself."

First of all the author, contrary to our affirmation, assumes that it is not the short duration of the interval between repetitions of the non-reinforced conditioned stimuli that contributes to the extinction of the conditioned reflexes, but the number of these repetitions. However, this is absolutely wrong. A non-reinforced conditioned stimulus without any repetitions, but merely prolonged for a period of three to six minutes, invariably ends in extinction to absolute zero, or as we term it, continuous extinction, in contrast to an intermittent one. Further, the author with the same arbitrariness supposes that extinction is not a constant fact, but an exception to the rule of frequency. And here, too, he is wrong. Extinction is one of the most constant facts of the physiology of conditioned reflexes. Having made both these conclusions contrary to reality, the author, so to speak, clears the field for himself and imagines the existence of other agents, which he does not define more precisely and which, together with the basic unconditioned stimulus, take part in the formation of the conditioned effect. It is probable that here, too, movements of the animal are implied, since mention is made of continuous and diverse movements of the animal in the course of the experiment. Thus, according to the author, the sum of the agents which determine the conditioned reflex constantly fluctuates, now increasing, now diminishing. When the number of these agents decreases and the conditioned reflex is absent or becomes weakened, then other, also unknown, agents become inhibitory, or what is actually the same, they become stimulators of other responses.

The author explains the interference of extraneous stimuli with extinction by asserting that these stimuli "disorganize posture and orientation," which appeared as inhibitors of the conditioned reflex at the stage of extinction and thus temporarily restore the reflex that is becoming extinguished.

The author does not consider it necessary to point out, even hypothetically, precisely which stimuli, along with the unconditioned one, support the conditioned reflex and which other stimuli that are also present inhibit this effect. When the author explains in his own way the interference of extraneous stimuli with the extinction, why does he not show how the extraneous stimuli, which remove the action of the agents inhibiting the conditioned effect, do not remove also the action of those stimuli supporting the conditioned response? After all, these are quite different stimuli.

Thus, the author has introduced, without any factual confirmation of their real value, a multitude of undefined, unknown stimulating agents.

It is to be assumed that the author has in mind just the same kinesthetic stimuli, but originating in different muscles. Of course, there are many skeletal muscles, and their movements form an almost infinite number of combinations, constantly sending special centripetal impulses to the central nervous system. But, in the first place, most of them proceed to the lower parts of the brain, and, in the second place, under usual conditions they in no way make themselves known to the cerebral hemispheres, serving only for the auto-regulation and greater precision of movements, for example, such as the continuous cardiac and respiratory movements. In our experiments only those movements, which form special motor reflexes, are effective in influencing our conditioned reflexes; chief and almost exclusive among these is the orienting reflex to the fluctuations of the surrounding medium, and sometimes also the defensive reflex arising in response to an accidental nocuous action on the animal during its movements in the experimental stand (a blow, some kind of pinch, etc.).

If, as the author assumes, the centripetal impulses from all the movements which we effect really reached the cerebral hemispheres to any considerable degree, then, by their very quantity alone, they would be a tremendous obstacle to the relations of the cortex with the external world and would render almost impossible this most important cortical function. Do the movements which indispensably arise when we talk, read, write, and in general when we think, disturb us to any extent? Are these actions ideally performed only when we are in a state of absolute immobility?

The constant fact of extinction is not the result of chance movements of the animal which are reflected in the work of the cerebral hemispheres; it is a law-governed manifestation of the fundamental properties of the cortical cells, as the most reactive cells in the organism, when they work for a more or less considerable length of time—even generally for a short period of time—without being accompanied by the fundamental inborn reflexes. The chief physiological function of the excitation of these cells is to serve as signals, to replace the special stimuli of the latter reflexes. Being the most reactive cells, they rapidly become exhausted, and from an active state pass into a state of inhibition, which, in all probability, not merely contributes to their rest, but accelerates their recovery. But when the activity of these cells is accompanied by unconditioned stimuli, the latter, as noted at the beginning of the article, immediately and, so to speak, in a preventive way inhibit them and thereby assist in the recovery.

That extinction is actually inhibition is proved both by its successive inhibitory action upon other positive conditioned reflexes and by the transition to a drowsy and sleeping state which, undoubtedly, is inhibition.

As for the remaining two points, where the author, instead of our explanation, offers a similar interpretation of his own, I can be more brief. With regard to the fact of the gradual intensification of the conditioned effect in the course of its formation, it must be pointed out that in this case it is the gradual elimination of extraneous stimuli that hinders the formation of the reflex, and not, as the author assumes, their ever-increasing participation in the creation of the effect. During our first experiments often from fifty to one hundred or more repetitions were needed to elaborate a complete conditioned reflex, while now from ten to twenty repetitions suffice, and often considerably fewer. In our present experimental conditions the first application of a new indifferent agent, which must later serve as a conditioned stimulus, results only in an orienting reflex, the motor component of which in the overwhelming majority of cases steadily and rapidly diminishes all the way to complete disappearance; thus, absolutely nothing remains of the constantly-growing sum of factors determining the conditioned effect, of which the author speaks. It is clear that the whole process consists in an ever-increasing concentration of excitation, and then probably in gradually beating a path between the points of the central nervous system which are to be connected.

Finally, as regards the independent acquisition of a conditioned effect from neighbouring stimuli, or near that to which the conditioned reflex was specially elaborated, the author again differs from us. In our view, this is nothing but an irradiation of excitation over a definite part of the cortex. The author, however, assuming that not a specific agent appears here as a conditioned stimulus, but an orienting reflex accompanying it, again gives his interpretation of the matter alleging that all the neighbouring agents become effective thanks to one and the same orienting reflex. But this completely contradicts the real facts. The neighbouring agents in most cases produce the conditioned effect directly, without any trace of the orienting reflex. On the contrary, when the latter is present, the conditioned effect is either fully absent, or considerably weakened; it manifests itself and grows only in proportion to the disappearance of the orienting reflex.

Thus, throughout the article, the author remains true to himself and to his habit of deduction.[111] Making erroneous use of a single physiological fact, he constantly and immediately derives from the principle of conditioning all the particulars of conditioned nervous activity which he utilizes for his subject of learning, while the factual aspect of these particulars are completely disregarded by him.

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It seems to me that the second article, "Basic Neural Mechanisms in Behaviour,"* to which I pass now, in large

* *Ibid.*, No. 1. *(Note by I. P. Pavlov.)*

measure treats the subject in the same way as the first. This article, by K. S. Lashley, was submitted in the form of a paper to the last International Congress of Psychology held in the United States (1929). Even though its material is almost exclusively physiological, the author's approach is exactly the same as in the preceding article. The material is sacrificed to the principal preconceived tendency to prove that "the reflex theory is now becoming an obstacle rather than a help to progress" in studying the cerebral functions; according to the author, of greater force and significance in this respect is, for example, the statements of C. Spearman[112] that "intelligence is a function of some undifferentiated nervous activity," or the analogy with the tissue of sponges and hydroids, which, being crushed and sifted through bolting cloth, subsequently, when settled or centrifuged down, again assumes the shape of a mature specimen with a characteristic structure.

First of all I must say in a general way, without going into detail for the time being, that this merciless judgement against the theory of reflexes is divorced from reality, an adamant and, one may even say, incomprehensible, refusal to take reality into consideration. Does the author really venture to affirm that all my thirty years' work, fruitfully carried on together with numerous colleagues, under the guiding influence of the idea of reflexes, is nothing but a hindrance to the study of the cerebral functions? No. No one has the right to say that. We have established a series of important rules relating to the normal activity of the higher part of the brain; defined a certain number of conditions of its wakeful and sleeping states; elucidated the mechanism of normal sleep and hypnosis; experimentally produced pathological states in this part of the brain, and found the means with which to restore it to normalcy. The activity of this part, as already studied by us, found and continues to find many analogies with the phenomena of our subjective world, as is evident from acknowledgements not infrequently made by neuropathologists, educators, psychologists-empiricists, as well as from the statements of academic psychologists.

Now before the physiology of this part of the brain there arises an infinite number of urgent problems, of strictly definite tasks in the field of further experimentation, instead of the almost blind-alley state in which this physiology undoubtedly found itself in the past decades. And this is the outcome of the application of the concept of reflexes to the experiments carried out on this part of the brain.

What, exactly, is the concept of a reflex?

The theory of reflex activity is based on three fundamental principles of exact scientific investigation: in the first place, the principle of determinism, i.e., an impulse, an impetus, or a cause for every given action or effect; in the second place, the principle of analysis and synthesis, i.e., the initial decomposition of the whole into its parts or units, and the subsequent gradual re-establishment of the whole from these units or elements; and finally, the principle of structure, i.e., the disposition of the activity of force in space, the adjustment of dynamics to structure. Therefore one cannot but regard the death sentence pronounced on the theory of reflexes as a misunderstanding or as bias.

You have before you the living organism, all the way to man, which produces a series of activities, manifestations of energy. You get a direct impression, difficult to overcome, of something spontaneous. In the case of man, as an organism, this impression is so strong that it is an obvious truth to almost everyone, and any contrary assertion seems absurd. Although Leucippus of Miletus* proclaimed that there is no effect without cause and that

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* I take this indication from Professor Kannabich's book *The History of Psychiatry*. (*Note by I. P. Pavlov*)

everything arises from necessity, is it not still believed, even leaving man aside, that there are spontaneously acting forces in the animal organism? As for man, do we not still here talk about free will and is there not a conviction rooted in many minds that there is something in us which is not subject to determination? I have always met and still meet not a few educated and intelligent people who are quite incapable of conceiving how it is possible to study the whole behaviour, say, of a dog quite objectively, i.e., solely by comparing the stimuli acting on the animal with its responses to them, and, consequently, disregarding the subjective world, which is presumed to exist in it by analogy with ours. What is meant, of course, is not the temporary difficulties of research, immense as they are, but the impossibility in principle of complete determination. It goes without saying that this same conviction is far more positive with regard to man. I should not be committing a great error in assuming that this conviction persists also among some of the psychologists, although masked by references to the *peculiar nature of psychical phenomena*, behind which, despite seemingly scientific arguments, can be detected the dualism and animism that is still immediately shared by many thinking people, to say nothing of religious believers.

The theory of reflexes is, as it has been doing ever since its inception, constantly and continuously enlarging the number of phenomena in the organism connected with the conditions that determine them, i.e., it is making clearer the integral activity of the organism. How, then, can it be an obstacle to the study of the organism in general, and the cerebral functions in particular?

Further, the organism consists of a mass of separate large parts and of billions of cellular elements producing corresponding masses of separate phenomena, which, however, are closely interconnected and ensure the integrated work of the organism. The theory of reflexes divides this general function of the organism into particular activities, connecting them both with internal and external influences, and then reunites them, one with another, thus making the activity of the organism as a whole and its interaction with the environment more and more comprehensible. How, then, has the theory of reflexes proved to be superfluous or inappropriate, or how can it be, if our knowledge of the connections between the separate parts of the organism is still insufficient, to say nothing of our incomplete comprehension of all the correlations of the organism with the surrounding medium? And all the internal, as well as external, relations in higher organisms are mostly effected by means of the nervous system.

Finally, if the chemist, when analysing and synthesizing, must, for the full understanding of the work of the molecule, imagine its invisible structure, and if the physicist, when analysing and synthesizing, must, for obtaining a clear idea of the mechanism of the atom, also visualize its structure, how is it possible to renounce the structure of visible masses and admit some kind of contradiction between structure and dynamics? The function of connecting both the internal and external correlations in the organism is accomplished in the nervous system which represents a visible apparatus. It is in this apparatus, of course, that the dynamic phenomena, which must be related to the most delicate details of its structure, are actually developed.

The theory of reflexes began the investigation of this apparatus with the definition of special functions, pertaining naturally to its more simple, grosser parts, and determined the general tendency of the dynamic phenomena occurring in it. Here is the general and fundamental scheme of the reflex: receptor apparatus,[113] afferent nerve,[114] central station (centres) and efferent nerve[115] with its effector tissue. Then came detailed elaboration of these parts, a process that is still going on. Of course, the most complicated and extensive work concerned, and still concerns, the central station, and in the parts of the central station—the grey matter, and in the grey matter—the cerebral cortex.[116] This work concerns the visible structure itself, as well as the dynamic phenomena developing in it, and, of course, does not lose sight for a single moment of the indispensable connection between structure and dynamics. In view of the difference of method in studying structure and dynamics, the investigation is naturally divided for the most part between the histologist and physiologist. No histologist-neurologist would, of course, make bold to state that the study of the structure of the nervous system, and especially of the higher part of the central nervous system, has reached completion; on the contrary, he will admit that the structure of this part still remains highly confused and obscure. Has not the cyto-architectonics[117] of the cerebral cortex revealed to us quite recently its extreme complexity and diversity? And are not all the numerous variations in the organization of different parts of the cortex without a definite dynamic importance? If the histologist is able to some extent to analyse them, how can the physiologist fully trace the movement of the dynamic phenomena along this inconceivable network? The physiologist, basing himself on the reflex scheme, never regarded the investigation of the central station as having been elaborated to any considerable extent, even in the simplest structures of these centres; he has always adhered to and has always been guided by the fundamental concept of the passage, the transmission of the dynamic process from the afferent to the efferent conductor. As for the higher centres, besides relating the functions to the details of structure as much as possible, he, of sheer necessity, concentrates his attention and work for the time being mainly on the dynamics, on the general functional properties of the brain mass. This has been done, and was continued until recent times, mostly by the Sherrington, Verworn and Magnus schools, as well as by other individual authors, on the lower parts of the brain; as for the higher part, this is being done predominantly and in the most systematic way by me and my colleagues in applying the conditioned reflex variation of the general theory of reflexes.

With regard to the cerebral cortex, the first reliable data relating to the detailed connection between its activity and structure, were obtained at the beginning of the glorious epoch of the seventies of the last century. While the existence of a special motor region in the cortex was then established and subsequently confirmed by successive investigators, the very exact and restricted localization of the sense organs in the cortex, as originally described, met with objections both on the part of physiologists and neuropathologists. To some extent these objections shook the theory of localization in the cortex. For a long time the situation remained uncertain, because the physiologist did not have purely physiological knowledge of the normal activity of the cortex; the use of psychological concepts, when psychology has not yet created a natural and generally accepted system of its phenomena, could not, of course, be conducive to the further study of the problem of localization. The situation radically changed when, thanks to the theory of conditioned reflexes, the physiologist was at last provided with the possibility of seeing the special, and at the same time purely physiological, activity of the cerebral hemispheres and thus could clearly distinguish the physiological activity of the cortex from the activity of the nearest subcortex, and the lower parts of the brain in general, in the shape of conditioned and unconditioned reflexes. Then all the separate facts could be definitely and strictly systematized, and the main principle of the structure of the cerebral hemispheres came clearly to the fore. From the seventies on, the special regions which had been located in the cortex as centres for the principal external receptors, remained the seat of higher synthesis and analysis of corresponding stimuli; however, along with these regions one had to admit the existence of representatives of the same receptors, dispersed, perhaps, over the entire cortex or at least over a considerable part of it, but capable only of simpler and quite elementary synthesis and analysis. A dog that had been deprived of the occipital lobes could not distinguish one object from another, but did distinguish degrees of light and simplified forms. A dog without temporal lobes was unable to discriminate complex sounds, such as its own name, etc., but was still capable of exact differentiation of separate sounds, for example, of one tone from another. What convincing proof of the fundamental importance of specialized structure!

Not without interest is the following experiment carried out by Dr. Elliason, and described in my "Lectures on the Work of the Cerebral Hemispheres"; it provides more detailed indications with regard to the functional significance of the structural properties of the special cortical regions. An acoustic complex of three tones of a harmonium—two extreme and one intermediate—covering more than three and a half octaves, and applied simultaneously, was used for the elaboration of a complex conditioned alimentary stimulus which evoked the secretion of a certain amount of saliva indicating the degree of intensity of the alimentary reflex. Subsequently, when separate components of the acoustic complex were tested, they also produced salivation, but to a lesser extent than the whole complex; the intermediate tones between these also caused a secretion of saliva, which was still less abundant. Then a bilateral extirpation of the anterior temporal lobes (gg. sylviaticus and ectosylvius with the anterior part of g. compositus posterior) was performed. The following was observed. When all the conditioned reflexes (to stimuli from other analysers) were restored after the operation, as well as the conditioned reflex to the chord (the latter reflex even before some of the others), the reflexes to isolated component tones of the chord were tested anew. The high tone and the neighbouring tones lost their effect, while the middle and low tones, as well as the intermediate tones, retained it, the effect of the low tone even increased, becoming equal to that produced by the whole chord. But when the high tone was accompanied separately by feeding, it soon (at the fourth essay) became again a conditioned alimentary stimulus and acquired a considerable effect —not a weaker, but a stronger one than previously. A number of exact conclusions can be drawn from this experiment. In the first place, that separate elements of the receptor acoustic apparatus are represented at different points of the special auditory region of the cortex; in the second place, that the complex stimuli use precisely this region; and, in the third place, that representatives of the same elements of the auditory apparatus, dispersed over a considerable part of the cortex, take no positive part in these complex stimuli.

When one sees, as I have seen, with the conditioned reflexes in hand, that a dog, with the posterior, greater part of both hemispheres removed, can orient itself with extreme precision by means of its cutaneous and olfactory receptors, losing only *complex* visual and auditory relations with the surrounding world, i.e., becoming unable to distinguish complex visual and auditory stimuli; that a dog deprived of the upper halves of both hemispheres, while fully retaining complex relations (auditory) with the surroundings, loses—in an astonishingly isolated manner—only the ability to orient itself amid the solid bodies met in the surrounding medium; and that, finally, a dog with extirpated anterior (minor) halves of both hemispheres, is, to all appearances, a fully disabled animal, i.e., deprived chiefly of proper locomotion, of the normal use of its skeletal movements, and yet reveals by means of another indicator, namely, the salivary gland, its complex nervous activity. When one sees all this how is it possible not to be impressed first of all with the paramount role played by the structure of the cerebral hemispheres in the fundamental task of the organism—to ensure proper orientation in the environment, proper equilibration with it? How, after this, is it possible to doubt the further significance of the more detailed features of the structure?

Were we to adopt the viewpoint of our author, described below in detail, we should have to advise the brain histologists to abandon their work as unnecessary and useless. Who would venture to draw such a conclusion? Otherwise, all the structural details, which are disclosed, must sooner or later find their own functional significance. Therefore, along with the further and deeper histological study of the cortical mass, it is necessary to carry on a purely and strictly physiological investigation of the activity of the cerebral hemispheres and of the adjacent part of the brain, so that the two elements—structure and function—could be gradually connected one with the other.

And this is done by the theory of reflexes.

Physiology long ago firmly established the permanent connection of definite internal and external stimuli with definite activities of the organism expressed in the form of reflexes. The theory of conditioned reflexes undoubtedly established in physiology the fact of temporary connection between diverse (and not merely definite) external and internal stimuli with certain units of activity of the organism, i.e., along with conduction of nervous processes in the higher centres, it also explicitly stated the possibility of their connection and disconnection. But this addition, of course, did not lead to any essential changes in the concept of a reflex. The connection between a definite stimulation and a certain unit of activity of the organism remains; however, it is manifested exclusively in the presence of a definite condition. That is why we distinguish these reflexes from inborn reflexes and term them "conditioned," in contradistinction to the older ones which have been termed "unconditioned." Thanks to this, the investigation of conditioned reflexes is based on the same three principles of the reflex theory: determinism, gradual and consecutive analysis and synthesis, and the structural principle. For us, the effect is always connected with the cause, the whole, in ever-increasing measure, is divided in parts and then synthesized anew, and the dynamics remain connected with structure, inasmuch, of course, as this is granted by the data of present-day anatomical investigation. Thus, one can say, boundless vistas are opened up for research into the dynamics of the higher part of the brain, i e., of the cerebral hemispheres and of the adjacent subcortex with its most complex fundamental unconditioned reflexes.

We consecutively study the fundamental properties of the cortical mass, define the essential activities of the cerebral hemispheres and elucidate the connections and interrelations of the cerebral hemispheres and the adjacent subcortex.

The basic processes of cortical activity are excitation and inhibition, their movement in the form of irradiation and concentration, and their reciprocal induction. The special activity of the cerebral hemispheres is expressed in a continuous analysis and synthesis of stimuli coming both from the external environment (for the most part) and from within the organism; thereafter the stimuli are directed to the lower centres beginning with the adjacent subcortex and ending with the cells of the anterior horns of the spinal cord.

Thus, under the influence of the cortex, the entire activity of the organism becomes more and more exactly and delicately correlated or equilibrated with the environment. On the other hand, the adjacent subcortex sends a powerful stream of impulses from its centres to the cortex which maintains the tonus of the latter. In the final analysis, the centre of gravity of research into the higher part of the brain is now being transferred to investigation of the dynamic phenomena taking place in the cerebral hemispheres and in the adjacent subcortex.

As stated above, the work of the cortex essentially consists in analysis and synthesis of the stimuli entering it.

The variety and number of these stimuli is truly innumerable, even in an animal like the dog. The best way to express the number and variety of stimuli would be to say that all the stages through which the states of separate cortical cells and of their diverse combinations pass, represent individual stimuli. By means of the cortex it is possible to form special stimuli from all gradations and variations of the excitatory, as well as of the inhibitory, processes, both in individual cells and in their various combinations. Stimuli formed from different intensities of one and the same stimulation, from the relationship of stimulations, etc., may serve as an example of the first case, and different conditioned hypnogenous stimuli—as an example of the second.

These countless states of the cells are formed not only under the influence of existing stimuli and manifest themselves not only during the action of external stimulations, but remain also in their absence, in the form of a system of different intermittent, more or less stable degrees of excitation and inhibition. Here is an illustration. A series of positive stimuli of different intensities, and of negative stimuli, are applied by us day by day for a certain period, in one and the same sequence and with the same intervals between them; in this way we obtain a system of corresponding effects. If in the course of the experiment we repeat only one of the positive stimuli, observing the same intervals, it will reproduce exactly the same fluctuations of the effect which were caused by all the successive stimuli taken together in the previous experiments, i.e., the same system of states of cortical excitation and inhibition will recur.

Of course, one cannot claim at the moment the establishment of any far-reaching conformity between the dynamic phenomena and the details of structure, but this conformity must necessarily be admitted, for the structure of the cortex is highly variegated throughout, and we are already convinced that certain degrees of synthesis and analysis of stimuli are accessible only to definite parts of the cortex, being inaccessible to others. This is also firmly established by the following fact. When a series of different acoustic stimuli (a tone, noise, beats of the metronome, a gurgling sound, etc.), or mechanical stimulations at different spots of the skin, are developed into conditioned stimuli, we can render one of the stimulated points pathological and invalid, while the other will be quite normal. We obtain this result not in a mechanical, but in a functional way, by bringing the point of stimulation into a difficult state either by an excessively intense stimulation or by a drastic collision at that point of the excitatory and inhibitory processes. And how can this be explained except by saying that the excessive work imposed by us on the given minute detail of structure, resulted in its destruction, just as rough handling spoils and destroys a very delicate instrument? How delicate and specialized must these details be when the points of application of other auditory and mechanical stimuli remain fully preserved and intact. Such isolated destruction can hardly ever be obtained mechanically or chemically. After this there can be no doubt that if at present, following mechanical destruction of the cortex, we sometimes do not observe any change in the animal's behaviour, this is due to the self-evident fact that we have not yet decomposed the animal's behaviour into all its elements, the number of which must be enormous. And for this reason, the disappearance of some of them naturally escapes our attention.

I have taken the liberty of dwelling at length on our data because I wanted, in the first place, to make further use of them in criticizing the experiments and respective conclusions of Lashley, and, in the second place, to show once more how fruitful at the present time is the investigation of the cerebral hemispheres based on the theory of reflexes with all its principles.

What, then, are the objections raised by Lashley against the theory of reflexes? What are the arguments with which he tries to smash this theory?* First of all it is absolutely clear that he conceives it in a peculiar way. Arbitrarily, disregarding physiology, he approaches it exclusively from the structural point of view, without a single word about its other principles. It is generally accepted that the idea of the reflex dates from Descartes. But what was known in Descartes' time about the detailed structure of the central nervous system, especially in connection with its activity? Indeed, it was only in the beginning of the 19th century that a physiologico-anatomical distinction was made between the sensory and motor nerves. It is obvious that the idea of determinism formed the essence of Descartes' notion of a reflex, and originated his concept of the animal organism as a machine. All later physiologists interpreted the reflex in a similar way; they related the individual activities of the organism to definite stimuli, gradually specifying the elements of nervous structure in the form of different afferent and efferent nerves and in the form of special paths and points (centres) of the central nervous system, and finally, collated the typical features of the dynamics of this system.

The main factual grounds which serve Lashley for his conclusion concerning the harmfulness of the reflex theory at the present time, and for recommending a new concept of cerebral activity, are drawn chiefly from the author's own experimental material. This material consists mainly of experiments carried out on white rats which learn to find the shortest possible way to a compartment containing food in a more or less complicated maze. These experiments showed that the learning became more difficult almost exactly in proportion to the degree of preliminary

* Since the monograph by K. S. Lashley entitled "Brain Mechanisms and Intelligence," published simultaneously with the abovementioned address, gives a fuller exposition of the author's experimental material, I shall, henceforth, refer to the address and monograph without any distinction, citing facts, conclusions and extracts. *(Note by I. P. Pavlov)*

destruction of the cerebral hemispheres, and, besides, irrespective of the parts subjected to destruction; in other words, the result was determined exclusively by the mass of hemispheres which remained intact. After additional experimentation the author reached the conclusion that "specific cortical areas, and association or projection tracts, seem unessential to the performance of such functions which rather depend upon the total mass of normal tissue." Thus, the author draws an original, but actually quite inconceivable, conclusion that the most complex activities of the apparatus are effected without the participation of its special parts and chief connections, or, in other words, that the whole apparatus functions somehow or other independently of its component parts.

And so the main question is: why is the accomplishment of the maze task regularly retarded, depending solely on the degree of destruction of the hemispheres, but irrespective of the location of destruction? In this connection it is to be regretted that the author did not keep in mind the theory of reflexes with its first principle of determinism. Otherwise, the first question he would have put to himself, when analysing his experimental methods, would be the following: how could the rat solve the maze problem in general? After all, it could not do it without some kind of directing impulse, without some kind of signal. Were we to take the contrary view, no matter how difficult this is, we would be obliged to show that the task can be actually accomplished without the help of any stimuli, i.e., it would be necessary first to destroy in the rat *all receptors at once*. But who has ever done this and how can it be done? If, however, as may be naturally assumed, signals, or certain stimuli, are inevitably required for the solution of the task, then the destruction of individual receptors or of their definite combinations is obviously insufficient. It is possible that all or almost all the receptors participate in the response, superseding one another separately or in certain combinations. And with rats, in view of the well-known conditions of their life, this invariably is the case. It is not difficult to assume that in accomplishing the maze task the rat makes equal use of olfactory, auditory, visual, cutaneous, and kinesthetic stimuli. And since special regions of these receptors are located in different places of the cerebral hemispheres, and individual representatives of their elements are possibly dispersed throughout the entire mass of the hemispheres, the possibility of solving the task always remains, no matter how much of the hemispheres has been extirpated; but, naturally, the less cortical tissue that remains intact the more difficult is the solution. But if one accepts the view that in the given case the rat uses only one receptor or a few of them simultaneously, then it is necessary to prove this by special experiments that would leave no room for doubt, i.e., by allowing each receptor to act separately or in certain combinations, and excluding all others. But, as far as I know, neither the author nor anyone else has ever performed such experiments.

It is very strange that the author completely disregards all these possibilities and does not even put the question: how does the rat overcome the mechanical obstacles, what stimuli, and what signals serve for the corresponding movements? He confines himself to experiments which involve the destruction of individual receptors separately or in certain combinations, but do not abolish the habit; he ends his analysis by stating that "the most important features of the maze habit are a generalization of direction from the specific turns of the maze and the development of some central organization by which the sense of general direction can be maintained in spite of great variations of posture and of specific direction in running." Truly, one can say, some kind of bodiless reaction!

Various incisions in the cerebral hemispheres and in the spinal cord were applied by the author as additional experiments in studying the maze reaction; their purpose was to exclude the association and projection tracts in the hemispheres and the pathways in the spinal cord. However, it should be pointed out that all these methods, as physiologists well know, are in no way decisive, but rough, approximate methods, and the more so the more complicated the structure. This applies also to the peripheral nervous system which is much more crude and simple. Physiologists know very well how difficult it is fully to isolate the organs from nervous connections with the whole organism, and often only complete removal of an organ gives full assurance in this respect. Various crossings, loops, etc., in the peripheral nervous system are familiar to physiologists. Let us recall, for example, the case of recurrent sensibility in the spinal roots and the innervation of a single muscle by fibres from different roots. How much more diverse and delicate, then, must this, so to speak, mechanical immunity be in the central nervous system with its immense connections. It seems to me that up to now this highly important principle has not been sufficiently recognized, particularly in the physiology of the nervous system, and has not been clearly and constantly formulated. Indeed, the system of the organism developed in the midst of all the surrounding conditions: thermal, electrical, bacterial and others, including mechanical conditions; it had to balance all these conditions, adapt itself to them, and prevent or limit their destructive action as much as possible. In the nervous system, and especially in its most complex central part, which controls the entire organism and unites all its particular activities, this principle of mechanical self-protection, the principle of mechanical immunity, had to attain the highest degree of perfection, and this, actually, is mostly the case. Since at present we cannot claim complete knowledge of all the connections which are formed in the central nervous system, our experiments with incisions, sections, etc., in many cases are, in essence, only of a negative character, i.e., we do not accomplish our aim of disjunction, since the apparatus proves to be more complicated, so to speak, more autoregulatory than we anticipated. It is, therefore, always risky to draw definite and far-reaching conclusions on the basis of such experiments.

In connection with our first question I shall touch on the problem of relative complexity of habits which the author investigated; I do so mainly for the sake of appraising his methods. The author finds that the maze habit is more complicated than the habit of distinguishing between different intensities of light. But how is this proved? Actually the reverse is the case: a habit in a highly complicated maze is formed in the course of nineteen trials, while the habit of distinguishing the brightness of light is formed in 135 trials, i.e., the maze habit is elaborated seven times easier. If a comparison is made with the simplest of the three mazes used by the author, the difference in the degree of difficulty amounts almost to thirty times. Despite this, the author arrives at the conclusion that the maze habit is more complex. He backs his conclusion with various explanations; however, in order to be convincing he should have determined exactly, that is, quantitatively, the value of the facts suggested in his explanations, so that, taken together, they should not only cover the actual difference, but transform the result into its opposite.

In a situation of this kind I would not venture to say what is complex and what is simple. Let us examine the essence of the question. In the movements of the animal in the maze and in the box with different intensities of illumination account is taken only of its turns to the right or to the left, but not the whole act of locomotion. In both cases signals or special stimuli are necessary to effect the turns. And in both cases they actually exist. But beyond this there is a difference. There are several turns in the maze, in the box only one. Consequently, in this respect the maze is more difficult. But that is not the only difference. In the maze the signs for the turns differ almost exclusively in quality; for example, in the act of turning the animal comes into contact with the openings of the partition now at the right side, now at the left side of the body, in making the turns the muscles of the right and left sides work alternately. The same is true in respect of the visual and auditory signals. In the box it is a matter of quantitative difference. These differences must somehow be equilibrated. And besides, of course, the life experience of the rat is bound to play a part, i.e., its greater or lesser preliminary acquaintance with one or another task, as the author rightly points out. But it is also impossible to overlook the fact that in the most complicated maze the accomplishment of the task is greatly facilitated by definite rhythm, by alternation of the turn now to the right, now to the left. On the other hand, inculcation of ability to distinguish between different intensities of light is bound to be considerably influenced by the fact that two impulses contribute to it: food and nocuous stimulation (pain), whereas in the maze the habit is determined only by food. This, of course, complicates the conditions of learning. And still another question arises: do the two impulses contribute to the formation of the habit, or, on the contrary, impede it? Besides, we have stated above that the development of a system of effects is a very easy and persistent phenomenon in nervous activity. So that in both methods, in the maze and in the box, we have different conditions, with the result that an exact comparison of the difficulty of the task becomes almost impossible. All this, plus the above-indicated vague character of the signals in the maze, makes the entire method of the author highly problematic.

That our author is more inclined to theorize and to generalize than to perfect and vary his experiments (an indispensable requirement in biological experimentation), can be seen from the following investigations carried out by him in relation to the same problem.* In one of these

* K. S. Lashley, "The Relation Between Cerebral Mass, Learning, and Retention," *Jour. Comp. Neur.*, Vol. 41, No. 1, 1926. "The Retention of Motor Habits after Destruction of the So-Called Motor Areas in Primates," *Archives of Neurology and Psychiatry*, Vol. 12, 1924. *(Note by I. P. Pavlov.)*

researches he investigates the visual habit elaborated to a certain intensity of illumination. Having destroyed the occipital third of the cerebral hemispheres in a rat, the author finds that even the speed of forming the visual habit is not diminished compared with normal animals. If, however, the same habit already existed in a normal animal before removal of the occipital part of the hemispheres, then the habit disappears and must be formed anew. From this the author draws the risky and somewhat inconceivable conclusion that in general the process of learning does not depend on the site of lesion, whereas the mnemonic trace or engram has a definite localization. But the matter is much more simple. As is known, the occipital lobes are the location of a special visual region, to which the stimuli from the eyes come first of all and where they enter into functional connections with one another for the formation of complex visual stimulations, as well as into direct conditioned connections with the various activities of the organism. But since the visual fibres extend much farther than the occipital lobes, probably over the entire mass of the hemispheres, beyond these special lobes they serve for the establishment of conditioned connections with the various activities of the organism, only in the form of more or less elementary visual stimuli. And should Lashley form a habit not to the intensity of light but to a certain object, this habit would disappear with the extirpation of the occipital lobes and would not reappear; thus, there would be no difference between the site of formation of the habit and the place of the mnemonic trace.

In the other research Lashley carried out experiments on the cortical motor region of monkeys. The motor habit does not disappear with the removal of this region. From this he draws the conclusion that the region has no relation to the given habit. But, first of all, in his three experiments he does not extirpate the motor region fully, and probably the remaining parts are still sufficient for a mechanical habit of the given complexity. He excludes this probability not by experiment, but by reasoning. Then, it is possible that in addition to the highly specialized motor region, ascertained by electrical stimulation, there is a less specialized and more extended region. These two considerations necessitate a considerable complication of the mechanical tasks. Finally, why did the author not blind his animals? For there is no doubt that vision also played a part in the formation of the habit, and stimulation of the motor apparatus located below might occur through the visual cortical fibres as well. We have a striking example of this in ataxic patients[118] in cases of tabes dorsalis. The ataxic patient can stand on one leg with his eyes open, but falls when his eyes are closed. Consequently, in the first case he replaces the kinesthetic fibres by the visual ones.

And here again, under the influence of the favourite negative attitude towards detailed localization, there is discontinuation of further necessary experimentation.

Let us now turn to the other experiments and arguments of the author aimed directly against the theory of reflexes. Analysing various adequate stimuli, the author states that most likely one and the same receptor cells do not take part in forming and reproducing a habit, and that this is most obvious in pattern vision. But, in the first place, we see objects, i.e., we receive certain combined visual stimulations with the help of each part of the retina, and not of the whole retina at once. The same applies to the projection of the retina in the cortex. Consequently, this is the reason why there is no definite connection between the given receptor cells and a definite reaction. Only when we study an object in detail, do we make temporary use of the fovea centralis,[119] and usually each part of the retina serves for a corresponding reaction to the given object. This principle is likewise true for the projection of the retina in the cortex. In the second place, as far as the identity of the reaction is concerned, in the case of a geometrical white figure on a black background or with the relations of brightness reversed, and in cases when the geometrical figures are replaced by corresponding contour outlines, and even partial outlines—this identity can be explained by what has just been said; on the other hand, this phenomenon was thoroughly studied long ago, and it means that at first the most general features of the stimuli act, and then, gradually, under the influence of special conditions, a further analysis takes place and more special components of the stimuli begin to act. In the given case, it is the combinations of white and black points alone, without exact relations and dispositions, that first act as stimuli. And this is demonstrated by the fact that in the course of further special experiments a white figure on a black background could, undoubtedly, be differentiated from a black figure on a white background, i.e., the mutual relationship of black and white would become a special stimulus. The same applies to the replacement of a geometrical figure by a contour outline, etc. All these are but stages of analysis, i.e., more and more detailed elements of the stimuli gradually become stimuli themselves.

Concerning the group of reactions, i.e., the motor apparatus, the author points out that the rat proceeds in the right direction in the maze, in spite of the fact that sometimes it runs very quickly, sometimes moves slowly, and finally, when its cerebellum is injured, makes circular movements. And this, in the author's judgement, is an argument against the existence of any definite connection between the stimulus and a given reaction. However, the rat constantly moves forward and makes turns to the right and to the left with the help of the same muscles in all the cases mentioned here, the rest are additional movements determined by other additional stimuli. Further, in the case of the exclusion of muscles by paralysis during the elaboration of a habit, and their subsequent use after curing the paralysis, it is necessary to find out why and where the paralysis arose. For we have a vast series of co-ordinated centres, extending from the end of the spinal cord up to the cerebral hemispheres, and conductive fibres may reach out to all of them from the hemispheres. Further, we know that every time we think of a movement, we actually produce it in an abortive way. Consequently, a process of innervation is possible, although actually it does not manifest itself. Then, if the stimulation cannot be realized through the shortest way, it must, in accordance with the principles of summation and irradiation, pass to the neighbouring points. Have we not known for a long time that a decapitated frog, when removing acid placed on the thigh of one of its extremities by using the foot of the same extremity, if unable to do so because of amputation of that foot, will use the other foot after a few unsuccessful attempts with the disabled extremity?

The author's reference to the absence of stereotype in certain forms of movement, for example, in the building of nests by birds, is also based on misunderstanding. The phenomenon of individual adaptation exists throughout the animal world. And this is precisely the conditioned reflex, the conditioned reaction which is effected on the principle of simultaneity. Finally, the author's allusion to the uniformity of grammatical forms fully accords with the above-mentioned fact of systematization taking place in the nervous processes of the cortical activity. This is precisely the combination or fusion of structure with function. And if we do not now have a clear idea of how this occurs, this is only because we do not yet possess complete knowledge of the structure and of the mechanism of the dynamic processes.

I consider it superfluous to dwell further on the author's arguments against the significance of structure in the central nervous system. The common feature of all his arguments is that he disregards the already known complexity of this structure, and even more so, its probable complexity; he constantly simplifies it in a preconceived way, reducing it to the simplest scheme of a physiological textbook, which merely shows the indispensable connection between the stimulation and its effect—and nothing more.

What then does the author propose in place of the theory of reflexes which he rejects? Nothing except extremely remote and fully unjustified analogies. Is it possible to seek a solution of the problem of the higher brain mechanism by references to the tissue of sponges and hydroids or to embryonal tissue, when in the higher part of the brain of higher animals, including man, we have the summit of differentiation of living matter? In any case, admitting absolute freedom of hypothesis, we have the right to demand of the author at least a preliminary and elementary programme of definite problems for successful experimentation on this subject in the immediate future, a programme offering more than the theory of reflexes and capable of vigorously advancing the problem of cerebral functions. But the author has no such programme. A truly scientific theory must embrace not only all the existing material, but open up a wide possibility for further research and, so to speak, unlimited experimentation.

This is precisely the position in which the theory of reflexes now finds itself. Who will deny the extreme, almost inconceivable complexity of the structure of the central nervous system in its highest representative, the human brain, and the necessity for a more profound study of it by improved methods? On the other hand, the human mind still remains overwhelmed by the mystery of its own activity.

The theory of reflexes seeks to provide a possible solution for both one and the other, to elucidate the remarkable mechanism, so difficult to conceive, of this most extraordinary instrument. The possibility of experimentation on the brain, and especially on its higher part, with the help of the reflex theory with its requirements of constant determination and unceasing analysis and synthesis of the underlying phenomena, is truly unlimited. This I have felt and seen continuously in the course of the past thirty years, and the further, the more distinctly.

Since this is my first appearance in psychological literature, it seems to me opportune, on the one hand, to dwell on certain tendencies in psychology, which, in my opinion, do not agree with the aims of successful investigation, and on the other hand, to emphasize with greater force my point of view on the subject of our common research.

I am an empirical psychologist and I know psychological literature only from the few manuals and the insignificant number—in comparison with the available material—of psychological articles which I have read. But throughout my entire conscious life I have been and still am a constant observer and analyser of myself and others within the range of life which is accessible to me, including belles-lettres and genre painting. I decidedly reject and dislike any theory claiming a complete inclusion of all that constitutes our subjective world, but that does not mean that I refuse to analyse it, or attempt to interpret it simply, in its individual points. This interpretation must consist in bringing its separate phenomena into accord with the data of our modern positive knowledge in the field of natural sciences. For this purpose it is necessary constantly to endeavour to apply in a most thorough way these data to every particular phenomenon. It is my conviction that a purely physiological interpretation of much of what was previously called psychical activity has gained sure ground, and that in the analysis of the behaviour of higher animals, man included, every effort should be legitimately made to interpret phenomena in a purely physiological way, on the basis of established physiological processes. However, it is clear to me that many psychologists, so to speak, jealously protect the behaviour of animals and man from purely physiological explanations, constantly ignore them and do not try to apply any of them in an objective way.

To prove what I have just said I shall take only two very simple cases: one mine and the other Prof. Koehler's,[120] although I could cite many much more complex cases.

When we were trying out the method of feeding an animal from a distance during experimentation, we employed different procedures, and among them the following: in front of the dog we always placed an empty plate to which a metal tube led from a vessel containing the powder of dried meat and bread with which we usually fed our animals at the time of experimentation. Located at the junction of the vessel and the tube was a valve which could be opened at the right moment by means of pneumatic transmission; in this way a portion of the powder came down the tube and spilt out into the plate where it was eaten by the animal. The valve did not work very well and when the tube was shaken some of the powder would fall into the plate. The dog quickly learned to make use of this, that is, to shake out the powder without assistance. And such shaking occurred almost constantly whenever the dog, while eating its portion of food, brushed against the tube. This, of course, is exactly what takes place when the dog is being trained to give the paw. In our experimental case the laboratory surroundings in general taught the dog, while here only a part of the surroundings—man. In the latter case the words "paw," "give," etc., the tactile stimulation which arises when the dog lifts its paw, the kinesthetic stimulation which accompanies this movement, and, finally, the visual stimulation coming from the trainer, are accompanied by feeding, i.e., they are linked up with an alimentary conditioned stimulus. What happened in the above-mentioned case is exactly the same thing: the noise of the shaking tube, the tactile stimulation from the contact with the tube, the kinesthetic stimulation resulting from knocking against the tube and finally the sight of the tube—all were likewise connected with the act of eating, with excitation of the alimentary centre. This, naturally, occurred through the principle of association by simultaneity and was a conditioned reflex. Two additional physiological facts are clearly manifested here. In the first place, the definite kinesthetic stimulation is connected, in the given case, probably in a conditioned way (while in the lower parts of the central nervous system in an unconditioned way), with performing the movement that originated the kinesthetic stimulation. In the second place, when two nervous centres are connected or joined together, the nervous processes move from one to the other in both directions. If we accept as absolutely valid the principle of one-way conduction of the nervous processes in all points of the central nervous system, then in the given case we shall have to assume an additional reverse connection between these points, i.e., to admit the existence of an additional neurone connecting them. When the lifting of the paw is followed by presentation of food, the excitation undoubtedly proceeds from the kinesthetic point to the alimentary centre. But when the connection is established and the dog, being in a state of alimentary excitation, gives the paw itself, the excitation obviously runs in the opposite direction.

I cannot interpret this fact in any other way. Why this is only a simple association, as psychologists usually affirm, and not an act of comprehension, of sagacity, even though elementary, remains obscure to me.

The other example is taken by me from W. Koehler's book (*Intelligenzprüfungen an Menschenaffen*) and also relates to a dog. The dog is kept in a large cage placed in an open space. Two opposite walls of the cage are solid and nothing can be seen through them. Of the other two, one is a grille through which free open space is visible, and the other has an open door. The dog stands in the cage facing the grille, and a piece of meat is placed in front of it at some distance. The moment the dog sees the meat it turns around, walks through the open door, around the cage and takes the meat. But if the meat is placed close to the grille, the dog vainly jostles about, trying to get to the meat through the grille, and does not make use of the door. What does this mean? Koehler makes no attempt to solve this question. But with the help of conditioned reflexes we can easily comprehend the matter. When the meat is near the grille, it strongly stimulates the dog's olfactory centre, and the latter, in accordance with the law of negative induction, greatly inhibits all the other analysers, all other parts of the cerebral hemispheres, and thus the traces of the door and of the roundabout way remain inhibited, or, in subjective terminology, the dog has temporarily forgotten them. In the first case, in the absence of a strong olfactory stimulation, these traces are either only slightly inhibited or not inhibited at all, and lead the dog more correctly to its aim. At any rate this interpretation fully deserves further exact verification in an experimental way. Should it be confirmed, it would be possible experimentally to reproduce the mechanism of our reverie, of strong concentration of thought on something, when we do not see or hear what is going on around us, or, what is the same, to reproduce the mechanism of the so-called blindness under the influence of passion.

I am sure that persistent experimentation will elucidate a number of other and more complex cases of the behaviour of animals and man from the point of view of many established rules of the higher nervous activity.

The second point, on which I shall dwell, concerns the significance of the aim and purpose in psychological research. It seems to me that with regard to this point different things are always confused.

Before us is the grandiose fact of the evolution of nature from its initial state in the form of nebulae in infinite space all the way to human beings on our planet, an evolution which, roughly speaking, passed through the following phases: the solar systems, the planetary systems, the inanimate and animate part of nature on earth. The phases of evolution in the form of phylogeny and ontogeny are most strikingly seen in living matter. We still do not know, and probably shall not know for a long time, either the general law of evolution, or all its successive phases. But observing its manifestations, we replace in an anthropomorphic, subjective way, both in general and in particular, the knowledge of the law by the terms "purpose," "intention," i.e., we merely repeat the fact without adding anything to our actual knowledge of it. But a genuine study of the separate systems of which nature consists, including man, implies a mere statement both of the internal and external conditions of existence of these systems, in other words, the study of their mechanism; to thrust into this investigation the idea of purpose in general, means to confuse different things, to hinder the fruitful investigation which is now accessible to us. In the study of each particular system the idea of possible purpose is not the final aim; it can only serve as an additional help, as a method of scientific hypothesis favouring the formulation of new problems, varying experiments, just as we do when we acquaint ourselves with a machine which is new to us and which is the work of human hands.

The question of freedom of will, naturally, is bound up with this point. Undoubtedly this question is of great vital importance. But it seems to me that it is possible to discuss it in a strictly scientific way (within the limits of modern exact natural science), and at the same time without contradicting the feelings common to all men and without involving confusion in its vital formulation.

Man, of course, is a system (roughly speaking, a machine), and like every other system in nature is governed by the inevitable laws common to all nature, but it is a system which, within the present range of our scientific vision, is unique for its supreme power of self-regulation. Among the products of human hands we already know a number of machines with various systems of autoregulation. From this point of view, the method of studying the human system is exactly the same as that of any other system; it includes decomposition into constituent parts, study of the importance of each part, study of the connections between the parts, study of relations with the external environment, and finally, on this basis, interpretation of its general functioning, as well as regulation, if this is within human possibility. But our system is a highly auto-regulating system, capable of maintaining, rehabilitating, repairing and even improving itself. The chief, the strongest and the lasting impression gained from the study of the higher nervous activity by our method is the extraordinary plasticity of this activity, its immense potentialities; nothing is immobile, unyielding; everything can always be attained, changed for the better, if only the proper conditions are created.

A system (machine) on the one hand, and man, with all his ideals, aspirations and achievements, on the other—what a terribly discordant comparison this seems at first glance. But is this really so? And does not it follow from our own point of view that man is the supreme creation of nature, the highest embodiment of the resources of infinite nature, the realization of her mighty and still unexplored laws? Is not this enough to enhance the dignity of man, to afford him the deepest satisfaction? And practically everything vital is retained that is implied in the idea of free will, with its personal, social and civic responsibility; for me there remains the possibility, and hence also the obligation, to know myself, and using this knowledge, always to maintain myself at the highest possible level of my abilities. Do not social and civic duties and requirements constitute conditions which are demanded of my system and which must evoke in it corresponding reactions favouring its integrity and perfection?

DYNAMIC STEREOTYPY OF THE HIGHER PART OF THE BRAIN[121]

Countless stimuli, different in nature and intensity, reach the cerebral hemispheres both from the external world and the internal medium of the organism itself. Whereas some of them are merely investigated (the orienting reflex), others evoke highly diverse conditioned and unconditioned effects. They all meet, come together, interact, and they must, finally, become systematized, equilibrated, and form, so to speak, a dynamic stereotype.

What truly grandiose work!

However, this work is accessible to detailed and exact investigation, naturally, at first, under simplified conditions. We are studying this activity on a system of conditioned reflexes, mainly alimentary, in the course of experimentation on dogs. This system consists of a series of positive stimuli, acting on different receptors and having different intensities, as well as of negative stimuli.

Since all these stimulations leave, after their action, more or less profound traces, precise and constant effects of the stimuli can be obtained in the system with the greatest ease and most rapidly only when the intervals between the stimuli are invariable and when the stimuli are applied in a strictly definite order, i.e., when they are externally stereotyped. In the end, a dynamic stereotype develops, i.e., a co-ordinated, equilibrated system of internal processes. The evoking and establishing of a dynamic stereotype is a nervous task of extremely variable intensity which, on the one hand, depends on the complexity of the system of stimuli, and, on the other hand, on the individuality and state of the animal.

I take as an example one of the extreme cases. (Virzhikovsky's experiments). In a stereotyped system well elaborated in an animal of the strong nervous type and composed of positive stimuli of various intensities, as well as of negative conditioned stimuli, we introduce a new stimulus, but in the following peculiar way: we apply it four times in the course of the experiment after different stimuli, i.e., at different moments of the experiment, but accompany it by an unconditioned stimulus only at the fourth application. The reflex is soon manifested and begins to develop; but this process is accompanied by extreme excitation of the animal, which tries to break away from the stand, throws off the apparatus attached to it, and howls. The previous positive stimuli lose their effect; the animal even rejects food, and it becomes more and more difficult to bring it to the experimental chamber and to emplace it in the stand. This painful state persists for two or three months, until at last the task is solved by the animal; the stereotype establishes itself: the first three applications of the new stimulus no longer produce any positive action, being inhibited, only the last (fourth) application is effective, and the animal fully calms down.

Thus, the establishment of the new dynamic stereotype requires an enormous expenditure of nervous energy, which can be sustained only by a strong nervous type.

But let us continue our experiment. When the first task is accomplished, we offer the animal another. Now also the first three applications of the new stimulus are accompanied by feeding, which means that the animal must transform them from inhibitory into positive ones. A state of excitation is again observed in the animal, but this time less intense and lasting for a shorter period, until all applications of the new stimulus begin to produce one and the same positive effect. Thus, the reshaping of the stereotype again requires certain effort. But since food has been presented to the dog this time, it is now not a question of inhibition of the alimentary excitation, as was probably the case, even though partially, with the first task, but precisely of the establishment of a new dynamic stereotype in the cerebral hemispheres. This is realized more rapidly and easily because the second task, obviously, is much simpler. Of course, simpler systems of conditioned reflexes are elaborated by the same animal with greater ease, in any case without marked efforts on its part.

To me it would be strange not to regard this nervous work as mental activity, just because the psychologists ascribe to the dog only associative activity.

But such is the state of affairs only with dogs possessing strong and equilibrated nervous systems. The picture is altogether different with dogs possessing a strong, but unequilibrated nervous system, dogs which are more or less feeble, sick, worn out, or aged. There are dogs which from the very outset, despite favourable conditions, are incapable of elaborating a dynamic stereotype—the effects of the conditioned stimuli constantly vary from experiment to experiment in a chaotic way. In this case we come to the assistance of the animal by simplifying the system of reflexes, for example, by reducing their number to two positive ones. A mere change in the sequence of the old stimuli in the course of the experiment also represents a difficult task, which in our experimental conditions leads sometimes to complete temporary discontinuance of the reflex conditioned activity. Even the maintenance of an already elaborated system calls for definite effort, which some dogs can endure only if there are intervals of two or three days in the course of experimentation, i.e., if the dogs are allowed regular rest. Without intervals the effect of the conditioned reflexes shows most irregular fluctuations.

The establishment of a stereotype in the cortical processes clearly manifests itself also in the absence of the real stimuli by means of which it was formed (experiments of Krzhyshkovsky, Kupalov, Asratyan, Skipin and others). Here is one of these interesting experiments. We elaborate in the animal a series of positive conditioned reflexes of different intensities, as well as of negative reflexes, applied at regular intervals and always in a strict sequence. Later, when in one of the experiments we apply only one of the positive (preferably weak) stimuli, the following occurs. The effect throughout the experiment shows the same fluctuations as exhibited by the whole system of different stimuli. The old stereotype persists for a time and then gives way to a new one, i.e., the repetition of one stimulus in the end produces a uniform effect. But the role of the old stereotype, if well established, does not end here. If the last stimulus is not applied for a time and then is tried once more, we get not a new stereotype but again the old one. Consequently, there takes place a certain supersession of stereotypes and a sort of rivalry between them.

Another, still more interesting, phenomenon is also observed. We have a stereotype elaborated from different stimuli. If alongside it a hypnotic state sets in in the dog during the experiment (this state easily develops in some dogs when a single stimulus, and, moreover, a weak one, is applied), then the stimulus now applied singly instead of the previous system, reproduces by its effects the entire system, but in a distorted way: a weak effect is obtained from the previously strong stimuli, and a strong effect—from the weak stimuli, i.e., there arises the paradoxical phase. As is known, we established this phase long ago for stimuli of different intensities in a hypnotic state. Thus, in the given case the dynamic stereotype is combined with the hypnotic state.

I believe that there are sufficient grounds for assuming that the above-described physiological processes in the cerebral hemispheres conform to what we use to designate subjectively as our *senses*, both in the general form of positive and negative senses, and in the form of their numerous nuances and variations due to different combinations and intensities. Among these are the senses of difficulty and facility, gaiety and fatigue, satisfaction and chagrin, joy, triumph, despair, etc. It seems to me that the painful senses, which often accompany a change in the habitual mode of life, an interruption of customary work, loss of close relations or friends, to say nothing of a mental crisis and collapse of beliefs, are, to a considerable degree, physiologically caused precisely by the change, the disturbance of the old dynamic stereotype and the difficulty of elaborating a new one.

In particularly intense and durable cases a morbid melancholy may also arise. In this connection I vividly recall the following fact which happened in my student days. We, three schoolmates, entered the university, and influenced by our literary inspirer at the time[122] chose the faculty of natural sciences. Thus, we began to study chemistry, botany, etc., i.e., we first began to learn definite facts. While two of us got used to this study, the third, whose favourite subject at school had been history and who had been very fond of writing compositions concerning the causes and consequences of various historical events, became more and more depressed and ended in deep melancholy and persistent attempts to commit suicide. His recovery was attained in the following way. We, his friends, at first almost forcibly, made him attend the lectures in the law faculty. After attending a few of them a marked improvement set in, until finally the normal state was fully recovered. Afterwards he entered the law faculty, graduated and was absolutely normal all his life. From our conversations with him before and during the disorder we learned that at school he had acquired the habit of freely relating one phenomenon to another, there being no serious obstacles in this respect, and that he had tried to do the same now, in studying the natural sciences. However, the implacable facts constantly opposed this trend and did not permit doing what could be easily done with the purely verbal material. Repeated failure brought on his bad mood and finally led to a morbid form of melancholy.

Similarly, when we offered our dogs difficult tasks, i.e., when we demanded of them the formation of new and complicated stereotypes, we met not only with the painful state described at the beginning of this address, we were also able to produce chronic nervous diseases—neuroses, which subsequently had to be treated.

CONCERNING THE POSSIBILITY OF FUSION OF THE SUBJECTIVE AND THE OBJECTIVE[123]

The physiology of the higher nervous activity developed before our eyes when the physiologist first began to study systematically by the objective method of conditioned reflexes the normal activity of the cerebral cortex and of the adjacent subcortex—the special apparatus of relations between the entire organism and the surrounding medium—and to establish the fundamental laws of this activity, i.e., to study it exactly in the same way as he studies the apparatus of digestion, circulation of the blood, and other subjects.

Since then there have gradually developed ever-increasing possibilities to superimpose the phenomena of the subjective world on the physiological nervous relations, in other words, to merge them. This was inconceivable when the experiments of the physiologist were confined to artificial stimulation of different cortical points and extirpation of different parts of the cortex in animals. On the contrary, at that time there was a strange situation. Two branches of human knowledge which deal with the activity of one and the same organ of the animal and human organism (who can dispute this now?) remained more or less isolated from each other and sometimes even regarded their independence of each other as a matter of principle. As a result, the physiology of the higher part of the brain stagnated for a long time; as for psychology, it could not even work out a common language to designate the phenomena investigated by it, although numerous attempts to elaborate a terminology that would be adopted by all psychologists were made.[124] Now the situation has radically changed, especially for the physiologists. Vast prospects for observation and experimentation open up before them. The psychologists will at last gain common firm ground—a natural system of the basic phenomena studied by them, a system that will enable them to classify more easily the interminable chaos of human emotions. There is coming into being, and there is bound to be realized, a natural union, a fusion of the psychological with the physiological, of the subjective with the objective. The problem that has troubled human thought so long will find its *real* solution. And the urgent task of science in the near future is to contribute to this in every possible way.

Naturally, the possibilities for this fusion are provided most of all by cases of disorder of the human brain when distortion of the human subjective world is linked, obviously, with anatomical and physiological disturbances of the higher part of the brain.

X

EXPERIMENTAL PATHOLOGY OF THE HIGHER NERVOUS ACTIVITY

EXPERIMENTAL PATHOLOGY OF THE HIGHER NERVOUS ACTIVITY[125]

First I should like to say a few introductory words concerning the complicated fate of our work in the sphere of physiology and pathology of the higher nervous activity, assuming that the adjectives "higher nervous" conform to the adjective "psychical."

Thirty-five years ago I was engaged in the investigation of digestion—previously a special subject of mine—and among other things I investigated the so-called "psychical secretion" of saliva. Intending to subject it to further analysis, I soon became convinced that if we adopted the psychological standpoint, that is, if we started guessing what the dog feels, thinks, etc., nothing would come of it and no exact knowledge could be obtained. It was then that I first decided to treat this psychical phenomenon, this "psychical salivation" as objectively, that is, solely from without, as everything else is studied in physiology. Soon Dr. Tolochinov became my associate and we began this work together. Helped by numerous collaborators we have been carrying on this work incessantly for the last thirty-five years.

At the outset the work was marked by a slight but interesting occurrence in our laboratory life. When I decided to continue the work along those lines, one of my collaborators, a very clever and alert young man who had worked with me on another, ordinary physiological subject, expressed his astonishment and even indignation. "How is that?" he said, "For goodness' sake! Is it conceivable to study psychical activity on dogs, and in the laboratory?" And this, as it appeared subsequently, was very significant. Twelve years later, when I travelled to London for the jubilee celebrations of the Royal Society, I met the leading British neurophysiologist Sherrington. "You know," he said to me, "your conditioned reflexes would hardly be popular in England, since they have a materialistic flavour."[126]

Well, and how do matters stand at present? I must tell you that these first impressions of our new work are still typical of the attitude to it of a considerable part of the educated public; and because of this work I am regarded by many as a very odious person.

Now, what about science? Here, too, the situation is far from being definite. True, in England, the country with which Sherrington tried to frighten me, there is an altogether different situation. There, the theory of the conditioned reflexes is now taught in all schools. It has been widely recognized also in the United States. But this is a long way from being the case in all countries. In Germany, for instance, the approach towards this theory is far from being such. Not very long ago a German professor of physiology visited Kharkov; in the course of a conversation with Professor Volborth—one of my former assistants—about conditioned reflexes, he plainly stated that this was "keine Physiologie."

It should be added that in general physiologists still cannot exactly determine the proper place of conditioned reflexes in the text-books on physiology. It seems to me that these reflexes must be rightfully advanced to the fore when expounding the physiology of the cerebral hemispheres, since they represent the normal, objectively established work of these hemispheres. Analytical data accumulated up to the present time by means of stimulations, extirpation[127] and other methods of investigating the cerebral cortex, must follow, naturally, the description of the normal activity.

I do not know what impression our modern physiology of conditioned reflexes expounded by Prof. Podkopayev has made on you, but in submitting the pathology of these reflexes, I make so bold as to think that you will see for yourselves how expedient and fruitful our method of treating the subject is. That is why I deemed it necessary to begin with this short introduction.

Now for the subject itself. I am very glad that Prof. Podkopayev delivered his lectures on the physiology of conditioned reflexes, prior to me, before this very audience; this relieves me of the need to make any preliminary explanations. I take it for granted that all of you are in possession of the basic physiological data, and so I shall proceed directly to an exposition of the purely pathological facts.

The nervous activity, as all physicians are aware, consists of two mechanisms, or two processes—excitatory and inhibitory. With regard to these two processes we distinguish three fundamental elements, namely, the strength of both the excitatory and inhibitory nervous processes, the mobility of these processes—their inertness or lability—and finally, the equilibrium between these processes.

Certainly, the entire normal higher nervous activity, or in the usual terminology, the psychical activity, not only of animals, but also of man, is based on the normal course of these processes with their inherent properties. At least our experiments with dogs, our usual objects of investigation, have convinced us that all their intricate and highly complex relations with the surrounding world fully come within the bounds of our research into the above-mentioned processes and their properties; they are comprehended by us to the extent permitted by the possibilities of our experiments.

We can divert all these processes with their basic properties from their normal path and cause them to become pathological. For this purpose we have quite definite methods at our disposal. There are three of these methods —overstrain of the excitatory process, overstrain of the inhibitory process and overstrain of the mobility of the nervous processes. As to the latter method, it should be pointed out that the expression "overstrain of the mobility of the nervous processes" is actually used by me for the first time; usually we refer to it as collision of the excitatory and inhibitory processes.

How to weaken the excitatory process, to make it pathological? For this purpose it is necessary to act upon the cell, within which the excitatory process is produced, by an external agent of a very great, extraordinary strength; by doing so we overstrain the work of the cell, its excitatory process, as a result of which the latter becomes pathological.

In a similar way, that is, by overstrain, the inhibitory process can also be made pathological.

You already know how we obtain inhibition by means of negative conditioned stimuli. Let us suppose that a given conditioned inhibitory stimulus has constantly evoked in its cell inhibition of half a minute duration and that the cell has endured it very well. I then expressly prolong the action of the same stimulus for five or ten minutes. A strong cell is able to sustain it, but in a weak cell the inhibition breaks down; the work of the cell becomes pathological and changes in different ways.

Finally, the third method. It is possible to make both the excitatory and inhibitory processes pathological by means of abruptly transforming, without any intervals, the inhibitory state of the cell into a state of excitation, or vice versa. We usually refer to this as a collision between the excitatory and inhibitory processes. It is obvious that a certain amount of time is required for a corresponding change in the activity of the cerebral cells, just as it is required for any other activity. Under such collisions only those cells may remain unaffected and intact where the basic nervous processes are strong and especially where these processes are highly labile.

Now, what results from the action of these morbific methods? How does deviation from the normal occur? How is the pathological state of the cells originated? A general weakening of the cell takes place. As to the excitatory process, the cell becomes incapable of performing the work which it performed previously, i.e., the limit of its working capacity decreases, and this manifests itself in the following pathological phenomena.

You are aware that if we have before us an absolutely normal cell and that if we apply external agents of different physical strength as conditioned stimuli, the conditioned effects of these stimuli more or less correspond to their physical strength.

Now, if we break this cell down, i.e., if we overstrain it and thus make it pathological, its relation to the stimuli becomes different. In some cases conditioned positive stimuli of different physical strength produce an equal effect, and then we say that this is the equalization phase of the cell's activity. In other cases, when the weakening of the cell, i.e., the decrease in the limit of its working capacity, is progressing, a state sets in in which strong stimuli produce a lesser effect than weak ones; this is the paradoxical phase. Finally, a further disturbance of the cell's activity manifests itself in the fact that the cell no longer responds to the positive stimulus, whereas the inhibitory stimulus produces a positive effect; we have termed this phase the ultra-paradoxical phase.

Besides the decline in the limit of working capacity, i.e., the weakening of the excitatory process in the cell, there can also be observed other changes of the excitatory process. One of the most striking of these—particularly interesting and particularly applicable in neurology and psychiatry—is the inert state of the excitatory process, i.e., a state in which the excitatory process becomes more tenacious, persistent, and gives way more slowly to normally arising inhibitory influences.

I shall dwell for a while on inertness. The excitatory process normally, even in healthy people, varies not only in strength, but also in another respect—in mobility. With some people the excitatory process is less mobile, i.e., it is more susceptible to stimulation, reacts more quickly under the influence of stimulation; at the same time, when the stimulation is over, the effect disappears sooner than with other types of normal people.

On this basis we, like Hippocrates, divide the equilibrated, strong animals into two categories—the phlegmatic and the sanguine. The phlegmatics, it follows, will be characterized by a relatively slow development of the excitatory process, the sanguines—by a quick one.

But this is within the bounds of normalcy. If, however, I act on the cell by means of morbific methods, I can make the inertness of its excitatory process excessive and pathological so that its state of excitation becomes exceedingly persistent.

Concerning the pathological changes of the excitatory process the following addition should be made. Two morbid changes in its mobility are observed. One of these I have just mentioned—pathological stagnation. Given other morbific conditions we get a diametrically opposite state of the nerve cell, namely, pathological lability. In neurology this is known as excitatory weakness, i.e., a state in which the cell becomes very alert, very rapidly reacts to stimulation, and at the same time quickly becomes bankrupt and weakens. We call this the state of explosiveness.

In the same way it is possible to break down (in our usual laboratory terminology) the inhibitory process as well, to make it pathological. By means of a sudden, and not gradual, considerable extension of the duration of the inhibitory state in a cell through the action of a corresponding external stimulus, we can greatly weaken the inhibitory function of the cell and almost fully destroy it.

It should be pointed out that in this respect the inhibitory process has been investigated to a lesser degree than the excitatory one.

The inhibitory process, too, usually manifests itself in different ways with regard to its mobility. Sometimes it develops rapidly and just as rapidly vanishes; sometimes, on the contrary, it assumes a more protracted character.

Thus, the inhibitory process is either normally inert or normally labile. However, it can also be brought to a pathological state with regard to inertness. In our laboratory there is a dog which has been exhibiting pathological inertness for the past three years. In this animal under the influence of frequent collisions the positive stimulus began to evoke, instead of the normal excitatory process, an inhibitory one, and of such a persistent nature that although we constantly reinforced it under favourable conditions in the course of the three years, we just could not restore its initial positive effect. Only recently did we find a means of changing this state of affairs, but I shall speak about that at the end.

Thus, you have before you, in general outline, the changes which occur under the action of morbific agents—the change in the excitatory process, the change in the inhibitory process and, hence, as a result, a derangement of the proper correlations between the excitatory and inhibitory processes. But the normal activity of the nervous system is, of course, determined by an equilibrium between these basic processes with their normal properties.

I must tell you that often it is quite easy to obtain a pathological state of the higher nervous activity with the help of the methods I have just mentioned. But, depending on the types of nervous system, one can observe a great difference in the facility with which this pathological state is attained.

In equilibrated and strong animals, i.e., those in which both the excitatory and inhibitory processes are of equal strength and whose lability is normal, it is, of course, likewise possible to produce a nervous disorder; however, it would take considerable time and labour, since it necessitates trying different methods. In excitable and weak animals this is very easily attained. As you already know, we classify as "excitable" that type of animal in which the excitatory process is very strong; the inhibitory process is probably also considerable, but the two processes do not conform. The excitatory process strongly predominates, and therefore in this type the negative stimuli hardly ever reach zero. This type can be broken down rather easily, i.e., made pathological. As soon as it is offered a series of tasks calling for a considerable degree of inhibition, it becomes quite weak—the animal can no longer discern anything, inhibit anything, i.e., it becomes neurotic.

As regards animals of the weak type, they can be easily made abnormal by all our methods.

The neurotic state manifests itself in the fact that the animal does not properly respond to the conditions in which it exists. This relates both to its laboratory characteristics and general behaviour. With regard to the latter everyone will admit that whereas previously the dog was normal, it is now ill.

In the laboratory we usually apply a system of conditioned reflexes—positive and negative—which are elaborated on the basis of various unconditioned stimuli: positive reflexes to stimuli of different physical strength and different kinds of negative reflexes. This entire system is normally governed by strict rules: the positive effect depends on the strength of stimulation; the inhibitory stimulus produces a greatly diminished or a zero effect, etc. Under the influence of our morbific methods all or many of the normal reactions become weakened and distorted.

The disturbed nervous equilibrium is clearly observed not only by us in the system of conditioned reflexes; our attendants also notice it. The dog obeyed them previously, behaved orderly, it knew where to go when led to an experiment. Now everything has abruptly changed. And the attendants simply say that the dog has become stupid and even has gone mad.

The pictures of neuroses in diseased animals vary considerably owing either to the different intensity of the disorder, or to the appearance in the foreground of this or that pathological symptom. Recently we have observed a particularly large number of such neuroses and neurotic symptoms on an organically pathological basis, namely, on castrated animals. It goes without saying that castration itself disturbs the normal relations within the nervous system. I shall, therefore, briefly touch on the post-operative state of our dogs, as far as their nervous system is concerned.

One of the most striking of the morbid, neuropathological symptoms appearing almost immediately after castration is an enormous decline of the inhibitory process, of the inhibitory function, so that the dog, which prior to the operation acted in an exemplary manner, in full accordance with the conditions influencing its nervous system, now becomes quite chaotic. Normally one sees day after day an absolutely uniform and perfectly exact system of conditioned reflexes, but after castration no day is similar to another; there is a series of entirely different days and there is no order whatsoever.

One more very important detail manifested itself shortly after castration and surprised even us. In the case of strong types, the action of the animals after castration, as I have just said, is extremely distorted and instead of being strictly regular, becomes chaotic. In the case of weak types the reverse is the case. For some time after the operation the dogs behave better and more orderly than before. True, this different condition exists only temporarily—for one, one and a half or two months. Then the nervous activity in these dogs, too, becomes weakened just as that in strong dogs. I shall revert to this question later and show on what this difference is based and how we interpret it.

Then, after months of entirely chaotic activity a circularity sets in which did not exist before, i e., the dogs do not work and manifest their system of conditioned reflexes in a disorderly manner constantly, i.e., from day to day, but their activity now periodically changes. It is chaotic for a while and then for a certain period it greatly improves in a spontaneous way and becomes more orderly. And as time goes on, the more distinct this periodicity becomes; the periods of better work are more frequent and of larger duration, until after some years everything becomes normal. This, obviously, denotes the existence of certain adaptability in the organism.

Of course, since we know the system of endocrine glands, which to a certain degree assist and replace one another, it is conceivable that in time the defect sustained by the organism immediately after castration becomes more or less levelled out. But the return to the apparent normal after castration takes place in different dogs after different periods; with some it occurs after one month, with other it takes years, and there are dogs in which this state has so far not set in at all. This is obviously connected with the initial strength of the nervous system.

It is clear that in these castrated dogs, after their full or partial recovery, it is possible to produce various neuroses much more easily than in absolutely normal dogs, since in the former the equilibrium has already been disturbed, and naturally they are, so to speak, more fragile than normal dogs. Thus, we can produce in them numerous neurotic disturbances by means of the above-mentioned morbific methods.

To a considerable degree the pathological nervous states produced by us conform to the so-called psychogenic diseases in human beings.[128] The same overstrain and the same collisions of the excitatory and inhibitory processes are also encountered in our own lives. For instance, somebody has deeply insulted me and I for some reason or other have not been able to respond to it by corresponding words, or, moreover, by a certain action, with the result that I had to overcome the struggle or conflict between the excitatory and inhibitory processes within myself. And this was repeated more than once. Or let us take another case from the literature of neuroses. A daughter is at the sick-bed of her father whom she loves deeply and who is living his last days, however, she must pretend that everything is all right and that everybody expects his recovery, whereas in reality she is weighed down by unbearable anguish and sorrow. This often leads to breakdowns, to neuroses.

Indeed, can we find any essential physiological difference between such breakdowns and those which we obtain in our experimental animals by colliding the excitatory and inhibitory processes?

But in addition to these neuroses, there must be, owing to the extreme complexity of our brain in comparison with that of the higher animals, special human neuroses, to which I ascribe psychasthenia[129] and hysteria.[130] These states cannot be produced in dogs, since in cases of this kind the division of the human brain into a higher, purely human part, connected with speech, and a lower part, which, just as in animals, receives the external impressions and directly analyses and synthesizes them in a certain way, makes itself felt. But neurasthenic states of different kinds can be fully reproduced in animals.

In view of the fact that our data seemed to me sufficient for a physiological interpretation of the mechanisms of nervous diseases, I decided, two or three years ago, to visit the neurological and the psychiatric clinics (of course, devoting only a little time to the matter). As far as the neurological clinic is concerned, I can say that practically all the neurotic symptoms and pictures observed there can be understood and connected with our pathophysiological laboratory facts. And this is not only my personal opinion, the opinion of a physiologist, it is also the opinion of neuropathologists who acquainted me with the clinic and who admit that our physiological interpretation of neuroses is not fantastic, that we are really laying a solid foundation for constant contact between our laboratory facts and human neuropathological phenomena.

Before passing to another category of our facts I shall explain a phenomenon which I have mentioned but have not analysed in detail.

Why is it that animals with strong nervous systems immediately after castration become chaotic, and only later, after a certain time, does their behaviour more or less level out, while animals with weak nervous systems, on the contrary, immediately after castration behave better, in a more regular manner than before castration, and only later become disabled?

We think that this phenomenon should be explained in the following way. Since an animal possesses sex glands, it experiences sexual excitation; consequently, additional impulses come to the brain and tonify it; but the brain is weak. Hence the deficiency in the general nervous activity. With the removal of the sex glands the additional stimuli disappear, the nervous system is eased, and its activity in all other respects assumes a more expedient character. There is nothing fantastic in this explanation. We clearly observe the same in another, more tangible case. The degree of appetite in the experimental dog is of great importance in our system of conditioned reflexes. If you have a strong dog and increase its food excitability by means of a certain method (while performing experiments with alimentary reflexes), then all its conditioned effects are increased. On the contrary, with a weak dog a heightened food excitability usually leads to a decline of the conditioned reflexes, i.e., the additional excitation cannot be endured by the dog, and is accompanied by inhibition, which we, therefore, call protective.

Now I shall proceed to another category of facts. The development of definite pathological states in the nervous system with the aid of our definite methods is, of course, based on the fact that our concept of the mechanism of this system is to a certain degree correct. The power of our knowledge over the nervous system will, of course, appear to much greater advantage if we learn not only to injure the nervous system but also to restore it at will. It will then have been really proved that we have mastered the processes and that we can control them. Actually, this is the case. In many instances we not only bring on disease, but eliminate it with great exactitude, one might say, to order. Of course, in this case it was necessary, above all, instead of reasoning and searching for various remedies at random, to be guided by the indications of medicine. Thus, bromide plays a very important role with us. But in order to apply this remedy accurately thorough knowledge of the mechanism of its action was necessary.

With regard to bromide we have definitely established, without the least doubt, that its action is quite different from that hitherto assumed and possibly still assumed by pharmacologists. The physiological effect of bromide consists not in decreasing excitability or in weakening the excitatory process, but in intensifying the inhibitory process. Bromide bears a special relation to the inhibitory process, and this can be proved by numerous experiments. Here, for example, is a very simple experiment which we always apply when need arises.

You have an excitable type of dog—the type in which the excitatory process is extremely strong and the inhibitory process relatively weak. Consequently, the dog cannot bring its inhibitory reflexes to a complete zero—its inhibition is insufficient. You administer bromide to the dog and immediately obtain complete inhibition. You often observe in this case also a greater positive effect than previously, before the administration of bromide. But there is another, no less important side to the effect of bromide.

Although bromide has been rightly used as a remedy for nervous diseases for years (I do not know exactly for how many years but not less than sixty or seventy), it is an absolute truth that to this day medicine has not always used this powerful instrument of nervous therapy in a proper way, often committing a very serious error.

You administer bromide in a case of a neurotic state. Let us suppose that the bromide produces no effect. Then you increase the dose thinking that the previous dose was too small. But this is true only in one series of cases. In other cases, and probably in the overwhelming majority of them, the dose must be decreased and not augmented. Often you must even decrease the dose to a very considerable degree. The gradation of the useful doses of bromide is highly extensive; in our dogs its limits are approximated to a thousandfold. This is absolutely true and we all guarantee it. Consequently, a very important correction must be made in medicine in this respect. If you administer an excessively large dose, you may obtain an injurious instead of a beneficial effect; you may cause the patient serious injury.

There can be no question, of course, that this is true only of dogs, and that with nervous people matters are different. The neuropathologists in our clinic have observed that when they took these facts into consideration it turned out that in many cases successful treatment necessitated not an increase in the doses of bromide but reduction to decigrammes and centigrammes. The general laboratory rule is: the weaker the type of nervous system and the given nervous state, the smaller must be the dose of bromide.

As is also well known in medicine, rest, too, provides a certain curative effect in laboratory neuroses. If a dog has been made neurotic by us, it is often helpful not to work with this dog every day, since a daily system of our conditioned reflexes is undoubtedly a difficult task, which in this state is beyond the dog's strength. As soon as you introduce a regular two or three day recess between the experiments, the nervous system begins to recover.

In some cases it has been observed that rest, as it were, substitutes bromide. Suppose you have a dog whose work after castration is chaotic. You can help it in two ways: either you make it work (that is, you experiment with it) not every day, but once in two or three days, with the result that its work considerably improves; or you administer a suitable dose of bromide which produces the same effect.

It should be pointed out that we are now applying another extremely important method of treatment, but as yet we are not entitled to say definitely that it is an agent of radical treatment. Still, it is impossible not to pay attention to it and not to look upon it with great hope.

With the help of our morbific methods, which make the whole cerebral cortex pathological, it is also possible to cause a completely isolated region of the cortex to become ill, this is an extremely important and highly impressive fact. Suppose you have a dog with a series of different acoustic conditioned stimuli: beats of the metronome, a noise, a tone, a crackling or a gurgling sound, etc. From all these stimuli it is not difficult to obtain only one which would prove noxious and evoke a sharp deviation from the normal. So long as you apply the other acoustic stimuli, the animal's behaviour is orderly and its work is quite regular. But the moment you touch the point of application of the morbific stimulus, not only is the reaction to it distorted in one degree or another, but thereafter the entire system of conditioned reflexes becomes deranged, and its harmful effect spreads over the whole cerebral cortex. This fact in itself leaves no room for doubts, since it has been frequently produced and is being produced now by many experimenters.

But here I would like to draw your attention to the following. When I enumerated all our sounds, it was obvious that they were of a more or less complex nature. How, then, are we to picture the disorder of the cerebral cortex in relation to separate sounds? It can hardly be assumed that to each sound applied by us there corresponds a particular group of nerve cells receiving the elementary acoustic stimuli of which the sound is formed. It is more probable that in the case of each of our acoustic stimuli it is a question of a dynamic structural complex, whose elements, the corresponding cells, enter also into other dynamic complexes when other complex sounds are applied. And it is the results of the difficulties created by our morbific methods in the processes connecting and systematizing the dynamic complexes that are responsible for the destruction and disturbances in those complexes.

Isolated pathological points can be obtained in all parts of the cerebral hemispheres. Here is an example. You elaborate conditioned positive stimuli from a mechanical stimulation of different spots of the skin. You can obtain such a state when in two points of the skin the excitatory process does not call forth any pathological effect while the third is functionally pathological.

We now have a dog of the excitable type, i.e., one in which the excitatory process is extremely strong but in which the corresponding inhibition is insufficient. This dog has been castrated. Being of a strong type it recovered rather quickly. Since it was excitable much time and effort was required prior to castration to elaborate in it a differentiation to the metronome. For a period after castration our laboratory sustained some trouble: there was a shortage of food for the animals and they became emaciated. Due to the general nervous exhaustion the reflex of our dog to the metronome, which had been complicated by a difficult differentiation, became morbid, while all other conditioned reflexes remained unaffected. As soon as metronomes were applied, normal work with conditioned reflexes became impossible. We tried to exclude the inhibitory metronome as the more difficult one, and to make use only of the positive metronome, but that did not change the picture. Bromide proved ineffective, which, for some unknown reason, is generally the case in disorders of isolated points of the cerebral hemispheres

Then the question arose whether the same thing would occur in another part, in another analyser of the cerebral hemispheres where the excitatory and the inhibitory processes would collide. In order to obtain an answer to this question we selected the cutaneous region, where we could apply an easier differentiation, i.e., make one spot of the skin positive and another inhibitory. The stimulation of one spot was reinforced by feeding, while that of the other spot was not. The effect was the same. So long as the positive conditioned stimulus alone was being elaborated, the dog behaved quite normally, and the entire system of reflexes was in order. But as soon as the inhibitory stimulus began to manifest itself, all the reflexes diminished and became distorted; the dog became extremely violent, so that the experimenter could not attach the apparatus to the skin or take it off without the risk of being bitten.

Now I wish to direct your attention to the following interesting phenomenon. When we had such isolated points in the cerebral cortex of other dogs, their harmfulness and morbidness were expressed only in the fact that their stimulation resulted in the derangement or destruction of our entire system; but our observations showed that this was never accompanied by a manifestation of pain in the animals. However, in this case there was a distinct impression that the touch to the skin became painful. How is this phenomenon to be explained?

As a matter of fact the only difficulty during the collision of the excitatory and inhibitory processes was in the brain, and this difficulty made itself felt in the system of conditioned reflexes. What, then, caused the pain in the skin? Apparently this may, and should, be explained in the following way. In a certain point of the cerebral cortex of the dog there arises a considerable difficulty, which must cause pain, just as you feel a kind of heaviness, a very disagreeable sensation in your head when you tackle an extraordinarily difficult problem. We must assume a similar state in our dog. But in the course of these experiments the dog apparently formed a conditioned connection between the attaching of the apparatus to the skin and the difficult state of the cutaneous analyser in the brain; conditionally the dog transfers the struggle against this difficult state in the brain to the moment of skin stimulation, exhibiting resistance to any contact with the skin. However, this is not a hyperaesthesia of the skin. Consequently, this is an extremely interesting case of objectification of an internal cerebral process, a manifestation of the strength of its connection with the stimulation of the skin. As for the brain, we must assume merely a special kind of heavy sensation in it, a peculiar kind of pain. It is not without reason that psychiatrists have described melancholia as a mental pain, or a cortical pain, the sensation of which differs from the pain caused by wounds or disorders of different parts of the organism.

Thus, for a long time we could not do anything with this dog. At last, however, a favourable way out was found thanks to the good fortune of one of my oldest and most valuable associates, Dr. Petrova. Formerly Petrova worked as a therapist, but later she was enticed into the study of conditioned reflexes and has devoted herself entirely to it for many years. I had an interesting experience in this connection. I must tell you that although I began my professorship as a pharmacologist,[131] I have always had a strong prejudice against introducing several substances at a time into the organism. It always struck me as strange whenever I saw a prescription containing three and more drugs. What a brew! And I had always been against such combinations of pharmaceutical remedies in the physiological analysis of phenomena; in this I proceeded form the principle that the simpler the conditions of the phenomena are, the better the chances for elucidating them. I admitted bromide to our laboratory as a single drug basing myself on medical practice; caffeine was also introduced as a separate stimulant related to the excitatory process. But I was always against using them in combination. However, the therapist, being used to combinations, insisted on a trial, and proved to be right. The effect was extraordinary and miraculous. When a mixture of bromide and caffeine was given to the dog mentioned above, the persistent neurosis immediately disappeared without leaving the slightest trace. We acted carefully. Having administered the mixture of bromide and caffeine for two days, we at first tried only the positive mechanical stimulation of the skin. The effect proved to be normal; the animal was absolutely quiet and no derangement of the system of conditioned reflexes was observed. A little later, being encouraged by the results of the trial of the positive stimulus, we applied the negative one. In this case too the effect proved to be the same—there was not the slightest trace of the former morbid reaction.

Post factum it was not difficult for me to build a respective theory. Now I presented the matter to myself in the following way. Certainly it must be assumed that in the overwhelming majority of cases a disorder of the nervous system is a disturbance of the proper correlations between the excitatory and inhibitory processes, as it appeared in the course of application of our morbific methods. Now since we have, so to speak, two levers in the form of pharmaceutical remedies, two communicators towards the two chief apparatus, i.e., towards the two processes of nervous activity, then by putting into action and correspondingly changing the strength now of one, now of the other lever, we have a chance of restoring the disturbed processes to their former place, into their proper correlations.

We have another similar case. I have already mentioned the case of the dog in which the pathological inertness of the inhibitory process lasted for three years, i.e., its positive process became pathological and the positive stimulus turned into an inhibitory one. Although we have been constantly reinforcing this stimulus for three years now, i.e., we have been creating the conditions under which it ought to be positive, we have always had it inhibitory. No matter what we tried—bromide, rest, etc.—nothing helped. Under the influence of the mixture of bromide and caffeine this stimulus which for such a long time produced a morbid reaction, has now assumed a normal positive effect.

In the same dog, parallel with the pathological inertness of the inhibitory process, there was pathological lability of the excitatory process on another stimulus, i.e., it developed its action not gradually but impetuously, in an explosive manner; but a negative phase set in quickly in the course of the excitation. At the first moment of the application of this conditioned stimulus the dog makes a violent effort to reach the food receptacle and exhibits a profuse salivary secretion, but soon, already in the course of excitation, the salivation stops; when you begin to reinforce the stimulus and offer food, it does not take it and turns away. This pathological phenomenon, too, disappears under the action of our mixture, the morbific stimulus becoming quite normal in its action.

Interesting too is the following fact. We administered the mixture to this dog for ten days and then decided to find out whether the cure was radical. But this was not the case. When we ceased to administer the mixture the old relations returned. Of course, much more time is probably required to eliminate the disturbances entirely. But one can also assume that we really establish correct relations between both processes changing them temporarily, but do not treat the processes themselves, or at least both of them simultaneously. It is clear that should it be the first case, it is a great triumph for therapy. In any event, in the present-day palliative, and possibly future radical treatment by means of a mixture of bromide and caffeine, it is necessary to take into account the extreme precision of the dosage of both drugs, reducing them, especially in the case of caffeine, even to milligrammes.

In conclusion, I shall briefly touch on the question of the application of our laboratory results to the neuropathological and psychiatric clinics. As for the first, there is no doubt that our human neuroses can be explained quite satisfactorily in the light of the laboratory analysis. But it seems to me that in psychiatry, too, certain things have been clarified by our laboratory research.

At present I am writing a series of booklets entitled "Latest Papers on the Physiology and Pathology of the Higher Nervous Activity." Two brief articles published in the last issue have been translated into foreign languages. One of them has already been published in French, the other has been sent to an English psychiatric journal, and it goes without saying that I eagerly await the reaction of our own and foreign experts.

Now you are aware that in the laboratory we are able to make pathological, and besides, in a functional way, an isolated point of the cerebral cortex, leaving all other points absolutely intact. I wish to make use of this phenomenon of isolated disorders for interpreting a very interesting and very enigmatic psychiatric form, namely paranoia. As is known, paranoia is characterized by the fact that a mentally normal person, who, like all healthy people, reckons with logic and reality, and sometimes may even be gifted, as soon as it comes to one definite subject, distinctly turns into a lunatic, acknowledging neither logic, nor reality. It seems to me that this form can be understood on the basis of our laboratory findings relating to isolated disorders of separate points in the cerebral cortex.

One can hardly dispute that the stereotypies of skeletal movement[132] can and should be understood as the expression of the pathological inertness of the excitatory process in the cortical cells which are connected with movement, and that perseverations[133] should be similarly looked upon only in the cells of speech movement. But at first sight it is more difficult to explain obsessive ideas and paranoia in the same way. However, it seems to me that the understanding of isolated pathological points of the cerebral cortex not only in a purely crude anatomical sense, but also in a structurally-dynamic one (as mentioned above) has eliminated this difficulty to a sufficient degree.

Here is another case of a neurosis which is very close to a psychosis.

In persecution mania the patient sometimes firmly regards as reality that which he fears and wants to avoid. For example, he wants to have a secret and it seems to him that all his secrets are constantly being disclosed in some way. He wants to be alone, and although he is alone in his room and everything lies open before his eyes, he still imagines that somebody else is with him. He wants to be respected, and it seems to him that at every moment he is being insulted in some way or other by signs, words, or facial expressions. Pierre Janet has described this as feelings of possession, as if somebody is taking hold of the patient.

In my view, this case is based physiologically on the ultra-paradoxical phase, which I have already mentioned and which, as you know, consists of the following.

Suppose we have two metronomes of different frequency which act as conditioned stimuli, one of them with 200 beats per minute being the positive stimulus and the other with 50 beats—the negative one. Now, if the nerve cell becomes pathological or simply falls into a hypnotic state, the effect is reverse: the positive stimulus turns into an inhibitory one, and the inhibitory becomes positive. This is an absolutely exact and constantly recurring laboratory phenomenon. Therefore, I interpret the state of the above patient in the following way: when he wanted to be respected or to remain alone, this was a strong positive stimulus, which evoked in him an opposite idea involuntarily and irresistibly in accordance with the rule of ultra-paradoxicality.

Thus you see that in the field of pathology our method of work, the method of an objective attitude towards the higher phenomena of the nervous activity, is fully justifiable for animals, and the more we apply it the more it is justified. At present we are making, as it seems to me, warrantable attempts to apply the same method to the human higher nervous activity which is usually called psychical activity.

That is all I wanted to tell you.

TYPES OF HIGHER NERVOUS ACTIVITY, THEIR RELATIONSHIP TO NEUROSES AND PSYCHOSES AND THE PHYSIOLOGICAL MECHANISM OF NEUROTIC AND PSYCHOTIC SYMPTOMS[134]

Of the vast material relating to the study of the higher nervous activity in dogs by the method of conditioned reflexes I shall now dwell only upon three points because of their particularly close connection with morbid disturbances of this activity. They are: the strength of the two basic nervous processes—excitation and inhibition—then the correlation of their intensities, or their equilibrium, and finally their mobility. These properties constitute, on the one hand, the basis of the types of higher nervous activity, types which play an important part in the genesis of nervous and so-called mental diseases, and on the other hand, typical changes taking place under pathological states of this activity.

Two thousand years ago the great genius of ancient Greece—the artistic genius, of course, not scientific—was able to discern in the immense diversity of variations of human behaviour its fundamental features in the form of four temperaments. And only now is the study of the higher nervous activity by the method of conditioned reflexes in a position to base this systematization on a physiological foundation.

According to the strength of the excitatory process (i.e., according to the working capacity of the cerebral cells) our dogs were divided into two groups—strong and weak. The strong group, in its turn, was divided into equilibrated and unequilibrated, depending on the correlations between the intensities of the excitatory and inhibitory processes. And finally the strong and equilibrated dogs were divided, according to the mobility of the processes, into quiet and lively ones. Thus, there are four basic types: the strong and impetuous type, the strong, equilibrated and quiet type, the strong, equilibrated and lively type, and the weak type. And they correspond to the four Greek temperaments—choleric, phlegmatic, sanguine and melancholic. Although there are different gradations of these types, life clearly shows that it is just these combinations that are more frequently met with and bear a more pronounced character. It seems to me that this coincidence of types in animals and human beings is convincing proof that such a systematization conforms to reality.

However, to obtain a full and clear idea of the variations of human behaviour, normal and pathological, it is necessary to add to these types, which are common in man and animals, certain particular, purely human types.

Before the appearance of the family of homo sapiens the contact of the animals with the surrounding world was effected solely by means of direct impressions produced by its various agents which acted on the different receptor mechanisms of the animals and were conducted to the corresponding cells of the central nervous system. They were the sole signals of external objects. In the future human beings there emerged, developed and perfected, signals of the second order, signals of these initial signals, in the shape of speech—spoken, auditory and visible. Ultimately these new signals began to denote everything taken in by human beings directly from the outer, as well as from the inner world; they were used not only in mutual intercourse, but also in self-communion. This predominance of the new signals was conditioned, of course, by the tremendous significance of speech, although words were and remain but second signals of reality. We know, however, that there are large numbers of people who, operating exclusively with words and failing to base themselves on reality, are ready to draw from these words every possible conclusion and all knowledge, and on this basis to direct their own life as well as the life of others However, without entering deeper into this important and very broad subject, it is necessary to state that thanks to the two signalling systems, and by virtue of the long-established different modes of life, human beings in the mass have been divided into artistic, thinking and intermediate types. The last-named combines the work of both systems in the requisite degree. This division makes itself felt both in individual human beings and in nations.

Let us pass now to pathology.

In our experiments on animals we constantly obtained convincing proof that chronic pathological derangement of the higher nervous activity under the influence of morbific agents arises with particular ease in the impetuous and the weak types, where it assumes the form of neurosis. Impetuous dogs become almost completely deprived of inhibition; in weak dogs the conditioned reflex activity either fully disappears, or is of a highly chaotic character. Kretschmer, who recognizes only two general types corresponding to our impetuous and weak types, correctly, as far as I can judge, associates the first with the manic-depressive psychosis, and the second with schizophrenia.

Having some very limited clinical experience (during the last three or four years I have regularly visited the nervous and psychiatric clinics) I take the liberty of advancing the following supposition concerning human neuroses. Neurasthenia is a pathological form inherent in the feeble-general and intermediate human types. A hysterical person is the product of the feeble-general type combined with the artistic type, and the psychasthenic (to use the terminology of Pierre Janet) is the product of the feeble-general type combined with the thinking type. In hysterical persons, general weakness, naturally, has a special effect on the second signalling system, which in the artistic type in any case yields pride of place to the first system, while in normally developed persons the second signalling system is the highest regulator of human behaviour. Hence the chaotic character of the activity of the first signalling system and of the emotional fund in the form of pathological fantasies and unrestrained emotivity with profound destruction of the general nervous equilibrium (sometimes paralyses, at other contractures,[135] or convulsive fits or lethargy) and in particular, synthesis of personality. In psychasthenics the general weakness, naturally, again affects the basic foundation of the correlations between the organism and environment, namely, the first signalling system and the emotional fund. Hence the absence of a sense of reality, continual feeling of inferiority of life, complete inadequacy in life together with constant fruitless and perverted cogitation in the form of obsessions and phobias.[136] This, in general outline, is how I conceive the genesis of neuroses and psychoses in connection with the general and particular types of human higher nervous activity.

Experimental study of pathological changes in the basic processes of the nervous activity of animals makes possible a physiological understanding of the mechanism of the mass of neurotic and psychotic symptoms, both taken separately or as components of certain pathological forms.

Weakening of the excitatory process leads to the predominance of inhibition, both general and diversely partial, in the form of sleep or of a hypnotic state with its numerous phases, of which most characteristic are the paradoxical and ultra-paradoxical phases. This mechanism, I believe, is responsible for a particularly large number of pathological phenomena, such as narcolepsy, cataplexy, catalepsy,[137] feelings of possession—les sentiments d'emprise (according to Pierre Janet), or inversion (according to Kretschmer), catatonia,[138] etc. The weakening of the excitatory process is caused either by its overstrain, or by its collision with the excitatory process.

Under certain laboratory conditions which are not yet quite clear there takes place a change in the *mobility* of the excitatory process in the form of *pathological lability*. This phenomenon, long known in the clinic under the name of excitatory weakness, consists in an extremely high reactivity or sensitivity of the process followed by its rapid consecutive exhaustion. Our conditioned positive stimulus produces an instantaneous and extraordinary effect, which, however, falls to zero and becomes inhibited already during the normal period of stimulation. We sometimes call this phenomenon explosiveness.

But in our experimental practice we also meet with quite the opposite pathological change in the *mobility* of the excitatory process—with *pathological inertness*. The excitatory process persists despite a prolonged application of conditions, under which normally the excitatory process is superseded by inhibition. The positive stimulus is not susceptible or slightly susceptible to successive inhibition evoked by preceding inhibitory stimuli. This pathological state is in some cases caused by a moderate, but continuously growing intensity of the excitatory process, and in other cases by collisions with the inhibitory process. It is quite natural to attribute the phenomena of stereotypy, obsessive ideas, paranoia, etc., to this pathological inertness of the excitatory process.

The inhibitory process can also be *weakened* either by its overstrain, or by collisions with the excitatory process. This weakening leads to an abnormal predominance of the excitatory process in the form of a derangement of differentiations, retardation and other normal phenomena in which inhibition intervenes; it also manifests itself in the animal's general behaviour in the form of fussiness, impatience and violence, and finally in the form of pathological phenomena, for example, neurasthenic irritability. In man it takes the form of submanic and manic states,[139] etc.

This year phenomena of pathological lability of the inhibitory process have been observed in our animals by my old colleague, Prof. Petrova, who has enriched experimental pathology and therapy of the higher nervous activity with quite a considerable number of important facts. A dog which previously took its food, placed at the edge of a staircase, with ease, without any hesitation, ceases to do so, hurriedly avoids the food and moves away from the edge. The matter is quite clear. When a normal animal, approaching the edge of a staircase, stops and does not move farther, this means that it is able cofidently to hold itself back, as much as is necessary to prevent it from falling down. In our case this retention is exaggerated; the reaction to depth is excessive and keeps the dog, to the detriment of its interests, much farther from the edge of the staircase than is actually necessary. Subjectively this is an obvious state of dread or fear, a phobia of depth. The phobia could be induced, and could be eliminated, i.e., it was under the experimenter's control. The condition responsible for its emergence is what we may call the torture of the inhibitory process. I will demonstrate this fact in a few days' time at the international physiological congress in Leningrad. I think that in many cases persecution mania can also be accounted for by the pathological lability of inhibition.

We have already examined the pathological inertness of the inhibitory process.

A difficult task still remains to be accomplished—it is necessary to determine with precision and in all cases when and in what particular conditions one or another pathological change arises in the basic nervous processes.

FUSION OF PRINCIPAL BRANCHES OF MEDICINE IN MODERN EXPERIMENTATION AS DEMONSTRATED BY THE EXAMPLE OF DIGESTION[140]

Despite the extremely complex nature of biological phenomena, and notwithstanding the difficulty of establishing their true causal relationship, and, consequently, of controlling them, the irresistible pressure of life forced medicine, even in olden times, to take charge of these phenomena, i.e., long before they began to be studied by natural science. And to a degree at least, medicine accomplished this task. At first sight the job seemed to be a huge one and hopelessly difficult, and yet it was partially accomplished. Among the countless possible solutions a few were seized upon as being truly fortunate. This incredible success was determined by two extremely vital conditions: in the first place, it was due to man's incessant and passionate striving for health and life, a striving which arose together with the first man, and, in the second place, to the participation of multitudes, of almost all mankind, in realizing this desire. But if the successes already achieved by medicine are astounding, who can doubt that they are still very insignificant compared with those which will be eventually attained. This, however, will take place not only because medicine utilizes, one might say, at every instance, and will continue in an ever-increasing measure, to use the general achievements of natural science for diagnostic and therapeutic purposes. Were medicine to remain purely practical, it is doubtful if it would attain complete triumph, since in most of its activity it would be doomed to apply only one instrument of natural science—observation; the other instrument—experimentation—is utilized by medicine with extreme caution and within relatively narrow bounds. But the method of observation is sufficient only for the study of the simpler phenomena The more complex the phenomenon (and what can be more complex than life?), the greater the need for experiment. Experiment alone crowns the efforts of medicine, experiment limited only by the natural range of the powers of the human mind. Observation discloses in the animal organism numerous phenomena existing side by side and interconnected now profoundly, now indirectly, or accidentally. Confronted with a multitude of different assumptions the mind must *guess* the real nature of this connection. Experiment, as it were, takes the phenomena in hand, sets in motion now one of them, now another, and thus, by means of artificial, simplified combinations, discovers the actual connection between the phenomena. To put it in another way, observation collects that which nature has to offer, whereas experiment takes from her that which it desires. And the power of biological experimentation is truly colossal. This experimentation has created in the course of some seventy or eighty years practically the entire modern, highly developed physiology of the organs of the complex animal. The ordinary educated man, even if he is not yet familiar with biology, upon acquainting himself with the usual, but somewhat more thoroughly arranged course of demonstrative physiology of animals, designed for medical students, would undoubtedly be extremely surprised at discovering the power which the present-day physiologist wields over the complex animal organism. And his surprise would be all the greater upon discovering that this power is the result not of millenniums or centuries, but only of decades.

Before our eyes this triumphant experimentation is steadily extending its power both to pathology and therapy. And it is difficult to imagine why this power of experimentation should not be equally effective in new branches of research. It seems to me that the remarkable success of modern medicine lies precisely in the fact that it is now in a position to develop all its principal branches by applying the method of experimentation. Bacteriology, in its turn, has given a tremendous impetus to this new tendency. Although pathology came to the laboratory somewhat earlier than bacteriology, the action of the still unknown, but highly pathogenic world of micro-organisms represented a very serious obstacle to experimental pathological investigation. We had in hand only the inanimate causes of the disease, such as mechanical force, heat, etc., but its living causes, the micro-organisms, escaped us. Only when the pathogenic organisms were discovered, did the domain of pathological physiology fully open up before the experimenter, and now nothing can prevent him from investigating almost the entire pathological world in laboratory conditions.

Although the clinic, as a result of its work of thousands of years, established with remarkable precision the forms of various diseases and gave almost the complete morphology of pathological states, although crude pathological anatomy, as well as the more recent microscopic and clinical investigations, collected and continue to collect rich material relating to the inner details of pathological processes, a full analysis and complete knowledge of the mechanism of such processes, of their entire development, can be obtained only by means of experimentation. In this respect the method of pathological anatomy alone is too crude, and the clinic alone, without experimentation, is powerless to penetrate deeply into the complexity of phenomena. Only laboratory experimentation makes it possible to discern in the general picture of a disease that which constitutes the protective mechanisms of the organism, and which compensates for the loss caused by the damage proper; only such experimentation can exactly reveal the inter-dependence of injuries, i.e., distinguish the initial injury from those subsequently evoked by it. Only this knowledge ensures expedient and effective aid to the diseased organism and precludes the possibility of any extraneous intervention which sometimes brings harm to the organism instead of relief. This—on the one hand. On the other, only experimentation will succeed in investigating and establishing all the real causes of pathological states, since it begins by isolating the cause which it deliberately induces to act. And it is precisely here that medicine is most vulnerable: it is a well-known fact that aetiology is medicine's weakest point. Indeed, do not the causes of the disease usually creep into the organism where they begin to act long before the patient becomes the subject of medical attention? Knowledge of these causes is, naturally, of extreme importance for medicine. In the first place, only when we know the cause, can we effectively struggle against it, and in the second place, what is still more important, prevent its action, its penetration into the organism. Only knowledge of the causes of diseases will turn the present-day medicine into the medicine of the future, i.e., hygiene in the broad sense of the word. In view of the obvious indisputability and significance of all this, one cannot but regret that pathology, being an exclusively experimental science—pathological physiology—has not yet generally taken its proper place, at times serving as an appendix to pathological anatomy and at others being completely lost in the programme of general pathology. The methods of pathological anatomy and those of experimental pathology differ to such a considerable degree that in practice, under university training conditions, they can hardly be applied by one and the same person and in one and the same premises on the principle of equality. It seems to me, on the other hand, that in the so-called general pathology special emphasis must be laid now precisely on experimental pathology, on the analysis of pathological processes by means of experimentation, and not on conclusions and abstractions drawn from facts of special pathology, which are often a mere enumeration, though in a new way, of these facts. One can hardly base serious scientific hopes on such a verbal treatment of the material of general pathology, especially at a time when such fascinating and fruitful experimental study of the world of pathological phenomena is taking place in the laboratory.

It is easy to imagine the embarrassing position of the physician when, in using a certain therapeutic method against one or another illness, against one or another symptom, he is often absolutely unaware of the effect that this method produces in the organism, and does not know in what way it helps in the given case. What inexactitude and uncertainty of action, what scope for fortuities! In these circumstances the striving of the clinicians to elucidate the mechanism of action of their therapeutic remedies is perfectly understandable; for decades experimentation has been rendering assistance to therapy and the therapeutic methods have been studied in the laboratory, where their action on healthy animals is subjected to analysis. This experimentation mainly concerns chemical medicines, hence the development of experimental pharmacology.

However, little by little the pharmacologist departed from his original purpose until he came to show little or no concern or interest in the therapeutic action of the substances handled by him. Pharmacology, naturally, developed into a department of physiology, studying the effect of chemical agents on the living body and pursuing purely theoretical aims. Actually, of course, there can be no objection to this. But thanks to this circumstance the connection between modern pharmacology and practical medicine, which might be described as the original mission of pharmacological experiment, a reminder of which is preserved to this day in the name—the study of medicines—is in many cases, at the present moment at least, weak and at times even purely scholastic. For example, in many text-books the exposition of the physiological action of one or another medicine is followed by an enumeration of the indications and contraindications of their therapeutic use, often completely unrelated to the previously described physiological action. This accounts for the complaints we sometimes get from physicians against modern pharmacology. Both experimenters and physicians would benefit by supplementing pharmacology with elements of experimental therapy. Having to do with the sick as well as the healthy animal, and administering drugs not only for the purpose of observing their effect in general, but also for the purpose of healing the sick animal, the pharmacologist, by way of analysis, will, for his own benefit, broaden and deepen his study of the reactions of the body to a given chemical compound, as well as his study of the body in general, and, for the sake of the physician, elucidate the actual value and real mechanism of action of the therapeutic agent. This necessity, at least so far as the study of the action of medicines on sick animals is concerned, was recognized and proclaimed long ago, the only obstacle to its realization being the difficulty of obtaining the necessary sick animals in laboratory conditions; at present this difficulty has been surmounted to a considerable degree thanks to the achievements of experimental pathology. Only if pharmacology is fused with experimental therapy, as mentioned above, will many therapeutical mirages be dispelled, as they rightly deserve to be; it will, on the other hand, preclude the sad possibility of many drugs being wrongly discarded for the sole reason that the pharmacological analysis of their effect on healthy animals has not yet touched the proper points of investigation or has failed to contact them altogether, since it was dealing exclusively with healthy animals. The programme of experimental therapy will, naturally, include the experimental analysis of various therapeutic means other than chemical agents, means which at present are wholly ignored in the vast course of medical academic education.

There is every reason to hope that we shall witness a tremendous growth of interest on the part of investigators when all the pathological processes, and not only the bacterial ones, are subjected in the laboratory to bold, unimpeded, and fully controlled treatment. We can be even more sure that outside bacteriology, no less success awaits the experimenter if he, in the field of therapy, assumes the role of initiator and not of interpreter, as has been the case so far. Some people hoped to bring pharmacology and medicine together by recommending the organization of, and by actually organizing, clinical sections in the pharmacological laboratories. But it seems to me that scientifically it is more logical and from the standpoint of practice more advantageous to create experimental therapeutic laboratories than special pharmacological clinics. Indeed, no matter what name you give the clinic, the patient cannot be subjected to a greater degree of experimentation in it than in any other. At the same time skill and system in the matter of treatment are obligatory for every clinical teacher, since precisely this distinguishes him from the ordinary practical physician. Thus, either the experimenter will be sacrificed to the clinician without any essential benefit, or, on the contrary, the clinician will be sacrificed to the experimenter, because a permanent and proportionate fusion of these two branches of activity is hardly within the bounds of practical realization.

I have come to the end of my paper. Only by passing through the fire of experiment will medicine as a whole become what it should be, namely, a conscious and, hence, always purposefully acting science. Modern surgery affords striking proof of this. What is responsible for its brilliant achievements? Its absolute consciousness of action. Proceeding from the plasticity of the organism and armed with antiseptics and aseptics against its chief enemy, the micro-organisms, surgery treats its subject from a purely mechanical point of view and strictly bases its methods on knowledge of the anatomical structure and physiological importance of this or that part of the body.

I am excited at the thought whether I have succeeded in convincing you of the extremely important role of experimentation in achieving the aims of practical medicine. But if you are convinced of this then your duty is to contribute in every way to the development of biological experimentation not only by possible personal participation, but also by actively assisting the experimenters in their work. The success of biological, that is, medical experimentation depends on adequate personnel, premises and means.

I draw your attention, gentlemen, to an essential difference between the clinicians and the experimenters. In the sphere of medicine scientists usually originate from the mass of the medical workers; every intelligent, energetic and talented practising physician can participate in the general science of medicine and may become a permanent and outstanding worker in this field. But the experimenters constitute an insignificant group of amateurs; their training and activity is almost impossible without special laboratory conditions. And this obliges you to render every possible help, both at college and beyond its walls, to the young champions of laboratory work who, by their laboratory specialization, take the risk of restricting, for the time being, their chances of success in life.

As is known, many clinicians, both therapeutists and surgeons, often have recourse to laboratory experimentation, now for the purpose of analysing a pathological process, now for elucidating the action of certain therapeutic agents, and for trying out a new surgical method, etc. This practice is, of course, commendable. At present the clinicians feel, even more so than the physiologists, the necessity of analyzing in laboratory conditions certain clinical pathological or therapeutic phenomena. That is why in numerous cases they now appear as initiators of experimental pathological and therapeutic investigations. This is their great merit and will remain so. However, for them this activity is not of primary importance; they devote to it only the leisure hours remaining after their principal therapeutic duty. Meanwhile, laboratory activity requires from any scientific worker full devotion, the sacrifice of all his strength. And I am of the opinion that our specialities (experimental pathology and experimental therapy) should be accorded the most favourable and absolutely independent status, since, broadly speaking, they are, in method and concept, nothing else but physiology. The course of medical sciences should, therefore, everywhere comprise three experimental chairs of physiology, namely, normal, pathological and therapeutic physiology.

XI

PHYSIOLOGY AND PSYCHIATRY

PSYCHIATRY AS AN AUXILIARY TO THE PHYSIOLOGY OF THE CEREBRAL HEMISPHERES[141]

My earlier researches on the circulation of the blood and on digestion led me to the firm conviction that the physiological mode of thinking may derive great help from the study of clinical cases, i.e., from the countless number of diverse pathological variations and combinations of the functions of the human organism. For this reason, during many years of work on the physiology of the cerebral hemispheres I often thought of making use of the world of psychiatric phenomena as an analytical auxiliary to this physiological study. Indeed, instead of applying our usual method which, as a mode of analysis, consists in destroying certain parts of the brain, and is very crude compared with the complexity and delicacy of the mechanism under investigation, one might expect in some cases to achieve a more distinct, precise and detailed decomposition of the work of the brain as a whole into its separate elements, to obtain a delimitation of its functions resulting from pathological causes, which sometimes reach a very high degree of differentiation.

In the summer of 1918 I had at last the opportunity to study a number of cases of insanity. And as it seems to me my former hopes have not been in vain. In some instances, I saw excellent demonstrations of points more or less explained in physiology; in others, new aspects of the work of the brain were brought to light, new questions and unusual problems for laboratory investigation arose.

My attitude towards the psychiatric material, however, differed greatly from the usual attitude of specialists. Due to a definite inclination of thought developed during years of laboratory practice, I always reasoned on a purely physiological basis and constantly explained to myself the psychical activity of the patients in definite physiological concepts and terms. This did not present any great difficulty for me, since my attention was concentrated not on the details of the subjective state, but on the principal features and phenomena of one or another state of the patient. How this was achieved will be partly seen from the following account.

In this article I shall describe and analyse the symptoms observed in two patients. One was an educated, well-bred girl, twenty-two or twenty-three years old. We find her lying motionless in bed in the hospital garden, her eyes half closed. At our approach she does not speak of her own accord. The physician accompanying me tells me that this is now her usual state. She refuses to eat without assistance and is untidy. She appears to understand our questions about her family, and remembers everything perfectly; she answers correctly, but with great effort and after considerable delay. The patient exhibits a strongly pronounced cataleptic state. She has been ill for years, at times almost fully recovering, at others relapsing and manifesting a considerable variety of symptoms; her present state is one of these relapses.

The second patient is a man aged sixty. He spent twenty-two years of his life in hospital, lying like a living corpse, without the slightest voluntary movement and absolutely speechless; he was fed artificially and was untidy. During the past few years, as he was approaching sixty, he began more and more often to make voluntary movements. At present he is able to get up and go to the lavatory; he talks volubly and quite reasonably, and sometimes eats without assistance. Recalling his former state, he said that he had been conscious of his surroundings, but had experienced such extreme and insuperable heaviness in his muscles that he could hardly breathe. And this was the reason why he could neither move, eat, nor speak. The disorder began to develop when he was thirty-five. Tonic reflexes[142] were recorded in the history of the case.

How should one assess the physiological state of the two patients?

In order to answer this question let us consider the strongly pronounced motor symptoms which are observed in both cases: the catalepsy of the first patient and the tonic reflexes of the second. When do these symptoms manifest themselves in animals? A long time ago Schiff[143] observed cataleptic phenomena in a rabbit deprived of the cerebral hemispheres. Decerebration[144] introduced by Sherrington is a simple method of obtaining distinct tonic reflexes in cats. Intoxication by certain narcotics, for example, urethan, also produces cataleptic phenomena. In all these cases there occurs an elimination of the activity of the cerebral hemispheres *without the suppression of the lower parts of the brain*; in the first two cases this is due to a specific property of the brain tissue of the given animals, as well as to the freshness of the operation, i.e., to the absence of subsequent reactive phenomena; in the case of intoxication by urethan it is due to the presence in the latter of an ammoniac grouping which produces a stimulating action on the lower motor centres. Such an isolated exclusion of the cerebral hemispheres, the nervous organ of the so-called voluntary movements, reveals the normal activity of the lower parts of the nervous motor apparatus. This activity is designed first of all for equilibrating the organism and its parts in space, which represents the equilibration reflex, always functioning under normal conditions and at the same time constantly disguised by voluntary movements. Thus, catalepsy is a normal and permanent reflex which manifests itself in a distinct and patent way only when the action of the cerebral hemispheres is excluded as in the above-mentioned case. As for the tonic reflexes, they are the elements of this complex reflex.

Consequently, the existence of the same mechanism must be assumed in our patients, i.e., exclusion of the activity of the cerebral hemispheres It is clear, however, that here only the activity of the motor region of the cerebral hemispheres is excluded, since our patients, unable to make any voluntary movements or suffering extreme impairment of this function, are able, at the same time, as can be seen or as they themselves acknowledge, to understand perfectly well what they are told; they also remember everything and are conscious of their state, i.e., the work of all the other parts of their cerebral hemispheres is quite satisfactory.

This strictly isolated inhibition of the motor region of the cerebral cortex is observed also in other cases, in other definite states inherent in man and animals. A subject in a certain degree of hypnosis understands your words quite well, remembers them and is willing to do something in connection with the conversation, but he has no power over his skeletal muscles and remains in the posture imparted to him, even though it is uncomfortable and he does not like it. Apparently, this phenomenon is essentially accounted for by a fully isolated inhibition of the motor region of the cerebral cortex, an inhibition which does not spread either over other parts of the hemispheres or to the lower levels of the brain mass. I have often observed a similar state in dogs when I worked in the laboratory with the so-called conditioned reflexes. Jointly with Dr. Voskresensky I studied these relations with particular precision and in a most systematic way on one of our dogs. For weeks and months this dog was often left alone in the room for a long time, strapped in the stand without being subjected to any experimental influence. As a result, the entire environment of the room became for the dog a hypnogenous agent, to the degree that the moment it entered the room, its behaviour immediately changed. Strictly measuring the influence of this agent by varying the duration of its action we could clearly see the separate phases in the development of the sleeping state. The following phenomena were observed. The so-called conditioned alimentary reflex for sound (association) was elaborated in the dog, i.e., at a definite sound the dog exhibited an alimentary reaction: it secreted saliva and made appropriate movements, licking its lips, turning in the direction from where the food was usually offered, and eating the food the moment it was offered. With the first signs of the sleeping state the conditioned salivary reflex to the sound disappeared, but the motor reflex to the sight of food remained quite normal, i.e., the dog began to eat without any delay. This first phase was followed by another one, which was quite unexpected and of considerable interest. Now the conditioned salivary reflex to the sound reappeared and became intensified with the addition of the natural conditioned stimuli proceeding from the food itself. But the motor reflex was absent—the dog did not take the food, even turned away from it and resisted its forcible introduction. In the following phase—the phase of profound sleep—all reactions to food, naturally, vanished. When the animal was deliberately awakened (by means of strong stimuli) the phases indicated above manifested themselves in reverse order as the sleeping state gave way. The second phase, naturally, could be interpreted as follows: the motor region of the cortex was already embraced by sleep inhibition while all other parts of the cerebral hemispheres still functioned quite satisfactorily and manifested their activity on an organ fully independent of the motor region—on the salivary gland. It is impossible not to see here a complete analogy with a person who is being awakened by you; he understands and even admits that you are rousing him at his own insistent request, but he cannot overcome the influence of sleep, begs you to leave him alone or he becomes angry and even aggressive when you persist in fulfilling his request and continue to disturb his sleep.

The first phase and its replacement by the second when the sleep becomes more profound, can be explained thus: since in our case the entire environment of the room, i.e., all the stimuli affecting the eyes, ears and nose, acts as a soporific agent, the parts of the cerebral hemispheres corresponding to these stimuli were the first to be subjected to sleep inhibition, the latter, though still superficial, was strong enough to suppress the conditioned action of the stimuli. At the same time the soporific influence was not yet sufficient to inhibit the more powerful part of the cortex—the motor region. But when monotonous cutaneous and motor stimuli (due to the limited movement in the stand) were added to the sleep-producing action of the room, sleep inhibition extended also to the motor region. And now this part of the cortex, being the strongest, attracted sleep inhibition from all other parts in accordance with the law of concentration of the nervous process; it thereby once more liberated them temporarily from this inhibition, until with the ever-increasing action of all soporific agents the sleep inhibition embraced all parts of the cerebral hemispheres with an equal and sufficient intensity.

And so we have sufficient grounds for granting the existence in the above-described patients of a concentrated and isolated inhibition of the motor region of the cerebral cortex as a result of the pathogenic cause.

What objections, from the clinical point of view, can be raised against our interpretation of the symptoms in our above-mentioned patients? I shall cite the arguments or the seeming inconsistencies with clinical cases which were pointed out by the psychiatrists when we informed them of the results of our analysis. Some of them were inclined to see in the cases cited by us a state of stupor evoked by strong emotions. But in the first place, this concerns the cause of the symptoms and not their mechanism. Evidently cases of stupor, i.e., of similar cataleptic states, may occur under the influence of strong, unusual stimulations caused by sounds of extraordinary intensity, by uncommon pictures, etc.; a very strong stimulation of certain parts of the hemispheres may lead to the inhibition of their motor region and thus create favourable conditions for the manifestation of the equilibrating reflex. In the second place, in the above-mentioned patients there are no indications of the existence of such a mechanism, and nothing reveals the presence of any extraordinary stimuli; one of the patients plainly points out the extreme difficulty, the impossibility of voluntary movements.

Further, it was stated that in progressive paralysis[145] a destruction of the cerebral hemispheres is proved even on a pathological anatomical basis, although catalepsy is absent. However, in this case there is no complete elimination of the motor activity of the cerebral hemispheres either. The patients are able to make many voluntary movements though they are badly co-ordinated; besides, they often exhibit phenomena of extreme motor excitability of the cortex in the form of convulsions. Consequently, here the main condition for the manifestation of a pure equilibrating reflex is absent.

Reference was made to cases of thrombosis[146] and haemorrhage in the cerebral hemispheres, which are accompanied by paralysis and not by catalepsy. But again this is not a condition which provokes catalepsy. In these cases one observes the absence of even spinal reflexes. It is clear that the inhibitory action of the destruction extends even to the spinal cord. And this inhibition must manifest itself all the more in the parts of the brain adjacent to the cerebral hemispheres.

Thus, the clinical cases of cerebral diseases do not reveal any actual inconsistencies with our analysis of the pathological state of the patients; therefore, in definite cases the mechanism of the pathological activity of the cerebral hemispheres suggested by us must be acknowledged as quite real. The fact that after more than twenty years of illness the patient described in our second case shows signs of returning to normal state, also leads us to interpret the general symptoms as an inhibition of the motor region of the cortex. This means that all the time his state was of a functional rather than of an organic, pathologico-anatomic nature.

Analysing further the state of our patients we must point to another essential circumstance. Although, according to present-day physiology, the cortical motor elements controlling different movements (skeletal, verbal, ocular, etc.), are localized in different parts of the cerebral hemispheres or, so to speak, scattered over them, nevertheless, in our patients they are all united by a common inhibition contrary to all other elements of the hemispheres which remain at the same time more or less free. This leads us to the important conclusion that all the motor elements possess common features in respect of their structure or chemical constitution, or, most probably, of both. Therefore, their relation to the cause originating the pathological symptoms is the same, and in this respect they differ from other cortical elements—visual, auditory, etc. This difference between certain elements of the cortex naturally manifests itself also in the above-mentioned phases of hypnosis and sleep when, influenced by one and the same cause, some elements are in one state and others in a different state.*

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* This difference between the cellular elements of the cerebral cortex must be regarded as being incontestable, especially since in the physiology of the peripheral nerves we constantly meet with a strongly pronounced individuality (excitability, relative strength, etc.) of the nerve fibres (and of their peripheral endings) of different functions. This individuality underlies the methods by means of which the differentiation of these different fibres of one and the same anatomic trunk can be effected. Let us recall, for example, the methods used in separating vasoconstrictor from vasodilator fibres. *(Note by I. P. Pavlov.)*

Let us now answer the following question: what cause actually determines the given symptoms? Different assumptions are, of course, possible There may be a definite toxic action, the sphere of influence of which is naturally limited by the individual peculiarities of the separate cortical elements that have just been mentioned. One can also assume exhaustion of the elements of the cortex resulting either from the general exhaustion of the organism, or only from overfatigue of the brain, from exhaustion concentrated in definite elements of the brain either because of the predominant part of these elements in the work producing the exhaustion, or again as a result of their specific nature. Finally, there is the possibility of direct or indirect (the last resulting from local changes in blood circulation or in general nutrition) reflex influences which may effect injuriously and also in an elective manner different elements of the cortex. Hence, in different cases, in spite of the similarity or even identity of the mechanism of the given complex of symptoms, the causes producing them may not be the same.

Finally, the following question is also of definite interest to us: what is the explanation for the case of our second patient, in whom the inhibition of the motor region of the cerebral cortex, having remained for twenty years almost at the same level of intensity, at last began drastically to diminish? This can be accounted for only by the patient's age. Approaching the age of sixty, when a sharp decline in the strength of the organism and the process of its aging usually becomes pronounced, he began to return to his normal state. How is this connection to be interpreted? If a certain toxic agent acted in this case, then, with the senile transformation of the body's chemism, there could take place a weakening, diminution of the agent producing this action. If the principal cause of the disease was chronic exhaustion of the nervous substance, then, with the senile transformation of the brain (lesser reactivity and lesser functional destructibility of the brain which is manifested in a sharp weakening of the memory for current events), this cause could now be less pronounced. Since sleep and hypnosis should be regarded as a kind of special inhibition, it may be admitted that our second patient presented an example of chronic partial sleep or hypnosis. With the advent of old age there is in evidence a relatively greater decline of the inhibitory processes, expressed in senile talkativeness, fantasticality, and in extreme cases, in dotage. In view of this, the recovery of the patient may be attributed to the senile decline of the inhibitory process.

It can hardly be disputed, I think, that the physiological analysis of the above cases raises before the physiology of the brain many new problems which can be investigated in the laboratory.

AN ATTEMPT OF A PHYSIOLOGIST TO DIGRESS INTO THE DOMAIN OF PSYCHIATRY[147]

In the course of the past thirty years I, together with my numerous colleagues, have been predominantly engaged in studying the activity of the higher parts of the brain, mainly the cerebral hemispheres; this study has been carried out on the basis of a strictly objective method, the method of the so-called conditioned reflexes. We have collected very considerable material relating not only to the normal activity of the above-mentioned parts of the brain, but to a certain degree also to their pathology and therapy. We are now in a position to produce obvious experimental neuroses in our experimental animals (dogs) and to treat them; and it is not impossible, in our opinion, to produce in the same animals states somewhat analogous to the human psychoses. It was this that induced me to make closer acquaintance with psychiatry, of which almost no traces have remained in my memory since my student days in the medical faculty. Thanks to the kindness of my medical colleagues, and especially of Prof. P. A. Ostankov and Dr. I. O. Narbutovich, I am now able systematically to observe different forms of mental disorders. Schizophrenia was the first disorder observed and studied by me. Here my attention was attracted, on the one hand, by the symptoms of apathy, torpor, inactivity, stereotype movements, and on the other hand, by playfulness, exaggerated familiarity, childish behaviour in general, which had not been peculiar to these patients before the onset of the disease (hebephrenia[148] and catatonia).

How can this be explained from the physiological point of view? Is it possible physiologically to generalize these phenomena and to find their common mechanism?

For this purpose it is necessary first of all to consider the facts obtained by the method of conditioned reflexes. This study has provided us with abundant data, particularly relating to the inhibitory process and its physiological and pathological significance.

Inhibition, which together with excitation constantly takes part in the diverse activity of the animal in its wakeful state, also guards the extremely reactive cells of the organism, the cells of the cerebral cortex; it protects them from highly strenuous work under the action of very strong stimuli, or even under the prolonged repetition of weak stimuli; it also ensures the necessary rest for the cells in the form of sleep after their daily normal work.

We have established the indubitable fact that sleep is inhibition, which irradiates over the hemispheres and descends along the brain to a certain level. Besides, we have been in a position to study on our animals also the intermediate phases between wakefulness and complete sleep—the hypnotic phases. These phases have been regarded by us, on the one hand, as different degrees of extensity of inhibition, i.e., of a larger or smaller extent of its irradiation over various areas of the hemispheres, as well as over various parts of the brain, and, on the other hand, as different degrees of intensity of inhibition in the form of different depth of inhibition in one and the same point. It is clear that owing to the tremendous complexity of the human brain, the diversity of separate hypnotic phenomena in man is much greater than in animals. It is possible, however, that some hypnotic phenomena are for one reason or another more manifest in animals than in man, especially since even the manifestations of human hypnosis vary considerably, depending on the peculiar features of the individual and the method of hypnotization. And so, taking into consideration the full complex of symptoms of hypnosis, I shall further deal with hypnotic phenomena observed both in man and in our animals.

Observing the above-mentioned schizophrenic symptoms I have come to the conclusion that they are an expression of a chronic hypnotic state, which I shall try to substantiate in my further exposition. Of course, apathy, dullness, inactivity, etc., are not in themselves proof of the hypnotic state of the patients, but at the same time they will not conflict in any way with this conclusion, provided my thesis is confirmed by a further comparison of more specific symptoms.

I shall first of all cite the following fact. Apathy and torpor are usually ascertained in a patient when he does not react to the questions addressed to him and gives the impression of being absolutely indifferent to them. However, if the same questions are asked not in a loud voice and not with the usual intensity, but in a low voice and in quiet surroundings, the patient reacts immediately with proper answers. This is a highly characteristic hypnotic phenomenon, to which, in my opinion, constant and proper attention is not being paid. And it is to be regretted that up to now the clinic, as far as I know, has no special term to designate this essential and important symptom as has been done with other symptoms. In our animals this symptom is one of the most frequent and persistent signs of the onset of hypnosis. In our experiments we constantly meet with the so-called paradoxical phase, when in the course of the given experiment or in one of its phases strong conditioned stimuli lose their usual action, while weak stimuli evoke in the animal a perfectly normal effect. In the well-known case of a five-year sleep, or properly speaking, hypnosis, described by Pierre Janet, the author made intellectual contact with his patient solely on the basis of this phenomenon. The patient herself emerged from the hypnotic state only at night, when all the daytime stimulations ceased.

Further phenomena of so-called negativism[149] were observed in the analysed patients. Similarly, in our experimental animals negativism is usually in evidence at the onset of a hypnotic state. In the case of an alimentary reflex when the conditioned stimulus is brought into action, and the food receptacle is placed before the dog, the latter persistently turns away from it. Not without interest is the following detail very clearly observed in a definite phase: when you begin to move the food receptacle away, the dog, on the contrary, reaches for it. And this is repeated several times in succession. But the moment the state of hypnosis is dissipated, the same dog devours the just rejected food. I shall analyse the mechanism of this, as well as other hypnotic symptoms, at another time; for the present I shall use them only as established facts constituting the hypnotic state.

Another symptom of schizophrenia in one of its variations is stereotypy—a persistent and prolonged repetition of definite movements. This, too, is an obvious hypnotic manifestation, and it is clearly observed in some of our dogs. When the dog is in a perfectly cheerful state, after being fed in the case of a conditioned alimentary reflex, it often continues for a certain time to lick the anterior part of its body, usually the breast and the forelegs. With the onset of a hypnotic state the licking assumes an extremely prolonged character and often lasts until the next meal. Certain other movements, effected by the animal at one time or other, are repeated with similar persistence.

Among usual phenomena observed in schizophrenics are the so-called echolalia[150] and echopraxia,[151] i.e., repetition by the patient of the words addressed to him by his interlocutor and the reproduction of gestures made by someone who attracts his attention. As is known, this phenomenon is also usual in hypnotized normal persons, and, it seems to me, manifests itself with particular ease and most frequently in hypnosis evoked by passes. Catalepsy is a very ordinary phenomenon in schizophrenics, consisting in prolonged retention by the patient of different postures, which are easily, i.e., without any resistance of the musculature, imparted to his body by another person; natturally this relates also to those postures which the patient himself assumes under the influence of certain temporarily acting stimuli. This, too, is a symptom very easily reproduced in normal persons subjected to hypnotism.

A particularly striking, pronounced and tenacious symptom in certain schizophrenics, constituting even a special form of the disease, is catatonia, i.e., a state of rigidity of the skeletal musculature strongly resisting any change in the given disposition of different parts of the body. Catatonia is simply tonic reflexes, as a result of which a hypnotized person can become as inflexible as a solid board.

Finally, it is necessary to include in this group of different variations of central inhibition the symptom of playfulness or silly mannerisms, mostly observed in hebephrenics, as well as the outbursts of aggressive excitation, met with in other schizophrenics in addition to the already mentioned symptoms. All these phenomena closely resemble the initial state of ordinary alcoholic intoxication, and the state peculiar to children and young animals, for example, puppies, when they are waking up, and especially when they are falling asleep. There is every reason to assume that these manifestations result from a developing general inhibition of the cerebral hemispheres; due to this the adjacent subcortex is not only liberated from constant control, from constant inhibition effected by the cerebral hemispheres in an alert state, but, because of the mechanism of positive induction, is even brought to a state of chaotic excitation affecting all its centres. That is why the state of alcoholic intoxication is accompanied now by a causeless and unusual playfulness and joviality, now by excessive sensibility and tearfulness, now by anger, and, in the case of children when they fall asleep, by capricious-ness. Particularly typical is a child in the middle of the first year of its life just going off to sleep. You can see on its face a truly caleidoscopic change of diverse expressions reflecting the chaotic activity of the child's primitive subcortex. Similarly the schizophrenic at definite stages and in definite variations of his disease exhibits this phenomenon now in a protracted form, now in the form of brief outbursts.

In view of what has been said, one can hardly doubt that schizophrenia, in certain of its variations and phases, is actually a chronic hypnosis. The fact that these variations and phases persist for years, cannot serve as a telling argument against this conclusion. Since there has been a case of a five-year sleep (described by Pierre Janet) and even of a twenty-year sleep (observed in Petersburg), why cannot hypnosis be of an equally lasting character, especially since the instances just mentioned must be regarded as states of hypnosis rather than sleep?

What is the reason for the chronic hypnosis of schizophrenics? What is its physiological, and especially pathological, basis? How does it develop and what are its consequences?

In the final analysis, of course, this hypnosis is profoundly based on the weak nervous system, and especially the weakness of the cortical cells. For this weakness various causes, both hereditary and acquired, may be responsible. We shall not touch here on these causes. But naturally, when such a nervous system encounters difficulties, more often in a critical physiological and social period of life, it inevitably becomes exhausted after excessive excitation. But exhaustion is one of the chief physiological impulses for the appearance of inhibition in the capacity of a protective process. Hence chronic hypnosis is inhibition in different degrees of extensity and intensity. Consequently, this state is, on the one hand, pathology, since it prevents the patient from normal activity, and, on the other hand, according to its mechanism, it is still physiology, a physiological remedy, since it protects the cortical cells from the danger of being destroyed as a result of too heavy work. In our laboratory we have now a striking example showing how prolonged inhibition restores normal activity for a time to weak cortical cells. There are reasons to assume that as long as the inhibitory process operates, the cortical cells are not gravely damaged, their full return to normal is still possible, they can recover from excessive exhaustion and their pathological process remains reversible. Using modern terminology, it is only a functional disease. That this is really the case, is proved by the following fact. According to Krepelin, a leading psychiatrist, of all the forms of schizophrenia the hebephrenic, and especially the catatonic form—which is of a particularly pronounced hypnotic character—show the highest rate of complete recovery (catatonics—up to 15 per cent), which is not observed in other forms, especially the paranoic one.

In conclusion I take the liberty of offering therapeutic advice more of a practical than sentimental character. Although enormous progress has been made since olden times up to our day in the treatment of the mentally ill, still, I think, something remains *to be desired* in this respect. To keep patients, already possessing a certain degree of self-consciousness, together with other, irresponsible patients, who may subject them, on the one hand, to strong stimulations in the form of screams and extraordinary scenes, and, on the other hand, to direct violence, in most cases, means creating conditions which to a still greater extent enfeeble the already weak cortical cells. Moreover, the violation of the patient's human rights, of which he is already conscious and which partly consists in restriction of his freedom, and partly in the fact that the attendants and medical personnel naturally and almost inevitably regard him as an irresponsible person, cannot but strike further heavy blows at the weak cells. Consequently, it is necessary as quickly and as timely as possible to place such mentally diseased in the position of patients suffering from other illnesses which do not offend human dignity so manifestly.

ESSAY ON THE PHYSIOLOGICAL CONCEPT OF THE SYMPTOMATOLOGY OF HYSTERIA[152]

To my dear comrade Alexei Vasilievich Martynov, in honour of his forty years of brilliant scientific, pedagogic and practical work

The grateful author,

Leningrad, April, 1932

The objective study of the higher nervous activity by the method of conditioned reflexes has made such progress and has been so widened and deepened that it no longer seems very risky to attempt a physiological interpretation and analysis of the complex, pathological picture presented by hysteria in all its manifestations, although hysteria is regarded by clinicians, fully or predominantly, as a mental disease, as a psychogenic reaction to the environment.

Thus, it is at the same time a test which enables one to judge to what degree the theory of conditioned reflexes is entitled to claim a physiological explanation of the so-called psychical phenomena.

Unfortunately, here again it is impossible to do without a physiological introduction, although a brief one. To this day conditioned reflexes are relatively little known even in the country of their origin; besides, the theory of conditioned reflexes is developing so rapidly that many of its important points have not yet been published and will be expounded by me here for the first time.

1

The conditioned reflexes continuously accumulated by human beings and animals in the course of their individual life are formed in the cerebral hemispheres, or, in general, in the higher part of the central nervous system. They represent a higher degree of complexity of ordinary unconditioned reflexes, i.e., reflexes which exist in the organization of the central nervous system from the day of birth.

The biological meaning of the conditioned reflexes consists in the fact that the few external stimuli of unconditioned reflexes, given a definite condition (coincidence in time), establish a temporary connection with the countless phenomena of the surrounding medium—signals of those stimuli. Because of this, all the organic activities representing the effects produced by the unconditioned reflexes, establish more delicate and more precise relations with the environment in wider and wider areas. The theory of conditioned reflexes or the physiology of the higher nervous activity studies the laws governing the dynamics of these reflexes both in normal and pathological life.

The activity of the cerebral hemispheres and of the entire central nervous system with its two processes—excitation and inhibition—is subordinated, in our view, to two fundamental laws: the law of irradiation and concentration of each of these processes, and the law of their reciprocal induction. Experiments carried out on the normal activity of the cortex enable us to draw the conclusion that if the intensity of these processes is weak, they at once begin to irradiate from the point of their origin; if their intensity is strong enough, they concentrate, and if it is excessively strong they irradiate again. When the processes concentrate they induce an opposite process both at the periphery during their action, and at the precise point of action upon its termination.

The irradiation of the excitatory process over the entire nervous system gives rise to a summation reflex. In spreading out, the wave of new excitation is summated with the already existing, manifest or latent, local excitation, revealing in the latter case the latent focus of excitation. In the cerebral hemispheres, which are of a more complex structure and possess extreme reactivity and impressionability, the irradiation of the excitatory process leads to the formation of a temporary conditioned connection, a conditioned reflex, association. While the summation reflex represents a momentary transient phenomenon, the conditioned reflex is a chronic phenomenon, gradually becoming stronger under the above-mentioned condition; it is a characteristic cortical process.

When the excitatory process concentrates in the entire central nervous system we meet with phenomena of inhibition—a manifestation of the law of induction. The point at which the excitation is concentrated is to a greater or lesser extent surrounded by an inhibitory process representing a phenomenon of negative induction. The latter makes itself felt both in the unconditioned and conditioned reflexes. The inhibition develops in full at once; it always arises and persists not only during the excitation by which it has been produced, but even for some time after it. The stronger the excitation and the lower the positive tonus of the surrounding brain mass the more profound, extensive and durable is its action. Negative induction acts both between the small points of the brain and its large parts. We call this inhibition external, passive, and, it can be added, unconditioned. Previously, this well-known phenomenon was termed the struggle of nervous centres, which emphasized the fact that at a particular time a physiological predominance, or, so to speak, priority of one nervous activity over the other takes place.

Along with the inhibition just mentioned the cerebral hemispheres exhibit other kinds or cases of inhibition, although there are grounds to assume that the physico-chemical process in all these cases is one and the same. This is, first of all, an inhibition which constantly corrects the conditioned connection and accordingly restrains the excitatory process, when the signalling conditioned stimulus is not accompanied, in some cases temporarily, by the signalized stimulus, or when it is accompanied by the latter with a considerable delay. This inhibition becoming highly fragmentary, also delimits, differentiates the conditioned positive agents from the countless analogous and related negative agents. It arises of itself in the conditions mentioned above, gradually grows and gains in intensity; it can train and perfect itself. This inhibition can also become connected with any indifferent external stimulus, if the action of the latter coincides for a certain time with the presence of inhibition in the cortex, this stimulus then begins of itself to produce an inhibitory process in the cortex. From what has been said it is clear that this purely cortical inhibition, along with the conditioned connection, plays an important role in the adaptation to the surrounding medium, constantly and expediently analysing the stimulations coming from there We have named this kind or case of inhibition internal, active inhibition. Generally speaking, the adjective "conditioned" could be accorded it as well. Then another, specific case of inhibition is observed in the cortex. All other conditions being equal, the effect of the conditioned stimulus is, as a rule, proportional to the intensity of the physical strength of the stimulus, but to a definite maximum level (and probably also to a certain minimum level). Beyond this maximum the effect no longer increases; it may sometimes even diminish. We then say that such a stimulus, on reaching this maximum level, begins to produce inhibition, not excitation. We interpret this phenomenon in the following way: the given cortical cell has a definite limit of functional capacity, i.e., of, so to speak, inoffensive, easily reversible functional wear, and the inhibition, which arises in connection with a super-powerful stimulation, does not permit overstepping this limit. The stronger the super-powerful stimuli, the more intense is the inhibition; in this case the effect of stimulation either remains at the maximum level, which is more often the case, or diminishes somewhat, if the stimulation is too intense. This inhibition could be called transmarginal.

The limit of functional capacity of the cortical cells is not of a constant nature; it may change abruptly, as well as in a chronic way. Inanition, hypnosis, disease and old age lead to a steady decline of this limit; at the same time in the surrounding environment more and more inhibitory stimuli appear which become super-powerful for the given cell. The following important fact must be also pointed out. When the excitability or lability of the cortical cells is augmented in a natural or artificial way, for example, by means of chemical substances, i.e., when a more rapid functional wear of the cortical cells is provoked, an ever-increasing number of stimuli which previously were below maximum or of maximum strength become super-powerful, leading to inhibition and a general decline of the conditioned reflex activity.

The following question remains unsolved: what is the relation between the two latter cases of inhibition and the first universal case of negative induction? If they are simply a modification of the first case, then what is the nature of the modification and how does it occur in relation to the peculiar properties of the cortex? It is probable that transmarginal inhibition is closer and more related to external, passive inhibition than to internal, active inhibition, since it, too, arises at once, and is not elaborated and trained as the latter.

These two kinds of cortical inhibition also move, spread over the brain mass. A very large number of diverse experiments were performed with the special object of studying the movement of the first kind of cortical inhibition—internal inhibition. In these experiments the inhibition spread out as if before the eyes of the experimenters.

There is no doubt that inhibition, while irradiating and deepening, develops different degrees of a hypnotic state, and that spreading from the cerebral hemispheres downward to the utmost over the brain, it produces normal sleep. The diversity and multiplicity of hypnotic stages, which at first can hardly be distinguished from the waking state, strikingly manifest themselves even in our dogs. In respect of the intensity of inhibition the so-called equalization, paradoxical and ultra-paradoxical phases are worth mentioning. Conditioned stimuli of different physical intensity, instead of producing effects in proportion to their intensity, as in the case of the waking state, now produce equal, or even inversely proportional and distorted effects. In rarer cases the distortion of the effects reaches such a degree that only the inhibitory conditioned stimuli produce a positive effect, while the positive stimuli assume an inhibitory action. In respect of the extensity of inhibition there are observed functional dissociations of the cortex, as well as of the rest of the brain, into larger and smaller parts. The motor area of the cortex is particularly often isolated from other areas, and even in this area sometimes a dissociation of functions comes to the fore.

It is a matter of sincere regret that up to the present time the impression produced by these laboratory experiments is weakened due to the rivalry of the so-called sleep centre suggested by clinicians and certain physiologists;[153] meanwhile, the matter can be interpreted in a satisfactory and conciliatory way from the following point of view, which seems to me fully justified by the facts. One can hardly doubt that there are two mechanisms responsible for the onset of sleep, and that it is necessary to distinguish active sleep from passive sleep. Active sleep originates in the cerebral hemispheres and is based on an active process of inhibition, arising in the hemispheres and spreading from there to the lower parts of the brain. Passive sleep results from the diminution or limitation of stimulating impulses reaching the higher parts of the brain (not only the cerebral hemispheres, but also the adjacent subcortex).

The stimulating impulses include, on the one hand, external stimuli, which reach the brain through the medium of the external receptors, and, on the other hand, internal stimuli, conditioned by the work of the internal organs and transmitted to the higher parts of the brain from the central nervous region regulating the organism's vegetative functions.

The first cases of passive sleep of a particularly pronounced character are Strümpel's well-known clinical case and the analogical, more recent experiment carried out by Prof. A. D. Speransky and V. S. Galkin when, after a peripheral destruction of three receptors—olfactory, auditory and visual—the dog falls into a profound and chronic state of sleep (lasting for weeks and months). The second cases of passive sleep are the clinical cases which lead to the recognition of what clinicians and some experimenters designate as the "centre of sleep."

The physiology of the muscular tissue offers us an example which in this respect is analogous to sleep. Owing to its specific physiological organization, the skeletal muscle only contracts under the influence of its motor nerve, but the relaxation of the muscle is of a passive nature; as to the smooth muscle, its contraction and relaxation are actively effected under the influence of two special nerves—one positive and the other inhibitory.

Just as in the case of concentration of the excitatory process, the concentration of the inhibitory process engenders, by virtue of the law of reciprocal induction, an opposite process, which in the given case is, naturally, a process of excitation. The point of concentration of the inhibition is surrounded, to a greater or lesser extent, by a process of heightened excitability—a manifestation of positive induction. The positive induction makes itself felt in the unconditioned, as well as in the conditioned reflexes. A heightened excitability arises either immediately or after a certain period during which the inhibition gradually concentrates; it persists not only for the duration of the inhibition, but for some time after its disappearance and in certain cases long after it. The positive induction manifests itself both between small points of the cortex and large parts of the brain.

I shall dwell now on some points of the physiology of the higher nervous activity which are of importance for physiological analysis of the symptomatology of hysteria.

The connection between the organism and the surrounding medium through conditioned signalling agents is the more perfect the more these agents are analysed and synthesized by the cerebral hemispheres in conformity with the extreme complexity and continuous fluctuations of the environment. The synthesis is effected through the process of conditioned connection. The analysis, the differentiation of positive conditioned agents from inhibitory ones is based on the process of reciprocal induction; the separation of different positive agents, i.e., of agents related to different unconditioned reflexes, is accomplished by a process of concentration (new experiments by Rickman). Thus, precise analysis requires a sufficient intensity both of the inhibitory and excitatory processes.

Further, of particular significance for the physiological study of hysteria are our data relating to the types of nervous system. First of all, we distinguish very strong animals, but unequilibrated, in which the inhibitory process always lags to a certain degree and, consequently, does not conform to the excitatory process. When these animals are confronted with difficult nervous tasks calling for considerable inhibition, they almost fully lose their inhibitory function (special neurosis) and become painfully restless; in some cases this restless state is periodically superseded by depression and drowsiness. In their general behaviour animals of this category are aggressive, provocative, and lacking in self-control. We call such dogs excitable or choleric. Next comes the type of strong and at the same time equilibrated animals, in which both processes are of equal strength; because of this, it is difficult and sometimes even impossible to induce neuroses in such animals by means of complex nervous tasks. This type assumes two forms—the quiet (phlegmatic) and the very lively (sanguine) forms. Finally, there is the weak inhibitable type, in which both processes are insufficient, particularly and more often the inhibitory process. It is this type which specially furnishes experimental neuroses, reproduced in them with extreme ease. Animals of this type are cowardly; they are constantly in a state of uneasiness or display excessive fussiness and impatience. They are incapable of enduring strong external agents acting as positive conditioned stimuli, any considerable normal excitation in general (alimentary, sexual, etc.), even a slight intenstiy (continuation) of the inhibitory process, and still less a collision of the nervous processes, any complex system of conditioned reflexes and finally any change in the stereotype of the conditioned reflex activity. In all these cases they exhibit a weakened and chaotic conditioned reflex activity and very often fall into different phases of hypnosis. Moreover, in these animals separate, even very small, points of the cerebral hemispheres can be easily rendered pathological, and when adequate stimuli affect such points a rapid and drastic decline of the general conditioned reflex activity takes place. Although the general behaviour of these animals is such that they cannot always be described as melancholic, nevertheless there is every reason to include them in the category of melancholic animals, i.e., those in which the vital manifestations are in many cases constantly suppressed and inhibited. In our exposition of the types of nervous system we implied, when we spoke of the equilibrium between excitation and inhibition, the so-called internal inhibition. In the weak type, with its weak internal inhibition, the external inhibition (negative induction) is, on the contrary, highly predominant and, above all, determines the entire external behaviour of the animal. Hence this type is called weak, inhibitable.

In concluding the physiological part of this article, I must point to the following circumstance which is of particular importance for the comprehension of some of the special symptoms of hysteria. There are sufficient grounds to assume that centripetal, afferent impulses produced by each element and moment of movement reach the cerebral cortex (motor region) not only from the skeletal motor apparatus, which makes possible an exact cortical regulation of the skeletal movements, but also from other organs and even separate tissues; because of this, the cortical regulation of the latter is likewise possible. At the present time, conditioning, which must be related to the higher part of the central nervous system, assumes greater biological significance since the possibility of conditioned leucocytoses, immunity and other various organic processes, has been demonstrated, even though we do not yet know the exact nervous connections participating, directly or indirectly, in this phenomenon. But this possibility of cortical influence is deliberately utilized and is revealed by us in very rare cases under exceptional artificial or abnormal conditions. This is explained by the fact that, on the one hand, the autoregulation of the activity of other organs and tissues, apart from the skeletal motor apparatus, is chiefly effected in the lower parts of the central nervous system, and, on the other hand, is disguised by the fundamental activity of the cerebral hemispheres aimed at regulating the most complex relations with the surrounding medium.

2

Let us turn now to hysteria.

Concerning the general concepts of hysteria held by the clinicians, some of them give a fundamental general characteristic of the pathological state and some bring forward certain particularly pronounced traits or symptoms of this state. Some clinicians speak, as it were, of a return to instinctive, i.e., emotional and even reflex life; others attribute the disorder to suggestibility, explaining the entire behaviour of hysterical persons and the so-called stigmata of hysteria (analgesia, paralyses, etc.) by suggestion and autosuggestion. Certain clinicians advance to the foreground the desire to be ill, to take refuge in illness; others regard as particularly important the manifestation of fantasticism, the absence of a real perception of life; still others look on the disease as chronic hypnosis, and finally there are clinicians who ascribe it to a reduced capacity for psychical synthesis or to split personality. I believe that all these concepts taken together fully cover the entire syndrome and the entire nature of hysteria.

First of all, we must consider as a generally recognized fact that hysteria results from a weak nervous system. Pierre Janet plainly states that hysteria is one of an immense group of mental illnesses caused by weakness and cerebral inanition.[154] If that is so, then the above characteristic—taking into account that the weakness mainly relates to the higher part of the central nervous system and especially to the cerebral hemispheres as its most reactive part—becomes comprehensible in the light of the physiology of the central nervous system and of its higher part as now presented by the theory of conditioned reflexes.

Usually the cerebral hemispheres which represent the highest organ of correlations between the organism and the surrounding medium and hence the constant controller of the executive functions of the organism, always exert influence on the adjacent parts of the brain with their instinct and reflex activity. From this it follows that the elimination or weakening of the activity of the cerebral hemispheres must necessarily lead to a more or less chaotic activity of the subcortex devoid of the right measure and of adequacy to the given surroundings. This is a well-known physiological fact which manifests itself in animals after the extirpation of the cerebral hemispheres, in adults when they are in different states of narcotization and in children when they fall asleep. Thus, using the above-mentioned physiological terms, the alert, active state of the cerebral hemispheres, manifested in the unceasing analysis and synthesis of external stimuli, of the influences of the surrounding medium, negatively induces the subcortex, i.e., inhibits its activity as a whole, liberating in a selective way only the activity needed by conditions of place and time. On the contrary, an inhibited state of the hemispheres liberates or positively induces the subcortex, i e., strengthens its general activity. Consequently, there are adequate physiological grounds for the occurrence of various affective outbursts and convulsive fits in hysterical persons under acute and abrupt inhibition of the cortex resulting from unendurable stimulations—and such stimulations are not infrequent in the case of a weak cortex. These outbursts and fits are sometimes expressed in more or less definite instinctive and reflex activities and sometimes in absolutely chaotic forms, depending on the varying localization of inhibition over the cortex and the adjacent or more distant subcortex.

But this is an extreme and active expression of the pathological state. When the inhibition spreads further down the brain, we witness another extreme, but passive state of the organism of the hysterical person in the form of profound hypnosis and, in the end, of complete sleep lasting for hours and even for days (lethargy). This difference between the extreme states is probably determined not only by various degrees of weakness of the excitatory and inhibitory processes in the cortex, but also by the force correlations between the cortex and subcortex, which sometimes vary in an acute or chronic way in one and the same individual, and sometimes are also related to different individuals.

This varying chronic weakness of the cortex, apart from being the cause of the extraordinary and extreme states of the organism just described, invariably conditions also the permanent peculiar state of hysterical persons—their emotivity.

Although our life and that of animals is directed by the basic tendencies of the organism—alimentary, sexual, aggressive, investigatory, etc. (functions performed by the subcortex adjacent to the cerebral hemispheres), nevertheless, for the purpose of co-ordinating and realizing all these tendencies, indispensably in connection with the general conditions of life, there is a special part of the central nervous system; this part moderates each particular tendency, harmonizes them and ensures their most rational realization in the conditions of the surrounding medium. These are, of course, the cerebral hemispheres. Thus, there are two ways of action. In the first place it is the way of rational action which is effected after, so to speak, a preliminary (though sometimes almost instantaneous) investigation of the given tendency by the cerebral hemispheres and its transformation, in the requisite measure and at the appropriate moment, into a corresponding motor act or behaviour with the help of the cortical motor region. It is, in the second place, the way of affective, passionate action, realized (perhaps even directly through the subcortical connections) under the influence of the given tendency alone, without the above-mentioned preliminary control. In hysterical persons the latter way of action predominates in most cases, and its nervous mechanism is quite clear. The tendency arises under the influence of external or internal stimulation and evokes the activity of a corresponding point or region of the cerebral hemispheres. Under the influence of emotion and due to the irradiation from the subcortex, this point becomes extremely charged. If the cortex is weak, this is sufficient to provoke a strong and greatly extended negative induction, which excludes any control, any influence of all other parts of the cerebral hemispheres. And it is precisely these parts which locate the representation of other tendencies and of the surrounding medium, the traces of previous stimulations and emotions, the acquired experience. This is joined by another mechanism. The strong excitation produced by emotion intensifies the excitability of the cortex; this rapidly leads the excitation of the cortex to the limit of its functional capacity and exceeds it. Consequently, negative induction is joined by transmarginal inhibition. Hence, a hysterical person lives to a greater or lesser degree not a rational but an emotive life, and is directed not by the cortical, but subcortical activity.

Suggestibility and auto-suggestibility are directly connected with this mechanism of hysteria. What are suggestion and auto-suggestion? They are a concentrated excitation of a definite point or region of the cerebral hemispheres in the form of a definite excitation, sensation or its trace—an idea now called forth by emotions, i.e., excited from the subcortex, now produced abruptly from the outside, now by means of internal connections, associations—an excitation which acquires a predominant, undue and irresistible significance. It exists and acts, i.e., passes over into movement, into one or another motor act, not because it is maintained by various associations, that is, connections with many present and past stimuli, sensations and ideas—this would produce resolute and sensible action, such as is usual with a normal strong cortex—but because in a weak cortex with a low, weak tone this concentrated excitation is accompanied by a strong negative induction which detaches and isolates it from all indispensable extraneous influences. This is the mechanism of hypnotic and post-hypnotic suggestion. During hypnosis we observe even in a normal and strong cortex a lowered positive tone owing to irradiated inhibition.* When the word or command of the hypnotist is directed to a definite point of such a cortex as a stimulus, the latter concentrates the excitatory process in a corresponding point and is immediately followed by negative induction, which, meeting little resistance on its way, spreads over the entire cortex; thanks to this, the word or command is completely isolated from all influences and becomes an absolute, irresistible stimulus, continuing to operate even subsequently, when the individual returns to an alert state.

Exactly the same as to its mechanism, but in lesser degree, takes place constantly and spontaneously in old age when the excitatory process in the cortex undergoes a natural decline. In a still strong brain the internal or external excitation, concentrating, even though considerably (but not excessively, as in exceptional cases), in a definite point or region of the cortex, is naturally accompanied by negative induction, which, because of the strength of the cortex, does not represent complete and widely spread inhibition. Therefore, along with the predominant excitation, some other concomitant excitations act, which evoke corresponding reflexes, especially old and fixed ones, or the so-called automatic reflexes. Usually our behaviour consists not of isolated, but complex reactions, corresponding to the constant complexity of the surroundings. The picture is altogether different in old age. When concentrating on a certain excitation, we exclude by means of negative induction the action of all other extraneous but simultaneous stimulations, and that is why

* Despite the very rich material accumulated by the physiology of the nervous system in general, and by the theory of conditioned reflexes in particular, the question of the relation between excitation and inhibition still baffles solution. Is it one and the same process interchanging under definite conditions, or a couple, strongly knit together, which, as it were, revolves under certain conditions and shows, to a greater or lesser extent, or even in full, now one of its sides, now the other? *(Note by I. P. Pavlov.)*

we often act not in compliance with the given conditions, i.e., our reaction to the entire surroundings remains incomplete. Here is a simple illustration. I look at the object which I need, I take it in my hand but at the same time do not notice all or practically all other objects surrounding and adjoining it; as a result I knock against the other objects, derange them without any need, etc. This is described, erroneously, as senile distraction, whereas, on the contrary, it is concentration, but involuntary, passive and defective. Precisely for the same reason an old man, who thinks of something or talks to somebody while putting on his outdoor clothes, sometimes forgets to put on his hat, takes one object instead of another, etc

As a result of constant extraneous and involuntary suggestions and auto-suggestions, the life of a hysterical person is overcharged with extraordinary and peculiar manifestations.

To begin with, let us take the case of war hysteria which was thoroughly studied during the world war. Being a permanent and serious menace of death, war, of course, is one of the most natural incentives to fear. Fear has definite physiological symptoms which in individuals with a strong nervous system either do not manifest themselves at all, being suppressed, or quickly disappear; in persons with a weak nervous system they are of a more prolonged character, with the result that such persons are no longer able to participate in military operations and thus are discharged from the obligation of exposing their life to danger. These persistent symptoms could also disappear of themselves with the lapse of time, but in a weak nervous system, precisely because of its weakness, a mechanism sets in which maintains them. The persistent symptoms of fear and the resulting temporary safety thus coincide in time and, by virtue of the law of conditioned reflexes, must become associated, interconnected. Hence, the sensation and representation of these symptoms assume a positive emotional shade and, naturally, are repeatedly reproduced. Then, on the one hand, according to the law of irradiation and summation, they, acting from the cortex, maintain and reinforce the lower centres of the reflex symptoms of fear, and, on the other hand, being emotionally charged they are accompanied, in a weak cortex, by an intense negative induction, and thus exclude the influence of other representations which could oppose the feeling of conditioned agreeableness or desirability of these symptoms. We, therefore, have not sufficient grounds to affirm that this case represents a deliberate simulation of symptoms. In effect, it is a case of fatal physiological relations.

But a hysterical person displays a multitude of similar cases even in his everyday life. Not only the horrors of war, but many other dangers (fire, railway accidents, etc.), numerous life shocks, such as the loss of close relations or friends, unfaithful love and other deceptions encountered, the loss of property, collapse of convictions and beliefs, etc., and in general difficult conditions of life—unhappy marriage, poverty, violation of self-respect, and so forth—all these factors produce in a weak individual, at once or eventually, violent reactions accompanied by different abnormal somatic symptoms. Many of these symptoms, which appear at the moment of strong excitation, are impressed in the cortex for a long time or forever, just as are many strong stimulations in normal persons (kinesthetic stimulations included). But other symptoms, which in a normal subject can be effaced with the lapse of time—whether because of fear of their abnormality, inconvenience, direct harmfulness and merely indecency, or, the reverse, because they are advantageous or simply interesting—become more and more intense, extended (through irradiation) and stable, owing to the same mechanism as in the case of the war hysteria mentioned earlier, as well as to their emotional reinforcement. Naturally, in a weak subject, an invalid in life, unable to win by positive qualities respect, attention and favour of other people, the latter motive acts most and contributes to the prolongation and fixation of the morbid symptoms. Hence, one of the most striking features of hysteria is the desire to be ill, to take refuge in illness.

Along with positive symptoms, there are negative ones, which are produced in the central nervous system not by the process of excitation, but by the inhibition process, for instance, analgesia and paralysis. They attract special attention, and some clinicians (for example, Hoche[155] in a recent article) regard them as specifically hysterical symptoms which seem absolutely incomprehensible. But this is an obvious misunderstanding: they do not differ in any way from positive symptoms. Do not we, normal people, constantly repress some of our movements and words, i.e., do not we send inhibitory impulses to definite points of the cerebral hemispheres? As pointed out in our physiological introduction, in the laboratory we constantly elaborate, along with conditioned positive stimuli, conditioned negative stimuli. In hypnosis by means of stimulating words we produce anaesthesia,[156] analgesia, general immobility or inability to move certain parts of the body, functional paralysis. A hysterical person often can and must be regarded, even in normal conditions, as being in a chronic state of hypnosis to a certain degree, since owing to the weakness of his cortex, ordinary stimuli become superpowerful and are accompanied by a diffused transmarginal inhibition, just as in the paradoxical phase of hypnosis observed in our animals. Therefore, besides the fixed inhibitory symptoms, which, like the positive ones, appear at the moment of violent nervous trauma, the same inhibitory symptoms may arise in a hysterical hypnotic as a result of suggestion or auto-suggestion. Any notion of an inhibitory effect evoked either by fear, interest or advantage, repeatedly concentrates and intensifies in the cortex and, owing to the emotivity of the hysterical person, just as in hypnosis the word of the hypnotist, provokes these symptoms and fixes them for a long time, until, finally, a stronger wave of excitation effaces these inhibitory points.

The same mechanism of auto-suggestion produces in a hysterical person a multitude of other symptoms, some of which are rather ordinary and frequent and some extraordinary and highly peculiar.

Any slight sensation of pain or the slightest anomaly in any organic function engenders in a hysterical person the fear of becoming seriously ill; and this suffices not only to maintain these sensations, again by means of the above-described mechanism, but to reinforce them and bring to such a pitch of intensity as to render the subject invalid. However, this time it is not the positive aspect of the sensation that is responsible for its frequent reproduction and predominant action in the cortex, as is the case in war hysteria, but, on the contrary, its negative aspect. This, naturally, makes no difference as regards the essence of the physiological process. Unquestioned cases of imaginary pregnancy accompanied by corresponding changes in the mammary glands, by an accumulation of fat in the abdominal wall, etc., are examples of peculiar manifestations of hysterical auto-suggestion. This is further confirmation of what has been said in the physiological introduction to this article concerning the cortical representation not only of the activity of all organs, but of separate tissues. At the same time this testifies to the extreme emotivity of hysterical persons. It is true that in this case the maternal instinct, powerful in itself, reproduces by auto-suggestion such a complex and specific state of the organism as pregnancy, at least certain of its components. The same mechanism is responsible for the states and stigmates of religious ecstatics. It is a historical fact that the Christian martyrs endured their tortures with patience, even with joy, and when dying, lauded those for whom they sacrificed themselves; this is striking proof of the power of auto-suggestion, i.e., of the strength of concentrated excitation in a definite cortical region, excitation accompanied by a very intense inhibition of all other parts of the cortex representing, so to speak, the fundamental interests of the entire organism, its integrity, its existence. If the power of suggestion and auto-suggestion is so great that even the destruction of the organism can take place without the slightest physiological resistance on its part, then, in view of the already proved high ability of the cortex to influence the processes of the organism, it is easy to understand from the physiological point of view the partial violation of the organism's integrity produced by suggestion and auto-suggestion by means of trophic innervation, the existence of which has been also proved.

It is, therefore, impossible not to see the erroneousness of the extreme point of view put forward by Babinsky,[157] although in general he correctly appraises the fundamental mechanism of hysteria. In his view the only symptom that should be regarded as hysterical, is the one provoked or eliminated by suggestion. This conclusion overlooks the extreme intensity and incessant action of the given emotivity, which cannot be produced in a full measure deliberately by suggestion, especially since the real cause and nature of this emotivity may remain unrevealed.

Finally, it is necessary to touch on the fantasticism of hysterical persons, on their detachment from reality and frequent twilight states. It can be assumed that these symptoms are interconnected. As shown by the observations made by Bernheim and others on hypnotized normal subjects, as well as by our observations on dogs mentioned in the physiological part of this article, we must distinguish in hypnotism a number of gradations, beginning with a state, which hardly differs from wakefulness and ending with complete sleep.

In order to embrace and fully understand all the degrees of hypnosis, especially in man, I think it is necessary to dwell on the following problems, which have not only been insufficiently elaborated by science, but are not even properly formulated.

Life clearly reveals two groups of human beings: artists and thinkers. There is a striking difference between them. The first group, artists of all kinds—writers, musicians, painters, etc., perceive reality as a single whole, i.e., the entire living reality without breaking it up or decomposing it. The other group, the thinkers, on the contrary, dismember it, thereby, as it were, killing it and making of it a kind of temporary skeleton; only afterwards do they gradually as if anew assemble its parts and try to revive it, but this, however, they are unable fully to accomplish. This difference is particularly manifest in the so-called eudetism of children.[158] I recall a case which greatly amazed me forty or fifty years ago. In a family of a marked artistic disposition the parents used to entertain their two or three years old child (and amuse themselves at the same time) by showing him a collection of twenty or thirty photos of different relatives, writers, actors, etc.—and simultaneously pronouncing their names. The effect was that the child memorized the photos and then called all the persons represented on them by their proper names. But how great was the general surprise one day when it was discovered that the child could give the right names by looking even at the back of the photo. Apparently in this case the brain, the cerebral hemispheres, perceived the optic stimulations in exactly the same way as a photographic plate reacts to the fluctuations of the intensity of light or as a phonographic disc records the sounds. And this, perhaps, is the essential feature of any kind of artistic faculty. Generally, such an integral reproduction of reality is inaccessible to a thinker. That is why the combination in one and the same person of great artist and great thinker is an exceedingly rare phenomenon. In the overwhelming majority of cases they are represented by different individuals. Of course, in the mass there are intermediates.

I believe that there are definite physiological grounds, although as yet not very convincing, for interpreting the matter in the following way. In the artist the activity of the cerebral hemispheres, while developing throughout their entire mass, least of all involves the frontal lobes and concentrates mainly in other parts; in the thinker, on the contrary, it is most intense in the frontal lobes.

Repeating what I have just said, for the sake of systematization, I view the higher nervous activity as a whole like this. In higher animals, including man, the first system establishing complex correlations between the organism and the external environment is represented by the subcortex adjacent to the cerebral hemispheres with its highly complex unconditioned reflexes (in our terminology), or instincts, drives, affects, emotions (in the usual diverse terminology). These reflexes are produced by a relatively limited number of unconditioned external agents, or in other words, those which act right from the day of birth. Hence, a limited capacity of orientation in relation to the surrounding world and at the same time a low degree of adaptation. The second system is represented by the cerebral hemispheres, excluding, however, the frontal lobes. It is here that a new principle of activity arises with the help of conditioned connection or association—the signalization of a limited number of unconditioned external agents by a countless number of other agents, which at the same time are constantly subjected to analysis and synthesis and ensure a very wide orientation in relation to the same medium and thereby a much higher degree of adaptation. This is the only signalling system in the animal organism and the first signalling system in man. In the latter another system of signalization is added; it can be assumed that this system relates to the frontal lobes, which in animals are much less developed than in man. It represents a signalization of the first signalling system by means of speech and of its basis or basal component—kinesthetic stimulations of the speech organs. In this way a new principle of nervous activity arises—abstraction and at the same time generalization of the countless signals of the first signalling system which is again accompanied by analysis and synthesis of the new generalized signals —a principle which ensures unrestricted orientation in relation to the surrounding world and ensures the highest degree of adaptation, namely, science, both in the form of human universal empiricism and in specialized forms. This second system of signalization and its organ, representing the latest acquisition in the process of evolution, are bound to be most fragile and susceptible to diffused inhibition when it arises in the cerebral hemispheres at the initial stages of hypnosis. Then, instead of the activity of the second signalling system, usually predominant in the alert state, the activity of the first system comes to the fore, liberated from the regulating influence of the second system; at first it takes the more stable form of reverie and fantastic imagination and subsequently the more acute form of a twilight state or light sleep (corresponding to the intermediate state between sleep and wakefulness or to the state of falling asleep). Hence the chaotic character of this activity, which no longer reckons with reality, or if it does, then only slightly, and is mainly dependent on the emotional influences of the subcortex.

From what has been said it will not be difficult to appreciate from the physiological point of view what the clinicians term disturbance of psychical synthesis in hysteria (the expression used by Pierre Janet) or the split "ego" (Raymond's expression). Instead of a co-ordinated and well-equilibrated activity of the three systems mentioned, in hysteria this activity is continually dissociated, and the natural and law-governed interdependence of the systems is deranged; meanwhile the interconnection and proper interdependence of the work of these systems constitute the foundation of a sane personality and underlie the integrity of our "ego."

In the final analysis, different combinations of the following three particular physiological phenomena are constantly manifest and make themselves felt against the fundamental background of cortical weakness in hysterical persons: quick susceptibility to varying degrees of hypnotic states due to the fact that even normal life stimuli are superpowerful and are accompanied by transmarginal diffused inhibition (the paradoxical phase); extreme fixation and concentration of the nervous processes in definite points of the cortex due to the predominance of the subcortex; and, finally, undue intensity and extensity of negative induction, i.e., of inhibition caused by low resistibility of the positive tone of other cortical parts.

In conclusion, I take the liberty of saying a few words about hysterical psychoses. A case of this kind of psychosis has been demonstrated to me; it is a case of hysterical puerilism[159] in a woman of more than forty, who became ill as a result of severe shocks experienced in family life. She was unexpectedly deserted by her husband who some time later also deprived her of her child. After an attack of stupor and a general prolonged paresis[160] the woman sank into dotage. At present she behaves like a child, without, however, manifesting any obvious general defects in the intellectual and moral sphere or in personal life. A closer examination of the patient shows that everything seems to be accounted for exclusively by the absence of the analytical inhibition which always accompanies our behaviour, our movements, words and thoughts and which distinguishes the adult from the child. Does not the development of our personality consist in the fact that under the influence of education and religious, social and civic requirements, we gradually learn to inhibit, to repress that which is not admitted, which is prohibited by the factors just mentioned? Is not our behaviour in the family circle or in the company of friends quite different in all respects from that under other conditions? The universal experiments of life prove this beyond all doubt. Do we not constantly encounter the fact that in fits of passion, which overcome the cortical inhibition, men speak and act in a manner which they regard as inadmissible when they are calm? And do they not bitterly regret such behaviour when the fit of passion recedes? This is particularly evident in the state of alcohol intoxication when all brakes are abruptly switched off, as aptly expressed in the Russian proverb: to the drunkard the sea seems up to his knee.

Will this patient ever return to a normal state? Well, it depends. The psychiatrists affirm that in youth such a state persists only for hours or days, although it is sometimes more protracted. In the given case it is a state of relative calm and satisfaction; it is probably determined by the previously described nervous mechanism, which makes the patient take refuge in illness in order to escape the difficulties of life and owing to which this pathological state may in the end become irremediably habitual. On the other hand, the disturbed and overstrained inhibition may weaken and disappear altogether.

Is hysteria in general a curable disease from the physiological point of view? In this respect everything depends on the type of nervous system. It is true that the predominant and encouraging impression produced by our work on conditioned reflexes in dogs is that the cerebral hemispheres offer great possibilities for their training, although naturally these possibilities are not unlimited. When dealing with an extremely weak type we can, in exceptional, so to speak, hot-house experimental conditions, obtain an improvement, a regularization of the animal's general conditioned reflex activity, and nothing more. A durable transformation of the type is, of course, out of the question. But since certain hysterical reactions of a general physiological character can also take place in more or less strong types as a result of powerful stimulations or violent shocks, a full return to the normal is, of course, possible in this case. However, the return can occur only if the series of shocks and excessive stimulations do not overstep their limits.

While it is impossible to read without keen interest the really brilliant pamphlet by Kretschmer on hysteria, in which the author reveals a strong and almost constant tendency to interpret the hysterical symptoms physiologically, Hoche's article in *Deutsche Medizinische Wochenschrift* in its January issue this year, makes a strange impression. Is it really the case that modern physiological knowledge does not throw any light on the mechanism of hysteria, that the clinic and physiology "have halted before hysteria as they would at closed doors?" The following reasoning in Hoche's article seems quite strange. Adhering to the view that analgesiae and paralyses constitute the fundamental feature of hysteria, he addresses the supporters of the theory of the pathogenic force of motives in hysteria with the question: why would the strong indignation felt by some of his listeners and readers in consequence of his adverse opinion of the above-mentioned theory, not render them insensitive to pain, if it were caused by a faradic current of high intensity? Then he cites other analogous cases: for example, why are the patients not cured by a similar method, i.e., by a strong desire to get rid of their illness, of their neuralgiae? In this connection I recall an instance from my student days which deeply impressed me and all who witnessed it. A young woman was undergoing a plastic operation on her nose which had been dreadfully deformed by some disease. Right in the middle of the operation the woman, to everyone's surprise, suddenly made a calm remark in response to something said by the professor performing the operation. Evidently, the anaesthesia (which was general) had practically no effect. Yet the same woman attracted general attention by the fact that during the daily dressing of the post-operative wound she exhibited extreme sensitivity to pain. Clearly the strong desire to get rid of the deformity, probably intensified by sexual emotion, rendered the woman insensitive to the operation trauma and made her hope and believe that the surgical intervention would end in complete success. But after the operation, at any rate for a period immediately after it, when the coarse, strange-looking artificial nose bitterly and cruelly disappointed her, the same emotion, on the contrary, rendered her highly sensitive even to what was now carefully done to her nose.

Many cases of this kind are met in everyday life, as well as in history. When dealing with such cases, it is necessary to take into account: in strong and normal individuals the harmonious complex of strong emotions and of predominant cortical associations accompanied by an equally strong negative induction in all other parts of the cerebral hemispheres; in the weak nervous type—the hysterical mechanism described above.

FEELINGS OF POSSESSION (LES SENTIMENTS D'EMPRISE) AND THE ULTRA-PARADOXICAL PHASE[161]

(OPEN LETTER TO PROF PIERRE JANET)

Would you deem it interesting to print this letter in your journal and at the same time express your views on the points made by me after careful study of the article published by you last year: "Emotions of the Persecution Delusion"?

I am a physiologist and of late, together with my colleagues, have devoted myself exclusively to study of the physiological and pathological work of the higher part of the central nervous system in higher animals (dogs), which corresponds to our higher nervous activity, usually called psychical activity. You are a neurologist, psychiatrist and psychologist. It seems that we should give proper consideration to our reciprocal work and co-operate in our research, for, after all, we are investigating the activity of one and the same organ (concerning which there can hardly be any doubt now).

The third part of your article attempts to interpret the feelings of possession. The basic phenomenon is that the patients objectivize their weakness, their imperfections, and attribute them to others. They want to be independent, but they are adamant in believing that other people regard them as slaves who are obliged to execute orders. They want to be respected, but it seems to them that they are being insulted. They want to have their own secrets, but it appears to them that their secrets are constantly being disclosed. Like everybody else, they have their own intimate thoughts, but in their imagination these thoughts are being stolen from them. They have annoying habits or painful fits, but they ascribe them to other people.

You interpret this phenomenon in the following way. Many of the ordinary circumstances of life are very difficult, unbearable and painful for these patients. For instance, the presence at the dinner table of two ladies of the patient's acquaintance, towards whom she had never been ill-disposed before. This constant difficulty and the natural frequent failures fill the patients with anxiety and fear, and inspire in them the desire to get away from it all. Like children or savages, they attribute all their troubles to the malignant actions of others, and this signifies deliberate objectification. Further, you devote attention to the following detail: in all the cases cited by you, we have to do, in your terminology, with binary social acts: to be master or slave, give or steal, strive for solitude or seek company, etc. These contrasts are confused by the patients when they are in a state of depression, the disagreeable opposite usually bearing an objective character and relating to other people. For example, the patient passionately wants to be alone, locked up in her room, and actually she remains alone, but she is tortured by the thought that some malevolent person has contrived to get into the room and watch her.

One cannot but agree with all the foregoing, which represents an extremely interesting psychological analysis. But I take the liberty of disagreeing with you on the interpretation of the last point. You repeat more than once that, contrary to the general belief, these contrasts are not so easily distinguishable. You say: "*To tell and to be told* form a single whole and the one is not easily distinguished from the other, as is usually believed." And further: "*The act of* *insulting and the act of being insulted* are united by the general concept of insult; but the disorder shows that they may be confused, that one may be mistaken for the other." You explain this confusion by a rather complex combination of feelings.

Availing myself of the facts established and systematized by you, I have resolved to take another way and to interpret them physiologically.

Our general notion (category) of contraposition is one of the fundamental and indispensable general notions, which, along with all others, facilitates and controls normal thinking and even makes it possible. Our attitude towards the surrounding world, social environment included, as well as towards ourselves, would be distorted to a very great degree if there were constant confusion of opposites: I and not I; mine and yours; I am simultaneously alone and in company; I offend and I am offended, etc. Consequently, there must be a profound reason for the disappearance or weakening of this general notion, and, in my opinion, this reason can and must be sought in the fundamental laws of nervous activity. I think that in present-day physiology there are definite indications to this effect.

In the course of our study of the higher nervous activity by the method of conditioned reflexes we observed and investigated in our experimental animals the following precise facts. In different states of depression, inhibition (more often in various hypnotic states) the equalization, paradoxical and ultra-paradoxical phases are manifest. This signifies that the cortical nervous cells, instead of normally producing (within certain limits) effects proportional to the intensity of the stimulating agents, in states of various inhibition, begin to produce effects either of equal strength, or inversely proportional to the intensity of the stimulus, and even of an entirely opposite character; this means that the inhibitory stimuli produce a positive effect, and the positive stimuli a negative effect. I make so bold as to suppose that it is precisely this ultra-paradoxical phase which causes the weakening of the notion of contraposition in our patients.

All the conditions necessary for the development of an ultra-paradoxical state in the cortical cells of our patients, are in evidence and have been clearly established by you. When these patients, being of weak constitution, come up against a multitude of life situations, they easily fall into a state of depression, anxiety and fear; they can, however, still desire or not desire something, they have their emotionally-reinforced and possibly concentrated ideas of what is desirable or undesirable (I am the master, not the slave; I want to be alone and not in company; I want to have secrets, etc.). And in such conditions this is sufficient to evoke in a fatal way an opposite idea (I am a slave; there is always somebody near me; all my secrets are being disclosed, etc.).

The physiological explanation of this phenomenon would be as follows. Let us suppose that a definite frequency of the metronome acts as a conditioned alimentary positive stimulus, since its application is accompanied by feeding and, because of this, evokes an alimentary reaction. Another frequency of the metronome acts as a negative stimulus, since it is not reinforced by feeding and produces, therefore, a negative reaction: the animal turns away when it is applied. The frequencies of the metronome beats constitute a physiological pair, the components of which, being opposites, are associated and at the same time reciprocally induced, i.e., one frequency stimulates and reinforces the action of the other. This is an exact physiological fact. Further, if a positive frequency acts on a cell which for some reason or other is in a weak state (or in a hypnotic state), then this frequency, according to the law of maximum, which is also a strictly established fact, inhibits the cell. This inhibition, in conformity with the law of reciprocal induction, conditions a state of excitation instead of inhibition in the other component of the associated couple.

That is why the stimulus related to the latter now provokes excitation, not inhibition.

This is the mechanism of negativism or contralism.

If food is offered to a dog when it is in a state of inhibition (or hypnosis), i.e., when you induce it to positive activity—to the act of eating—it turns away and rejects the food. But when the food is moved away, i.e., when you give the dog a negative impulse aimed at inhibiting the corresponding activity, at discontinuing the act of eating, the dog, on the contrary, begins to reach for the food.

Evidently this law of reciprocal induction of opposite actions must also be applied to contrary ideas, which, naturally, are connected with definite (verbal) cells and also constitute an associated pair. Due to a state of depression or inhibition (in our experiments any difficulty arising in the higher nervous activity is usually reflected by inhibition), more or less intense stimulation of one idea leads to its inhibition and, by means of the same mechanism, induces the opposite idea.

It is easy to see that this explanation naturally embraces the peculiar symptom of the schizophrenics—ambivalency[162]—which arises under a highly extended and profound ultra-paradoxical state.

Many people, even scientifically-minded people, are moved almost to the point of anger by the attempts to give a physiological interpretation of psychical phenomena; they retort that such explanations are "mechanical", since they want to stress as strongly as they can the obvious inaptitude and absurdity of trying to link subjective feelings and mechanics. In my view this is an obvious misunderstanding.

At present, of course, there can be no talk of representing our psychical phenomena *mechanically, in the full sense of the word.* We are also far from being able to do this with regard to all physiological manifestations; the same thing applies, although in lesser degree, to chemical phenomena, and it applies fully to physical phenomena. A truly mechanical interpretation is still the goal of natural-science research; the study of reality as a whole, including ourselves, is advancing very slowly towards this goal, and much time will be required before it is reached. Modern natural science as a whole is but a series of many *stages of approximation* to this mechanical interpretation, stages linked throughout by the supreme principle of causality or determinism, according to which there is no action without cause.

And if possibilities are now opening up for explaining the so-called psychical phenomena physiologically, they can be regarded as a certain, slight, very slight degree of approximation towards a mechanical interpretation. It seems to me that in many cases these possibilities are opening up.

Being now at the psychological stage of your research, you are interpreting the feelings of possession, establishing the conditions under which they arise, reducing them to their elementary components and, in this way, elucidating their general structure, i.e., you are also dealing with their mechanics, with their general structure, but in your own way. I, in the physiological stage of my research, am trying to bring our common problem a bit nearer to true general mechanics, interpreting your fact concerning confusion of opposite ideas, as the specific interaction of elementary physiological phenomena—nervous excitation and inhibition. In their turn chemistry, and, finally, physics, will further disclose these phenomena and their mechanism, thus steadily approaching the solution of our problem.

XII

FRAGMENTS OF STATEMENTS AT THE "WEDNESDAY" GATHERINGS

STRUGGLE OF I. P. PAVLOV AGAINST IDEALISTS

[EXPERIMENTS WITH ANTHROPOIDS. CRITICISM OF THE CONCEPTS OF YERKES[163] AND KOEHLER]

EXTRACT FROM THE STENOGRAPHIC RECORD OF THE "WEDNESDAY" HELD ON MAY 16, 1934

Academician *I. P. Pavlov:* Here we have before us "Raphael"[164] the chimpanzee. You say to him: "Work," and he takes a seat in a definite place, near a big rectangular box. The box has sliding lids with openings of different shape—circular, quadrangular and triangular. The lower part has a small door and through this we place food which rouses "Raphael's" interest. Near the box we put fifteen or twenty sticks with sections of different shape—round, quadrangular and triangular. We place the food into the lower part of the box, with "Raphael" looking on, and then we close it. The box is of a special construction: it is necessary to insert a corresponding stick into the opening in the lid, and press heavily on it; only then the door in the lower part opens, and "Raphael" is able to take the food. This is what we call his work, and his work lasts for a very long time, for two, three months and even more.

Thus, the food is placed in the box in the presence of the ape in order to rouse his interest, and a bundle of sticks is placed alongside the box, some of them circular, some tetrahedral, and some trihedral. At present "Raphael" has brought his work to a high degree of perfection.

For example, when a lid with a quadrangular opening is placed on the box, "Raphael" takes a tetrahedral stick and opens the box.

When we complicate the job by placing among many sticks only one with a square section, he makes an error and picks up a triangular stick instead of the square one. He makes the same error three times and then picks up the square stick and gets the food. We repeat the experiment. This time "Raphael" makes the same error only twice, and then takes the right stick. After several trials and several errors, in the course of subsequent experiments, he insists on taking the square stick only, no matter how deeply it is concealed in the bundle. Thus you see that "Raphael" errs, but in a uniform manner. There are round and triangular sticks in front of him, but he never takes a round one.

We then place on the box a lid with a round opening. "Raphael's" choice is excellent: he immediately finds the right stick, even if it is hidden in the bundle.

We change the lid once more. This time it has a triangular, not a round opening. At the first go "Raphael" confuses it with the square opening, which means that as yet he cannot differentiate precisely between angular forms; he takes a quadrangular stick, tries it and, since it does not fit, throws it away. But he does not repeat this error; he finds the triangular stick no matter how it is hidden among the others. The following detail should be added. Here I shall have recourse to a certain degree of imagination, although in a way which seems to me absolutely legitimate. "Raphael" is a very gluttonous fellow, indeed; he accomplishes the entire procedure only if he is well recompensed. As a rule, he shows no inclination to waste time on trifles. Along with "Raphael," there is "Rosa," a female ape, who, unlike him, is more disposed to exercise her intelligence than to satisfy her tummy. When offered food, she often pushes it away. We can say, therefore, that if she displayed any interest in these exercises she would in all probability solve the task merely for the sake of curiosity.

In telling you all this I wish to stress the following. The activity of this ape is in no way inferior to that described with such great satisfaction by Mr. Yerkes and Mr. Koehler. They regard this activity as a manifestation of the special intelligence of apes and sharply distinguish it from the activity of dogs which they regard as an associative process. But what reasons did they have for this viewpoint? What is the difference between dog and ape in this respect? I would even ask what difference is there between this activity and the activity of a child? And again, in what way does this activity differ from that of the dog?

The fundamental difference is that the lower extremities of the ape can perform functions similar to those performed by the upper extremities. Consequently, the ape accomplishes his task with greater ease, that is, finds the right sticks, chooses one, and inserts it in the opening, etc. "Raphael's" success, is due, above all, to the highly-developed mechanical possibilities of his body compared with those of the dog; the dog has no hands, and lacks the mobile extremities provided with five separate digits which make it possible to choose, to seize, to place, etc. This means that the motor apparatus of the ape is much more perfect than that of the dog.

And then? Then the spectator is impressed by the fact that the apes greatly resemble human beings—hands and general behaviour. However, were we to trace the entire path traversed by "Raphael" before he attained his complex equilibration with the surrounding world in conformity with his sense organs, we would be able to say that, wherever we were successful in observing his behaviour step by step, we found nothing, absolutely nothing, that had not already been studied by us on dogs. This is a process of association followed by a process of analysis effected with the help of analysers and accompanied by an inhibitory process which facilitates differentiation and rejection of that which does not correspond to the given conditions. Nothing more than this was observed by us in the course of our experiments. Consequently, there are no grounds for affirming that apes have some kind of "intelligence" which brings them closer to man, while dogs have not and are capable only of an associative process. I have gone back to the grudge that I had against certain psychologists. At first I renounced them, then I became reconciled with them to a degree, but now the facts have turned me against them once more. They, apparently, want their subject to remain unexplained. How strange, indeed! How they love the mysterious. Everything that can be explained physiologically they reject. But all the facts mentioned here took place before our eyes. "Raphael" analysed the actions which he had to perform with the box gradually and for quite a long time. He began by discerning the visual images of the sticks lying horizontally on the floor; he distinguished the angular, trihedral stick from the flat and tetrahedral one, as well as from the round one. When he had to choose the right stick he began with what I call a chaotic reaction. It seems to me I have already said that if we wish strictly to adhere to objective terminology, we must replace the American term "trial and error"[165] by the term "chaotic reaction." There is a nuance of subjectivity in the first term. But from the objective point of view this is a chaotic reaction. Take, for example, infusoria which move hither and thither in their medium in pursuit of definite aims—food, favourable conditions, better temperature, more appropriate composition, oxygen, and who knows what else; suddenly one of the infusoria gets caught in a certain noxious substance—in a cold or hot current; it begins to zigzag back and forth, and from side to side until it finds a suitable medium. This is termed the "method of trial an error." But I would prefer to call it "chaotic reaction," especially since every child begins with chaotic reaction.

"Raphael's" connection with the stick as an instrument of action was probably established long ago. He takes the stick, and this is quite comprehensible, all the more so since it was previously inserted in the opening in his presence; consequently, we have here the action of imitative excitation. He tries the stick but it does not fit; since his action is not reinforced by food he discards the stick and picks up another one which is also thrown away; however, he now begins to distinguish between the sticks. After several trials he no longer takes the wrong stick; this means that extinctive inhibition has been elaborated to them. The third trial is crowned with success; he gets the food, and thus the action is reinforced. After several repetitions a connection is formed between the visual image of the stick and the success of the action. But then the lid of the box is changed; "Raphael" adheres to the stick which has already brought him success for several times. But this time its use is not reinforced with food; he is able to differentiate, rejects the stick and looks for another one in the same way, etc. Consequently, everything begins with the formation of an association, with analysis of the shape of the sticks. In the subsequent experiments he selects the sticks at random, since he does not relate them to the opening in the lid; but whenever the stick does not fit, he throws it aside, and thus extinction manifests itself. He tries another stick, and if it does not fit either, it too is discarded, and finally he finds the proper one. Thus he easily distinguishes one stick from another, but that is not sufficient to solve the problem. So far "Raphael" only analyses the visual images of the sticks, but does not connect them with the opening. Then the second phase begins: the formation of a connection between the visual images of the sticks and the shape of the opening. Obviously "Raphael" cannot establish the connection between the shape of the sticks and the shape of the openings for a long time, because he does not see the shape of the section of the stick, while he clearly sees the lid opening, which is either round, square or triangular.

Further, an association must be formed between the opening and the visual images of the sticks. When one of these associations proves to be correct, when it is reinforced by food, "Raphael" begins to establish a connection between the visual stimulation produced by the opening and the visual images of the sticks, i.e., he begins to analyse. At one stage he distinguishes only a round opening from angular ones, but he still confuses different angular sticks. This means that the analysis must be further perfected. "Raphael" finally learns to distinguish the sticks precisely, and then the problem is fully accomplished.

Consequently, in this task there is simply constant association between the opening and the stick. There you have the whole of "Raphael's" man-like activity; his entire behaviour is based on analysis and association.

M. A. Ussievich: A dog with which I am working, upon being placed in the stand for the first time and seeing a revolving food receptacle, immediately began to turn it with his paw.

I. P. Pavlov: That is in complete accord with what I have said. The tendency to draw a psychological difference between ape and dog based on the process of association is nothing more than the secret desire of the psychologists to evade a clear solution of the problem and to render it mysterious and extraordinary. In this pernicious, I should say, disgusting tendency to depart from the truth, psychologists like Yerkes or Koehler fall back on such barren notions as, for example, that the ape went away, "meditated at leisure" in a human-like way, "has found a solution," etc. Of course, this is absolute nonsense, child's play, most unbecoming. We know very well that a dog often tries to accomplish a definite task but is unable to cope with it; however, it suffices to let the dog rest, say, for two days, and the task is successfully accomplished. Can we seriously say that the dog spent the two days considering the problem? Of course not. Simply fatigue has brought on inhibition, which confuses everything, renders difficult and destroys. This is a very ordinary phenomenon.

A long time ago someone told me—I think it was Speransky—that musicians studying a new melody often have a lot of trouble—nothing comes of their efforts. The more persistent they are the worse the result. In utter despair they abandon the effort. But after some time, when they resume work, all the obstacles are easily surmounted. This is merely accounted for by the fact that in the course of the study one becomes fatigued and the fatigue disguises the immediate result. But after a rest the ready result clearly manifests itself.

It is necessary to add that these facts can be explained without any difficulty. It is worth noting that when these experiments were performed one after another and in great number, "Raphael" erred much more frequently; he fell into a state of despair and picked up the sticks at random, just like a man when he is upset. This was an obvious manifestation of fatigue.

Then my attention was attracted by the following fact. When "Raphael" was unable to perform his task he did look away for a while, after which he turned again and accomplished the task. This fact can also be explained quite simply. When "Raphael" moved he had before his eyes the real images of the sticks; but when he turned away from these real impressions, he had before him only the persisting images of the traces of different sticks, and the association in this case was easily accomplished. This is perfectly natural. And that is how the matter should be actually interpreted.

Therefore, basing myself on the study of the apes, I affirm now that their somewhat complex behaviour is a combination of association and analysis, which I consider to be the foundation of the higher nervous activity. So far we have seen nothing else in their behaviour. The same can be said of our thinking. Beyond association there is nothing more in it.

THE NATURE OF INTELLIGENCE IN ANTHROPOIDS AND THE ERRONEOUS INTERPRETATION OF KOEHLER

EXTRACT FROM THE STENOGRAPHIC RECORD OF THE "WEDNESDAY," HELD ON SEPTEMBER 12, 1934

Academician *I. P. Pavlov*: ... I have two incidental subjects: one about apes, the other about Mr. Sherrington. The apes are connected with Koehler. It would be more correct perhaps if I say: on the one hand, about Koehler, and, on the other, about Sherrington. I believe it will be more useful if I talk about Koehler first.

This summer I devoted some time to the study of apes. We began with experiments concerning the analytical ability of apes. But these data are not new and are of no great interest. During the last month we reproduced Koehler's experiments, for example, the superposition of boxes in order to take hold of suspended fruit, etc. Prior to this I had read very thoroughly, and as usual, not once but several times, Koehler's article "Investigation of the Intellect of Anthropoids."[166] Thus I was able to read about the experiments and to have the facts of the given experiments before my eyes. I must say that I am really amazed at the degree in which human minds can differ.

In my opinion, Koehler saw nothing of what was actually demonstrated by the apes. I say this without any exaggeration: he simply did not understand anything.

Koehler, as indicated by the very title of his article, tried to prove that apes are intelligent, that in this respect, unlike dogs, they are close to man. He even mentions a special experiment which showed that the dog, unlike the ape, is not intelligent, and the latter, therefore, is rightly called an anthropoid animal.

What proof does he advance?

His sole fundamental, but peculiar proof is this. When the ape is given the task of taking hold of fruit suspended at a certain height, and when for the purpose of accomplishing it he needs definite instruments, for example, a stick and some boxes, all his unsuccessful efforts to get the fruit are not, according to Koehler, proof of intelligence. This is simply the method of trial and error. When the ape becomes tired, as a result of his unsuccessful efforts, he gives up and remains for some time in sitting posture. When he has rested he tries again and succeeds in accomplishing his task. According to Koehler, the ape's intelligence is proved by the fact that he sits for a period without doing anything. He literally says that, gentlemen. In his view the ape accomplishes some kind of intellectual work when it is sitting, and this proves its intelligence. How do you like it? It turns out that nothing but the silent inaction of the ape proves its intelligence!

And the fact that the ape uses a stick and places several boxes one on top of the other, this is not a manifestation of intelligence. When the ape acts, moves the boxes from one position to another, these are associations and not manifestations of intelligence; this is the method of trial and error. Koehler absolutely disregards these facts —all this is simply only association. But when the ape rests, when he is inactive, he performs certain intellectual work. Naturally, the only explanation one can offer for such reasoning is that Koehler is a confirmed animist, he simply cannot become reconciled to the fact that this soul can be grasped by hand, brought to the laboratory, and that the laws of its functioning can be ascertained on dogs. He does not want to admit this.

In reality the matter is quite different. For it is the processes disregarded by Koehler that are of greatest importance. I grasped and realized this while observing the behaviour of the ape. And I say that all this activity of the ape in trying now one, now another way of solving the task, is the intelligence, the reasoning in action, which you can see with your own eyes. This is a series of associations; some of them have been acquired in the past, others are formed before your eyes, and are either combined, united into a positive whole, or, on the contrary, are gradually inhibited and lead to failure. One can clearly observe the manifestation of some of the associations formed earlier in the ape, in the course of his life in the jungle, in his native surroundings.

It is clear that the ape is the perfect equilibrist, capable of maintaining his centre of gravity even in most incredible positions on a vertical support. When putting the boxes in position the ape first of all finds out in an empirical way whether they are firmly fixed. He places one box on top of the other, just as if they were stones or stumps, and then tests their firmness. He does not verify whether the surfaces of the boxes coincide; he simply gets up on them and begins to sway. In the event of failure, he rearranges the boxes in order better to adjust their parts, then jumps on to them again and tests their firmness once more. You are witnessing associations—associations acquired in the past and used by the ape as ready-made ones. These are the tactile, muscular, visual and other associations.

The ape continues piling up the boxes, depending on the height of the construction. It so happens that he picks up an extra box, climbs the pyramid and places the box on his head. This, you see, is an error, made in the process of elaborating the right association, the necessary connection.

One erroneous and very ancient association causes him great difficulty. He cannot overcome this association on the basis of reality.

He is given boxes of different dimensions; to attain stability it is necessary to place them in strict order, beginning with the largest one which must be placed at the bottom. But up to now he is unable to do this. If, for example, he mistakenly places, say, the sixth box instead of the second, there is no association which could tell him that this is inconvenient, that the wrong box must be removed; so he continues building. In this case only a lucky chance comes to his assistance. As for newly acquired associations, their successful elaboration depends exclusively on the exact order of the boxes. This is a visual association, and it is formed before our eyes. The sight of a regular pyramid leads to success. This visual association favours success. The act of placing the boxes directly under the suspended fruit, is an association which already existed. Thus, we clearly witness the formation of our thinking, we see all the reefs it encountered on the way, and all its methods. This is actual intelligence, but Mr. Koehler disregards it: for him this is simply trial and error.

There are some essential details. If the ape is in a state of excessive alimentary excitation, his actions become very disorderly—he takes the boxes as they come to hand, for example, the sixth instead of the second, etc. External inhibition exerts a very great negative influence. All this is well known. It is only necessary properly to observe definite facts and to accord them their actual significance. Then everything becomes clear and unfolds before the eyes. Such is the entire activity of the ape. His thinking is clearly observed in his actions. And this is the proof of his intelligence. It shows that there is nothing in intelligence but correct or erroneous associations, proper or distorted combinations of associations. Koehler, however, maintains that it is not a matter of association. Meanwhile the entire intelligence consists precisely of associations. What distinguishes it from the development of a child, or from our inventions? For the ape the problem consists in getting the fruit without the help of a stick, and he does this before your eyes by the method of trial and error, i.e., by means of associations. It is absolutely clear. In what way does this differ from our scientific discoveries? It is exactly the same thing. Evidently, this is elementary intelligence, differing from ours only by the poverty of associations. The ape has associations which relate to the interaction of mechanical objects in nature.... Thinking once more about the reason for the ape's success compared with other animals, and why he is closer to man, we would say that it is precisely because he has hands, actually even four hands, i.e., more than we have. Because of this, the ape can enter into very complex relations with the surrounding objects. And that is why a multitude of associations are formed in him, associations which do not exist in other animals. Since these motor associations must have their material substratum in the nervous system, in the brain, the cerebral hemispheres of the apes are more developed than those of other animals, this development being due to the diversity of their motor functions. We, humans, in addition to the diverse movements of our hands, possess a complex of speech movements. As is known, the ape is less capable of imitating speech than many other animals. Compared with him the parrot can have a greater stock of words. That is how I see the matter.

Koehler, evidently, is a victim of animism. Sherrington is another victim, but I will speak about this next time.

Such is Koehler's interpretation of the matter. That does not mean, of course, that he is not a highly intelligent man. These are quite different things. Quite a lot of intelligent people have also been animists.

I have had the opportunity of meeting Koehler. He is a most intelligent man, a man of vast erudition; he has profound knowledge, particularly in the domain of natural science. Will his intelligence enable him to overcome this animism? In his book he constantly refers to another volume to be written by him. I do not know whether it has appeared or not (*a voice*: "No, it hasn't"). Then I will venture the following supposition. His book, apparently, was written under animist influence, but subsequently he overcame his animism and in all probability he now takes a different view of the subject. That is why his second volume has not yet appeared.

Read Koehler's book and see for yourselves. To close one's eyes to the ape's activity, which is plain for all to see and the meaning of which is absolutely clear, is the height of absurdity, sheer nonsense. Koehler speculates that the ape goes into meditation when it is in a state of inactivity. But we observed this state time without number, it signifies mere extinction and nothing else.

Good-bye.

[CRITICISM OF SHERRINGTON'S IDEALISTIC CONCEPTS]

EXTRACT FROM THE STENOGRAPHIC RECORD OF THE "WEDNESDAY," HELD ON SEPTEMBER 19, 1934

Academician *I. P. Pavlov*: I shall now turn to a criticism of Mr. Sherrington. I have taken my time over this, quite deliberately; I re-read his book several times so as to avoid any exaggeration, any superfluous statement or judgement. However, two weeks have passed and my opinion has not changed in the least.

There is no doubt that Sherrington is handling a subject with which he, obviously, is quite familiar—*The Brain and Its Mechanism*.[167] He has been a neurologist all his life, engaged in the study of the nervous system, though, more of its lower part, the spinal cord, than the higher part.

Comparing the laws of the brain and its mechanisms, he draws a very strange conclusion. It appears that up to now he is not at all sure whether the brain bears any relation to our mind. A neurologist who has spent his whole life studying the subject is still not sure whether the brain has anything to do with the mind. This is clearly expressed by him in the following words: "If nerve activity have relation to mind. . . ." I did not trust my knowledge of English and so I requested others to translate it for me.

How can it be that at the present time a physiologist should doubt the relation between nervous activity and the mind? This is the result of a purely dualistic concept. This is the Cartesian viewpoint, according to which the brain is a piano, a passive instrument, while the soul is a musician extracting from this piano any melodies it likes. Obviously this is his viewpoint. Probably Sherrington is a dualist who resolutely divides his being in two halves: the sinful body and the eternal, immortal soul. I am all the more surprised that for some reason or other he regards knowledge of this soul as something pernicious and clearly expresses this point of view; according to him, if the best of us acquire some knowledge of the nervous system this would be a most dangerous thing threatening the extinction of man on earth. He makes the following statement which appears to me rather strange: if man learns to know himself and on the basis of this knowledge to govern himself in an economical way (such economy is not bad since it means that he will preserve himself for a longer time), then our "planet will be re-liberated, free for the next era of animal domination." What do you think of that? What does it mean? Why, it's simply preposterous!

Very good, we will suppose that the relation of the soul to the body is similar to that of the pianist to the piano, but it still leaves us in the dark as to why knowledge of the soul may be pernicious. I would like to know how on earth it can lead to the extinction of man. Socrates counselled: "Know thyself." How, then, can a scientist, a neurologist, say: "Do not dare know thyself"? Strange as it may seem, Sherrington adheres to the motto proclaimed at one time by Dubois-Raymond, a man who was always ready to sacrifice truth to eloquence, to a witty phrase, and who said that the function of the brain should never be made known,—"ignorabimus."[168] Sherrington, it seems, gets pleasure from repeating the same words fifty years later. What does this signify?

"If nerve activity have relation to mind," then he is inclined to think that this concerns only inhibition. Thus, positive work is of no significance whatever, while inhibition, discontinuance of work seems to go very well with the soul. He literally says: "If nerve activity have relation to mind, we can hardly escape the inference that nerve inhibition must be a large factor in the working of the mind." Why is, then, the essential positive activity rejected as having no relation to intelligence, while inhibition is regarded as having such a relation? Gentlemen, can anyone of you, who has read Sherrington's booklet, say anything in defence of the author? I believe that this is not a matter of some kind of misunderstanding, thoughtlessness or misjudgement. I simply suppose that he is ill, although he is only seventy years old, that these are distinct symptoms of old age, of senility.

Take my wife, for example. She is an obvious dualist. She is religious, but at the same time her attitude to things is not distorted.

How is it possible to come out against studying the problem, to affirm that this can lead to the end of mankind, to the triumph of the animal world? Indeed I would like to see this book translated by someone who reads English fluently. Why is such nonsense published, especially since Sherrington is regarded by many as an outstanding authority? I ask you to read it and, if you can, to say something in his defence. To me, it is strange in the highest measure.

Now I can prove to you that he is a dualist and animist. This is clear from the fact that in 1912, that is, twenty-two years ago, he said to me when we first met in London: "Your conditioned reflexes will hardly be popular in England, since they have a materialistic flavour." As I understand it, he obviously spoke for himself.

Here is another interesting passage. He puts the matter thus: "Strictly we have to regard the relation of mind to brain as still not merely unsolved but still devoid of a basis for its very beginning." He clearly states that we do not possess any starting-point, even the slightest one, for the solution of this problem.

Precisely this explains why towards the end of his life he has become a confirmed dualist and animist.

As for Descartes and his dualism, he, when he speaks of animals, regards them as genuine machines. This gave us the idea of reflexes on which we base our entire analysis of nervous activity. But when he speaks of man, Descartes is a dualist, believing in all seriousness that the brain is a piano, while the soul is the pianist, and that there is no direct connection between them. Thus, for Descartes' great intellect this was a complicated problem. He drew a distinct demarcation line between the animal and himself. In animals, the simple people used to say, there is only smoke or vapour, while man has a soul. When I mentioned this in a conversation with Richet,[169] he, apparently anxious to uphold French thought, told me that this was not Descartes' real viewpoint, that the clergy had forced him to think in this way and to express such ideas, that in reality he obviously advocated our point of view.

Voice: There are indications that Descartes burnt his last book, an outstanding work written in an absolutely materialistic spirit, because he had a presentiment that he would be denounced by the church. This was his conclusive philosophical work.

I. P. Pavlov: I have never read anything about that. In those days, of course, this was no joke: They could easily burn him, do away with him. That is quite on the cards.

N. A. Podkopayev: There are indications that Descartes deliberately inserted corrections in his work to suit the censorship; there was a difference between his real views and what he wrote.

I. P. Pavlov: I have never heard of that.

Well, gentlemen, that is all. I recommend those of you who know English to read this book.

P. S. Kupalov: Of course Sherrington is a dualist. That is absolutely clear. It explains why the sense he generally imparts to certain words differs from yours.

I. P. Pavlov: But he literally writes: "If nerve activity have relation to mind."

P. S. Kupalov: What does he imply by the term mind?

I. P. Pavlov: He simply uses the term "mind."

P. S. Kupalov: But you interpret the concept mind in your way, while he regards it somewhat differently. He chiefly takes into account, so to speak, the subjective emotions. He agrees that behaviour is law-governed. But he is preoccupied with what we call—near sensations.

N A. Podkopayev: That makes it all the worse.

I. P. Pavlov: Podkopayev is quite right when he says it makes it all the worse. What is meant by the statement that sensations have no relation to nervous activity? If by the mind he implies not proper orientation in the surrounding world, but exclusively subjective emotions and subjective manifestations, for example, direct sensations, then it follows that sensations have no relation to the nervous activity.

P. S. Kupalov: At the beginning of his book he states that the mind is directed by the external world and that all human behaviour is conditioned by the laws of that world. So, as far as the first part of the book is concerned, there is no misunderstanding. But I don't quite comprehend the final part of the book. He puts the following question: if I start investigating the brain from a purely physiological point of view, will I discover anything other than the mechanisms of which I already know in the nerve cell? As he sees it, mind signifies rather spirit than intelligence.

N. A. Podkopayev: Such a formulation of the problem is, in itself, dualism. In order to throw a bridge over two things, it is necessary that they should greatly differ, that there must be a gulf between them.

I. P. Pavlov: He is right from the physiological point of view when he admits that the mind is a most delicate correlation between the organism and the surrounding world. I should like to ask you then, what remains for your subjective sensations? If all our relations with the surrounding world, even the most delicate ones, are nothing but our physiological brain, then there is no room for any other interpretation of the word "mind." And this is the kernel of the contradiction.

P. S. Kupalov: He expresses the following idea: if we thoroughly knew one another, to the very core, life would become absurd, stupid, impossible.

I. P. Pavlov: Your words reveal the torments of the thought expended in trying to solve this problem, but they lack clarity.

P. S. Kupalov: I see it in a different way. Let us take his very last phrase which begins with the words "May I be forgiven. . . ." and concerns the social type. . . .

I. P. Pavlov: That is really stupid; it has nothing to do with the question we are discussing and is further proof that we are dealing with an abnormal mind. The fact that you, his advocate, despite your attempt to comprehend his viewpoint, are baffled by his phrase, simply reinforces my position. He is an out and out dualist. I know other dualists, but they have not gone so far as to affirm that if you analyse this sort of thing, i.e., the mind, you will be threatened by annihilation and by animal domination on earth. His talk about liberating the planet from man is sheer nonsense. It means that we, who are crowning the evolution of living matter, are evil, a species of tyrants. It is from this point of view that his words about the planet which "will be liberated" can be understood and interpreted.

P. S. Kupalov: Liberated in order to be developed anew.

I. P. Pavlov: "Free for the next era of animal domination."

No, gentlemen, if you undertake to defend him you must at least fully understand what he writes.

P. S. Kupalov: He maintains that there is "animal domination" on earth at present, meaning the reign of the animal world, including man. Who governs the earth? It is man as the supreme representative of the animal world.

I. P. Pavlov: When he speaks of animal domination he implies not man, but animals, which are inferior to us.

E. A. Asratyan: Perhaps, this can be explained in a simpler way; perhaps, the social aspect of the problem is involved here too. Spengler[170] and other reactionary trends have tremendous influence in the West. It is rather an attitude towards science.

I. P. Pavlov: That simply shows the course of his abnormal thought. He speaks of this, obviously proceeding from a dualistic point of view, and then inappropriately quotes Spengler and others. He says that we need not be prophets to foresee the fast approaching extinction of man. But Spengler and others say something quite different, namely, that in view of the complexity of life, to which the human system is not adapted, such excessive work may lead to the destruction of man. That is a different thing and has absolutely nothing to do with the study of the mind and of the nervous system. I can't understand it. In my opinion this phrase simply proves his abnormal thought. For it is a question of study, of scientific research, of economizing the nerves and intellectual forces, while Spengler speaks of the deformation of nervous activity as a result of excessive work which is beyond its strength.

E. A. Asratyan: I believe this stems from the same root.

I. P. Pavlov: As you like. But this is of definite interest. If these are torments of thought under the influence of dualism, then it is of general human importance and deserves to be given consideration....

To solve particular, comparatively limited problems is one thing; but to tackle a problem involving the entire history of human intelligence is quite a different thing.

We have just begun more or less to liberate ourselves from dualism. The human mind has for a long time been a prisoner of idealistic concepts. This should be borne in mind. And now it is time to end our discussion.

[CRITICISM OF THE GESTALT PSYCHOLOGY[171]]

EXTRACT FROM THE STENOGRAPHIC RECORD OF THE "WEDNESDAY," HELD ON NOVEMBER 28, 1934

Academician *I. P. Pavlov*: Today our discussion will be devoted to psychology, or to be more precise, to the marriage of psychology and physiology....

I shall devote myself specially to the gestaltists....

First of all, what do the gestaltists represent? They are representatives, advocates, adherents of the idea of integrality. According to them, it is necessary to consider and to keep in mind the integral whole, the synthesis, the system, and not isolated manifestations, which for some reason they dislike. Gestalt means design, pattern or image. The word is differently translated into different languages. For example, the English translate it as "form" or, even better, as the author calls it, "configuration." The English gestaltists are "configurationists." Gestalt is a German word and it has the same meaning....

A few words about the radicalism of this psychology. I must tell you that it is quite young. It originated in 1912, and is but twenty-two years old. It represented a revolt against Wundt, i.e., against associationism—the system of psychology which dates from the 16th or 17th centuries and which to this day is, to a degree, predominant among psychologists.

"The Gestalt psychology revolted against analysis as the fundamental problem of psychology," as its principal task. This is a somewhat strange approach in view of the fact that all positive, modern science is fully based on analysis and inevitably begins with it.

We shall never arrive at any psychology if we do not analyse the human behaviour or experience.

Further, the Gestalt psychology has proclaimed that the notion of association is simply a misconception.

What a queer radicalism, indeed!

"Gestalt psychology was as opposed to the simple reflex as to the simple sensation." Such is their true radicalism! It cannot be expressed in a more distinct, in a more definite form. The gestaltists attacked Wundt and associationism solely because the latter defended the principle of analysis. Wundt stated that he first identified the elements and then worked up to larger and larger compounds, which is what science does in general. But the gestaltists refer to this approach as the "brick psychology"—a play upon words—or the "mortar psychology" crumbling everything with its pestle. Very nice, indeed!...

The author goes on to inform us that a still more important impulse to the development of this psychology was given by the notion which appeared on the intellectual horizon in 1890 and which was introduced by a certain Ehrenfels, namely, the notion of "form quality."

This "form quality" means that elements remain elements, and are not worthy of any attention; of real importance only is the fact that from one and the same elements it is possible to obtain different wholes. The following example is cited. Take definite musical notes and compose from them different melodies. The melodies, of course, will be absolutely different. But this does not mean that the elements are of no value; it is thanks to them that the melodies can be formed and if they did not exist the melodies would be inconceivable. But this is no novelty! Why does the notion of form quality date only from 1890? Good gracious, how often do we meet the same in organic chemistry? Carbon, oxygen, hydrogen are elements which form carbon hydrates, acids, alcohols, etc. Where, then, is the novelty? And how can it be affirmed that the notion of form quality originated in 1890? Actually the idea is a very, very old one. But it made quite an impression on the psychologists. Woodworth even finds that it played the role of an impulse.

I must say that we have to deal with rather strange psychologists. At present I know them fairly well; I have also frequently met some of them. When I pointed out in my book the mosaic of the cerebral hemispheres, on the one hand, and the dynamic system, on the other, M. Piéron, a Paris psychologist, was greatly astonished and confused. Thereupon I wrote. let him open any page of any book on organic chemistry and consider any formula of chemical compound. He will see, on the one hand, a mosaic of hydrogen, oxigen, carbon, and on the other hand, their combination, the formation of a dynamic system. Do not all bodies represent dynamic systems?

Such is their thoughtlessness! They amuse themselves with a play on words, but disregard reality. This is absolutely clear.

Such, according to the author, is the origin of this "form quality"; it greatly attracted them and they chose it as their fundamental principle.

Since 1912 the Gestalt psychology has endeavoured to prove that any distinction in psychology between the elements and the whole is a misconception, that psychology invariably and exclusively deals with the study of the whole. But how can anyone get to know the whole without breaking it up? Take, for example, the simplest machine. How can the principle of its working be understood, if it is not dismantled, if the interdependence of its parts is not considered?

This is truly strange reasoning and it passes my comprehension.

The next chapter deals with the organic integrity of the psychical activity as the fundamental feature of the Gestalt psychology.

I must tell you that Woodworth endeavours to convey the ideas of others with astonishing thoroughness, I should say, even scrupulously. . . .

"We can certainly recognize in the Gestalt psychology a strong and valuable addition to the varieties of psychology." The gestaltists adhere to the point of view that an isolated feature should by no means be studied; they prove this in splendid fashion by stating that if a single feature of the face is exposed, and the rest of the face is covered, nothing will come out of it. This is self-evident. Separate features assume different importance in the entire whole; some of them stand out in bold relief, others are disguised, recede into the background, etc. That is quite obvious. But the features must be identified. In the end, when one analyses a face, one must on the basis of definite features depict it as quiet, calm, wilful, very tender, etc. Of course, without analysing the constituent parts one cannot comprehend anything. The same thing applies to the human character. If one takes isolated traits and analyses each of them separately, naturally it will be impossible to determine the given character; for this purpose it is necessary to take a system of traits and to establish which of the traits in this system are more prominent, which are hardly visible, etc.

The author adds that human and animal organisms are a "Gestalt." But nobody doubts their integrity. At the same time, however, nothing prevents the decomposition of this system into its component systems of blood circulation and digestion, as well as the decomposition of the latter into the stomach, intestines, gastric glands, etc. This is simply forcing an open door.

Further, it is said that our behaviour is not a mere sum of reflexes. Again, what a truth! That is a commonplace. But they picture the matter in such a way as if a system were a sort of a sack filled pell-mell with potatoes, apples, cucumbers, etc. Nobody has ever expressed such an idea. The moment one has to deal with an organism, it is clear that all its elements act one upon another, just as hydrogen, oxygen and carbon act in a chemical body depending on their location—from above, from the sides, from the right, from below, etc. This is a well-known, long-established fact. . . .

The gestaltists have given much attention to perception. What is perception? Some fifty or sixty years ago when I was studying in the seminary, and when there was not even a sign of the gestaltists, I learned from the same old professors and psychologists all about perception and what distinguished it from sensation, which is a more elementary process. The course of psychology at the seminary taught us that sensation is a kind of purer, so to speak, physiological stimulation, produced by a certain external agent on the sense organs; perception, however, is that which arises in the brain, when this stimulation is not single, but connected with other stimulations and old traces. It is this that enables us to get an idea of an external object. Such is perception. The final result of internal elaboration constitutes its very essence. So you see this is quite a commonplace fact, well known to everybody. . . .

But this has already been exhaustively elucidated from the physiological point of view. I am not acquainted with their works and their bibliography, and I don't know whether they made corresponding allusions or not; but they should have cited Helmholtz and his physiological experiments with the sensation of tones. Another of his classical books deals with the eye and the ear. There the explanations are not so vague; they are absolutely exact from the physiological point of view, from the point of view of the whole of the system. And so some fifty years prior to them everything had been explained physiologically.

These gentlemen should have made a proper study of physiology, i.e., they should have thoroughly read Helmholtz. But instead, they content themselves with a play on words: "the sensory brain process is . . . subjected to the influence of the distance,"—but how—about this not a single word is said.

Perception, if considered profoundly, is simply a conditioned reflex; however, since Helmholtz knew nothing of conditioned reflexes he called them unconscious conclusions. . . .

So you see that the gestaltists, far from contributing anything new to the problem, are not even aware of what was regarded as a truth more than fifty years ago. This is an indisputable fact. I defy the physiologists or psychologists to prove that I am not right.

Now let us turn to another point—to the gestaltist study of behaviour.

The following is literally stated: "Gestalt psychology dislikes the stimulus-response conception." What does this mean? These are scandalous words.

"It objects, first of all, to the idea that behaviour can properly be analysed into stimulus-response units." Thus they do not admit stimulations and reactions, i.e., they deny, for example, the fact that if I choke, it is because something is irritating my throat. They do not want to make any distinctions. But this won't get them very far! And what do you think of this? "It objects to the notion of a bond between stimulus and response." This is literally stated. Read it and see for yourselves. They object to the notion, consequently, to the importance of a connection between stimuli and responses, whether provided by nature or inculcated by practice.

These are not my words; they are the words of the author.

I shall read one more passage, since it is a collection of magnificent absurdities.

They object to the theory that an instinct is simply a chain of reflexes; they object to the theory that learned behaviour consists of reflexes linked together by the process of conditioning. Moreover, they object to the loose way in which the term "stimulus" is used by psychologists.

A psychologist speaks of a definite complex object as the stimulus. But they say: you have no grounds for affirming this, since various stimuli proceed from this object. But no one disputes that. For when I look at a certain object it may act simultaneously on my eyes and on my sense of smell if it has an odour. Why, then, is it impossible to use the term stimulus?

Now we pass, so to speak, to the pillars of Hercules, i.e., to the analysis of behaviour. Here we encounter another gestaltist. Apparently, gestaltists are recruited from among very superficial persons. Such, for instance, is Professor Kurt Lewin of Berlin University—don't trifle with this! Lewin has devoted himself to the study of the psychology of action. His arguments against the associationists, and in particular against the linkage between stimuli, do not go so far as to deny their existence in general. No, he does not go as far as that. But he affirms that the stimuli do not produce any action. This is also remarkable! "They are not the insufficient causes of action." He illustrates this point of view by splendid experiments—observations carried out on himself. All the experiments and observations of the teachers, and of the pupils, are cited in his book. So you can imagine what an intellectual beauty it is!

Suppose, for example, that I have put a letter in my pocket, impressing on myself the necessity of dropping it into the letter-box I pass in the street. In this way I have established a connection between the sight of the letter-box, as a stimulus and my response, my reaction, i.e., the necessity of placing the letter in the box. And when I see the letter-box I post the letter. The associationist or psychologist would cite this reaction as excellent confirmation of their doctrine. But he begins to argue.

"According to the association psychology the exercise of this stimulus-response connection should strengthen it." It is not bad, of course, that he is aware of reinforcement. "Therefore, when I reach the second letter-box my response of reaching in my pocket for the letter will be even stronger" *(laughter)*.

Tell me, please, what does this mean? Is it not really absurd?...

Had he studied the matter more attentively, he would have said: I put the letter in my pocket. I had it with me, but being absorbed in my thoughts I forgot all about it and went past the letter-box. Later, I came to another letter-box which caught my eye; then my thoughts coincided, and I dropped the letter into the box. This is a true association. But he confused everything. The devil's own brew! Such are the gentlemen who analyse the higher psychological activity. They won't go very far!

[CRITICISM OF THE GESTALT PSYCHOLOGY]

(Continued)

EXTRACT FROM THE STENOGRAPHIC RECORD OF THE "WEDNESDAY," HELD ON DECEMBER 5, 1934

Academician *I. P. Pavlov*: Today we shall continue discussion of last Wednesday's subject. It merits consideration and is quite opportune since we are now trying hard to link up the psychological with the physiological.

First of all, I shall discuss in more detail what I mentioned in passing last time.

I have in mind a chapter in Woodworth's book devoted to Gestalt psychology. It is entitled "Insight Essential in Learning According to Gestalt Psychology." Learning, the concept of learning—such is the fundamental subject. I shall read a few excerpts.

"The trend of psychological theory, from the day of Ebbinghaus had been in the direction of a mechanical conception of learning."

And he adds: "On the other side, the work of Pavlov, and the enthusiasm with which psychologists took up the idea of the conditioned reflex, reinforced the old associationist conception of learning as consisting of linkages formed between . . . stimuli and responses."

"The Gestalt psychology is the chief opponent of associationism. It has no faith in these elementary linkages, whether native or acquired. Not that it dislikes brain mechanics—or dynamics; but it believes the brain to work in large patterns, by 'closing gaps' (I shall explain this later), etc., rather than by the operation of nerve paths linking this and that little centre in the brain."

What is meant by the phrase: "it believes the brain to work in large patterns, by 'closing gaps'?"

You will remember—I mentioned this last time—they took into consideration the fact that we distinguish cortical phenomena as a whole, and that if there is any hint indicating the existence of certain gaps, we fill the gaps ourselves. From this they have deduced a rather peculiar principle which they call "closing gaps." Koffka, one of the gestaltists, has written a book entitled *The Growth of the Mind.* Now, I ask those of you who are fluent in English, to tell me what the word "growth" means: does it mean increment or origin? According to the dictionary, it has both meanings. But there is a world of difference between them.[172]

Discussing the problem of learning, Koffka bases himself exclusively on the anthropoid studies of Koehler. He comes to the conclusion that all learning consists in insight, and that Thorndike's supposed learning by trial and error is simply a mistake, one may even say, a misunderstanding. How do you like that?

Further he states: "Thorndike had pointed to the gradual improvement shown in his learning curves as evidence against any sudden insight."

Thorndike, just as we did in our experiments, kept his cats in cages; they learned to open the door, etc. Naturally, with the passage of time they performed this act more rapidly. And this is his learning curve. He finds that the fundamental learning curve, i.e., ability to open the door grows, improves quickly, and becomes more and more precise. That is why he says that there is no sudden insight, but gradual learning.

Koffka re-examined Thorndike's experiments and found that in some cases the solution was of a sudden nature. He cavilled at this point. Thorndike himself admits that many things, naturally, complicate the task. The learning and the final goal of learning can be attained at times more rapidly, at others in a more gradual way.

Further, Koffka gives his own exposition of the entire method employed by Thorndike and comes to the conclusion that "there is no learning except through insight" Insight, he says, does not simply exist alongside of trial and error as an additional mode of learning. The trial and error principle is merely displaced.

As Koffka sees it, Thorndike's trial and error principle means in the first place that nothing new can ever be learnt by the animal. The elimination of unsuccessful movements and fixation of the successful ones (according to Koffka) must go forward without any participation on the part of the animal. You see how preposterous his deduction is! The animal has not the slightest notion why its behaviour is being modified. The entire process, in which the successful acts are preserved and the unsuccessful ones gradually eliminated, is purely mechanical.

That is how Mr. Koffka puts the matter in his exposition of Thorndike's trial and error principle.

He seizes on an inexact expression of Thorndike's and traps him. In reality Thorndike says something altogether different:

"When put into the box the cat would show evident signs of discomfort and of an impulse to escape from confinement. It tries to squeeze through any opening; it claws and bites at the bars or wire; it thrusts its paws out through any opening and claws at everything it reaches;

it continues its efforts when it strikes anything loose and shaky."

This is something quite different from what Koffka pictures, with the result that he wages a struggle not against the real Thorndike, but against an imaginary one, i.e., created by himself.

Such is the attitude of the gestaltists, as represented by Koffka, towards the object of learning.

Further, Woodworth has recourse to a proposition which they advanced with real success against associationism. I learned of this long ago from my correspondent in Paris, who acts as my liaison with Paris physiologists. There is much talk there to the effect that the gestaltists have adduced very serious and strong arguments against associationism. These arguments are based on the fact that conditioned reflexes are supposedly elaborated only to isolated stimuli, but not to relations between objects.

I reproduced this experiment jointly with S. V.[173] and we found that a conditioned reflex could be elaborated to a relation as successfully as to an isolated stimulus.

Their experiment consists in the following. They take two grey boxes, one of them having a darker shade than the other. Food is placed in one of the boxes, say, the one of a lighter shade. At first the animal confuses the two boxes; then, on the basis of the usual procedure of conditioned reflexes, it moves towards the box of lighter shade.

After that they take some other shades of grey, and the animal rushes towards the lightest of them, although this is not the same stimulus that acted in the experiment with the first two boxes. Consequently, a reflex to a relation has been elaborated in the animal. And this, in their view, is a strong argument.

However, the substance of the experiment refutes their conclusions.

Together with S. V. Kleshchev we verified these facts on dogs. We elaborated a reflex to two tones with an interval of a quint. Then we began to differentiate other pairs of tones, some of them separated by a quint and some—by a third. It turned out that the pair of tones separated by a quint became differentiated more rapidly. Thus, a relation may of itself become a conditioned stimulus. There is nothing special in this. They, however, decided that on the basis of this experiment all the older theories of learning should be rejected and, consequently, that Thorndike's interpretation of his experiments should likewise be rejected.

The next section is devoted to the theory of learning itself.

I am compelled to enter the lists against the author.

The title of one of the paragraphs reads: "The Theory of Learning More Uncertain than Ever." I am most grateful to him. Because this means that he himself acknowledges his bankruptcy.

He affirms that there are now three theories: our theory of conditioned reflexes, Thorndike's theory, and the Gestalt theory. Each of these theories explains some of the facts; each is justified to a degree by corresponding experiments, but this justification is not sufficient to elucidate the questions raised by others.

I shall cite now his final conclusion: "We can certainly recognize in the Gestalt psychology a strong and valuable addition to the varieties of psychology.... There is probably profound truth in their contention that besides sensations and motor responses and bonds between them—besides and including all these—there is the process of "dynamic organization."

What do you think of it? Besides sensations, besides responses and bonds, there is a dynamic organization. This is a connection, and if not, then it is the soul which you consider, that is, something imperceptible, something you cannot feel with your hands. The connection is nothing else but this dynamic organization. I say that this idea of imperceptibility, of the soul, is ingrained in all of them.

I have told you what he says in his book.

Now for our interpretation of the question.

It must be assumed that the formation of temporary connections, i.e., of associations, as they are usually called, precisely represents this insight, knowledge, or the acquisition of new knowledge.

When a connection, or an association, is formed, this undoubtedly represents the knowledge of the matter, knowledge of definite relations existing in the external world; but when you make use of them the next time, this is what is called insight. In other words, it means utilization of knowledge, utilization of the acquired connections.

Consequently, gentlemen, the gestaltists start not at the beginning, but at the end. There are inborn connections; but when it is a question of a connection that is not inborn, it proves that if one thing follows another, you can establish such a connection. This is absolutely clear. All learning consists in the formation of temporary connections, and it is this which constitutes thought, thinking, knowledge. Thus, association, thinking, are the fundamental factors; this has long been known and has been properly appraised by some psychologists. The significance of the Gestalt psychology, which denies associationism, is wholly negative; it contains nothing positive.

I draw your attention to one idea to which the gestaltists stubbornly cling. In the concluding paragraph we find the following words:

"Where the existentialists are interested in sensory analysis, and the behaviourists in motor performances, the Gestalt group stress the importance of the topic that has usually been called perception, neglected by the behaviourists and handled very meagrely by the existentialists." The whole of this sentence discloses absolute incomprehension of the matter. What does the word perception mean here? The connection between the kinesthetic stimulation of the cell and all other stimulations, etc. All these are perceptions, and all of them develop in the brain. It would be absurd to suspect Woodworth of believing that it is the contracting musculature itself which participates in the process of perception. Clearly, this takes place in the brain.

I am fully convinced that thinking is an association and I challenge anyone who disagrees with me to prove the contrary. Association is knowledge, it is thinking, and when you make use of it, it is insight. But beyond this there is considerable confusion.

The essence of the question is: how can the experimental forms of Thorndike be brought into line with ours? We apply the conditioned reflexes in a way that first a certain conditioned stimulus is put in action and then the unconditioned reflex is added. Thus, the stimulation produced by us is a signal of this unconditioned reflex. In the brain there takes place a prolongation of the path between the cells of the external excitation and the cells of the unconditioned reflex. That is our understanding of the matter.

Thorndike's experiments are quite different.

They are carried out in the following way. A cat is placed in a cage the door of which is bolted in a certain way. The cat tries to get out, prompted by the desire to be free, just like any other animal kept in confinement and whose movements are restricted, or possibly it is excited by the food placed outside the cage. How does it act? It makes numerous chaotic movements, and then, by chance, in the course of these movements, contacts the bolt; the cat acts on it mechanically also in an accidental way. Finally the door opens and the cat escapes.

It is clear that in this case a connection is formed between the definite contact with the object and the mechanical pressure on it, say, the latch or bolt and the opening of the door. This is an association. The association consists in nothing but this, thus it is knowledge which the cat will use next time; this is comprehension of the relations between objects in the external world.

In the given case the cat was attracted by a piece of meat. But our ape "Rosa," who displays little interest in food, would do the same thing solely for the sake of escaping from the cage. In this case the connection is different. If, for the sake of getting a piece of meat, a dog or a cat succeeds in unbolting a door, it will act in the same way when satisfied, when it wants to be free. . . .

How should these facts be interpreted? It is necessary that at the given moment the brain should be in an active state, in a state of certain excitation. There is no doubt that the longing for freedom or for food is a reflex. It is a manifestation of the instinct. Take any animal, even the lowest, in which it is impossible to admit the slightest degree of intelligence, it will not turn away from food, on the contrary, it is attracted by it. And in exactly the same way it will not move towards any noxious agent, fire, for example. This is an unconditioned, inborn connection. When a dog reaches for meat or longs for freedom, this is an unconditioned reflex, an inborn instinctive connection. When the brain is in such an active state, an association is bound to arise, and this is intelligence, thinking, intellectual activity. At first it may be of a minimum degree, but subsequently it becomes more considerable owing to the formation of a connection. From this moment on thinking and comprehension make their appearance. Thus, it is association which underlies all these phenomena. It is in this way that our experiments must be compared with those of Thorndike. Precisely here lies the essence of the matter. In our experiments with artificial and alimentary conditioned reflexes, when connections are formed which act as alimentary signals and signals varying in accordance with the conditions of the experiment, the connections are purely temporary, bearing the character of signals. In Thorndike's experiments the connections are of a more permanent nature. This is already the beginning of scientific knowledge, since it is a question of more constant connections. At the beginning they may be rather accidental, but science in general has at first a superficial character and subsequently becomes more and more profound, getting rid of everything accidental.

According to the mechanism of formation this is the same connection, the same association, but of different significance.

When you repeat one after another two words taken at random and having no definite meaning, one of the words finally begins to evoke the other. The mechanism responsible for the elaboration of this connection is the same—the formation of a path between definite cells. The gestaltists deny this. It follows that genuinely profound analysis is absolutely beyond them. In their eyes the matter is so complicated that one must not even touch it, must not try to analyse it....

I shall pass now to our experiments on apes. Here it becomes even more clear that this "insight," this "thinking" (obviously one and the same thing) at first consists exclusively of elementary associations and then of connections between elementary associations, i.e., of complex associations.

We suspend a fruit at a certain height in "Raphael's" chamber. The connection established between him and the fruit is an unconditioned reflex, an instinct. He is attracted by the fruit, but the considerable distance between him and it is an obstacle. There are boxes on the floor. After a number of unsuccessful attempts "Raphael" soon notices the boxes. At first he stands on one of them and tries to reach the fruit, but the distance is still too great. Then he rejects this box as unsuitable and begins to pile one box on top of the other.

It must be granted that in this case the ape very likely proceeds from previous life experience. This is probably an old connection elaborated by life....

As to the given association it can be assumed that either this experience existed previously, i.e., the ape already knew how to go about the matter, or being in a state of strong motor excitation, he seized one box, threw it accidentally on top of another and, standing on them, came nearer to the aim. Consequently, this is the same method of trial and error. Indeed, it is impossible to admit that the new connection was formed of itself, from things never seen by the ape before. In future we shall observe the actions of another ape right from the beginning.

The first association was thus established. In order to diminish the distance between himself and the bait, it was necessary to place one box upon another. The ape could do this in a firm and stable manner, or, on the contrary, he could place the upper box on the edge of the lower box. A truly useful association is obtained solely by the method of trial and error. Should the ape place the upper box on the edge of the lower, that is, without making their surfaces coincide, nothing would result. And this is another association. A connection must necessarily be formed in the head between the positions of both boxes. "Raphael's" complicated task consisted in piling six boxes one upon another in order to reach the fruit. Now he is able to accomplish this task. All these particular associations were elaborated by the method of trial and error. When the connection coincided with achievement of the aim, it was reinforced and fixed. In the final analysis, it is quite clear that different connections arise. This is clear to all.

In Koehler's experiments all the apes were kept together. In some of them the association was elaborated rapidly, in others—slowly, in still others it could not be formed at all; everything depended on the physiological properties of the brain.

Besides these constant connections, even if consisting of various associations, one more important association is required: to place the box directly under the fruit.

When one of the apes reached the fruit, all the others saw it, and one of the most sluggish of his companions, directed by the imitation reflex, also piled the boxes one on top of the other, not, however, directly under the fruit, but a little to the side; thus, he made a fool of himself.

When he climbed on to the boxes the apple was beyond his reach. Evidently this is the elaboration of isolated associations.

Along with the elaboration of such isolated associations, there must also be formed a chain of associations, linking one association with another. Thus, the entire mechanism of thinking consists in the elaboration of elementary associations and in the subsequent formation of chains of associations.

The importance of imitation must also be emphasized. One of Koehler's apes did not practise the method of trial and error—he simply watched the work of another ape. In this way he elaborated new connections, so to speak, at the expense of the work of another ape.

An amusing incident took place one day with "Rosa." "Rosa" is cleverer than "Raphael." She has a fairly high degree of "intelligence," while "Raphael" is simply a glutton. Food is the only thing that attracts him. With "Rosa," on the contrary, food is of minor importance. She often manifests an overwhelming desire to play, or even to "contrive"—to open a small box, etc. When she is busy with something or other, she rejects the food you offer her; the aim of her work is quite different. Unfortunately, this entails certain complications since the use of food is the simplest method.

In one of our experiments we utilized her playing instinct. We wanted to reproduce the experiment with boxes. For this purpose we constructed in a warm chamber a kind of a well. A limited space was surrounded by high walls. "Rosa" was brought in through a door. She wanted to play, but there was absolutely nothing in this space, except high walls and a few boxes on the floor. And so the natural impulse of breaking loose came to the fore. She acted in a very curious and amusing way, just like Koehler's ape which, reproducing only part of the associations, made a fool of himself—he succeeded in constructing something but not in the right place. "Rosa" observed the door through which she had been let in. At first she simply tried to open it, but without success, since it was tightly bolted. She then discovered a tiny hole in the door, and availing herself of an old association, inserted her digit into it and made several attempts to force the door. However, these attempts, too, were in vain—the door was solid.

Then, picking up one of the boxes she carried it to the door, climbed on to it and putting her digit into the hole, began to tug at the door.

What is the significance of these actions? The significance is that in the large cage in which "Rosa" was kept, she frequently watched "Raphael" solve his problem. One of the elements of this solution was familiar to her, and she "thought" that it would help her to open the door. That was her aim, but she had seen "Raphael" reach his goal—the suspended apple—by carrying the boxes and piling them one on top of the other. This temporary connection was fixed in her, and she used it, but without success. Such is the explanation, such is the sole meaning. The ape performed this action once, and then repeated it. That is how I understand the matter.

This means that up to a point thinking is nothing else but associations; at first these are elementary associations connected with external objects, and subsequently they become chains of associations. Thus, each slight association, including the very first one, is the moment of the birth of thought. As I said at our previous gathering, these associations grow and increase in number. We then say that the thinking becomes more profound, more extensive, etc.

However, this is but half the process of thinking. It is what the philosophers, including Locke in his work on the human mind, call synthesis.[174] And that is exactly what it is. It is, indeed, a union of impressions produced by two external objects and the subsequent utilization of this union.

But in addition to this association another process sets in—the process of analysis. As you know, the analysis is based on the analysing capacity of our receptors, and besides, on the disintegration of connections, which is also effected by the cerebral cortex. We know this process perfectly well from our experiments with the conditioned reflexes. If in experimenting with a dog you elaborate a temporary alimentary connection to a definite tone and then apply other tones without reinforcing them by food, the first thing observed is a temporary irradiation bringing the adjacent points to a state of excitation. We call it generalization. When the connection with the other tones is not justified by reality, the process of inhibition takes a hand. In this way the real connection becomes more and more precise.

The same thing applies to the process of scientific thought.

The habits of scientific thought consist, above all, in obtaining a more constant and precise connection and in the subsequent rejection of all accidental connections. From this point of view everything can be easily understood. The process of thinking begins with associations, with the synthesis; then the analysis joins in. The latter is based, on the one hand, on the analysing capacity of our receptors, of the peripheral endings, and, on the other hand, on the process of inhibition which develops in the cerebral cortex and sorts out that which does not correspond to reality from that which does. That is how I understand the matter, basing myself on the data of our research.

Gentlemen, if any of you would like to add to what I have said, or to amend any of the points, you are welcome!

From my point of view, the Gestalt psychology is one of the most unsuccessful essays of the psychologists. Its role, I should say, is definitely negative. Indeed, what has it contributed to the knowledge of the subject? Absolutely nothing. On the contrary, it is destroying that which is most essential and most correct—associationism, synthesis, connection. Such is my attitude to this Gestalt psychology.

In any case, you should give thought to the matter, since it is of vital interest to us. All of us are studying the problem of the higher nervous activity, and you, our "conditionists," are taking part in solving it. I would recommend you, therefore, to concentrate on it, to consider all the pros and cons and to express your views, since this is the only way to establish the truth.

I think that the point of view which I have just expressed fully accords with reality. I cannot think otherwise now....

If you have no points to raise at the moment, keep the subject in mind and think it over. It is a fundamental question. Psychology is covered here by physiology, the subjective is interpreted in a purely physiological, objective way. And this is a great gain. We are beginning to understand the process of human thinking, which has been the object of so much talk and of so much twaddle.

In any case, I am grateful to this book: it made me consider all these questions anew, in a more profound way and, in the end, enabled me to reach the conclusion I have laid before you.

[CONCERNING THE ARTISTIC AND THINKING HUMAN TYPES]

EXTRACT FROM THE STENOGRAPHIC RECORD OF THE "WEDNESDAY," HELD ON JANUARY 9, 1935

Academician *I. P. Pavlov*: ... Now, gentlemen, let us turn to the following question. When we analysed nervous patients in the neurological clinic, I came to the conclusion that there are two specifically human neuroses—hysteria and psychasthenia; I related this conclusion to the fact that man offers two types of higher nervous activity, namely, the artistic type, consequently analogous and close to that of animals which also perceive the external world in the form of impressions exclusively and directly by means of receptors, and the other, intellectual type which functions with the help of the second signalling system. Thus, the human brain is composed of the animal brain and of the purely human part relating to speech. It is this second signalling system which is beginning to prevail in man. It can be assumed that under certain unfavourable conditions, when the nervous system is weakened, this phylogenetic division of the brain takes place anew; then probably one individual will use predominantly the first signalling system, while the other will use predominantly the second signalling system. And it is this that divides men into artistic natures and purely intellectual abstract natures.

In unfavourable conditions when this divergence attains a high degree, a morbid manifestation of this complexity of the higher human nervous activity takes place in the form of, so to speak, exaggerated artists and exaggerated thinkers (pathology). I think that the former are related to hysterical persons, and the latter to psychasthenics. I have seen many neurotics. As to the unsuitability for life or inactivity of these patients, it must be said that the psychasthenics are particularly feeble compared with the hysterical persons. This is confirmed by facts. Many hysterical persons become "prominent public men" (for example, the American lady who, being a typical hysterical person, founded a new religion, amassed millions and became famous).[175] On the contrary, psychasthenics, who limit themselves exclusively to words, are in most cases unsuitable for life and absolutely helpless. Of course, there are also hysterical individuals whose activity becomes so chaotic that they, too, cannot find their proper place in life, and who are nuisance both to themselves and others.

I put myself the question: and what about our animals? The existence of psychasthenics among animals is excluded, since they do not possess the second signalling system.

In the final analysis, all complex relations in man have passed into the second signalling system. Verbal and abstract thinking has been elaborated in us. The second signalling system is the most constant and ancient regulator of human relations. But there is nothing of the kind in animals. Their entire higher nervous activity, with its supreme manifestations, is included in the first signalling system. In man the second signalling system acts on the first signalling system and upon the subcortex in two ways: in the first place, by inhibition which is greatly developed in it, and is absent or almost absent in the subcortex (and which, it can be assumed, is less developed in the first signalling system); in the second place, by its positive activity—by the law of induction. Since in man the activity is concentrated in the region of speech—in the second signalling system—its induction must act on the first signalling system and the subcortex.

Such relations cannot exist in animals. But they can assume this form when the inhibitory process in the first signalling system (which in animals stands above the subcortex) is weak. Since the first signalling system in animals is also the regulator of the subcortex, a relation essentially similar to that in hysterical persons may arise; if the inhibitory process in the first signalling system of the animal is weak, the subcortex falls into a state of violent excitation and its activity does not correspond to the action of external stimuli. Consequently, something analogous to hysteria may also take place in animals. While in man we meet with pressure of the second signalling system on the first signalling system and on the subcortex, in animals it is the first signalling system which exerts pressure on the subcortex. Essentially this is the same thing, but in the second case there is a single source of inhibition, while in the first case there is a double source (partly the positive system and partly the intense activity).

This thought occurred to me when observing one of the dogs in Koltushi; this was "Verny," a truly violent, impetuous dog, a typical watch-dog, which would not let anyone but his master approach him. He also exhibited a violent alimentary reflex. For a long time we have not been able to obtain in him a more or less stable system of conditioned reflexes. This is similar to what was observed in castrated dogs by M. K.[176] No dependence on the intensity, no complete differentiation, and quite often manifestation of the ultra-paradoxical phase. The course of reflexes in the period of retardation, i.e., in the period of the isolated action of the conditioned stimulus, is also of definite interest. During the first five seconds the dog exhibits an abundant secretion of saliva, and during the subsequent five seconds—complete absence of salivation (zero). I am ready to affirm that this is a hysterical dog, in which the first signalling system regulating the nervous system and the energy of the subcortex is absolutely feeble. There is no correspondence here between the action of the signalling system and the emotional fund of the subcortex. This is proved by the fact that when we reinforced the inhibition in the first signalling system (by means of bromide), a certain order began to set in. The administration of a large dose of 6 grammes resulted in the elimination of the chaos to a very considerable degree.

And so it is possible to regard "Verny" as being a really hysterical dog, since he does not possess in any sufficient measure the vital regulator of the subcortical emotional fund.

[EXPERIMENTS ON APES AND CRITICISM OF KOEHLER'S CONCEPTS]

EXTRACT FROM THE STENOGRAPHIC RECORD OF THE "WEDNESDAY," HELD ON JANUARY 9, 1935

Academician *I. P. Pavlov:* ...Now I shall busy myself with Koehler and our apes. The point that Mr. Koehler fully disregarded is to us, on the contrary, of special importance. The way in which the ape makes acquaintance with the surrounding world is of no interest to him He ignores this, while we concentrate on it. When the ape sits without doing anything, he is, probably, resting, but not meditating, as Koehler has it. We see how "Raphael" makes expedient acquaintance with the surrounding medium. Under the influence of alimentary excitation he explores the conditions of the external environment.

"Raphael" was solving a rather complicated problem —the piling up of boxes of different size with the object of reaching suspended food. The dimensions of the boxes varied in the proportion of 1 to 16. The boxes had to be so placed as to form a kind of stable stairway. The structure was a fairly high one—three and a half metres. But "Raphael" solved this problem before our eyes. He realized that the surfaces of the boxes should coincide as much as possible, and that the boxes should not be placed on their edges or angles. He assembled them by the method of trial. The experiment lasted about two months. Now "Raphael" builds quite well. The structure must be erected at the place where the fruit is suspended. And "Raphael" builds directly under the suspended pear, placing all the boxes in the proper order: the first, then the second, etc. When the boxes are scattered, he reassembles them and places them in the required order. Can there be any doubt that this is similar to our thinking? Koehler, however, ignores this.

Carried away by our experimentation, we are now enlarging in every possible way "Raphael's" "naturalist researches"; our assistance takes the form simply of diminishing the adventitious elements, that is, in creating certain favourable conditions.

We experiment with a fire which bars the ape's way to the food. "Raphael" rapidly considered the situation; he burnt and licked himself at the first unsuccessful attempts. His method was quite clear: he made use of various solid objects, chips, nails, etc. When the food was placed in the centre of a circle formed by burning candles, he knocked the candles over or blew them out. Recently he has learnt to extinguish fire with the help of water. Here is how he did it.

A vessel containing water was placed in a box. A tap connected with the vessel was attached to the top of the front part of the box. Fruit was placed in this box and was visible through an opening in the front. Just below the opening there was another small oblong vessel resting on a support; spirit was poured into this vessel and a match applied to the wick. The flame prevented the ape from getting the fruit. "Raphael" had to extinguish it. He tried one way, then another, and still another. Finally he noticed the tap, seized it and turned it. Water poured from the tap—the opening was so arranged that the water flowed direct on the vessel containing the spirit. After one or two repetitions "Raphael" began immediately to turn the tap at the right moment. In this way we came to his aid. When he turned on the tap for the first time he did it not with the purpose of setting the water in motion. However, he subsequently related the action of water to the extinction of the flame. And when the tap ran dry he took a bottle with water and poured it on to the flame. Is not this convincing enough?

In this way we shall acquaint "Raphael" with the properties of different phenomena and with the relations between them. And he will be able to make use of them. Koehler, however, ignores this, although it is the essence of the matter. This is the genesis of our thinking, which enables us to function. What distinguishes "Raphael's" experience from our own experiments, when we try now one way, now another until, finally, we establish the proper connection. Is there any difference? In my opinion, there is none.

After reading about the intellect of anthropoids, and witnessing these experiments I cannot understand how a psychologist, a man who deals with the problems of thinking, can overlook all this, and at the same time advocate the nonsensical idea that when the ape stops working he meditates just as we do. Is not this a peculiar way of thinking, of treating the subject? Yet, that is the way things are, and that is the way they remain. For some reason, our physiological interpretation of these phenomena does not concern the psychologists in the least.

Koehler's latest book appeared in 1933, under the title *Psychologische Probleme.*[177] I have not read all of it. Part one is entitled "Behaviourism," part two—"Psychology and Natural Science" Koehler delivers broadsides against behaviourism. He mentions in passing that the behaviourists have accepted our conditioned reflexes with great enthusiasm. He makes this reference to conditioned reflexes: "I presume that the research carried out by Pavlov and his school is known." Just a single line! Thus, although he knows of our experiments, he does not say a single word about them; on the contrary, he vilifies them in every possible way.

He violently attacks the behaviourists. He affirms that the latter are guided by two commandments: "In science one must never admit any phenomenal world!", that is, must not admit our own manifestations as subjective phenomena. And further: "In the nervous system one must never rely on any functions except reflexes and conditioned reflexes." I am not sure whether he deliberately exaggerates this or not. But here is something which concerns us: "Observers are unlikely to find the reflexes and conditioned reflexes closely related to the study of complex forms of behaviour of animals and men or worthy of attention."

How do you like that? This means that they are so remote from the study of the behaviour of animals and man that he can hardly them *in Betracht nehmen* or consider them to any degree *nächst liegender*—closely related.

What strange blindness to affirm that this is not "nächst liegend" and "nicht in Betracht nehmen," when everybody knows that all the habits, all the connections (omission in text of the stenographic record)....

... "But those who are firmly convinced (he implies the behaviourists and us) that the original theory of conditioned and acquired reflexes is the whole truth (we have never claimed this for the nervous system), have no grounds whatever for observing natural behaviour. They will have to begin a new study; they have no other functional notions."

How absurd! How can a professor of Berlin University, not an old man living out his days but a young man in the prime of life, talk such nonsense!

Whereas every one of our experiments is aimed at enlarging our concepts, he thinks that our knowledge of reflexes precludes any aspiration for further knowledge. This is strange, something truly amazing! And yet he pretends to know our conditioned reflexes. What is one to make of it?

For some inexplicable reason our way of interpretation is regarded as being "conservative." But how is that? How can it be "conservative" if lots of people argue with us and show no desire to comprehend our viewpoint? They regard our views as something monstrous, something they cannot accept.

"On the other hand, these conservative ideas are upheld and protected by the adherents of Pavlov and by all behaviourists, since they limit observation." This means that nothing more is desired by us. How could he reach such an absurd conclusion? "All the reactions of the animal nervous system are reduced to a couple of reactive forms—to conditioned and unconditioned reflexes."

Such is his attitude towards our conditioned reflexes. Please, tell me what it all means. As for me, I don't understand it. I have learnt from F. P.[178] that the author lectures on psychology at the theological faculty of Berlin University. Evidently that is not a place where our point of view is likely to be accepted. And that, I think, is the sole explanation of his thoughtlessness.

There are even more amazing and still less comprehensible things. In the chapter "Psychologie und Naturwissenschaft" he regards the naturalist hypothesis as a working hypothesis only, but, at the same time, audacious. He begins with the statement that our subjective world and our emotions can and must be subjected to observation, but it is useful to systematize them, and then, on the basis of physiological data, somehow to superimpose the system of our subjective emotions of this objective system of physiological facts concerning the physiology of the nervous system. That is quite correct. Our aim is to study the objective, purely physiological facts, whereas the aim of psychology, if only it is capable of considering and comprehending this subjective world, is to superimpose one system on the other, which is what we are trying to do. We explain the phenomena of our subjective world on the basis of our physiological data. But imagine, his system is exactly the same. For he says that he has every ground to observe our emotions, our subjective states, to systematize them, to superimpose them on a corresponding physiological system and to establish a connection between them. Apparently he is aware of our research, since the results have been published in foreign languages. Nevertheless, for him all this is but a working hypothesis, an audacious hypothesis, and he adds the following, which seems to be his criticism: "We only see how on the basis of general ideas it is possible to deduce the real system of personal emotions superimposed on the structural properties of corresponding cerebral processes." This, it appears, is his criticism. But with us this is a constant fact; we know plenty of subjective manifestations that can be connected with objective data. Even in conversation during a private visit to him I told him how I interpreted the fact cited by him about a dog kept in a cage but within sight of meat which was placed beyond the grille. When the meat was placed at a distance from the cage, the dog immediately found a way out of the cage and took the meat. But when the meat was placed near the cage and greatly excited him, he behaved stupidly, trying all the time to get the meat through the grille. This means that a strong stimulus apparently produced negative induction. But for Koehler that is but an audacious hypothesis. He concludes: "... The system of personal emotions superimposed on the structural properties of corresponding cerebral processes, which are of decisive importance for the interpretation and observation of the behaviour" ... and adds: "doch solang bis jetzt nicht beobachtet worden," i.e., which so far, however, have not been observed. What does this mean? Please explain it to me. I simply cannot understand him. The only possible explanation is that the torments of animism, deeply rooted in him, make him inconsistent, slow-witted and contradictory. There can be no other reason. I have met any number of medical people who were simply incapable of realizing that the entire behaviour of a patient could be explained without recognizing the active and independent role of the inner world. They could not understand how it was possible to take into consideration exclusively the influence of external stimuli, their summation, etc. This, I think, is the sole explanation of his highly inconsistent conduct.

Gentlemen, I recommend those of you who know German to read this book and express your opinion. I can understand this only as the torments of an animist obliged to adopt a scientific point of view. The spirit of the times demands it of him, but he lacks the necessary inner resources....

When I visited Koehler in Berlin I was astonished at the reluctance with which he acceded to my explanations of the behaviour of his dog: "Yes, yes," he murmured making an obvious effort.

There is no need, however, to look for examples so far away. I had a close friend, a psychiatrist, to whom I ardently demonstrated our principles. I used to visit him on Sundays after my laboratory work. This lasted for several years. However, up to his death he was convinced that we were committing a grave error, since we did not take the inner world of the dog into consideration. And this man was a psychiatrist who knew very well how our soul changes and breaks when the brain is disordered. Such is the force of a habitual point of view.

This can be explained only by the fact that in the present case a fierce struggle takes place against the deeply-rooted prejudices of human thought in the form of dualism. It is an interesting book, don't fail to read it. It contains glaring contradiction and inconsistency. Many interesting things will come our way when our interpretation of the behaviour of apes is published.

[CRITICISM OF KOEHLER'S IDEALISTIC CONCEPTS]

EXTRACT FROM THE STENOGRAPHIC RECORD OF THE "WEDNESDAY," HELD ON JANUARY 23, 1935

Academician *I. P. Pavlov:* ... Now, gentlemen, we shall pass from peaceful affairs, if we may say so, to matters of war, to Mr. Koehler. We are at war with him. This is a serious struggle against psychologists. Koehler is professor of psychology at Berlin University. A scientist of minor authority would hardly be elected to a chair in Berlin University; they respect hierarchy there. They look on Koehler as an outstanding psychologist. I have been in his laboratory which is located in Wilhelm's palace. Don't trifle with it!

When I read his *Psychologische Probleme* published in 1933, I was about to write an article on our experiments with apes. It was my intention to say something in the preface about the Gestalt psychology, and I even wrote on this subject.

Here it is:

"The most important and indisputable of the oldest acquisitions of psychology as a science is the establishment of the existence of connections between subjective phenomena—the association of words, the most obvious phenomena—and then the connection between thoughts, emotions and impulses to action. One cannot but be surprised, therefore, that nowadays this scientific merit of psychology is depreciated or greatly belittled by a new fashionable trend in psychology—the Gestalt psychology. The fact of association, established by the psychologists, becomes all the more important since it fully coincides with the physiological fact of temporary connection, the formation of paths between different points of the cerebral cortex; thus, it represents a fundamental case, a moment of contact, or to be more precise, a synthesis, identification of the psychical with the somatic, of the subjective with the objective. This is an event of great significance in the history of human thought, on the horizon of the single and exact human knowledge. The attitude of the Gestalt psychology is an obvious misconception."

Such was my opinion when I read his book.

That which is correct in the book is as old as the hills. It is doubtful if the psychologists-associationists ever numbered in their ranks anyone who regarded the world of subjective phenomena with their endless interconnections as a sack filled with apples, cucumbers and potatoes which do not exert any action one upon another. The psychologists-associationists know very well that different combinations of three elements alone—oxygen, hydrogen and carbon—originate countless systems in the form of different substances, each with its peculiar properties. And the eduction of elements together with their diverse synthesis enable the chemist to penetrate deeper and deeper into the structure of our planet as a gigantic hole. The animal organism, ours included, is, in like manner, a closely interconnected whole. Is not its study made possible thanks first and foremost to its decomposition into larger or smaller units and to their subsequent intermittent composition? Why, then, should the product of the higher animal organism, the phenomena of our subjective world, be studied by other methods which exclude decomposition and analysis? Precisely for this reason the new trend in the Gestalt psychology, its violent opposition to associationism, is an obvious scientific error. The unmerited success of this psychology among modern psychologists is to be explained solely by the fact that dualism still makes itself felt in their midst; dualism is manifested in the form of animism which admits the existence of a peculiar substance opposed to the rest of nature and therefore requiring special treatment on the part of researchers compared with material phenomena.

My following categorical statement also bears a direct relation to this subject: "There is but one way to a truly scientific comprehension of phenomena in psychology—the way of analysis."

That is my opinion of the Gestalt psychology. Thinking it over, it struck me as being a very harsh view, since the conclusion might be drawn from it that that which is old is true, while the new is worthless. So I resolved to re-read the book. As is my habit, I carefully read over and over again the chapter specially devoted to association.

And I must say that this chapter greatly perplexed me. In my view it reveals utter thoughtlessness and inconsistency. . . .

Undoubtedly, close contact has been established between our physiology of the higher nervous activity, in the form of the theory of conditioned reflexes, and psychology. We are studying one and the same problem. Of this there can be no doubt. But whereas from the factual point of view our concepts and notions are fully grounded, practically indisputable, theirs are not. I should like to attach great importance to this fact, clearly emphasizing that in some things physiology at present offers more truth than psychology, that is, if Koehler is to be regarded as a serious psychologist.

Koehler considers the entire problem in its historical aspect. He focuses attention on the fact that it is much more difficult to memorize a series of meaningless syllables than those which make sense. He is unable to deny this fact. It has been affirmed by competent psychologists whose authority is beyond all doubt. Unable to refute this fundamental fact, he switches to the factors which contribute to this association. There are many of them. Since connections are ready to hand, the association is either found at once or fixed quite easily. Koehler bases all his objections on the fact that the existing connections contribute to the formation of the given connection.... But that goes without saying. The old connections constitute, according to Koehler, a gestalt, i.e., a system of organization.

Summing up we can say that wherever there is a solid organization, combination or a gestalt right from the outset, there is, naturally, a ready association. But where there is no proper organization from the outset, the association is absent; it must be elaborated.

Koehler then passes over to physiological notions. Generally he admits the formation of paths between two excited centres of the cortex. "This hypothesis may enable us to understand why the excitation, after some repetition, takes such a definite direction and thus augments the conductivity of the connected fibres. On the contrary, one cannot see—'Sieht man gar nicht'—why the stimulus takes this direction from the very first."

Why does it take this direction from the very first? What do you think of that?

It reminds me of a passage from *Nedorosl** where Prostakova begins to argue with the tailor; when the latter explains that he spent much time in mastering the art of tailoring, etc., she counters with the peremptory question: "Tell me, then, who taught the first tailor?"

What does this mean? How is it that an intelligent man, a professor of psychology, cannot grasp the matter, cannot comprehend it? His attitude differs in no way from the question "Who taught the first tailor?"

Can any of you, gentlemen, dispute this? How can one say that no coincidence is required, that the gestalt is formed of itself, at one stroke!

And here is another of his stunts.

He says that the idea of a path formed as a result of numerous repetitions is obsolete, that now there is a new hypothesis: when two centres somehow fuse, the tonus of one cell is communicated to the other, and this forms a gestalt system, an organization—instead of two systems there is now one. But this means that the association forms the gestalt and not vice versa.

He, however, draws the conclusion: "The new concepts of Woodworth are invalid. Association as a special independent and theoretical notion loses all significance." What do you make of all this? Explain it if you can.

It is precisely the process of association that Koehler describes. The activity of two cells, previously detached, is fused into a single system because of a coincidence in time. Consequently, this is association. But according to Koehler, there is no association at all.

In my opinion this is complete misconception. I fail to

* Comedy by Fonvisin, 18th century Russian satirist.—*Tr*

see in it any sensible human thought, any impartiality and logic.

Further, he cites the example of meaningless syllables, which, when repeated one after another, are connected and memorized with great difficulty, while many other things are grasped in a flash and retained in the memory. But everything depends on the conditions, as well as on the old connections. Is there anything incomprehensible in it?

The next point directly concerns us and is, therefore, of special interest to me. I would ask you to look at it closely and to try to understand it.

"From our point of view conditioned reflexes would sound better than associations. However, I cannot regard this notion as being more fundamental than the notion of association. It can even be said that the so-called conditioned reflexes are simply particular cases of association."

But that is exactly the case; this is not something that can be said, but which must be said,—"since, evidently, the stimulus indirectly connected with the reflex reactions becomes a stimulus only when it begins to act in connection with an adequate stimulus evoking the same reflex in a natural way. Thus, an association of two sensory processes takes place."

So far his point of view coincides with ours.

Further, we read: "This association can become so strong that in the final analysis the new stimulus might be fit only to follow the trace of the adequate sensory process, but not to evoke it." What does this mean? What do you think of this Egyptian enigma? And how are we to understand his words that the new stimulus is fit only to follow the trace of the adequate stimulus, but not to evoke it? Can you explain this physiologically or in any other way?

N. A. Podkopayev: Maybe he wanted to say that the conditioned stimulus does not wholly reproduce the picture evoked by the unconditioned stimulus, that its effect is somewhat weaker, and the reaction not so intense.

I. P. Pavlov: But he literally says: "nicht diese nachrufen." He speaks of our facts but in a way that it is impossible to comprehend him.

E. A. Asratyan: Perhaps he wants to say that an extraneous stimulus no longer evokes the orienting reaction, previously evoked by it, but a conditioned reflex.

I. P. Pavlov: He mentions a true reaction, conditioned by an adequate stimulus, and says that it follows the trace of the adequate stimulus, but does not evoke it.

E. A. Asratyan: Maybe it is a misprint. (*Laughter.*)

I. P. Pavlov: That's a pretty poor defence. This is truly astonishing!

Nevertheless, fundamentally the question is of definite importance. This is a real struggle between psychology and the physiology of the higher nervous activity.

I should like to see this book translated. We would then be able to circulate it widely and invite the psychologists to read it. Let them come here and defend one of their leading men. Is Zeliony here? (*Voice:* He is absent.) What a pity, I'd have given it to him.

E. A. Asratyan: The thing is really absurd.

I. P. Pavlov: Our task is a very definite one; we clearly see that due to association a system, an organization, or, in Koehler's terminology, a gestalt, arises. Consequently, it is the associations which form the gestalt and not the gestalt which forms the associations. The latter concept is absurd. Recall, for example, our delayed reflex. Is it not a gestalt, a system, in which one and the same stimulus first acts in an inhibitory way and afterwards positively? This is a gestalt, a system, and we know how it was formed. Then take our dynamic stereotype. We apply our stimuli in regular succession. They become connected and, as a result, a gestalt arises, a system, formed by us on the basis of associations. How, then, can anyone deny such an obvious fact?

[CONCERNING THE ANIMISM OF SHERRINGTON AND THE CONSERVATISM OF ENGLISH SCIENCE]

EXTRACT FROM THE STENOGRAPHIC RECORD OF THE "WEDNESDAY," HELD ON FEBRUARY 6, 1935

Academician *I. P. Pavlov:* ... Here is another interesting fact concerning the general significance and interpretation of our work. When the German edition of my lectures on the higher nervous activity appeared, a characteristic notice, written by one of Sherrington's adherents, appeared in the English magazine *Nature*. It begins with different compliments and then says that the correctness of the interpretation accorded this vast and grandiose material is open to doubt. For this reason, it continues, some people doubt whether the Pavlovian terminology can contribute to clear understanding of the matter. It is possible that in view of the present state of our knowledge it would be more advantageous to interpret these discoveries in psychological terms, such as association, distraction, interest, consciousness, attention, memory, etc.

What do you think of that? They themselves erect this structure, fully convinced that they are doing useful work. Sherrington himself investigated the reflex activity of the spinal cord, but he is decidedly against attributing this activity to the higher parts, to the brain; in the latter case this structure becomes in their eyes hypothetical.

This is animist reasoning. Sherrington has built a nest of animism. This is proved by the fact that he doubts whether the mind has any relation to the nervous system. Hence the mind is something beyond and above the nervous system, something that can be detached from the nervous activity altogether.

I can understand the influence usually exerted by a teacher on his pupils. But must all the pupils necessarily be animists if their teacher is an animist? Is there really such intellectual serfdom among Englishmen? How are we to understand this reasoning? The man who makes it is one of Sherrington's adherents, he also cites his colleagues. And he affirms that it is better to systematize from the psychological rather than from the purely physiological point of view. This is all the more astonishing in view of the fact that the conditioned reflexes have won particular success in England, where they are even included in the programmes of the secondary schools.

In my view the stand taken by Sherrington is manifestly harmful, since he trains such disciples. He is at liberty to think as he pleases, but why should he confuse others?

No, we can confidently rely upon our conditioned reflexes.

Good-bye.

[CONCERNING THE IDEALISM OF PIERRE JANET]

EXTRACT FROM THE STENOGRAPHIC RECORD OF THE "WEDNESDAY," HELD ON FEBRUARY 20, 1935

Academician *I. P. Pavlov:* ... I read now Pierre Janet's latest book *The Sources of the Intellect.* Pierre Janet is a remarkable man. He is not a physician, but a psychologist, and at the same time a celebrated neurologist. He is, undoubtedly, an outstanding personality. I shall deal with the essence of his book next Wednesday. The book itself, in conception and analysis, is most interesting. I shall devote more time to it, since it deals with the highly important problem of correlations between the physiology of the higher nervous activity and psychology.

I am waging a violent war against Pierre Janet as a psychologist. And in my next talk I shall do all in my power to deal him a shattering blow. But as a neurologist he is of great interest. He has collected a considerable quantity of extremely interesting and important pathological cases. I am sure that as a neurologist he will always remain in the memory of science, but as a psychologist, I think, he will be discarded as a result of our work, the work of the physiologists of the higher nervous activity.

Janet describes two extremely interesting pathological cases of which the first is as follows.

It relates to a lady who was in a state of great exhaustion after the pains of childbirth. While travelling by train to her destination she was tormented by the thought that she was going in the opposite direction, although there were no grounds whatsoever for this, and her fellow-travellers confirmed that the train was going in the right direction.

What does this signify? This is a pathological phenomenon, a kind of obsession. It is one of the variants of the series mentioned earlier. Suppose, for example, that the patient wants to be respected, but he has the impression, without the slightest reason, that he is being insulted. Or he wants to be alone and secludes himself, but is convinced that there is someone in the room. As I have already explained, this is the ultra-paradoxical phase. And all these phenomena are categories of opposition. We have here a fundamental stimulus in the form of the idea: I am travelling in a definite direction; then comes the hypnotic phase: the monotony of stimuli acting in the car. Exhaustion of the nervous system as a result of a difficult delivery is also in evidence. All this brings on the ultra-paradoxical phase and the rise of an opposite idea, or distortion of the fundamental idea. For example, the idea: I am alone, becomes reversed: I am not alone; the idea: I am respected or I want to be respected, turns into its opposite: I am not respected. The thought: I am going in the right direction, assumes a contrary meaning. In my open letter to Pierre Janet I gave my interpretation of this phenomenon. It is an old story and there is nothing special in it.

The second case interested me particularly.

It concerns a French officer who during the war was wounded in the occipital region of the brain. The bullet passed through the posterior cerebral part and became embedded in the opposite side. For some reason or other it was impossible to remove it.

The officer lost his sight. It was restored later only to be followed by the so-called "physical blindness." He could see, but could not understand; this is the so-called "Munk blindness." At a later stage he began to comprehend everything he saw, namely, that a man is a man, a table is a table, etc. Thereafter his visual comprehension became extremely concentrated, and the following occurred. I quote Pierre Janet: "The patient enters my consulting room leaning on the arm of a soldier, since he is convinced that he cannot walk without assistance. He recognizes me, greets me amiably and correctly, seats himself in an armchair and immediately begins to complain, unburdening himself of an "extraordinary grievance." This is what he said: "I am most miserable, because I have lost the ability to orientate myself in the world. I never know where I am." These are his exact words. They mean complete absence of orientation in space. . . .

This is an extremely interesting case, but how to interpret it? Basing myself on our observations I made two suppositions. This, apparently, is a matter of the occipital region, of the patient's visual relation to the surrounding world.

The same phenomena which we observe in our "Rebus" are manifest in his visual region: this region is inhibited to such a considerable degree that it cannot endure two simultaneous stimulations. You will recall that "Rebus" was unable to form more than one conditioned reflex—a stronger reflex destroyed the weaker one, for example, the defensive reflex destroyed the reflex to acid and the latter destroyed the alimentary reflex.

Consequently, the visual region of the brain has such a low tonus of excitation that it is able, influenced by the given stimulus, to concentrate its activity at one point only, while all other points remain as if they were non-existent. Hence, the patient sees a distinct person, a distinct object, but he is unable simultaneously to perceive anything else, since the notion of space escapes him. Everything is confined to the point which is stimulated at the given moment. There are no traces whatever and that is why the patient feels "lost in the world"....

The fact that there is a complete or almost complete absence of traces in this officer is very interesting. Due to a low cortical tonus, only the existing stimulations are effective in him; when he is subjected to certain stimulations, the inhibition spreads to the other parts of the analyser. The remainder disappears from his consciousness. And this is what gives him the feeling of being "lost in the world."

In the few minutes left to me I shall tell you something that may interest you. Next Wednesday I shall make a general attack upon Pierre Janet; today I shall confine myself to a brief statement about him.

Of course he is an animist, i.e., he believes in a specific substance which is not subject to any laws and which is unknowable. In his explanations he refers to Bergson,[179] a rather violent French philosopher.

He writes: Bergson has presented us with a very beautiful model in order to make us understand how nature could create such a miracle as the eye. To us the eye seems to be extremely complicated, and we are inclined to think that in order to comprehend it we must accumulate fact upon fact and combine them in various ways. However, when I want to lift my arm, it is not necessary for me to analyse this or that organ, this or that nerve or muscle in order to have the desire to do something with its help. All that is needed is the desire to act and everything fits into place. The living substance longed for light, wanted to grasp light, and this desire took shape in the eye.

He literally states: "This desire took shape in the eye." "This is a creative force, a substance of great power."

And further: "We have lost much of this primitive power, but we still use some of it in our imagination." We use the negligible remnants in our imagination! Does he, then, resemble us? Is it possible to agree with him? Of course, not. According to him, the imagination is a particle of the creative force which has resulted in my eye!

[EXPERIMENTS WITH "RAPHAEL"]

EXTRACT FROM THE STENOGRAPHIC RECORD OF THE "WEDNESDAY," HELD ON MARCH 6, 1935

Academician *I. P. Pavlov:* ... Now I should like to say a few words about our apes.

As you know, "Raphael" has added greatly to his knowledge of the surroundings. He has learned to open locks with the help of the corresponding tools. This is an old accomplishment, but he has become quite proficient in the art. He has learnt to appreciate the significance of the keyhole, to insert the key and to turn it. Now he does this quite easily. He has learnt to put out fire by means of water—his own "scientific invention." He is now able to construct a tower with the help of cubes, arranging them in the form of a staircase, and to climb it. This was not acquired at once; many difficulties had to be surmounted.

He has elaborated numerous more or less elementary associations. Now he has been given the more complicated task of forming an association of associations.

This involves opening the door with the key, entering the room, extinguishing the fire blocking the way to the landing; then he must get out of the room and build a tower on the landing in order to reach a suspended fruit. In this way he is called upon to effect an association of associations.

It is interesting to note that he usually performs all the operations without a hitch until he gets to the landing where he sprawls on the boxes and only after some time does he begin to build the tower. This repeats systematically. Obviously it is a difficult mental effort for him and greatly fatigues him. Hence, rest is necessary. This fact is absolutely clear.

We have known for quite a long time that our conditioned reflexes also represent nervous work. We know also that a dog which prior to castration responds perfectly to our complex system of conditioned stimuli, is unable to cope with the same system after castration. It needs rest.

Thus, you see that we are penetrating deeper and deeper into the higher nervous activity, and we are now dealing with its rather complex manifestations.

[CRITICISM OF CLAPARÈDE'S BOOK *THE GENESIS OF THE HYPOTHESIS*]

EXTRACT FROM THE STENOGRAPHIC RECORD OF THE "WEDNESDAY," HELD ON MARCH 27, 1935

Academician *I. P. Pavlov*: ...Now, gentlemen, let us turn to the psychologists. They are, assuredly, experts at playing with words. But they fully disregard facts, and are an exceptional type of thinking people.

I have received from a very amiable psychologist a copy of a new book. The author, whom I have met on several occasions, is the permanent secretary at all international psychological congresses. His name is Ed. Claparède—Geneva psychologist. He has sent me his book, *The Genesis of the Hypothesis*. I have read those parts which directly concern us. What a strange thing it is to use the word "mind" without having any idea as to what it actually signifies! How can I speak of the mind if I do not know its real meaning?

He begins with the words. "According to different authors, the essence of mind..." and then he enumerates the definitions of mind given by different authors.

One psychologist regards mind as the knowledge of the aim to be attained, another as ability to combine, a third as the power of abstraction, a fourth as ability to form a correct judgement—the latter being a particularly clever definition, etc. It is also defined as the formation of a general idea, as the faculty of analysing and synthesizing, of comprehending, inventing, making tools, utilizing experience, learning, giving the right answers from the point of view of the truth, predicting with high precision, knowledge of the relations between phenomena, and so on—there is no end to the definitions.

"If we wanted to come to common understanding about these definitions (not counting all others), we should never be able to do so, moreover, we should never begin the empirical study of the act of intelligence." This is interesting, but the author himself could not refrain from giving a new definition: "The concept of the new situation" seems to me to be essential to the definition of mind; if the situation or the problem which is to be solved were not new, there would be no question of mind, but of some other process: memory, habit, routine, repetition, etc., in brief, automatism. "Our definition is in close harmony with the general usage which opposes the mind to instinct and habit."

He then begins to expound his definition which for some reason or other he considers better than all others. It is really amazing: They heap up words and cannot agree on their meaning. I am greatly surprised at this because I happen to know that many years ago the Americans displayed purely American daring and wanted to compose a psychological dictionary. But, as conditions were then, this was an impracticable task. For a long time no progress was made with the dictionary which passed through the hands of different editors. Finally, an energetic man was found; this was Warren, he is now dead, I think. Warren completed the editing of the dictionary, but it is not worth spending money on it. It is no good, being a complete failure.[180]

I shall read to you the author's judgement on our conditioned reflexes. You will see his lamentable juggling with words. One cannot but shrug his shoulders in amazement.

First of all, he coined a new term to designate our conditioned reflexes. I do not know whether he uses the term implication for the first time or whether it has been used by others as well. This is a Latin word. In his terminology our conditioned reflexes are not associations but implications.

Here is what he writes. I shall take up some of your time, gentlemen, because he devotes three pages to the point.

"Implication is a process which is indispensable to our needs of adaptation. Without it we would not be able to avail ourselves of our experience. Our life would resemble Sisyphean labours: no acquisition would help us properly to choose the mode of further behaviour. Indeed, what would actually occur if we did not tend to ascribe necessity to the combinations and connections which arise before us, if we were not inclined to regard as indispensable attributes the qualities of the object we meet for the first time? How would we react to it if we met it a second time?"—Do you get the point?—"For example, we find a fruit in the forest and taste it. It is sour and unpleasant. Our spirit does not limit itself to associating the acidity with the form and colour of the fruit to a degree that would evoke memory of the acidity upon meeting the fruit again." Thus, you see, for some reason it does not limit itself. It would seem that this is precisely what happens, that we do recollect that the sour taste is connected with the appearance. But Claparède affirms that it "does not limit itself."

... What does this mean? How can this be brought into accord? We recall that the acidity in question is connected with the form and the colour; however, according to Claparède, the matter is not confined to this.

Further, he states: "If this implication were not itself implicated in the first relation experienced by us, then what basis would there be for a reaction in future?" What is this? Word-play? Instead of saying that these phenomena are interconnected, he affirms that if they were not implicated in this relation we should have no basis for further reaction! This is something incomprehensible.

A veritable deluge of words follows.

"Implication is based on the law of reproduction of the analogous which expresses the fact that the individual tries to repeat the previously favourable reactions of the past, to repeat them under identical or analogous situations. At the same time implication is a principle of generalization and induction which takes place in accordance with the law of reproduction of the analogous."

Reading this one would say: "Good gracious! What profound wisdom! It passes my comprehension." In reality, however, this is sheer nonsense, nothing but a cloudy haze. I beg your pardon, later on you will see this for yourselves. The ordinary man would think: "Evidently I lack education. That is why I know nothing about this and cannot understand it." But my conviction is that this is simply playing with words....

"The reaction to a new situation on the basis of old experience—'experience meaning association'—clearly shows us that implication roots in the motor layers of being." What is one to make of all this? (*Laughter.*) He neither explains nor proves anything, all he does is to trot out a phrase!

But there is worse to come: "One can say that life implicates implications." Indeed, this is an intolerable word-play. What does it mean?

... "Implication is not a tardy, evolved and superior phenomenon ... this is clearly demonstrated by the conditioned reflexes." What do you think of that? Are not all conditioned reflexes gradually formed, developed and reinforced before our eyes?

"Usually they are regarded as a peremptory argument in favour of the doctrine of association." He is most anxious to uphold this association. And without a moment's hesitation our conditioned reflexes and associations are bundled into implication, with the result that we get not association but implication.

I have read the three pages and see no grounds for making any distinction between implication and association, especially since he is speaking about our facts.

"While implication is determined by the notion of adaptation, it is governed by the need of adaptation, it produces a certain action. To implicate means to await; and this, in its turn, means to strive for that which you await." Is not this sheer twaddle? Gentlemen, there are many of you here. Who can find in these three pages the slightest grounds for making any distinction between associations and these implications? So far as I am concerned I fail to see any, although I have read the passage more than once.

E. A. Asratyan: The main thing is that he has not understood the conditioned reflexes.

I. P. Pavlov: No, that is over-simplification. I cannot agree with you.

... Undoubtedly, this is a special breed of people, a special sphere in which there is no place for genuine thought, where it is always buried in the devil knows what. That is quite clear.

... No, here it is not a matter of lack of knowledge. It is a matter of playing with words. These gentlemen never bother about the real meaning of their words, they are unable to give words a concrete sense. That is the main point. They really have a specific tendency to play with words, while ignoring reality. Our controversy with Claparède has been going on for twenty years. You probably remember that Zeliony translated his first ideas and that right from the very beginning I resolutely opposed zoopsychology. Now the position is this: we accumulate a multitude of facts and systematize them, disregarding psychology completely. All this takes place before his eyes, and he constantly studies it. No, lack of knowledge is out of the question, since our controversy has been in progress for more than twenty years.

It follows that psychological thought is quite a peculiar matter; it does not regard words as signs and does not observe the principle that in using words one must always remember the reality implied by them. But Claparède does not adhere to this rule and has no desire to do so. There can be no other interpretation.

[CONCERNING KRETSCHMER'S BOOK *Physique and Character*[181]]

EXTRACT FROM THE STENOGRAPHIC RECORD OF THE "WEDNESDAY," HELD ON OCTOBER 23, 1935

Academician *I. P. Pavlov:* ...Not long ago I took another look at Kretschmer's book *Physique and Character*. I read it when it first came out; at that time I said more than once that it puzzled me. Kretschmer committed an error (despite the fact that he is a talented man, and possibly even because of his artistic talent) by trying to reduce all humankind, all the inhabitants of the globe, to two of his clinical types: schizophrenics and cyclothymics. Of course, this is a strange approach to the problem: why should the types which predominate in certain disorders and which sooner or later find their way to the psychiatric hospital be regarded as the fundamental human types? Actually the majority of mankind has no relation whatever to these hospitals. That is his obvious error; carried away by the clinic he overlooked the rest of the world.

I failed to understand why all outstanding personalities must necessarily be regarded either as schizophrenics or as cyclothymics. I put the same question to others but nobody could help me to attain comprehension; so I gave up the attempt as hopeless.

Now, ten years later, when typology has made considerable progress, I decided to read the book once more, but the task proved impossible, I had to abandon the intention. It is an absolutely fruitless occupation. His book cannot be comprehended because it is permeated through and through with his fundamental error: he wants to reduce everything to his two types. However, even the dogs showed us that there exist not two, but at least four types. Besides, he never deals with normal individuals, does not think or speak of them.

There is another strange thing about his book He makes no distinction between type and character, and that, too, is a blunder.

Nowadays we firmly adhere to the view that man has inborn qualities and, on the other hand, qualities that he has acquired in the course of life. That is clear. Consequently, if we are dealing with inborn qualities, this would be a matter of the type of nervous system, and if we are dealing with character it would be a matter of a combination of inborn inclinations and those acquired during lifetime under the influence of diverse impressions.

Therein lies his error: he has confused everything; he makes no distinction between the study of the inborn type and the qualities acquired by man in the course of life.

Let us turn now to our dogs. We always relate the study of types to three phenomena: to the strength of the opposed nervous processes, to their reciprocal equilibrium (equilibrated and unequilibrated types) and, finally, to their mobility.

On the other hand, we also possess data indicating the factors which constitute character.

Take, for example, the dog "Ratnitsa." "Ratnitsa" belongs to the strong type, but, as the experiments revealed, her character prevents her from working in an ordinary chamber, since everything distracts her to no purpose.

We can point to another phenomenon of great importance for the dog's character and which imparts to it a strictly definite physiognomy.

We encountered this phenomenon for the first time some years ago. We had two dogs which exhibited a strongly pronounced guarding reflex. They recognized only one person—their mistress—with whom they were most friendly, and who could do anything she liked with them. To all others they were ferociously hostile. But this connection with the mistress manifested itself only under certain conditions.

Now let us take the dog "Ussach," whose behaviour has been thoroughly studied by us. When placed in the stand in an isolated chamber with M. K.[182] in front of him, no one else could approach him. It was an ordeal to sit beside M. K. and attend the experiment. The dog would bark furiously and gave the impression that he would tear me to pieces were he to break loose.

But the moment it was led out of the room, its attitude to people abruptly changed. What convincing proof of adaptation to definite conditions!

At the moment V. K.[183] has a similar kind of dog in his laboratory, and none but he can treat it, since it is ready to bite anyone who tries to approach.

Consequently, this is a particular dog and its behaviour underlies a special trait in its character—its ferocity.

An interesting point is that there is a special condition which reconciles the dog to V. K.—the noose of a rope thrown around its neck with the end held by V. K. At first nobody could approach this animal. Then the noose was thrown round its neck through the grille of the cage, with V. K. holding the end of the rope. It was this act that gave him power over the dog. From that time on he was able to lead the dog, to make it obey orders, etc. Thus you see to what a great degree this is specialized.

In this connection I recall a past impression. In the courtyard of our house in Ryazan we kept a dog in a kennel. Since we wanted it to be a good watch-dog not everybody was allowed to approach it. The janitor alone was accorded the privilege of chaining and unchaining it. It was ready to tear anybody else who would try to approach it. But a dog of this kind rushes at everybody only when it is chained; the moment the chain is taken off, it pays no attention to anyone, it simply enjoys its freedom.

Thus we have here, on the one hand, a pronounced trait of the character, and, on the other hand, an acquired quality.

The guarding reflex is an excellent illustration of a trait of character, but not of type. Similarly, the passive-defensive reflex is not a trait of type, but of character, and is acquired in the course of life.

[THE INFLUENCE OF THE IDEALISTIC WORLD OUTLOOK ON THE ATTITUDE OF SCIENTISTS TOWARDS THE THEORY OF CONDITIONED REFLEXES]

EXTRACT FROM THE STENOGRAPHIC RECORD OF THE "WEDNESDAY," HELD ON NOVEMBER 6, 1935

Academician *I. P. Pavlov*: As you are aware—I mentioned this in dealing with the history of the theory of conditioned reflexes—our conditioned reflexes encounter strong opposition in the heads of those imbued with dualism. What is taking place here is a collision between physiological law and psychological law, between the dualistic conception and the monistic conception of man. I am speaking of the fact which I pointed out a long time ago and which I recently included in my lectures at the Institute for Perfection of Physicians. The attitude of some people to our physiology of the higher nervous activity—and who will deny that this is physiology?—is quite different. You probably remember that in my first laboratory for the study of conditioned reflexes one of my colleagues resented our attempts, our new methods of studying the behaviour of dogs. He is still going strong and feels somewhat ashamed when we meet.

On the other hand, the Englishman Sherrington displays similar scepticism. In 1912, in the course of conversation he said to me: "You know, your conditioned reflexes would hardly be popular in Britain, because of their materialistic flavour," because they oppose the dualistic concept. There you have the reason for his unbelief; this is confirmed by the lectures[184] which he delivered last year and in which he manifested his dualistic concept by affirming that man is a complex of two substances: the supreme spirit and the sinful body. Strange as it may seem for a modern physiologist, he clearly says that there is probably no connection between the mind and the brain....

...We must understand that the conditioned reflexes occupy an exceptional place in the world of physiology because there is a dislike for them on the part of many who have a dualistic world outlook. This is quite obvious. The conditioned reflexes force their way to the forefront. They wage a continuous fight against this dualism which, of course, does not surrender.

This is seen in greater or lesser degree from the fact that the conditioned reflexes are accepted by physiologists with a certain reluctance. Strange as it may seem, many physiologists, authors of text-books, do not cite any data concerning our experiments with conditioned reflexes. Not long ago Heber's reputable manual was translated in Moscow; this manual makes no mention of the conditioned reflexes. In view of this, Prof. Shaternikov, the editor, specially commissioned one of us to write a chapter on the subject. The same thing can be observed in other text-books where practically no mention is made of conditioned reflexes. This shows how deeply dualism is rooted in the minds of scientists.

In the category of such scientists one can include, for instance, Mr. Bethe, a rather prominent German physiologist from Frankfort on the Main. I think that his battle against conditioned reflexes has led him to commit a serious error in his work, although, generally speaking, he is an able man. E. A.[185] is now correcting him; this, naturally, will put him to shame and make him remember that his general world outlook should not be brought into scientific thought. For the time being these are quite different things.

Bethe destroyed the extremities of dogs partially or fully and in various combinations.

Naturally, after each mutilation the dogs were disabled for a time, depending on the operation. Subsequently, however, the disability was gradually obliterated, and the dogs recovered their ability to move, sometimes even in a quite satisfactory way; in other words, their locomotion was restored.

But this can be observed in human beings. And, as I have already said, these experiments were undertaken to no purpose whatever. When Bethe described them in Stockholm in 1926, I, sinner that I am, was indignant: what on earth makes you mutilate the unfortunate dogs? It is of no value at all and it does not prove anything.

The entire mass of human beings know this from their own experience. Was it worth while then to cripple twenty or thirty dogs merely for the sake of reproducing this fact?

Bethe's analysis of this phenomenon as applied to dogs is very simple. He ascribes everything to the plasticity of the spinal cord. It is a well-known fact that any mutilation becomes levelled out with the passage of time. But to this he added the words: "This must be ascribed to some mysterious property (since it is without further analysis) of the spinal cord." This is what all his works and all his talk about plasticity is reduced to. I am likewise inclined to see in this a manifestation of dualism. After all, what did he achieve by his absolutely useless experiments? Nothing, and yet he found some followers. There is no gain here at all, only idle talk. The dualistic ardour directed against monism, which manifests itself in our conditioned reflexes, has clouded Bethe's mind to the degree that the idea of the necessity of devoting attention to the conditioned reflexes did not even enter his mind. But, as a matter of fact, everything he said about the plasticity of the spinal cord is fully applicable to our cortical conditioned reflexes as well. Consequently, the first thing that needed doing, had he not been under the spell of the dualistic world outlook, and had he paid attention to our conditioned reflexes, was to put the question, after the dogs had learned to move again and recovered their faculty of locomotion: will they preserve this faculty if their cerebral hemispheres are removed? Had he done so everything would have been reduced to the conditioned reflexes of the cortex. But he did not do that. E. A. did so, and he proved to be absolutely right. All the dogs recover their locomotion with the help of the cerebral hemispheres, i.e., with the help of the conditioned reflexes. If a mutilated dog which has subsequently learned to move anew, is deprived of the cerebral hemispheres, it will become an incurable invalid.

So you see how Bethe plays on words, being fully satisfied with them, how he uses the term "plasticity" and rests content with it.

That is a very instructive fact....

NOTES AND COMMENTARY

I

PUBLIC AND SCIENTIFIC SPEECHES AND ADDRESSES

Being a successor to the progressive traditions of the revolutionary Russian intelligentsia of the 19th century, Ivan Petrovich Pavlov throughout his entire lifetime was a partisan of true democracy; he frequently opposed the reactionary tsarist officials, and fought vigorously for a progressive science free from prejudice.

In the period of Soviet rule Pavlov became an ardent supporter of the new regime created by the Great October Socialist Revolution under the wise leadership of the Communist Party The great scientist became an outstanding public figure. In his brief, but brilliant addresses, speeches and letters he revealed his attitude towards social developments, his firm belief in the great historical role of his motherland, in the greatness of the Russian people

[1] The salutatory message of Pavlov, then President of the Organizing Committee of the First Sechenov Physiological Congress, was published in the Russian Physiological Journal, Vol. I, Nos. 1-2, 1917. Pavlov, jointly with V. I. Vartanov and A. A. Likhachev, spent years in vain attempts to obtain permission from the authorities to convene an all-Russian congress of physiologists. This proved possible only after the overthrow of tsarism. Pavlov was unable to attend the congress because of a fractured thigh, the result of a fall on December 27, 1916. p. 47

[2] The apocryphal phrase "The republic needs no science!" ("La République n'a pas besoin de science!"), usually attributed to the president of the revolutionary tribunal which tried Lavoisier, is, apparently, simply an invention of bourgeois historians; the latter, pursuing their own ends, frequently used this phrase in a tendentious way, to discredit by all means the era when the poor classes of town and countryside in France appeared on the historical arena. K. A. Timiryazev, outstanding Russian naturalist, correctly stated: "The French

people, driven to despair by foreign intervention, treason within and ruin, but ready for new sacrifices, prosecuted in the person of Lavoisier a representative of the hateful estate of tax-farmers, whom they regarded as internal enemies and accomplices of their foreign enemies. Lavoisier was one of the twenty-six fermiers généraux, or tax-farmers, who mounted the guillotine on that day, he paid for the misdeeds of others, for the misdeeds of generations of plunderers who had been sucking the blood of the French people There is no doubt that he was not guilty of their crimes . . ." (K A Timiryazev, *Complete Works*, Vol. I, 1937, p 215) p 48

[3] The letter was written by Pavlov in reply to congratulations from the Academy of Sciences of the U.S S R. on the occasion of his 85th birthday p 49

[4] The letter is a reply to a telegram sent by the members of the Leningrad Sechenov Physiological Society on the occasion of a special session dedicated to the 85th birthday of Academician I. P Pavlov, founder of the society and its honorary president. p 50

[5] Reply to greetings sent by the Presidium of the Academy of Sciences of the U S.S R. on the occasion of I P. Pavlov's election to the post of Director of the Institute of Physiology and Pathology of the Higher Nervous Activity p. 51

[6] The letter to the youth was written by Pavlov in connection with the request of the Central Committee of the Leninist Young Communist League of the Soviet Union that he should say something about the tasks of young scientists. p. 52

[7] The Fifteenth International Physiological Congress was held in 1935 in Leningrad and Moscow. Pavlov was president of the Organizing Committee of the congress and its honorary chairman. p. 53

[8] In August 1935 Academician Pavlov visited his native town—Ryazan. The District Executive Committee of the Soviet of Working People's Deputies gave a banquet in honour of the great Soviet scientist which was attended by representatives of the local community—physicians, teachers and others. This speech was Ivan Petrovich's response to the congratulations p. 58

[9] July 1935. This is what Pavlov said about the prospects of his work. He had just recovered from a long and serious illness. p. 59

II

WORKS ON BLOOD CIRCULATION AND THE TROPHIC ACTION OF THE NERVOUS SYSTEM

I. P. Pavlov's works on the circulation of the blood relate to the earliest period of his activity (1874-1889); they are of great interest since they reveal true richness of observation, perfect technique of investigation, as well as audacity and originality in formulating the problems In this experimental research the great physiologist, long before many foreign scientists, elucidated new and still unknown aspects of the reflex regulation of blood circulation. The fundamental idea of this research was the concept of auto-regulation of blood circulation in the integral organism aimed at maintaining the blood pressure at a definite level corresponding to the given conditions. This concept was confirmed by the discovery of centripetal nerves which reflexly accelerate the cardiac activity and thereby increase the blood pressure; their existence was first established in the work of I. P. Pavlov and V. N. Veliky. This discovery was an important addition to the so-called depressor nerve (i.e., the nerve declining the blood pressure) previously discovered by the Russian scientist Cyon and by K Ludwig, a nerve the excitation of which provokes in a reflex way retardation of the cardiac activity and dilation of the vascular system.

It is worth noting that Pavlov's first works on blood circulation revealed his tendency to treat the organism as a single whole, they also reveal the exceptional importance which he attached to the nervous system in the regulation of the organism's functions

Pavlov's ideas concerning the centripetal (sensory) nerves of the vascular system and of the internal organs, first formulated in these works, were further developed in the works of his disciple, Academician K. M. Bykov and his colleagues.

[10] This abstract of a paper prepared by V. N Veliky and I. P Pavlov was published in the *Collected Papers of the St. Petersburg Society of Naturalists, 1874*, Vol. V, p. 66. The problem of a reflex intensification of the cardiac activity and of an increase of the blood pressure was raised in this paper for the first time. In his student days V. N. Veliky (afterwards professor of the Tomsk University) worked with Pavlov in the laboratory of the prominent physiologist and histologist Academician F. V. Ovsiannikov. p. 63

[11] N. accessorius Willisi—the eleventh pair of craniocerebral nerves. p. 63

[12] Ganglion stellatum—the star-shaped ganglion, a big sympathetic ganglion from which the sympathetic nerves accelerating the cardiac activity originate. p. 63

[13] The inferior laryngeal nerve and the pneumogastric nerve p 63

[14] The article "Experimental Data Concerning the Accommodating Mechanism of the Blood Vessels" was published in *Pfluger's Archiv fur die gesamte Physiologie*, Vol 16, 1877, pp 266-271 This work elucidates the idea of a reflex accommodation of the cardiac and vascular activity. The laboratory headed by Professor A. O Ustimovich was one of the first Russian experimental laboratories in the sphere of physiology. Already this work reveals Pavlov's tendency, so characteristic of all his subsequent activity, to carry out investigations "on non-intoxicated intact dogs" p. 64

[15] Karl Ludwig—outstanding physiologist-experimentalist of the 19th century. The object of his research was blood circulation. Many Russian physiologists, among them I. M Sechenov and I. P Pavlov, worked in his laboratory. p. 64

[16] Curarization—a motor paralysis brought about by the introduction of curare into the organism. Curare, a poison used by Indians on their arrow tips, prevents the transmission of excitation from the nerves to the muscles. p. 65

[17] N. ischiadicus—the sciatic nerve, contains numerous afferent, i.e, sensory, fibres p. 71

[18] The paper "Concerning Trophic Innervation" was read by Pavlov at a session dedicated to the 50th anniversary of scientific and medical activity of A. A. Nechayev, held on December 31, 1920 at the Obukhov hospital. It was published in the "Symposium of Scientific Works Dedicated to the 50th Anniversary of Scientific and Medical Activity of Prof. A. A. Nechayev," Part I, Petrograd, 1922.

The theory of the trophic action of the nerves, i.e., of their capacity to increase or decrease the vitality of tissues, is closely connected with the observations described by Pavlov as early as 1883 in his doctor's dissertation "The Centrifugal Nerves of the Heart" He discovered that along with nervous influences accelerating or retarding the cardiac activity there are nervous influences of the state of the heart's functional activity. Subsequently, the immense experience accumulated by Pavlov as a result of his observations on the effects of operations performed on the internal organs of dogs ("trophic reflexes," according to Pavlov), as well as his numerous observations on the changes which take place in the composition of the saliva under the influence of the secretory nerves, confirmed the existence of a trophic influence of the nervous system.

This theory greatly influenced the development of normal and pathological physiology by Soviet physiologists and clinicians. p. 72

III

WORKS ON DIGESTION

The present edition reproduces the first and the last chapters of I P Pavlov's classical work *Lectures on the Work of the Principal Digestive Glands.* These lectures were delivered first at the Institute of Experimental Medicine, and later repeated in a concise form at the Military Medical Academy Their publication in 1897 brought Pavlov world-wide fame; he was awarded the Nobel Prize in 1904 for his works on digestion—the highest scientific award of that time. Pavlov was the first to apply strict asepsis in physiological experimentation and to elaborate delicate surgical methods, as well as highly complicated operations on different parts of the digestive canal, this enabled him to study the secretion of digestive juices in conditions of normal activity of the animal organism, taking into consideration the finest relations between the functions of the different organs and the influence of the external environment. In the course of ten years Pavlov actually created anew the modern physiology of digestion. At the same time these works marked the beginning of a new surgical trend in experimental biology. They have played and still play a prominent role in the solution of important practical problems in medicine and animal husbandry Thanks to the application of Pavlov's methods, the Soviet physiologists have made a considerable contribution to the knowledge of the laws of digestion and livestock feeding.

The first, introductory lecture describes the new methods of integral study of physiological processes in conditions of a chronic experiment. The eighth and last lecture gives a profound theoretical interpretation of the material, shows the causal dependence of the secretory and motor activity of each section of the digestive canal, the biological expediency, and the adaptability of the glandular activity to the given alimentary conditions.

The lectures distinctly reveal Pavlov's constant tendency to link the results of physiological research with the practical tasks of the clinic.

[19] Brücke—a prominent German physiologist of the 19th century. p. 84

[20] Claude Bernard (1813-1878)—celebrated French physiologist, one of the founders of experimental physiology. The method of obtaining pancreatic juice was described by him in his book *Leçons de physiologie opératoire,* 1879. p. 86

[21] Rudolf Heidenhain—outstanding German physiologist who devoted much attention to the study of digestion In 1884-1886, while on a scientific mission abroad, Pavlov worked in Heidenhain's laboratory. Heidenhain actually repeated the operation of a permanent pancreatic fistula which Pavlov had performed earlier. p. 86

[22] The American physician Beaumont studied for nine years the process of digestion in the organism of a Canadian hunter whose stomach had an unhealable fistula as a result of an accidental wound. His work was published in Boston in 1834. p. 91

[23] In 1889 Minkovsky (jointly with Mehring) extirpated the pancreas for the first time and proved the connection of diabetes with the activity of this gland. Minkovsky also studied the external pancreatic secretion. p. 100

[24] The operation of a fistula between the inferior vena cava and the vena portae, or of the so-called Eck fistula, was proposed by the Russian surgeon Eck in 1877. The method of the operation consists in the formation of a communication between the two veins in question and in a simultaneous ligature of the vena portae above the fistula. Owing to this, the blood current is transmitted from the alimentary canal direct into the inferior vena cava, without passing through the liver. Pavlov thoroughly studied the effect on the organism of the exclusion of the liver and made considerable improvements in the method of the fistula operation, consequently, this operation must be actually called the Eck-Pavlov fistula. (See the following works by I. P. Pavlov: "The Eck Fistula Between the Inferior Vena Cava and the Vena Portae, and Its Effect on the Organism" (jointly with M. Gan, V. Massen and M. Nentsky), *Complete Works*, Vol. V, 1949, pp. 3-25; "A Modification in the Operation of an Eck Fistula Between the Vena Portae and the Inferior Vena Cava," *Ibid*, pp. 34-35; "Remarks on the Eck Fistula from the Surgical Point of View," *Ibid*, pp. 36-38.) P 100.

[25] Pavlov's Nobel speech was first published in the symposium "Les prix de Nobel en 1904," Stockholm, 1905. This speech expounds the fundamental principles of the physiology of digestion from the integral biological point of view which became predominant in this branch of science thanks to the works of I. P. Pavlov and his school; in addition, the Nobel speech is of considerable interest since it establishes a direct succession between Pavlov's study of the digestive process and his passage in the beginning of the 20th century to the study of the higher nervouvs activity. p 127

[26] In vitro—literally in glass, i.e, outside the organism. p. 128

[27] Pia desideria (Latin)—pious desires p. 136

[28] Markel Vilhelmovich Nentsky (1847-1901)—celebrated biochemist. From 1891 to 1901 he was in charge of a division at the Institute of Experimental Medicine. Pavlov, jointly with M. Nentsky, carried out a number of investigations relating to the role of the liver in the formation of urea in the organism The works of M. Nentsky on the chemical relationship between the blood pigment of animals—haemoglobin, and the vegetable pigment of plants—chlorophyll, were highly appreciated by K. A. Timiryazev. p. 138

IV

THE PROBLEM OF THE STUDY OF HIGHER NERVOUS ACTIVITY AND THE WAYS OF ITS EXPERIMENTAL SOLUTION

29 This speech was delivered at a plenary session of the International Medical Congress in Madrid, in April 1903. It was first published in the "Proceedings of the Military Medical Academy" 1903, p. 103.

Having elaborated special methods for the study of reflex relations in the organism and of the secretion of the digestive glands in a practically normal animal, Pavlov already in his works on digestion pointed out the possibility of "psychical secretion" along with the secretion conditioned by purely physiological factors At the end of the nineties, Pavlov began his experimental study of the mechanism of "psychical secretion."

The famous Madrid speech contains the maximum programme which the great creator of the theory of the higher nervous activity set himself and which he consistently and steadfastly carried out in the thirty-three years of his subsequent purposeful scientific activity. This speech fully revealed I. P. Pavlov's materialistic attitude towards psychical phenomena: he considered psychical activity from the biological, evolutionary point of view and rejected the "mechano-physical" and vitalist views

It is in this article that Pavlov first gave his definition of the conditioned and unconditioned reflexes p. 149

30 Teleology—an idealist theory proclaiming that all phenomena existing in the world are conditioned by the influence of expediently acting forces which direct them to predetermined ultimate goals. p. 152

31 Animism—a theory recognizing the existence of souls in inanimate objects. Pavlov considered animism equivalent to idealism. p. 152

V

METHODS OF INVESTIGATION AND FUNDAMENTAL LAWS OF DEVELOPMENT

32 The "Lectures on the Work of the Cerebral Hemispheres" were delivered by I. P. Pavlov at the Military Medical Academy in the spring of 1924 for physicians and biologists. They were published in 1926 and republished without any changes in 1927 and 1937.

In the preface to the third edition Pavlov characterized the "Lectures" as "... a fundamental exposition of our facts, systematized for the first time, it covers more than three-quarters of the entire period of our work in the field of the physiology and pathology of the higher nervous activity."

In his "Lectures" Pavlov substantiated the physiological mechanisms which determine the properties of the higher nervous activity and indicated the vast prospects of utilizing laboratory experience in the neurological and psychiatric clinics Since the factual material contained in these lectures was subsequently considerably supplemented by further research, which was generalized by Pavlov in corresponding articles and papers, the present edition contains only the first two chapters, brilliantly illustrating the history of the problem and the Pavlovian method of studying the higher nervous activity p 169

[33] Munk and Ferrier studied the functions of different parts of the cerebral cortex; they showed that the cortical zones, which do not react to electrical stimulations, perform definite functions, confined to the given region and connected with the reception of external stimuli (the visual zone, the auditory zone, etc) Munk was the first to establish the existence of cortical regions with more complex sensory functions, the derangement of their integrity leads to the so-called "psychical" blindness or deafness when the patient sees the objects but can neither recognize nor name them See also the article "Summary of Results of the Experiments with Extirpation of Different Parts of the Cerebral Hemispheres by the Method of Conditioned Reflexes," published in this edition p. 170

[34] William James (1842-1910)—American psychologist, founder of the so-called philosophy of pragmatism, an idealistic system close to empirio-criticism, but possessing typical traits of the ideology of American capitalism. p. 172

[35] Wilhelm Wundt (1832-1920)—celebrated German physiologist and psychologist. He advocated the erroneous point of view that in studying the psychical activity of animals we should proceed from our own mental activity. p. 172

[36] René Descartes (1596-1650)—celebrated French philosopher and naturalist, a mechanist in the sphere of natural science and an idealist in philosophy. He was the first to establish the notion of reflex as an automatic reaction of the organism to an external stimulation, a reaction which takes place due to the transmission of excitation by the nerves to the brain. The mechanist nature of this notion was combined by Descartes with the concept of a human "intelligent soul."

See also the article "Reply of a Physiologist to Psychologists" published in this edition. p. 172

[37] Charles Sherrington—English physiologist known for his research concerning the reflex function of the spinal cord. His philosophical outlook is thoroughly idealistic. The reactionary character of Sherrington's views stands out with particular clarity in his latest works where he openly opposes Pavlov's theory of conditioned reflexes and affirms that psychical activity cannot be made known by the methods of natural science Pavlov's attitude to Sherrington was made quite clear at the "Wednesday" gatherings (see pp. 563-69).
p. 173

[38] R Magnus—well-known Dutch physiologist who showed that the displacement of an animal in space (its "locomotor activity") and the distribution of tension (tonus) in the skeletal muscles are connected with reflex reactions, whose centres are located in the brain stem and the cerebellum p. 173

[39] Comparative physiology is a branch of physiology which studies the functions of animal organisms at various levels of evolutionary development with the aim of disclosing the peculiarities of the unity of the organisms and of the environment at different stages of evolution, of determining the fundamental factors of development and reproducing the picture of evolution of physiological functions. Comparative physiology, which is based on a genuinely historical approach to the development of physiological functions in the organic world, has made particularly great progress in Soviet biological science.
p. 174

[40] The theory of animal tropism was elaborated by the American physiologist-mechanist J. Loeb. According to Loeb, an organism exists in a medium with various lines of force (luminous rays, diffusive currents in the case of chemotaxis, etc.). The symmetrical structure of their bodies obliges the animals to orient them relative to these lines of force; otherwise one of the sides would be subjected to greater influence and this would lead to an intensification of physicochemical changes on this side, and would provoke more intensified movements. This is why animals always move to the source of stimulation along the straight line.

Loeb did not limit himself to lower animals; he applied his concepts also to higher animals emphasizing the forced character of their movements. He tried to explain the effect of a stimulus by its action on the muscular system of one of the symmetrical sides through the sense organs. Similarly he sought to explain by means of tropism even more complicated processes, such as instincts and conditioned reflexes, which he reduced to primitive physicochemical reactions,
p. 174

[41] Herbert Jennings—American zoologist, known for his research into the physiology of reproduction and behaviour of lower animals.

He was close to behaviourism, and his philosophical views approximated to pragmatism. Here, apparently, Pavlov has in mind his work published in 1906 and entitled *Behaviour of the Lower Organisms*
p. 174

[42] E. Thorndike—American psychologist, one of the founders of the so-called "behaviourism," a trend in comparative psychology. (See note 44)

Thorndike believed that apes and other animals solve any new problems arising before them by the method of numerous "trials and errors." Definite appropriate movements, accidentally effected by them, are fixed owing to associations which either persist or, on the contrary, disappear, depending on subsequent experience.

Pavlov had a high opinion of his research, which he regarded as the first attempt by a psychologist to make an objective study of the psychical activity of animals and as renunciation of the anthropomorphic views which attribute human motives to the behaviour of animals (see Pavlov's statements at the "Wednesday" gatherings). However, striving to give a single and universal explanation of habits, learning and intellect at all levels of evolution, Thorndike in a mechanist way equalized all animals and denied the existence of specific properties of human psychical activity. His book *The Process of Learning in Man* was translated into Russian and published in 1935.
p. 174

[43] Concerning this episode Pavlov in his preface to *Twenty Years of Objective Study of the Higher Nervous Activity (Behaviour) of Animals* wrote: "I began to investigate the question of this (psychical—*Ed*) stimulation of the salivary glands with my colleagues, Drs. S. G. Wolfson and A. T. Snarsky. While Wolfson collected new and important facts relating to the peculiarities of the psychical stimulation of the salivary glands, Snarsky undertook to analyse the internal mechanism of the stimulation from the subjective point of view, i.e., he assumed that the internal world of the dog (we performed our experiments on dogs)—its thoughts, feelings and desires—is analogous to ours. It was this that brought us face to face with an incident which had no precedent in our laboratory. We considerably diverged in our interpretation of this internal world; further attempts failed to bring us to a common conclusion, contrary to the usual laboratory practice, according to which new experiments undertaken by mutual consent generally led to a settlement of all differences and disputes. Dr. Snarsky clung to his subjective interpretation of the phenomena, while I, astonished at the fantastic character and scientific barrenness of this approach to the problem, began to seek for another way out from this difficult situation"
p 175

[44] Behaviourists—zoopsychologists who advocate behaviourism (a derivative from the word "behaviour") The theory of behaviourism appeared as a reaction against the hitherto existing anthropomorphic notions of the psychical activity of animals The behaviourists strove to study the behaviour of man and animals by objective methods and to exclude from their explanation of the behaviour all psychological notions connected with consciousness (sensation, attention, will, etc). From the point of view of the behaviourists, consciousness is behaviour and nothing else. They considered that their task was to study the relations between the stimulus and the reaction to it. The behaviourists utilized Pavlov's theory of conditioned reflexes, however, they simplified it and rendered it primitive, since they did not take into account the laws of the higher nervous activity disclosed by Pavlov and connected with the peculiarities of the physiological processes in the central nervous system. The behaviourists were equally incapable of appreciating the Pavlovian concept of the second signalling system, which qualitatively distinguishes the human psychical activity from that of animals.

Being far removed from the dialectical interpretation of the phenomena of life, the behaviourists take the mechanist view of the vital activity of animals, they reduce consciousness to latent motor reactions and biologize the human personality. p. 176

[45] The depressor nerve terminates in the walls of the initial part of the aorta. Its excitation, as proved by the investigations of Cyon, the Russian physiologist, provokes a reflex dilation of the vessels and a decline of the blood pressure, i e., a depressor effect. p 180

[46] V. I. Vartanov (1853-1919)—outstanding Russian physiologist, Professor at the Petrograd Women's Medical Institute. p 192

[47] Bahnung—the formation of a path in the nervous system, the facilitation of conduction of a certain reflex reaction as a result of its frequent repetition. p. 196

[48] Pavlov's famous speech "Natural Science and the Brain" was delivered at the plenary session of the Twelfth Congress of Naturalists and Physicians in Moscow on December 28, 1909. It was first published in the *Journal of the Congress of Naturalists and Physicians* in 1909.

In this speech Pavlov substantiated the necessity of an objective approach to the study of psychics and brilliantly characterized the significance of conditioned reflexes as a biological act which creates the necessary conditions for a regular metabolism between the organism and the external environment.. In this speech, along with the elucidation of the mechanism of a temporary connection, he also formulated the fundamental law of concentration and irradiation of the process of excitation in the cerebral cortex.

K. A. Timiryazev highly appraised Pavlov's speech. In this connection the following exchange of friendly letters took place between the two great Russian naturalists.

I. P. Pavlov wrote to K. A. Timiryazev:

"Dear Kliment Arkadievich,

"I left the Moscow Congress on December 29 and only yesterday I learnt from the *Journal of the Congress* of your appreciation of my speech. I deem it natural and appropriate to tell you of the great joy which your appreciation brings to my heart Harmony of scientific thought and recognition by our colleagues of the correctness and value of our views are to us the most legitimate source of assurance and satisfaction. I am very conscious of these feelings, since, to my regret, I belong to the type who always tend towards uneasiness and doubt, which is, evidently, the result of my neurasthenia. Allow me to express my deepest gratitude

"With all my heart I wish you full recovery and a rapid return to your usual activity.

"Respectfully and sincerely yours,

"Ivan Pavlov"

K. A. Timiryazev replied.

"Dear Ivan Petrovich,

"Words fail to express the joy and satisfaction which your kind letter brought me. When, under the deep impression made by your speech, I sent you my telegram, it occurred to me that the opinion of one who does not understand a thing about your problems would be of no importance to you. But later I consoled myself with the thought that no one can be prevented from expressing his admiration. Your friendly, comradely attitude has definitely reassured me and filled me with joy not only for myself but for our science in general. I, too, am obliged to wage a struggle against the botanists, young and old, Russian and German, who advocate the view that plant physiologists must renounce 'the strict rules of naturalist thought' and replace them by an absurd 'phytopsychology' which, fortunately, does not exist. Now that I can refer to the fact that you, the man recognized by the whole world as the 'great physiologist of the Russian land,' have undertaken to withdraw the psychological method from its last stronghold in physiology, I feel sure ground under my feet and new strength for further struggle.

"I regard your speech as a great event in the history of natural sciences and deeply regret that I could not attend the congress. In general, it was the thought that I would be able to see you and have a talk with you that attracted me most to the congress.

"Allow me to thank you once more from the bottom of my heart for your kind and warm letter

"Respectfully and sincerely yours,

"K. Timiryazev."

p. 204

[49] The notion of "unconscious conclusions" was introduced by Helmholtz to designate the reactions elaborated as a result of repeatedly evoking a definite situation forgotten by a man. Owing to this the reactions penetrate unconsciously, against one's will Helmholtz considered that the process of "unconscious conclusions," being the most elementary in the nervous activity, underlies the process of thinking. (See his book *Physiologische Optik*, second edition, p 601.)

Being a dualist and a Kantian, Helmholtz was far from adhering to the materialist conception of psychical activity and this concept resulted exclusively from empirical observation, which was not developed in his works. Helmholtz's idealist theory of symbols was subjected to criticism by V. I. Lenin in his book *Materialism and Empirio-Criticism* p. 213

[50] The article "'Pure Physiology' of the Brain" was a paper originally prepared by Pavlov for the Congress of Psychiatrists, Neurologists and Psychologists scheduled to be held in Switzerland in August 1914, but postponed on account of the outbreak of the war. The article was first published in the Russian magazine *Nature*, No. 1, 1917, pp. 27-38. p. 219

[51] Edouard Claparède, Professor of Psychology of Geneva University, was the president of the Organizing Committee of the Congress of Neurologists and Psychologists which was to be held in 1914. Pavlov refers to Claparède's article "La psychologie comparée est-elle légitime?" ("Is the Existence of Comparative Psychology Justified?") published in the *Archive de Psychologie*, Vol. V, 1905, p. 13. See also Pavlov's statement at the "Wednesday" gathering of March 27, 1935 (p. 611). p. 219

[52] The article "Relation Between Excitation and Inhibition, Delimitation Between Excitation and Inhibition, Experimental Neuroses in Dogs" was published in the *Skandinavische Archiv fur Physiologie*, Vol. 47, 1926, pp. 1-14, and was dedicated to the memory of Robert Tiegerstedt, a well-known physiologist, professor of Helsingfors (Helsinki) University. The article appeared in Russian in the book *Twenty Years of Objective Study of the Higher Nervous Activity (Behaviour) of Animals*, 4th ed., 1928.

The article is of special interest, since it characterizes the evolution of Pavlov's views and gives a detailed analysis of one of the fundamental problems of the theory of conditioned reflexes, namely, of the problem of the relations between the processes of excitation and inhibition in the cerebral cortex It also stresses the importance of the law of irradiation of the inhibitory process in the cerebral cortex discovered by Pavlov

According to Pavlov, it is precisely the relations between the excitatory and inhibitory processes, or their equilibrium, which determine all our behaviour, the normal and the pathological p 230

53 This refers to the *Skandinavische Archiv fur Physiologie* p. 230

54 Pavlov implies here the centres in the brain stem and in the cerebellum which regulate the displacement of the animal in space, as well as the equilibration and distribution of the muscular tension in the skeletal musculature. p. 242

55 Pierre Janet—French psychologist and psycho-pathologist, Professor of Psychology at the Collège de France, Paris. He showed that the pathological phenomena which are observed in neuroses are of a psychogenic origin, i e, are not accompanied by pathologico-anatomical changes Janet was the first to establish the form of neurosis known as psychasthenia and connected with the weakening of the psychical tonus In respect of his gnosiological views Janet is close to the philosophy of subjective idealism p 243

56 Nikolai Yevgenievich Wedensky (1852-1922)—brilliant Russian physiologist, professor of Petersburg University. Studying the development of excitation in the nerve fibres he demonstrated that excitation and inhibition are stages of a single excitatory process in the protoplasm of nervous formations both of a peripheral and central origin Wedensky's classical work *Excitation, Inhibition and Narcosis* was published in 1901 and marked the beginning of a new progressive trend in the theory of the physiological nature of the inhibitory process as a stage of development of a single excitatory process.

Although Pavlov did not agree with certain points of Wedensky's concept, he highly valued his researches. In one of his works (*The Latest Successes of the Objective Study of the Highest Nervous Activity in Animals*) Pavlov wrote: "... while investigating these deviations in the direction of the preponderance of the inhibitory process and the weakening of the excitatory, we found that one of the discoveries of our distinguished, late physiologist N. E. Wedensky was absolutely justified. Wedensky did much to advance the physiology of the nervous system; he succeeded in bringing to light important facts, but for some reason he has not received due recognition in foreign scientific literature. He is the author of the book *Excitation, Inhibition and Narcosis* in which he described the changes in the nerve fibres caused by strong stimuli and distinguished several phases of these changes. It proves now that these peculiar phases are entirely reproduced also by the nerve cells if there is an intense collision between the processes of excitation and inhibition. I have no doubt that after this the researches of Wedensky will receive the recognition they merit" (I. P Pavlov, *Complete Works*, Vol. III, 1949, pp 331-332) p 244

[57] The article "The Conditioned Reflex," written by Pavlov in 1934 for the Big Medical Encyclopedia, gives an exceptionally profound and extensive review of the theory of conditioned reflexes, and shows the tremendous general biological importance of the principle of temporary connection, as well as the significance for psychology and psychopathology of the objective method of studying the higher nervous activity in animals p 245

[58] See note 33. p. 246

[59] The law of Weber and Fechner establishes a numerical interrelation between the intensity of stimulation and the strength of sensation According to this law, the strength of sensation increases in proportion to the logarithm of the intensity of stimulation and not parallel to the variations of its absolute value. p 246

[60] See note 35 p 247

[61] See note 42. p. 247

[62] See note 47 p. 249

[63] See article "Open Letter to Pierre Janet" published in this edition p. 266

[64] See note 83 p 266

[65] See note 138 p. 267

[66] See note 87. p 267

[67] The term circularity is usually employed to denote a peculiar constitution of the psychics which manifests itself in periodical fluctuations of mood. When these fluctuations overstep the normal limits, disorder sets in—the manic-depressive psychosis (see note 86). p. 269

[68] See note 84. p. 269

[69] See note 86. p. 269

[70] The article "Physiology of the Higher Nervous Activity" represents Pavlov's address to the Fourteenth International Congress of Physiology in Rome on September 2, 1932. p. 271

[71] The German physiologist F. Goltz, for the first time on record (1892), succeeded in keeping alive several dogs in which both cerebral hemispheres had been extirpated The animals were able to move and to eat; they preserved their sense of smell, hearing, cutaneous sensibility and the muscular sense They reacted to light but did not distin-

guish the objects In 1912, at Pavlov's request, an operation for the removal of both cerebral hemispheres in dogs was carried out in his laboratory by G. P. Zeliony The experiments proved that elaboration of conditioned reflexes in such animals after complete recovery from the bilateral extirpation of the cerebral cortex, was impossible p 272

[72] The case of the "famous patient" was described by Dr. Strumpel of Leipzig, I. M. Sechenov cited this case as proving the fundamental idea of his brilliant work *Reflexes of the Brain*, according to which "all acts of conscious and unconscious life are reflexes by the nature of their origin."

In 1900 Sechenov wrote. "A case of this kind was recorded by physicians in Germany It concerned a young man all of whose sense organs, with the exception of one eye and one ear, no longer functioned, the intact eye and ear being his sole means of communication with the external world So long as the eye could see and the ear hear, he remained in an alert state. But whenever the physicians, in the course of experimentation, closed his intact eye and stopped his ear, he rapidly fell into a sleeping state from which he could be awakened by means of sensory influences upon these very organs." Sechenov cited a similar case related to him by S P. Botkin. "The patient, a woman of education, retained only the sense of touch and the muscular sense of one of the arms. As testified by the personnel of the hospital, she was almost continuously in a state of sleep; she communicated with other people in the following way: a pillow was placed on her abdomen, then someone would take her hand which preserved sensibility and passing it over the pillow made her write the question to which she had to give an answer. . . Is it possible, then, given these facts, to doubt that a wakeful state, with the interchange of all kinds of sensations which inevitably accompany it, is maintained by luminar, acoustic, thermic, olfactory and often mechanical, external influences upon the sense organs?... The loss of all senses must necessarily result in complete loss of consciousness, since consciousness is expressed by none other than sensations of which the individual is conscious Complete loss of the senses is bound to be followed by deep dreamless sleep." "The Participation of the Nervous System in the Human Working Movements," 1900. I. M. Sechenov, *Selected Works*, Vol. I, U.S.S.R. Academy of Sciences, 1952. Moscow, pp. 511-512.

These views of Sechenov are in full accord with the propositions advanced by I. P. Pavlov. p. 279

[73] The notion of dynamic stereotype was examined in detail by Pavlov in his article "Dynamic Stereotypy of the Higher Part of the Brain" (see p. 448 of the present volume). p. 283

[74] Pavlov has in mind his article "Essay on the Physiological Concept of the Symptomatology of Hysteria" (see p. 516 of the present volume). p. 285

VI

THEORY OF ANALYSERS, LOCALIZATION OF FUNCTIONS AND MECHANISM OF VOLUNTARY MOVEMENTS

[75] The paper "Summary of Results of the Experiments with Extirpation of Different Parts of the Cerebral Hemispheres by the Method of Conditioned Reflexes" was read in 1911 at a gathering of the Society of Russian Physicians in Petersburg. It was first published in the *Proceedings of the Society of Russian Physicians in St. Petersburg*, 1912-1913.

The combination of the method of conditioned reflexes with the extirpation of certain parts of the cerebral hemispheres enabled Pavlov to study the problem of localization of functions in the cerebral cortex of the dog in quite a new way. An exposition of the principal results of this study is given in the present article. p. 289

[76] Gyrus sigmoideus, g g coronarius and ectosylvius—the sigmoid, coronal and ectosylvian convolutions situated in the anterior part of the cerebral cortex of the dog. Stimulation of these cortical zones by means of an electric current makes the animal's extremities or trunk move. p. 295

[77] The article "Physiological Mechanism of the So-Called Voluntary Movements" was published in the *Collected Papers of the Physiological Laboratories of Academician I P. Pavlov*, Vol. VI, No. 1, 1936. p. 307

[78] Kinesthetic stimulations—signals entering the nervous system from the skeletal muscles. By means of these signals we can judge of the state of muscular contraction or relaxation, the position of the extremities, the resistance which they have to overcome, etc. The significance of the kinesthetic stimulations, which are also called "muscular sense," was first demonstrated by Ivan Mikhailovich Sechenov. Kinesthetic cortical cells are those to which impulses are transmitted along the paths conducting the muscular sense. p. 307

[79] Numerous tricks with the so-called "thought transmission" are based on this phenomenon. p. 309

VII
THEORY OF TYPES

On the basis of the rich experience accumulated in the course of almost thirty years of work on conditioned reflexes, Pavlov and his school thoroughly elaborated the typology of the higher nervous activity in dogs As pointed out by Pavlov, this typology coincided with the four human temperaments described by Hippocrates Already in Pavlov's lifetime investigation was started with the aim of elucidating the biological importance of these types in dogs, as well as the problem of their transformation and hereditary transmission. At present this work is carried on at the Pavlov Institute of Higher Nervous Activity in the village of Pavlovo (formerly Koltushi)

80 The article "General Types of Animal and Human Higher Nervous Activity" was published in the form of a booklet in the series *Latest Papers on the Physiology and Pathology of the Higher Nervous Activity*, Paper No III, 1935. p. 315

81 Hippocrates' four temperaments are implied here A more detailed description of them is given on p 340. p. 317

82 Stereotypy—uniform repetition of definite stimuli in one and the same succession and of corresponding reactions to them. p. 339

83 Ernst Kretschmer—German psychiatrist, author of the well-known book *Physique and Character*, a dualist in the interpretation of the psychical activity Criticizing Kretschmer's type Pavlov stresses only one weak point of this concept. It should be added that Kretschmer, like all bourgeois psychologists and psychiatrists, ignores the influence of social environment on man He is an adherent of the anti-scientific Morganist anthropo-genetics p. 343

84 Cyclothymics, according to Kretschmer's classification, are individuals of a sociable, jovial disposition, energetic, sometimes violent; persons susceptible to manic-depressive psychosis usually belong to this type (see note 86). p. 343

85 Schizothymics, according to Kretschmer's classification, are individuals of a reserved disposition, absorbed in their internal world, fantasts, persons susceptible to schizophrenia belong to this type. p. 343

86 Manic-depressive, or circular, psychosis—a mental disorder characterized by interchange of periods of violent excitation and depression. p 343

87 Schizophrenia—mental disorder which manifests itself in hallucinations, fantastic emotions, and split personality, however, with the intellect remaining relatively unchanged. p. 343

VIII

PROBLEMS OF SLEEP AND HYPNOSIS

The works included in this chapter are a striking example of the useful role played by the Pavlovian theory in solving problems relating to the physiology of the nervous system. Basing himself on laboratory observations in the course of the elaboration of conditioned reflexes in dogs, Pavlov developed an original theory of sleep, regarding it as inhibition originating in the cortex and spreading over the lower parts of the central nervous system. At the same time he showed that sleep and hypnosis are one and the same phenomenon, differing only in intensity and extensity of inhibition.

88 The Paper "Some Facts About the Physiology of Sleep" was read by Pavlov at a meeting of the Petrograd Biological Society in 1915. It was first published in French in *Comptes rendus de la Société de Biologie*, Vol. 79, 1916, pp 1079-1084. The sleep of animals, which had long been an obstacle to the experiments with conditioned reflexes, itself became the object of investigation. It was found that both sleep and hypnosis could be evoked in dogs by means of conditioned reflexes. p. 347

89 The article "Concerning the So-Called Hypnotism in Animals" was published as a supplement to the proceedings of the Physics and Mathematics Department of the Russian Academy of Sciences, of November 9, 1921. p. 354

90 Experimentum mirabile—a "miraculous experiment," which was first performed in the 17th century by Athanas Kircher. A hen, suddenly placed on her back, remains for a long time in a state of stupor and immobility. p. 354

91 The article "Physiology of the Hypnotic State of the Dog" was first published in the *Collected Papers of the Physiological Laboratories of I. P. Pavlov*, Vol. IV, 1932. p. 356

92 Catalepsy—a state of stupor. p. 356

93 See note 149. p. 357

94 The subsequent works of Academician K. M. Bykov's school showed that the internal organs dispatch nervous impulses to the brain, signalizing their state; on the basis of these signals it is possible to elaborate conditioned reflexes, and the function of the given organ can be modified by means of conditioned reflex regulation. (See K. M. Bykov's *The Cerebral Cortex and the Internal Organs*, Moscow-Leningrad, 1947. State Medical Publishing House.) p. 364

95 The motor nerves stretching to the muscles originate in the nerve cells of the anterior horns of the spinal cord. p. 365

[96] This is a reference to the article "A Brief Essay on the Higher Nervous Activity" written by Pavlov in 1930. In this article he explained why under hypnosis it is the strong stimuli which are subjected to inhibition first (as, for example, in the equalization and paradoxical phases). He stated "that the exhaustion of the cortical cell always results in the development of an inhibitory process in this cell. Thus the inhibition irradiating from the cells continually stimulated by the conditions of the experiment is summated with the inhibition of the specially stimulated working cell proper and reaches here maximum intensity" (I P. Pavlov, *Complete Works*, Vol. III, 1949, p. 403). p. 366

[97] A case analogous to war-time neurosis was described by V. V. Rickman in his article "Disclosure of Earlier Traces of Stimulation in the Centres of a Defensive Reaction as an Analogue of Traumatic Neurosis." (*Collected Papers of the I. P Pavlov Physiological Laboratories*, Vol. IV, 1933, p. 102) This work shows the prolonged and heightened excitability of the centres of defensive reaction after the action of a powerful destructive stimulus, as well as the conditions which make this state manifest Hypnotic inhibition of the cortex is one of these conditions. p 369

[98] The paper "The Problem of Sleep" was read by Pavlov at a conference of psychiatrists, neuropathologists and psychoneurologists in Leningrad in December 1935. The stenographic record of this paper was first published in the *Complete Works* of I. P. Pavlov, Vol I, 1940. p. 371

[99] Narcolepsy—a periodically appearing overwhelming desire for sleep p. 378

[100] Cataplexy—a state of stupor evoked in certain animals by extreme fright or arising under the so-called animal hypnotism, i.e., when the animal is forcibly kept for a certain period in an unnatural position. Some regard it as a state analogous to catalepsy which is peculiar to human hypnotic sleep. p. 378

[101] Fili olfactorii—olfactory fibres extending from the olfactory bulbs of the brain to the olfactory conches and mucous membrane of the nasal cavity. p. 379

[102] Corpora geniculata—geniculated bodies, formations in the brain stem which represent the intermediate centres of the auditory nerves (internal geniculated bodies) and of the optical nerves (external geniculated bodies). p. 371

[103] Encephalitic sleep—pathological sleep developing in a patient suffering from epidemic encephalitis. p. 382

[104] Hypothalamus—part of the diencephalon situated under the optic thalamus and forming the bottom of the third ventricle of the brain. In the hypothalamus are located the centres of many vegetative functions of the organism water metabolism, thermo-regulation, etc. According to the data of Hess, obtained as a result of the stimulation of this region by means of an electric current, the "centre of sleep" is situated here, too In epidemic encephalitis, which is accompanied by pathological somnolence, certain changes of the nerve cells are observed in this region. p. 382

[105] Rapport—special faculty of a hypnotized person for perceiving in a selective way exclusively the words of the hypnotist without maintaining any contact with the rest of the external world. Pavlov showed that this state is not the exclusive property of hypnotic sleep, and that it is sometimes also observed in normal sleep. p 390

IX

PHYSIOLOGY AND PSYCHOLOGY

Pavlov considered that the principal object of his study of the higher nervous activity in animals was to disclose the physiological laws of human psychical activity and to include psychology in the sphere of natural sciences. In this, however, he was far from being inclined to apply in a purely mechanical way to man the laws of higher nervous activity observed in dogs, he pointed out that the peculiarities of the human higher nervous activity "sharply distinguish man from other animals." He wrote: "It would be the height of presumption to regard these first steps of the physiology of the cerebral hemispheres—complete in relation to programme but not to content—as solving the grandiose problem of that supreme mechanism of human nature." (*Complete Works*, Vol. IV, 1949, p. 326.) Towards the end of his life, Pavlov, basing himself on a thorough study of human mental pathology and on a profound biological consideration of the problem of mental evolution, clearly formulated certain fundamental physiological distinctions between the higher nervous activity of man and that of animals. He wrote: "When the developing animal world reached the stage of man, an extremely important addition was made to the mechanisms of the nervous activity. In the animal, reality is signalized almost exclusively by stimulations and by the traces they leave in the cerebral hemispheres, which come directly to the special cells of the visual, auditory or other receptors of the organism. This is what we, too, possess as impressions, sensations and notions of the world around us, both the natural and the social—with the exception of the words heard or seen. This is the first system of signals of reality common to man and animals. But speech constitutes a second signalling system of reality which is peculiarly ours, being the signal of the first signals On the one hand, numerous speech stimulations have removed us from reality, and we must always remember this in order not to distort our attitude to reality."

It should be pointed out that objective study of the second signalling system was but begun by Pavlov. The articles in this chapter indicate the ways of applying physiological methods to the study of the laws governing human mental activity. At the same time Pavlov vigorously combated the animism and dualism of psychologists who denied the material foundation of the psychical processes.

106 The paper "Physiology and Psychology in the Study of the Higher Nervous Activity of Animals" was read at a meeting of the Petrograd Philosophical Society on November 24, 1916 It was published in the *Journal of Psychiatry*, No. 6, 1917, pp. 141-146. This paper combines scientific exactitude with brilliant popularization of the Pavlovian methods of objective study of the higher nervous activity designed for an audience not familiar with biology. The above journal also published the discussion which followed the reading of this paper and in which the celebrated neurologist V. M. Bekhterev, as well as the philosophers-idealists N O. Lossky, A I. Wedensky and others, took part. p. 395

107 Modest Nikolàyevich Bogdanov (1841-1888)—well-known Russian zoologist and traveller, Professor of Petersburg University. p. 395

108 Retina—the part of the eye which is sensitive to light. p. 404

109 The article "Reply of a Physiologist to Psychologists" was published in the magazine *Psychological Review*, Vol. 39, No. 2, 1932, in connection with the works referred to in the text, namely, the article by Guthrie "Conditioning as a Principle of Learning" and Lashley's "Basic Neural Mechanisms in Behaviour." In this article Pavlov gave a particularly exhaustive formulation of the fundamental methodological principles underlying the reflex theory—the principle of determinism, the principle of analysis and synthesis, and the structural principle Pavlov counterposes these materialist principles to the confusing idealistic concepts of the idealists. In his reply published in 1934 in Volume 41 of the same magazine ("The Pavlovian Theory of Conditioned Reflexes"), Guthrie expressed his idealistic concepts in even more distinct form, insisting that it is impossible to disclose the nature of psychological processes by objective physiological methods. p. 414

110 Pavlov repeatedly pointed to the indispensable existence of internal analysers (see his articles: "Summary of Results of the

Experiments with Extirpation of Different Parts of the Cerebral Hemispheres by the Method of Conditioned Reflexes" and "The Physiology of the Hypnotic State of the Dog"). Whereas the external analysers link the organism with the external world, the internal analysers, which receive signals from all the organs and systems of the animal, enable the latter "to analyse also that which takes place inside it." p. 417

[111] That is, proceeds not from experimental facts, but from a preconceived point of view. p. 424

[112] Charles Spearman—psychologist, Professor of London University. Spearman's idealistic point of view, cited by Pavlov, characterizes the vitalist conception of this bourgeois psychologist with regard to the nature of the intellect. p. 425

[113] Receptor apparatus—the sense organs or the sensory nerve endings. p. 428

[114] Afferent nerves—sensory or centripetal nerves conducting the excitation to the central nervous system. p. 428

[115] Efferent or centrifugal nerves conduct the impulses from the central nervous system to the effector organs (muscles, glands, etc.). p. 428

[116] The central nervous system (the brain and the spinal cord) consists of the white matter—the nervous fibres—and the grey matter in which the nerve cells are mainly concentrated. The grey matter forms the cerebral cortex, as well as the nuclei of the brain stem. p. 428

[117] Cyto-architectonics—branch of the histology of the nervous system which studies the cellular structure of the cerebral hemispheres. In animals and man there are different cortical zones which are distinguished by their structure and cellular composition. p. 429

[118] Ataxic patients—patients who suffer from tabes; owing to a deranged conduction of the muscular (kinesthetic) sense in the spinal cord, the normal co-ordination of movements is abolished in them. Such patients can effect well-co-ordinated movements only when they control them visually. p. 444

[119] Fovea centralis—the region of the retina most sensitive to light. p. 444

[120] Wolfgang Koehler, Professor of the Berlin Institute of Psychology. Basing himself on his own experiments, Koehler stressed the importance of integral structures in the behaviour of chimpanzees, and attributed to the latter human-like intellectual faculties. Koehler

and his followers criticized associationism (i e., the conditioned reflex theory of behaviour) and behaviourism. Koehler's observations formed the experimental base of the idealistic concept of contemporary bourgeois psychology, the so-called "Gestalt psychology." His book *Investigation of the Intellect of Anthropoids* was translated into Russian and published in 1930 Pavlov subjected Koehler's views to ruthless criticism (see his statements at the "Wednesday" discussions included in the present edition). p. 449

[121] The article "Dynamic Stereotypy of the Higher Part of the Brain" was a paper read by Pavlov at the Tenth International Congress of Psychologists in Copenhagen on August 24, 1932. It was published posthumously in the book *Latest Papers on the Physiology and Pathology of the Higher Nervous Activity*, Vol. I, pp. 33-39.

In his paper Pavlov for the first time substantiated the notion of the so-called "dynamic stereotype" which, in his definition, is a well-co-ordinated and equilibrated system of internal processes; he also indicated the way to the study of the integral higher nervous activity in animals. p. 454

[122] The "literary inspirer", to whom Pavlov alludes here, is D. I. Pisarev, writer and sociologist. p. 458

[123] This is an extract from Pavlov's preface to the book by Prof A. G Ivanov-Smolensky *The Fundamental Problems of the Pathophysiology of the Higher Nervous Activity*, published by the State Medical Publishing House in 1933

Pavlov's views on the "fusion" of the psychical and physiological, the subjective and the objective, reflected his consistent materialistic tendency to bridge the gulf created by the idealists between the objectively existing material reality and the human consciousness. Here again Pavlov expressed his fundamental idea of a material base underlying all psychical manifestations and of the possibility of making the higher nervous activity known by means of the method of conditioned reflexes which he created, reflexes which combine the features of subjective phenomena and those of an objective physiological process. Affirming the necessity of creating a materialistic psychology, based on the physiological laws of activity of the nervous system, Pavlov wrote: "I am convinced that sooner or later the physiologists studying the nervous system and the psychologists will become united in their close and common work.... The more attempts that are made in this direction, the greater the chances that we shall finally come together to our mutual delight and benefit." (*Complete Works*, Vol. III, p. 359.)

A profound analysis of Pavlov's views on the close connection between objective and subjective phenomena was given in the report delivered by A. G. Ivanov-Smolensky at the Joint Session of the

U.S.S.R Academy of Sciences and the U.S.S.R Academy of Medical Sciences, devoted to the problems of the physiological teachings of Academician I. P. Pavlov (June 28-July 4, 1950). p. 460

[124] See also corresponding statement by Pavlov at one of the "Wednesday" gatherings, pp. 612-13, as well as note 180. p. 461

X

EXPERIMENTAL PATHOLOGY OF THE HIGHER NERVOUS ACTIVITY

The articles included in this chapter reflect Pavlov's tendency not to rest content with the experimental study of conditioned reflexes, but also to elucidate the causes of human nervous and mental disorders by means of the physiological laws of the higher nervous activity disclosed by him. The analysis of experimental neuroses gave rise to Pavlov's exceptionally fruitful notion of protective inhibition—a physiological mechanism protecting the weakened nerve cells from over-excitation and lesion. Protective inhibition underlies many pathological phenomena in mental disorders, at the same time, as demonstrated, artificial intensification of this process is a powerful therapeutic remedy in a number of nervous disorders.

Also of great value is Pavlov's analysis of the action of bromide and caffeine on the higher nervous activity, as factors which vary the relative intensity of the inhibitory and excitatory processes. These investigations make possible precise dosage of the above-mentioned preparations and their higher therapeutic effect. Thus, just as in the period of his research into digestion, Pavlov endeavoured to bring together physiology and clinical medicine. "We must be able to repair the damaged mechanism of the human organism on the basis of exact knowledge of it"—wrote Pavlov, and this was the motto of the great physiologist.

[125] A lecture delivered on May 10, 1934 at the Institute for Perfection of Physicians in Leningrad published in 1935 as a booklet. p. 465

[126] See Pavlov's statements on this subject at the "Wednesday" gatherings, published in the present volume. p. 466

[127] Extirpation—in this case removal of certain parts of the brain. p. 466

[128] Psychogenic diseases are those which result from psychical traumatism, not connected with pathologico-anatomic changes in the organs. p. 474

[129] Psychasthenia—literally "mental feebleness"—a functional nervous disorder pertaining to the group of so-called "psycho-

neuroses." It was first described by the French psychoneurologist Pierre Janet. The typical symptoms of this disorder are feeling of inferiority, morbid diffidence, reasoning, obsessive ideas Pavlov considered that the symptoms of psychasthenia depend on a pathological rupture between the first signalling system and the second, as well as between the latter and the subcortex. p. 475

130 Hysteria—a functional nervous disorder pertaining to the same group of psychoneuroses as psychasthenia. Its symptoms are high suggestibility and auto-suggestibility, leading to development of diverse disturbances of physiological functions. p. 475

131 Pavlov was elected professor to the chair of pharmacology in the Military Medical Academy in 1890 and occupied this position up to 1895. p. 482

132 Involuntary repetition of the same movements in certain diseases. p. 485

133 Perseveration—forcible repetition of one and the same syllable, word or phrase as a result of certain disturbances of the cortical zones connected with speech. p. 486

134 A paper on this subject was read by Pavlov on July 30, 1935 at the plenary session of the Second International Congress of Neurologists in London, it was published in the book *Twenty Years of Objective Study of the Higher Nervous Activity (Behaviour) of Animals*, 6th ed., 1938. p. 488

135 Convulsive contraction of certain groupings of muscles which in hysterical persons sometimes lasts for months and years. p. 491

136 Phobia—pathological imaginary fear. p. 491

137 Catalepsy—the petrifaction of the entire body or of its parts in positions artificially imparted to them and a simultaneous loss of ability to effect voluntary movements. It is observed in the state of hypnosis, as well as in certain mental disorders (for example, catatonia). p. 491

138 Catatonia—a mental disorder pertaining to the group of schizophrenia and accompanied by stupor, psychical depression and negativism. p. 492

139 A state of pathological excitation peculiar to the manic-depressive (circular) psychosis. p. 493

140 The article "Fusion of Principal Branches of Medicine in Modern Experimentation as Demonstrated by the Example of Digestion" is a paper read by Pavlov at a special meeting of the Society of Russian Physicians dedicated to the memory of S. P. Botkin in 1899. It was first published in the *Proceedings of the Society of Russian Physicians*, 1900, Vol. 67, November-December, pp. 197-242. p. 494

XI

PHYSIOLOGY AND PSYCHIATRY

Pavlov regarded a disease as a state of the organism in which specific relations arise between different organs and systems, and which cannot always be reproduced in experimental conditions. According to Pavlov, " ..clinical practice will always be an abundant source of new facts. It is, therefore, quite natural that the physiologist should desire a closer union between physiology and medicine." This desire is expressed by Pavlov with particular force in his paper "Psychiatry as an Auxiliary to the Physiology of the Cerebral Hemispheres," read in 1919.

Taking into consideration the specific property of the human higher nervous activity, which distinguishes man from higher animals, Pavlov by no means regarded the data obtained in the laboratory (experimental neuroses in dogs) as fully explaining the disturbances in human mental activity.

He emphasized the existence "of specifically human neuroses"—psychasthenia and hysteria. The latter circumstance gave him an added interest in psychiatry, which, according to him, is an auxiliary in the study of the physiology of the cerebral hemispheres and helps to comprehend certain aspects of the higher nervous activity peculiar to man.

The objective approach of the physiologist-materialist in studying symptoms of mental disorders enabled Pavlov to elucidate a series of pathological processes in man, proceeding from the fundamental laws of the higher nervous activity previously disclosed in the course of experimentation; it also enabled him to indicate new and effective methods of treatment, now being successfully elaborated by Soviet clinicians.

[141] The article "Psychiatry as an Auxiliary to the Physiology of the Cerebral Hemispheres"—originally a paper read by Pavlov at a meeting of the Society of Psychiatrists in Petrograd in 1919. It was published in the *Russian Physiological Journal*, Vol. II, 1919, pp. 257-260.
p. 505

[142] Tonic reflexes—reflex augmentation of tension in certain groups of the skeletal muscles—one of the symptoms of catatonia.
p. 507

[143] Maurice Schiff (1823-1896)—Swiss physiologist, who made a study of the central nervous system and the trophic action of the nerves on the tissues. p. 507

[144] Decerebration—removal of the cerebral hemispheres and of the anterior parts of the brain stem in animals by means of sectioning

the brain stem at the level of the anterior edge of the pons varolii. In this connection the tonic reflexes, whose centres are situated below the level of the sectioning, become intensified. p. 507

145 Progressive paralysis—an affection of the nervous system accompanied by profound anatomical changes in the cerebral cortex and developing in certain cases of syphilis. p. 512

146 Thrombosis—obstruction of a blood vessel by a clot of blood, or by the so-called thrombus. p 512

147 The article "An Attempt of a Physiologist to Digress into the Domain of Psychiatry" was published in the booklet *The Physiology and Pathology of the Nervous Activity*, Moscow-Leningrad, 1930. Brilliantly proving his idea of the unity of physiology and pathology, Pavlov interprets the catatonic stage of schizophrenia as "chronic hypnotic inhibition" protecting the cortical cells of the patient's weak nervous system, which are open to injury, from further destruction. p. 515

148 Hebephrenia—a form of schizophrenia characterized by impoverishment of mental life and nonsensical silly mannerisms. p 516

149 Negativism or contralism—a negative attitude towards the influences of the surrounding world; one of the fundamental symptoms of catatonia and of other schizophrenic forms, it is also met with in other mental disorders. p. 518

150 Echolalia—automatic repetition by the patient of words heard by him p. 518

151 Echopraxia—automatic repetition by the patient of the actions of other people. p. 518

152 The article "Essay on the Physiological Concept of the Symptomatology of Hysteria" was published in booklet form by the U.S.S.R. Academy of Sciences (1932, 36 pages). Professor A. V. Martynov, to whom this work is dedicated, had operated on Pavlov for gallstones. p. 522

153 Concerning the centre of sleep, see "The Problem of Sleep" in this volume. p. 527

154 Pierre Janet regarded hysteria as a derangement of consciousness, mainly as its splitting, which leads to the emergence of symptoms characteristic of this state Janet attributed great importance to weakness of the nervous system and to emotions in the development of hysteria. p. 532

155 The German psychiatrist Prof. A. E. Hoche in an article "Ist die Hysterie wirklich entlarvt?", published in the *Deutsche Medizini-*

sche Wochenschrift, 58, p. 1, 1932, endeavoured to prove that no progress had been made in the comprehension of hysteria (see p. 540). p. 539

[156] Anaesthesia is total loss of cutaneous sensibility (the opposite state is hyperesthesia—heightened sensibility). Analgesia is incapacity to feel pain. p. 539

[157] Babinsky believed that suggestion and auto-suggestion play the principal role in the development of hysteria. p. 541

[158] Eudetism is a specific psychical phenomenon which is close to the memory of images, i.e.; when the image of an object persists long after the disappearance of the object from the field of vision. Eudetism is a normal phase in the development of memory, through which all children pass at a certain age. p. 542

[159] Puerilism—a form of hysteria characterized by a naive, puerile conduct. p. 545

[160] Paresis—incapacity to effect voluntary movements. p 545

[161] The article "Feelings of Possession (Les sentiments d'emprise) and the Ultra-Paradoxical Phase" was published in the *Journal de Psychologie*, Nos. 9-10, 1933, pp. 849-854. Pierre Janet was one of the editors of this journal. p. 549

[162] Ambivalency—a symptom of schizophrenia when the patient simultaneously experiences diametrically opposed emotions (for example, joy and sorrow, etc.). p. 552

XII

FRAGMENTS OF STATEMENTS AT THE "WEDNESDAY" GATHERINGS

STRUGGLE OF I P PAVLOV AGAINST IDEALISTS

Pavlov's famous "Wednesdays" began in the spring of 1921 when, after the victorious termination of the civil war, the Soviet country embarked on the path of peaceful and creative work aimed at building the new, socialist society. Of particular significance for the rapid restoration of the normal activity of Pavlov's laboratories was the special decree issued by Vladimir Ilyich Lenin on January 24, 1921, providing for favourable conditions for the scientific work of Pavlov and his colleagues.

Twice weekly (on Wednesdays and Fridays from 10 o'clock in the morning till noon), with the strict punctuality for which he was renowned, Pavlov visited the small physiological laboratory of the Russian Academy of Sciences, which he headed and which occupied several rooms in the main building of the Academy overlooking Mendeleyev Avenue.

Taking part in the usual laboratory experiments of his small group of scientific colleagues (there were only four of them), he at the same time acquainted them with the results of the experimental research carried out in the other laboratories under his charge (at the Institute of Experimental Medicine and at the Physiological Chair of the Military Medical Academy).

This practice remained unchanged after the reorganization of the physiological laboratory into the Institute of Physiology in 1924, when the latter was accommodated in the premises it occupies now (Vasilievsky Island, Tuchkov Quay, No. 2A).

The numbers attending the "Wednesdays" grew steadily not only because of the increase in the staff of the Institute and the other Pavlov laboratories, but because of the presence sometimes of many physiologists and physicians who had received invitations.

Unfortunately, there are no records of the "Wednesdays" for the period from 1921 to 1929. From the end of 1929 until May 1933, V. K. Feodorov, one of the scientific workers in the Institute, regularly recorded the "Wednesday" meetings. Afterwards from the autumn of 1933 up to February 27, 1935, the day of Pavlov's death, these physiological discussions were taken down in shorthand. They are of great scientific value, revealing as they do the very process of Pavlov's scientific creative activity, his everyday "thinking" in close personal contact with numerous pupils and colleagues.

The present edition contains fragments of Pavlov's statements, mainly devoted to the interrelation of physiology and psychology, as well as to his tireless struggle against the idealistic concepts of some scientists abroad.

The stenographic records were edited in a manner that has preserved the peculiarity of Pavlov's expressions and turn of speech. The minutes of the Pavlovian "Wednesdays" were published in three volumes in 1949 by the U.S.S.R. Academy of Sciences (see "Pavlovian Wednesdays," Vols. I-III).

163 R. Yerkes—American scientist, author of numerous works on problems of general and comparative psychology, and especially the psychology of apes. He affirmed that the psychical processes in chimpanzees differ qualitatively from the associative higher nervous activity of other animals, while the difference between the mental activity of the chimpanzee and that of man is only quantitative.
p. 557

164 "Raphael" and "Rosa"—the chimpanzees used in Koltushi for experimentation with the aim of studying the higher nervous activity of anthropoids. This experimentation is now being carried on at the Pavlov Institute of Physiology of the U.S.S.R. Academy of Sciences in Pavlovo (Koltushi). p. 557

165 See note 44. p. 560

[166] W. Koehler's book under the same title is implied here. It was translated into Russian in 1930. p. 564

[167] The book by Charles Sherrington *The Brain and Its Mechanism* was published in 1933 His next book *Man on His Nature* appeared in 1942, it deals with the problems of the history and philosophy of natural sciences Basing himself on the idealistic concepts of J. Fernel, 16th century physician and philosopher, Sherrington proclaimed the reactionary idea that the world is unknowable. p 569

[168] Dubois-Raymond—well-known German physiologist of the 19th century In his speech "Seven Enigmas of the World" he declared that the mysteries of mental life would never be disclosed by natural science. Since then the term "ignorabimus" has become the motto of all agnostics and avowed idealists. p. 570

[169] Richet Charles—outstanding French psychologist. He was professor of the Medical Faculty at the University of Paris and president of the Paris Biological Society. p. 572

[170] Spengler—reactionary German philosopher-idealist, one of the ideologists of German fascism. p. 575

[171] Gestalt psychology—reactionary trend in contemporary bourgeois psychology According to the adherents of this trend, a psychical state constitutes an integral structure—a "gestalt," or "configuration." The latter cannot be decomposed into separate elements and is inaccessible to analysis, owing to which it cannot be made known. Koehler and Koffka, who head this trend, deny that behaviour consists of different reactions to these or other stimuli: The external situation and the reaction to it constitute a single structure which tends to a state of equilibrium. In this connection the adherents of the Gestalt theory reject the doctrine of the behaviourists, their theory of "trial and error," as well as the very principle of associationism (i.e., the formation of functional links between the sensations in the course of individual experience). The principles of the Gestalt theory, which affirms that mental activity is unknowable, and which, therefore, admits the existence of a particular, non-material and spiritual source, are applied by the adherents of this theory (for example, by Koffka) to all biological and even physical phenomena. They endeavour to prove that the latter represent definite structures, i.e , close integral processes, which cannot be decomposed into elements, since each part is fully determined by the whole to which it belongs.

In his statement Pavlov analysed the concepts of the Gestalt psychology and cited a book written by one of its adherents, the American Robert Woodworth, *Contemporary Schools of Psychology*, 1932; Pavlov subjected this idealist theory to annihilating criticism. p. 576

[172] The book by Kurt Koffka *The Growth of the Mind* published in 1924, is a translation from the 1921 German edition which appeared under the title *Die Grundlagen der psychischen Entwicklung*... p 584

[173] S. V. Kleshchev. p. 586

[174] Pavlov has in mind the treatise by the English philosopher John Locke, "Essay on Human Understanding," written in 1687. Locke denied the existence of inborn ideas and affirmed that all knowledge is acquired from experience However, according to Locke, true knowledge originates not only from sensations, but also from another source—from reflection, i.e., the synthesis of sensations p 594

[175] Mary Becker-Eddie, who founded a reactionary religious current in the United States, the so-called "Christian Science." p. 597

[176] Maria Kapitonovna Petrova—a prominent Soviet scientist, one of Pavlov's closest co-workers. p. 599

[177] W. Koehler's book *Psychologische Probleme* was published in Berlin in 1933. p. 602

[178] F. P. Mayorov. p. 603

[179] Henri Bergson—reactionary French philosopher-idealist According to Bergson's erroneous views, the methods of the natural sciences serve only as means of practical application, they do not elucidate the essence of phenomena; real knowledge of the world is effected through intuition. All the vital processes are governed by a vital torrent, or the so-called élan vital. There is a free indetermined connection between phenomena Bergson strongly opposed the natural science theories of evolution and regarded evolution as a phenomenon of a psychical nature p. 617

[180] Howard Warren—author of a number of reference books on psychology. In 1934 he published the "Dictionary of Psychology" mentioned by Pavlov. p. 621

[181] See note 83. p. 624

[182] Maria Kapitonovna Petrova. p 626

[183] V. K. Feodorov. p. 626

[184] Sherrington's book *The Brain and Its Mechanism.* p. 628

[185] Ezras Asratovich Asratyan—pupil and colleague of Pavlov, Corresponding Member of the U.S.S.R. Academy of Sciences, Member of the Academy of Sciences of the Armenian Soviet Socialist Republic. p. 629

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